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LECTURES

ON

THE PRINCIPLES AND PRACTICE

OF

PHYSIC;

DELIVERED AT KING'S COLLEGE, LONDON,

BY

THOMAS WATSON, M.D.,

FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS, PHYSICIAN TO THE MIDDLESEX HOSPITAL, AND FORMERLY FELLOW OF ST. JOHN'S COLLEGE, CAMBRIDGE.

PHILADELPHIA:

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ADVERTISEMENT.

THE following Lectures were put together, with unavoidable haste, during the Medical Session of 1836—37, in which they were first delivered. They were repeated, with slight variations, for four successive years; the Author always meditating, but never finding time to accomplish, their thorough reconstruction and revision. They were afterwards printed, to fulfil a rash promise, in the pages of the *Medical Gazette:* and they are now published, in a collected form, at the request, formally conveyed to him in writing, of many who had heard or read them, including several of his Colleagues at King's College.

Writing for mere beginners, and without any thought of future publication, the Author took no pains to note authorities as he went along. He may often therefore have used, without acknowledgment, not only the facts and reasonings, but sometimes, perhaps, the very words of others. This omission he regrets, but is now unable to supply. Neither has he leisure to correct, if that were desirable, the colloquial and familiar style in which the Lectures were originally composed.

Should they attract the notice of any who are no longer *in statu pupillari*, he would request such readers to bear in mind for whom these lessons were intended. They do not profess to present a formal and complete treatise on the Practice of Physic, much less to exhaust the various subjects upon which they touch. His chief hope is that they may prove useful as a text-book for Students.

As they were passing through the press, such additions and alterations have been introduced as the Author would have made, had he continued to deliver the Lectures orally.

Henrietta Street, Cavendish Square,
September, 1843.

NOTICE BY THE AMERICAN PUBLISHERS.

The high reputation which the Lectures of Professor Watson have acquired while passing through the columns of the Medical News, has induced the publishers to issue them in this collected form before their completion in that periodical. They are nearly all reprinted from the Author's revised copy; that portion which was taken from the columns of the Medical Gazette is suited to the Author's revision by the insertion of an Appendix at the end of the volume.

January, 1844.

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LECTURES

ON THE

PRINCIPLES AND PRACTICE OF PHYSIC,

DELIVERED AT KING'S COLLEGE, LONDON,

BY THOMAS WATSON, M.D.

PROFESSOR OF MEDICINE.

INTRODUCTORY LECTURE.

In approaching any new subject of inquiry, gentlemen, there are certain points concerning which the inquirer should always be careful to satisfy himself. He should comprehend, distinctly, what it is that he proposes to learn; its subject matter, and its objects; he should consider whether he is about to adopt the most easy, direct, and effectual means for obtaining his purpose; and whether he is qualified, by the possession of the requisite preliminary information, for pursuing his inquiries with intelligence and success. To these points, and to some others, as they are connected with the duties with which I have been entrusted in this College, I wish briefly to direct your attention on the present occasion. It will be my endeavour to furnish you, at the outset, with clear notions of the nature and the ends of that branch of study upon which you are now about to enter; to explain why it is taught, and in what degree it may be taught, by oral discourses; to point out to you what may reasonably be expected from me, and what, to render my attempts prosperous, will be required on your parts. Something also it is expedient that you should know respecting the general order and arrangement of the course; and a short explanatory comment upon some of the terms that we shall constantly be employing, will clear the way for the succeeding lectures, which, forming more strictly than the present a part of the series, will also be more strictly didactic in their character.

The subject of our study is that wonderful thing, the animal body—and more particularly the human body; its construction and qualities; its actions and its sufferings; its derangements; its decay.

In this most interesting, and surely awful study, you have already advanced a certain way. You know the outward form and fashion of the body: you have investigated its internal composition and structure: you have learned what is known of its various endowments; the uses and the actions of its several parts.

This amount of knowledge is indispensable to your further progress. It forms a portion only of what you desire to learn—or rather it is a preparation for the ends you are seeking. To the student of natural theology, indeed, it would, in itself, be sufficient and complete; and who can enjoy the privilege which each of you has enjoyed without becoming, even involuntarily, a student of natural theology? Who can look into the mechanism of this intricate but perfect work, and contemplate the evident marks of exquisite contrivance, of which it is full—the endless examples of means adjusted to ends, of prospective expedients, of compensation for inevitable disadvantages, of direct provisions for animal happiness and enjoyment—without the deepest conviction of the power, and wisdom, and benevolence of its Maker? But beyond, though not above, these higher objects

of a diligent examination of man's bodily fabric, we have another and still a noble end; and it is my business to take you one step nearer to that end. Hitherto you have heard of structure and of function. Henceforward our theme must be of health and of disease. Of health, that we may understand disease; of disease, that we may, under Providence, restore health. Our objects are to preserve the one;—to prevent, remove, or assuage the other.

What then do these contrasted terms denote?

Health we regard as a standard condition of the living body. But it is not easy to express that condition in a small number of words; nor is it necessary. My object is to be intelligible rather than scholastic: and I should probably puzzle myself as well as you if I were to attempt to lay down a strict and scientific definition of the term health. It is sufficient for our purpose to say that it implies freedom from pain and sickness; freedom also from all those changes in the structure of the body that endanger life, or impede the easy and effective exercise of the vital functions.

It is plain that health does not signify any fixed immutable condition of the body. The standard of health varies, in different persons, according to age, and sex, and original constitution; and in the same person, even from week to week, or from day to day, it may shift and vary within certain limits.

Health, again, does not necessarily imply the perfect integrity of all the bodily organs, and is not incompatible with great and permanent alterations, and even loss of parts that are not vital: as of an arm, a leg, or an eye.

If we can form and fix in our minds a clear conception of the state of health, we shall have no difficulty in comprehending what is meant by disease, which consists in some deviation from that state; some mode of action, or of being, or of feeling, different from those which are proper in health.

I use the word *disease* generically. Various terms in our language bear nearly the same meaning; and endeavours have been made to appropriate some of these more distinctively. Thus the word *disorder* has sometimes been applied to simple derangements of function, where no alteration of structure is seen, or can reasonably be inferred to exist; while the term *disease* has been restricted to maladies that run a definite course, or to such as are attended with appreciable change of texture. I see no great utility, but, on the contrary, some risk of confusion, in tying ourselves rigidly down to such distinctions. Indeed, we cannot always make them. During life it is often no easy thing to determine whether the parts, of which the functions are disturbed, preserve their integrity of structure or not: and even when the peccant organ is placed before our eyes after death, and the most careful scrutiny fails to discover in it any faultiness of texture, there may still be ground for suspecting that some material change, too subtle for detection by our senses, may have been wrought in its finer and more delicate organization. I shall take care to point out to you, as we go along, the cases in which we can trace organic change, and the cases in which we cannot; but, for the sake of simplicity, I shall call all deviations from the healthy standard, whether of function or of structure, by the generic term disease; and to avoid the perpetual and tiresome recurrence of the same word, I shall not scruple to employ the several terms disorder, complaint, malady, distemper, illness, as synonymous with the term disease.

The number of these deviations from the standard of health, (in other words, the whole number of *diseases*,) if we include all their differences in kind and in degree, is truly infinite; and the first thing requisite towards investigating the laws that govern their phenomena, is, that we should break them into groups, and dispose them according to some principle of order.

Now there are various methods in which this first broad classification of diseases might be framed.

The most cursory examination of the animal economy suffices to show that it is made up, not merely of separate parts, but of several distinct systems. There is one set of organs for the mechanical circulation of the blood; there is an apparatus expressly designed for exposing the blood repeatedly to the air; a system for regulating the movements and the feelings of the body; another system for receiving, preparing, and appropriating its nourishment; another for the elaboration of matters that are useful or essential to its

functions; another system for carrying off its impurities, and for removing its superfluous or effete materials; another for the continuance of the species.

Now each of these systems is liable to changes of structure and interruptions of function, peculiar to itself; and these peculiarities must be taken into account, whatever may be the order adopted in treating of diseases in detail. But I shall not divide the subject, as some have done, into diseases of the circulating system—diseases of the respiratory system—diseases of the nervous system—and so on: for this, among other reasons, that there are many forms of disorder that affect all these systems in common, or simultaneously, and comparatively few that are strictly confined to any one of them.

Neither, in the lectures which I am about to commence, shall I classify diseases according to the several *tissues* of which the animal frame is composed. In speaking of diseases in general, it will, indeed, be both proper and necessary to explain in what manner the same morbid process may be modified by the nature of the special tissue affected. But the entire body is more or less penetrated and pervaded by several of these tissues together, so that no useful or lucid arrangement of diseases could be founded on this basis.

Nor shall I attempt to construct a nosological system by grouping together certain sets of symptoms, and calling each set, in its collective form, a disease.

To say the truth, I shall consider convenience and usefulness, in framing my plan, rather than an appearance of scientific precision; and if I make one principle of arrangement more prominent than another, it will be that which relates to the anatomy of regions —the place and situation of organs. At the same time I shall not omit to borrow in part from some of those other plans to which I have just been referring.

Before, however, we treat of the nature of particular diseases, it will be requisite to give some general account of the different ways in which the various parts of the body are liable to be altered in structure, or disordered in function; and before we speak of the *signs* of particular diseases, it will be proper to take a general view of symptoms, and of their ascertained relations with the several forms of altered structure; for doubtless you are aware that, although diseases are not *constituted* by symptoms, they are, in the living body, *disclosed* by symptoms. Sometimes the symptoms are outward signals which alone reach our senses, and through which internal changes declare themselves, and we then have to decipher and interpret those signals. Sometimes we *see* the morbid changes themselves, on the surface of the body, or in parts within our view. Some internal changes we can appreciate as surely by the touch, or by the sense of hearing; and of some we infer the existence from alterations in the chemical or in the sensible qualities of the natural excretions.

After death diseases are often to be traced, by visible changes of structure in the internal parts of the body. These changes are extremely interesting, as illustrative of morbid processes: they throw light upon what is past; they afford some guidance for the time to come. But, for obvious reasons, those signs which reveal diseases during life are, practically, of chief moment. In truth, the great object of our art is to prevent the disclosure of the others. The instruction afforded by the dead body comes too late to be of use in that particular case.

I have already intimated that the morbid conditions from which the symptoms flow, are not always to be detected, either before or after dissolution. Neither, when they are detected, is their connection with the symptoms always evident.

Besides inquiring into the modes in which the various organs and textures of the body may be spoiled, and into the signals or symptoms by which the presence of disease may be ascertained, it will be expedient to premise something, in a general manner, of the *causes* of disease, both with a view to its cure, and to what is much better, its prevention. We shall also find it very useful to institute a short inquiry into the different ways in which death may take place—the different processes of dying.

There is one morbid condition or process, to which all parts of the body are liable, and which contributes so largely and so frequently to alterations both of texture and function, that it claims our especial attention when discussing the more general facts and doctrines of pathology; I allude to that change, or series of changes, which we comprehend under the term *inflammation*.

It will be necessary, therefore, in the preliminary part of the course, to give a general account of inflammation; and this account must chiefly be drawn from those of its pheno-

mena which are most familiar to us—which we can see and handle; those which we witness when disorder is seated in or near the surface, in the skin, in some of the mucous membranes, or in the subjacent cellular tissue. Then we shall pursue the examination of its peculiar phenomena as they are presented in the other tissues of the body—the mucous, serous, fibrous, parenchymatous, muscular, and nervous tissues; and here the *general* principles of treatment applicable to inflammation may be laid down, with the modifications required according to the tissues interested.

In this part of the course may also be conveniently discussed the modifications of inflammation, and of morbid conditions generally, by the influence of certain *diatheses*, or peculiar dispositions of the body. Some constitutional morbific tendencies we shall find to be innate or hereditary;—such are the scrofulous and the cancerous dispositions: others, again, are plainly acquired, as that in which the whole system is tainted for a longer or shorter period by the venereal poison.

Hæmorrhages, also, and serous accumulations, or *dropsies*, as they are liable to occur in all parts of the body, require to be treated of generally, before they pass under our notice in the list of particular maladies. There are certain facts and reasonings common to all inflammations, to all hæmorrhages, to all dropsies. By combining these "generalities" into one comprehensive statement, we help the memory, avoid needless repetitions, and find room for the exposition of principles.

Diseases themselves, in the mass, are sometimes distinguished according as they are *local*, or *general*.

Taking these epithets in their popular sense, we should say that local diseases are those which occupy a definite portion only of the body; general diseases those which pervade the whole body.

But let us endeavour to obtain clear notions upon these points.

Certainly there are many diseases which occupy a definite portion only of the body, leave all the remaining parts, and the system at large, healthy both in texture and in function. Such diseases we have no hesitation in calling local.

Again, there are many other diseases which occupy a definite portion only of the body, yet occasion a manifest and serious disturbance in the functions of various other parts, and (it may perhaps be said) of the whole system. Inflammation of a small portion of the frame may give rise to much secondary or symptomatic fever; but here also we properly speak of the disease as being local; the secondary general disorder results from the local and primary, follows it in point of time, and subsides upon its cessation.

But there are still other forms of disease which show themselves, not like inflammation now in this and now in that part, but in many or most parts of the body *at the same time*. I will take the complaint called purpura, characterized by the universal appearance of purple spots, as an example of what I mean. It is in truth a hæmorrhage affecting many or all the tissues of the body simultaneously. For this reason it is commonly regarded as a general disease.

But if we look somewhat closer into the matter, we shall, I think, perceive that most, if not all, those which have been thus reputed general, are, in fact, reducible to the class of local diseases. The fluids are as much parts of the body as the solids; and if it be true, as I believe it is, that the essential and primary change in purpura is a change in the blood, its characteristic phenomena will be apt to present themselves wherever there is blood circulating—that is, throughout the whole system. The disease is local inasmuch as its seat is in that particular fluid, the blood: it appears to be general, because the morbid blood is everywhere present.

The same observations apply to a large class of febrile contagious diseases; to that state of the general system which is sometimes called anæmia; also to certain spasmodic affections, where the seat of the actual disease is in the whole nervous system.

What are called general diseases, therefore, are those in which the whole of some one system that pervades the entire body happens to be similarly deranged. Whether diseases can ever be truly called general in any more strict or absolute sense than this, is much to be doubted.

I have mentioned dropsy as a malady which, like hæmorrhage or inflammation, may occur in various parts of the body separately. It may also extend at once to all parts capable of receiving and retaining serous effusions: *i. e.* besides filling the large serous cavities,

the effused fluid may occupy the universal cellular tissue. But even this apparently general dropsy will be found, upon careful investigation, to resolve itself, in most cases at least, into local disease within the thorax, or the abdomen.

The diseases which, in the sense now explained, may be called general, I shall arrange among the diseases of those parts of the system from which they have been ascertained, or may be presumed, to flow.

The first part then of the course will embrace an outline of general pathology, with an especial reference to those morbid conditions which fall to the care of the physician. In its relations to surgery, and to midwifery, pathology will be more particularly taught by the respective professors of those distinct though kindred departments of medicine. Do not however imagine that I take no interest in these; or that there can be anything different in the principles upon which the several branches of pathological knowledge are founded. The truth is, that you cannot, if you would, separate the one from the other. You can neither understand what may be called medical, without learning much which as strictly belongs to surgical pathology; nor can you be ignorant of either without being in many important respects deficient also in the other. But the open field of pathology is of wide extent, and though we may, and must, survey the whole, its artificial divisions, its enclosures and allotments, will be cultivated best, and most improved, by a division of labour.

Afterwards, separate diseases are to be described and considered: all such, at least, as admit of being individualized, or presented under a definite shape. And here, I repeat, I shall chiefly pursue an anatomical order, as being comprehensive, and inartificial, and as tending to facilitate diagnosis. The diseases of parts which lie near each other are the most liable to be confounded.

I shall begin therefore with the diseases of the parts that appertain to the head and spinal cord; and then proceed in succession to those of the parts belonging to the neck, the thorax, and the abdomen; to those of the joints, the muscles, and the skin. I shall not hesitate however to deviate from this order whenever, by doing so, I can promote your convenience or advantage.

With that portion of the course which relates to particular diseases, I shall also interweave certain pathological considerations, applicable not so much to the whole body as to the several great systems of which it is made up. Thus, when I come to the brain, I shall speak of the functions peculiar to the *nervous system*, and of the obstructions and disturbances to which those functions are obnoxious; by way of preface to a detailed examination of the various affections of the several parts of that system. Before discussing the diseases of the chest, I shall bring before you, in a general view, the manner in which the great functions of *respiration* and of *circulation* are liable to be impeded, or otherwise disordered. As preparatory to the consideration of the diseases of the abdomen, I shall treat, in the same way, of the functions of *nutrition;* and of *waste*, which implies an interruption of those functions.

Still there would remain certain diseases, which would not necessarily find a place in this arrangement, inasmuch as their seat is uncertain, or only guessed at. Ague is one of these. Cholera perhaps another. It is quite unimportant whereabouts in the course such maladies are considered. I feel no concern about any imputations of imperfect or clumsy arrangement with which the plan that I propose to adopt may appear chargeable. I had rather not be cramped and hampered by attempting what abler heads than mine have failed to achieve, and what, in truth, I believe to be impossible, a complete and methodical system of nosology. My object will be to furnish as much instruction and information as I can, in the way that seems most likely to be practically useful to you.

Ague I shall venture to include among the disorders of the nervous system: and with it, the important subject of malaria will necessarily engage much of our attention.

The great questions of infection and contagion I shall consider in connection with continued fever, which I rank among that remarkable class of diseases, the contagious exanthemata of Cullen.

Of sympathetic and of hectic fever, I must speak when upon the subject of inflammation.

This, then, is a sketch of the method I propose to follow. In the earlier lectures, with the general pathology, I shall endeavour to lay down principles. To these principles I

shall continually refer, as occasions offer, both in those prefatory remarks with which I purpose to introduce the diseases belonging to the several great systems that contribute to form the body; and also in what I shall subsequently have to say concerning those diseases themselves in detail. In this way I hope to combine the advantage of repetition, which was the peculiar advantage of two short courses in a season, with that of greater completeness, which forms the recommendation of a single extended course. The same great advantage of repetition—or I should rather say of recapitulation—will be farther aimed at in the stated examinations of the class.

Such being a summary account of the topics to be embraced in the ensuing series of lectures, and of the order in which I hope to pursue those topics, it seems proper that I should now say a few words in explanation of the scope and objects of the course. The prospectus informs you that it will comprehend the *Principles and Practice of Physic.* What is the true import and promise of those words?

By the *principles* of medicine are meant those general truths and general doctrines which have been ascertained and established, slowly indeed, and irregularly, but still with considerable precision, by the continued observation of attentive minds throughout the entire progress of medicine as a science. These principles I profess to teach you. The *practice* of medicine, or the particular application of those general facts and doctrines, I shall *describe* to you; but I cannot profess to *teach* it in this room: nor can you learn it, except in a very imperfect sense, from my description of it. It is the science that I shall here endeavour to unfold. Skill and facility in turning that science to useful purposes I am unable to impart. These are qualities that do not admit of being communicated from one mind to another. The practice of physic, like every other practical art, is to be learned by its repeated exercise; by habit; by carrying its various acts into direct effect again and again; or, if they happen to require no manual dexterity, by looking on, and seeing them done again and again. There is this capital difference, however, between the art of healing and some other arts: that the blunders of early attempts may be both grievous and irremediable—may chance to spoil the goodly and precious machine they are intended to repair. There is this also peculiar to our art—that it proceeds upon observations made at the very time when its exercise is wanted; and that it requires *skill in observing* as well as *skill in acting*. You will find what, perhaps, previously to positive trial, you might not suspect, that the senses—the eye, the ear, the touch—however sharp or delicate they may naturally be, require a special system of training and education before their evidence can be trusted in the investigation of disease. I do not know that these views are capable of being rendered plainer by illustration; for you must have observed a similar distinction between the science and the art in various other branches of human knowledge. The principles of navigation may be thoroughly comprehended by a person who scarcely knows a sail from a rope, and who would not be trusted, nay who would not trust himself, with the management of the simplest boat. A man may master the beautiful science of astronomy—may acquire the power of working with ease its sublimest and most abstruse problems—and yet remain in complete ignorance of the method of adjusting or using a telescope, and unable to ascertain for himself the position or movements of a single star. But place such a person night after night in an observatory—let him notice and imitate the proceedings of some one already skilled in examining the phenomena of the heavens—and he will soon acquire the requisite tact and facility himself. Just so it is with that branch of knowledge with which we are concerned. It is in the wards of a hospital, or in the domestic chamber—it is among the sick and the dying—and there alone—that you can either thoroughly or safely learn to practise physic.

In what, then, you may fairly ask, consists the value or the use of lectures on the practice of physic, if the practice of physic cannot be taught by lectures?

The main object of systematic lectures, explanatory of the principles, and descriptive of the practice of medicine, is to prepare the hearer for observing to the best advantage the actual phenomena of disease, and the power of remedies over it. They are intended to fit him for seeing with intelligence—to enable him to read, and understand, and interpret, the book of nature when it is laid open before him—in short, to qualify him for clinical study. One man shall travel into a foreign land, knowing nothing beforehand of its scenery or its climate, of its natural productions, its manufactures, or its works of art, and ignorant

alike of the manners, customs, history, laws, and language of its inhabitants; another shall visit them after having furnished his mind with information on these subjects by reading, and by conversing with men who have already passed over the same ground. Supposing the visit to be limited in each case to a certain, but not long period of time, and I need not ask your opinion as to which of these travellers will reap the greatest crop of enjoyment and of profitable knowledge from his journey. Not less striking is the difference, in point of instruction and of interest, perceived by different students, upon their first admission to the bedsides of the sick, according as they have been well or ill prepared for the multiform spectacle of bodily suffering then disclosed to them. There are persons, indeed, who seriously, and I make no doubt in perfect good faith, warn the student against bringing to the contemplation of disease any preconceived opinions; who tell him that he must come with a free and unprejudiced mind, and see, and note, and judge of all things for himself. I also would have him exercise, and ultimately abide by, his own judgment; but surely if every man were to depend upon his own unassisted observation for his knowledge of disease, every man would be marvellously ignorant, and the *science* of medicine would stand still, or cease to be. "If no use be made (says Dr. Samuel Johnson) of the labours of past ages, the world must remain always in the infancy of knowledge." In truth, a person who, without any previous information concerning diseases, should betake himself to the wards of a hospital with the design of impartially and resolutely investigating their phenomena, such a person, however clear and strong his intellect might be, would find himself, for a long time, more puzzled than instructed by what he saw around him. He would be perplexed by the shifting and seemingly contradictory characters presented by the same malady in different patients; or in the same patient at different times; and not less so by the outward resemblance of disorders essentially unlike. He could not but be confused by the multitude of symptoms that crowded upon his attention on every side; and at a loss to distinguish important facts from those which were trivial, or useless, for the chief ends of his pursuit.

The business, therefore, of a lecturer upon the *Principles and Practice of Medicine*, or, as it is sometimes worded, *the Nature and Treatment of Diseases*, is first to fix upon some order in which to treat of the various subjects comprised in his course. The simpler and less artificial his arrangement, the better. The chief use of this classification is to facilitate the recollection of particular facts; and I have already told you that if I can distribute the multifarious forms of disease in such a manner as that they shall appear plain to your understanding, and take a secure hold upon your memory, I shall not trouble myself or you with a vain search after that phantom—a perfect methodical nosology. "In all such classifications," writes Lord Brougham, "we should be guided by views of convenience rather than by any desire to attain perfect symmetry; and that arrangement may be best suited to a particular purpose which plants the same things in one order, and separates them and unites them in one way, when an arrangement which should dispose those things differently might be preferable, if we had another purpose to serve."

Having settled this framework of his discourses, the next aim of the lecturer must be to collect and arrange from the voluminous and bewildering records of medicine, and from the necessarily more slender stores of his personal experience, whatever it may seem of consequence that his hearers should know concerning each distinct form of disease as it comes before them for consideration: to state all the facts which are well ascertained, and which tend to explain its symptoms, to elucidate its origin, to identify its nature, to direct its treatment, to accomplish its prevention: to sift the true facts from the false, the important from the trivial, the essential from the accidental: to analyze the relations of these facts, and ascending from particulars to generals, to point out those great principles and precepts which constitute the keys, both to the knowledge and to the management of all diseases of the same kind. It may even sometimes be his duty to notice and discuss mere theoretical opinions; to express his own sentiments upon disputed and undecided questions; and to admonish his audience against the danger of being led away by ingenious refinements, by the speciousness of novelty, or the boldness of speculation, from the more secure and settled results of careful observation improved by patient thought.

These duties of a lecturer on medicine are metaphorically, but aptly, expressed in the following passage from Lord Bacon:—

"Formica colligit, et utitur, ut faciunt empirici; aranea ex se fila educit, neque a parti-

cularibus materiam petit, ita faciunt medici speculativi ac mere sophistici; apis denique cæteris se melius gerit. Hæc indigesta e floribus mella colligit, deinde in viscerum cellulis concocta maturat, iisdem tamdiu insudat, donec ad integram perfectionem perduxerit."

I may venture to paraphrase it thus:—

The lecturer must not be the ant, collecting all things indiscriminately from all quarters, and using them as provender for his discourses;

Nor the spider, seeking no materials abroad, but spinning his web of speculative doctrine from within himself;

But rather the bee, extracting crude honey from various flowers, storing it up in the recesses of his brain, and subjecting it to the operation of his internal faculties, until it is fit to be produced, digested, and ready for use.

Such, gentlemen, are the main objects which I shall endeavour to keep steadily in view during the series of lectures I am about to commence; and I should ill deserve the chair I have the honour to occupy, if I did not feel the great responsibility under which I speak to you. The subjects with which we have to deal are not matters of mere speculative curiosity or intellectual amusement—to be taken up to-day and dismissed with unconcern to-morrow—but they relate to questions of life and death. The opinions you are now to form or to embrace, are for the most part the opinions upon which in after life you will confidently and constantly be acting. The comfort or the misery of many families may probably hang upon the notions that each of you will carry from this place. Therefore it is that I feel myself to be engaged in a very serious undertaking. Doctrines and maxims, good or bad, flow abroad from a public teacher as from a fountain, and his lessons may become the indirect source of incalculable evil and suffering to hundreds who have never even heard his name. These reflections have impressed upon my mind an almost painful sense of the obligation imposed on me, by my present office, of closely sifting the facts, and of carefully examining the principles to be derived from those facts, which I propose to employ for your instruction and guidance.

But amid all the responsibities, gentlemen, both of teacher and of learner, the profession which you and I have chosen, or which circumstances have prescribed to us, is one with which we may be well satisfied. It has its own peculiar privileges and advantages, and if rightly pursued may prove to each of us a lasting source of mental and moral profit. I have already reminded you how it brings under our minute and daily notice that most remarkable portion of matter which is destined for a season to be the tabernacle of the human spirit, and which, apart from that singularly interesting thought, is replete in itself with the materials of wonder and admiration. Our daily vocation tends also, by the constant exhibition of human pain and weakness, to awaken the best emotions of our nature, to foster the benevolent affections, and to promote the charities of social life. It affords us continual opportunities (and woe to him who shall wantonly or selfishly neglect or abuse them) of showing kindness to our afflicted fellow creatures, of manifesting love towards our neighbour. It is free, moreover, or it may and should be free, from party turmoil and sectarian jealousy. Pain and danger are the only passports necessary to its good offices. It regards no political differences; and the poor no less than the rich are embraced in its ministrations. Hallowed, as it ought to be, by religious motives, and by the sense of duties fulfilled, it offers its peculiar benefits and blessings without stint or scruple, to men of every religion, and to men of no religion at all. It conveys to ourselves the most salutary lessons; teaching us in how many instances, forming a large majority indeed of the whole, bodily pain and anguish proceed directly or remotely from evil courses; the sins of our fathers, our own unbridled passions, the malevolent spirit of others. It shows us too the uses of such sufferings, which are mercifully designed to recall men from the strong allurements of vice, and the slumber of temporal prosperity: teaching that it is good for us to be sometimes afflicted. It is full at the same time of the most solemn warnings, and speaking to us day by day of death, it reminds us that our time also is short and uncertain, and but scantily proportioned to our moral wants and intellectual aspirations. Followed up in the true and Christian spirit of gratitude towards God, and love towards man, this noble profession is second only in usefulness and dignity to that of the servants of the church, and like it, when faithfully administered, it will assuredly become its own abundant reward.

LECTURE II.

PATHOLOGY—MEANING OF THE TERM. PATHOLOGY, GENERAL AND SPECIAL. MORBID ALTERATIONS OF THE SOLID PARTS OF THE BODY. ALTERATIONS IN BULK. HYPERTROPHY,—LAW OF ITS PRODUCTION—ITS EFFECTS. ATROPHY—ITS CAUSES AND CONSEQUENCES. CHANGES IN FORM. ALTERATIONS IN CONSISTENCE. INDURATION —ITS VARIOUS KINDS.

I PROPOSE to devote several lectures, in the commencement of the course, to pathology, as it relates to medicine.

And I must first of all explain to you what I mean by the word pathology.

Many persons speak of pathology as if it were the same thing with morbid anatomy. That is not the sense in which I purpose to use the term. Pathology *is* morbid anatomy, but it is something more.

A knowledge of pathology (in the full and proper acceptation of the word) implies indeed a knowledge of altered structures and of diseased conditions;—but it implies also an explanation of these—a knowledge of what precedes them, and a knowledge of what results from them.

It comprehends therefore the following particulars:—1. A knowledge of the material changes to which the several parts of the living body are subject: 2. A knowledge of the processes or actions by which these changes may be wrought: 3. A knowledge of the causes which set these processes on foot: and 4. A knowledge of the consequences of the same changes, or of the symptoms they occasion.

On some of these points our actual knowledge is still scanty and imperfect. Yet a good deal of valuable information concerning each of them has been collected: and this I shall endeavour to place before you as distinctly, and at the same time in as small a compass, as I can.

Pathology is general, or special. General pathology treats of the morbid conditions which are common to the whole system, or to the several tissues that compose the system. Special pathology contemplates particular diseases. An acquaintance with general pathology prepares us for, and conducts to, that which is special; and when I say that the earlier lectures of the course will be given to a consideration of the facts and doctrines of pathology, you will of course understand me to speak of *general* pathology.

And I shall begin by inquiring what are the changes to which the component parts of the living frame are liable.

There are various ways, capable of intelligible description, in which the different parts of the body may be altered by disease.

The solid parts may be altered in *bulk;* in *form;* in *consistence;* in *their intimate texture*, i. e. in the qualities and arrangement of their component particles; and in *situation*.

The fluid parts may also be altered in *quantity;* in *quality;* and in *place*.

And many of these alterations may exist in combination with each other.

Let us first consider the solids.

They may be simply altered in bulk without any change of texture; and that in two ways. They may become larger than natural, or smaller than natural. In the one case the change is called *hypertrophy*, in the other *atrophy*.

Hypertrophy.—We find the best illustrations of hypertrophy in the muscular system.

The huge muscles visibly prominent in the arm of a blacksmith or pugilist, and in the leg of an opera dancer, afford familiar examples of it. In these cases the increased bulk, although it may be unsightly, as being out of proportion to other parts, is not disease, and does not interfere with the most perfect health. By constant exercise the muscles acquire preternatural volume, and weight, and power. It seems to be a law which prevails extensively in the animal economy, that increase of function should lead to augmentation of bulk. The function of the muscular system is contraction, and more frequent and energetic contraction begats an addition of substance. But the same principle obtains in various other parts and tissues. It is especially noticeable in some of the organs which are double. If one kidney wastes, or is spoiled by disease, an increase of function is thrown upon the other, and by a beautiful law of compensation, the sound organ, without any alteration of its peculiar fabric, enlarges. The same is observed to be the case with the lungs. The law resembles, somewhat, one that is familiar to political economists, and is expressed by them in the maxim—that the supply of a marketable commodity is regulated by the demand for it. If, in respect to a muscle, increase of force be habitually needed, the necessity generates the requisite addition of bulk, which implies an augmentation of force. One kidney becoming inefficient, it is necessary the other should secrete a larger quantity of urine; and this faculty is obtained by the enlargement of the secreting organ.

I say this law is of extensive operation in the living body: but it is not universal. It does not hold, for instance, in respect to the organs of the special senses. One eye does not become hypertrophic when the other is blind; nor one ear grow larger or longer because the other is deaf. And we see at once why the law in question does not apply in such cases. These organs differ from such as I mentioned before—from muscular and glandular parts—in this; that increase of their size would not augment or facilitate the function or purpose they are designed to serve. A muscular arm will strike a harder blow, or lift a heavier weight, in proportion to the bulk of its muscles: but we should gain nothing in distance or distinctness of vision by the enlargement of an eye; nor should we hear more acutely or more clearly if our ears were of twice the ordinary magnitude.

Hypertrophy of this unmixed kind, unattended by any change of texture—(and it is to this that the term should as much as possible be restricted)—is believed to depend upon more active nutrition of the part, (υπερ τροφη). More materials are laid down in the part by the blood, and assimilated, than are received back from the part into the blood to be taken out of the body. The nutritive process preponderates over the reabsorbent. That hypertrophy does thus result from an excess in the process by which parts are nourished and built up, and not from a defect in the process by which they are decomposed and removed, is rendered probable by the fact that an increased quantity of nutrient blood is sent to the hypertrophied part; its arteries grow larger: this we perceive by comparing these vessels with others where no accession of bulk has occurred. This opinion is further strengthened by the converse effect produced upon an hypertrophied part (the thyroid gland, for instance) by tying its principal nutrient artery. The magnitude of the bronchocele diminishes. It is curious that no such alteration of size has been noticed in the nerves supplying the hypertrophied parts.

Now these examples of hypertrophy clearly have not the nature of disease. But hypertrophy is often plainly connected with disease, while still it is not itself a morbid process. Thus we have it in the hollow contractile organs, the office of which is to propel fluids:—in the heart when the progress of the blood suffers some mechanical obstacle; in the bladder when the urine, and in the intestinal canal when its contents, are somehow hindered in their natural course; or when, from some undue stimulus or irritation, these parts respectively are urged for a long time together to excessive, or too frequent, action. I show you preserved specimens of each of these changes. You will find that muscular structure sometimes becomes apparent, under the influence of disease, where very slight traces of it, or none at all, were visible before. You may sometimes observe this in the air tubes, the trachea and bronchi, when the respiratory functions have been long embarrassed; and in the gall bladder, when the exit of the bile has been chronically impeded. And it is worth remarking that this new, or greatly exaggerated appearance of muscular tissue, which is the product of disease in the human body, is a part of the natural and healthy structure in some of the inferior animals.

The several instances of hypertrophy that I have now been mentioning, if they are to be looked upon as morbid, are morbid in a particular and limited sense—morbid, merely as being associated with disease, and not so either in their own processes or in their tendencies. Many indeed of the writers who notice them, speak of the hypertrophy as constituting a source of disease, and a cause of danger to the patient. But I shall have occasion to show you hereafter that in most cases it is really a compensatory change, and conservative of life;—a resource of nature by which impending danger is postponed, and existence prolonged.

It may be said of hypertrophy, that it has to do with disease or not, very much according to its seat. As regards the muscular system—in the voluntary muscles it is generally innocent, in the involuntary it is generally connected with disease, sometimes as a cause, much oftener as a consequence, sometimes as both cause and consequence. One way in which hypertrophy may manifestly be a cause of disease is by the pressure of an enlarged organ upon the parts in its neighbourhood, and a consequent interference with the functions or the sensations of those parts.

I am not sure, whether, to those among you who are beginners, I make myself understood. An example or two will render what I mean obvious.

It often happens that the aortic orifice of the left ventricle of the heart becomes narrow and constricted, in consequence of disease in the semilunar valves there situate. Under these circumstances, more forcible contractions of that ventricle are required for the due propulsion of the obstructed blood, and the walls of the ventricle become, accordingly, thicker and stronger. Here the hypertrophy of the left chamber is evidently a consequence or *effect* of the disease that previously existed at its outlet.

On the other hand, when the thyroid gland is enlarged, it sometimes presses so much on the parts that lie behind it, as to impede the breathing, or the swallowing. In this case the hypertrophy is the *cause* of consecutive disease.

Hypertrophy is exceedingly common in other tissues as well as in the muscular. Of its affecting the glandular system we have good examples in what I have just mentioned, the true bronchocele—in certain forms of enlarged prostate—in the thymus gland not unfrequently. Hypertrophy of the adipous tissue displays itself in partial accumulations of fat—or in that general and extreme obesity which may amount to disease, and which nosologists have called *polysarcia*. Of a state of the brain which is considered to constitute hypertrophy, I shall speak more particularly when we come to the morbid conditions of that organ. Hypertrophy is also said (I am not certain with how much propriety always) to occur in the cutaneous, mucous, and vascular systems, in the bronchial, mesenteric, and mammary glands, in the liver, spleen, and pancreas. I suspect that the enlargements of these parts to which the term hypertrophy has been sometimes applied, most frequently combine some alteration of texture with the increase of size, and therefore are not strict examples of hypertrophy.

It is of importance that you should be aware that hypertrophy of one or more of the component tissues of an organ may exist, while the others either remain unaltered, or are changed in some other way. It frequently happens that when one component part is thus over nourished, it is so at the expense (as it would seem) of another which becomes atrophied. There are parts of the heart upon which a certain quantity of fat is usually deposited. It is not uncommon to meet with this in excess, and at the same time to find the muscular texture of that organ pale, flabby, soft, and wasted. What has been deemed hypertrophy of the female breast consists, almost always I believe, in excessive development of its adipous tissue, without any enlargement of the gland itself—or even with its diminution.

In the majority of cases the size of an hypertrophied organ is augmented; it has a larger superficies than is natural: and, therefore, I have introduced hypertrophy to your notice among the alterations to which parts are liable in *bulk*.

But it is not always so. There may be hypertrophy of an organ without enlargement—in at least three different ways:—

1st. In hollow organs, where the additional substance is deposited centrally, and the hypertrophy takes place at the expense of the cavity:

2dly. In any organ, whereof the hypertrophy is confined to one or more tissues, while the others are proportionably wasted: and,

3dly. Hypertrophy may even be consistent with no alteration of shape, or increase of bulk in any direction, the organ occupying exactly the same space, and preserving the same absolute dimensions as before, but becoming more full of component particles, more compact, heavier. This state is well exemplified in certain cases of hypertrophy of bone: the spongy or cancellous texture of the bone disappears—its specific gravity is increased—it becomes hard, firm, and like ivory. The structure appears, to the eye, to be changed, yet remains the same, except in respect of its density.

I have told you that hypertrophy is usually a conservative and salutary change. We shall meet with many illustrations of this as we proceed. But I may take the present occasion of pointing out to you some of the beneficial tendencies of this change when it takes place in bone. For, since the diseases of the bones do not belong to my province, I shall have no other opportunity.

You probably know that in the disorder called rickets, occurring principally in childhood, the bones are soft, and deficient in their more solid ingredient; so that they bend under the weight of the body or the contraction of the muscles attached to them: after a certain period this disproportion in the constituent particles of the osseous tissue ceases; but the bones are permanently distorted, and, therefore, less adapted to their office, and less strong, than if they had remained straight. Now the natural remedy that ensues is very striking and beautiful. The bent bones become *hypertrophied* in certain places; they grow thicker, denser, harder, and, consequently, stronger, at the very concave part where the stress of the pressure is the greatest.

The following experiment showed the same thing in a somewhat different manner. An inch of the middle part of the fibula of a quadruped was cut out. A long time afterwards the animal was killed. The tibia was then found to have become considerably thickened exactly in that part of it which corresponded to the defect in the fibula.*

The same principle appears still more conspicuously in a case of disease related by Cruveilhier. He saw in the hospital at Limoges a young man who had lost (from necrosis with suppuration) the middle third of his tibia; of the larger of the two bones of the leg. The lost bone had not been reproduced, but the fibula, the naturally slender bone, had become thick and strong enough to support the whole weight of his body.

I was explaining to you that hypertrophy may exist, without enlargement. On the other hand there may be enlargement, without any change of structure, and yet no hypertrophy. The liver and spleen are apt to acquire a considerable increase of bulk from mere congestion and distension of their vessels by blood. An immense spleen will shrink into its proper size in a few hours, after hæmorrhage from the stomach, whereby the gorged venous system of the abdomen has been relieved. Dr. Townsend mentions a remarkable example of the same kind respecting the liver. The inferior cava had been compressed by an aneurismal tumour, so that the passage of blood from the liver was greatly impeded. Under these circumstances the liver became so large as nearly to reach the crest of the ilium. Suddenly the aneurism burst, the pressure was taken from the cava, the hepatic veins were allowed to empty themselves, and before the body was opened for inspection, the liver had nearly resumed its natural situation and dimensions.

Of the causes of hypertrophy little more is known than I have already told you. The most important circumstance for you to remember is that increase of function produces increase of nutrition. This is nearly a general fact; but whether the converse proposition be as generally true—whether hypertrophy of a part always denote increased activity in its function—is much less certain. If that were ascertained, we might hope to discover the actual office of certain parts of the body, the uses of which we do not yet understand (of the thyroid gland, for example), by investigating the circumstances under which they become subject to hypertrophy. In Mr. Mayo's *Outlines of Human Pathology*, a case is related of hypertrophy of the tongue, in a young child, treated by Mr. Hodgson, of Birmingham. It would seem to be impossible to account for this by any increased energy in the known functions of that member.

A few isolated facts, bearing upon some points connected with this inquiry, have been made out.

* Mr. Stanley's Lectures, Coll. Surg.

In the first place, certain localities appear to be influential in the production of certain forms of hypertrophy. Thus bronchocele is very frequent among the inhabitants of certain districts—especially in close or marshy valleys at the feet of high mountains. Its real cause is to be sought in some condition, hitherto undetermined, of the air of those places, or of the water, or of both.

2dly. Certain congenital or acquired conditions of the body tend to produce local hypertrophy. In that peculiar diathesis which we call the strumous—and of which I shall have much to say hereafter—certain parts of the body, as the upper lip, and the extremities of the long bones, undergo a kind and degree of enlargement that seems properly to fall within the definition of hypertrophy.

3dly. Certain habits of life have a distinct effect in promoting certain forms of hypertrophy. Full diet, with bodily inactivity, leads to hypertrophy of the adipous tissue. So general is this tendency, that we confidently act upon it in the fattening of animals. Shut a healthy pig up in a small sty, and give him as much food as he is willing to eat, and you insure his rapid pinguescence. If you cannot so certainly attain the same result by similar means in the human animal, it is chiefly, I believe, because moral causes, and especially mental anxiety, will effectually counteract those means. A healthy man, with a quiet mind, using habitually a full nutritious diet, and leading a sedentary life, will fatten, I apprehend, as unfailingly as a calf, or a turkey. Sometimes, indeed, fat accumulates, to an enormous extent, in spite of abstinent habits, and very active exercise.

4thly. It is a curious fact that the removal of certain parts of the body, as the testicles in male animals, and the ovaries in females, increases the disposition to accumulate fat. The same tendency appears to be given, for a time, by the extirpation of the spleen.

Of the curative methods that hypertrophy may require it would be premature to speak at present.

The *bulk* of parts may also be *augmented* in various other ways. The hollow organs may be inordinately distended by an undue accumulation of their natural contents,—or by matters that do not enter them in health:—the solid organs may have their size increased by the presence of matter foreign to their natural composition, collected in their interior, or distributed through the interstices of their proper tissues, or deposited upon their surface: and in each case the functions of the part itself may be disturbed or suspended; or the functions of parts immediately contiguous to it may sustain damage from its pressure; or the functions of distant parts connected with it by dependency of office may be disordered; or all these consequences may exist together. Numerous examples of them all will hereafter be brought under your notice.

Atrophy.—Let us next attend to that condition which is the opposite of hypertrophy—to *atrophy*, namely, in which parts become notably smaller than natural, without other alteration of texture.

The two conditions contrast strongly with each other in their nature and origin, as well as in their physical character.

Hypertrophy depends essentially upon an increase—atrophy upon a diminution or defect, of the nutritive functions. You will find that atrophy plays an important part in altering the bodily organs, both in health and in disease.

Of the effect of atrophy in causing alterations consistent with health, I shall merely remind you of some instances, that you may the better comprehend its morbid operation.

There are parts of the body, as you well know, destined for a temporary purpose only. Upon the cessation of their especial function they dwindle, or disappear. We have examples of this in the thymus gland, in the supra-renal capsules, and in those parts of the mechanism of the circulation that are peculiar to the fœtal state. The atrophy here begins as soon as the child is born, and not only is consistent with, but is necessary to, its perfect health. As life advances, we see the same principle at work, remodelling from time to time those structures of which the office has only a limited duration. After the child-bearing period in women is over, when the functions of the ovaries expire, these organs shrink, through atrophy. It is so with the testes of old men. Indeed atrophy, to a certain extent, pervades all parts of the system in old age: the muscles diminish in size, the whole body is less plump, the bones lose a portion of their substance, and become brittle.

Even in the period of fœtal life this process, by which parts are starved and stunted,

sometimes displays itself. But here it is no longer compatible with the integrity and well-being of the system. The arrest or retardation of the nutritive function produces changes of great interest, and gives rise to various kinds of monstrosity. Harelip—fissure of the palate—certain malformations of the heart—are familiar examples of the consequences of intra-uterine atrophy.

Atrophy, considered as a morbid process, is conspicuous, no less than hypertrophy, in the muscular system. We see it in the voluntary muscles, whenever a limb remains long in a state of inaction—whether from palsy dependent upon disease in the brain or spinal cord; or from pain connected with disease of a joint; or from perversion of the will, as in the self-inflicted penance of the Fakir. The same law, therefore, obtains here, which was previously announced; the development of a part is proportioned to the activity of its function. In most cases I believe the atrophy will be found to resolve itself into a deficient supply of healthy arterial blood. Mere inaction will produce it; but it is probable that the inaction oper ites simply by diminishing the flow of arterial blood to the muscle. It has, indeed, been supposed that in paralysed limbs the altered condition of the nervous influence has also some share in determining the atrophy. The alleged facts on which this supposition is founded are of this kind. In the first place, when the palsy is imperfect, the diminution of bulk in the muscles is not always (they say) in proportion to the degree in which their power of action is impaired. But it is not very easy to measure and compare, in different cases, either the amount of action remaining to the muscles, or the degree in which they waste. Secondly, it has been remarked that when a limb is deprived of the power of motion by some injury that destroys or interrupts the functions of its principal nerve, atrophy takes place to a greater degree, and more rapidly, than when the palsy results from disease of the brain. But any local injury which affects the functions of the nerve will be likely to impair the functions of other parts of the limb also, and among the rest, those of the arteries. I therefore look upon the proposition, that what is called a "*change in the innervation*" of a part tends to occasion its atrophy, as at least a questionable proposition: and even if its truth could be established, the change of innervation would be most likely to operate by somehow reducing the supply of healthy arterial blood. The nerves belonging to palsied and atrophied muscles are said not to diminish in size. It is with the arterial circulation, certainly, that atrophy is most concerned. It is upon a diminution of the number of the smaller, and, perhaps, also of the capacity of the larger arteries, that senile atrophy depends. We find atrophy of the brain accompanying certain diseased conditions of its main arteries. So the testicle withers when the spermatic artery is tied for the cure of varicocele.

Pressure of any kind, exercised either upon the larger arterial trunks, or upon the capillary vessels, so as to lessen without completely preventing the supply of blood, will be found to give rise to atrophy, whenever the due quantity of blood is not furnished by the establishment of a collateral circulation. *Chronic inflammation* is sometimes attended by the wasting of the part which it occupies. It acts, in all probability, by unfitting the capillary arteries for transmitting the requisite quantity of blood. Various diseases, by which the supply of nutriment to all parts of the body is checked at its source in the digestive organs, or by which some unnatural drain upon the system is kept up—by which, in short, the quantity of the nutrient fluid is diminished, or its quality impaired—produce a greater or less degree of *general* atrophy: but to this universal wasting we usually apply the term *emaciation*.

Atrophy, then, such at least as is morbid in its nature, may be the consequence of inaction, of compression, of chronic inflammation, and of various diseases: but in all cases the defect of nutrition which causes the atrophy seems to be resolvable into a diminished supply of healthy blood through the arteries.

As in hypertrophy, so likewise in atrophy, the change may be limited to some one or more of the component tissues of a part:—and from this altered proportion of its constituent tissues the appearance of the part may be remarkably modified.

So also, as hypertrophy may exist without any increase of absolute size, atrophy may occur without any diminution: as in the heart, when the cavities are dilated in the exact degree in which their walls become thinner. Bones, externally sound in appearance, have had their specific gravity so greatly reduced by internal atrophy, that they would float upon water like a cork.

It is a curious fact—which I mentioned in other terms before—that an atrophied part is sometimes plentifully encompassed by fat. But this is by no means a necessary accompaniment. Why it happens in one case, and not in another—whether the adipous hypertrophy is ever the cause of the atrophy associated with it, or the atrophy the cause of the hypertrophy:—these are questions which, in the present state of the science of medicine, do not admit of any certain solution.

It is scarcely necessary to observe that the changes of *bulk* which we have been considering, imply often, though not always, changes of *form* also. You will have one or two of the chambers of the heart greatly enlarged, while the others remain of their natural size. Of course this altered proportion modifies the shape of the organ.

Signal changes of form are produced also by inflammation, by pressure, and in various other ways. But after all, modifications of figure are rather to be considered as *accidents* of disease than among its important *elements:* and I pass on to other alterations.

Induration.—Various parts of the body are liable to be changed in *consistence*. They may become harder and firmer than before: or they may become softer. To the state of increased or unnatural hardness the term *induration* has been applied: the same word is used also to express the process of hardening. To the state of diminished consistence we give the name of *softening*. The French pathologists, who first noticed this condition as an element of disease, call it *ramollissement*.

You are already aware—those of you who have attended the lectures of the Professors of midwifery and of anatomy—that a slow process of natural and healthy induration is going on throughout the body from the earliest period of uterine life to extreme old age.

There are several ways in which *un*natural induration may take place.

Induration of an organ may happen, without any other alteration of its proper tissue, in consequence of inordinate fulness of its blood-vessels. This is apt to occur in the lungs, or liver, whenever the free exit of blood from these organs is in any way impeded. They become stretched, tense, resisting, hard.

In like manner induration of the hollow organs, or of cellular parts, will arise (without any change of their texture) from an undue accumulation of fluids within them:—of bile, for example, in the gall-bladder—of urine, in its receptacle—of gases in the stomach and intestines—of serosity in the cellular tissue.

In either of these kinds of induration the unnatural hardness may be temporary only—or it may be the permanent accompaniment of other disease. It is necessary that you should be aware of its occurrence, and of its nature. I say of its *nature*, because this is not always understood. In the induration arising from the last circumstance I mentioned—viz., from infiltration of the cellular tissue with the serous or albuminous parts of the blood—from *œdema*, in short—the hardness has sometimes been erroneously ascribed to some other morbid condition. Dr. Carswell has shown that in the curious disease of new-born children who are said to be *skin-bound*, the hardness of the surface is the consequence of simple œdema of the subcutaneous cellular tissue. The same phenomenon is remarkable in œdema of the tongue. I believe the induration belonging to œdema will be found to be the greater in proportion as the effusion is recent, and has taken place rapidly.

Again, induration may accompany, and be a consequence of, simple hypertrophy. Of this I have already shown you examples: especially in the eburnation (as it has been called) of hypertrophied bone.

Induration of an organ may also result from the *ex*pression of its fluid, and the *com*pression of its solid parts. We see this extremely well in the lung, when it has been thrust and flattened against the vertebral column by fluid effused into the pleura—or when it is still more tightly bound down by an investing layer of plastic lymph. In this way, therefore, induration may be consistent with atrophy. That the natural structure of the hardened lung is not always lost in these cases we know, because we can restore, to a certain extent at least, its bulk and spongy feel, by forcibly inflating it. The spleen sometimes exhibits the same kind of induration, under the constricting force of an investing false membrane. I am mentioning samples only of these changes.

More frequently induration depends upon the presence, in the internal texture of parts, in the little spaces left between their component tissues, of fluid or solid matters which

are not found there in the healthy state. Bony or earthy particles are sometimes laid down, and the part thus changed is said to be ossified. There are few parts of the body in which this kind of induration does not occasionally take place. It is especially common in the coats of arteries, and in the subserous tissues. Blood, or fluids separated from the blood, may fill and obliterate the natural interstices, and concreting, tend to solidify and harden the part which they occupy. What is called *hepatisation of the lung* is a good instance. I need not tell you that the healthy lung is spongy and crepitant under pressure: in this altered state it no longer crackles between the fingers—its spongy character is lost—it resembles liver in its compactness and colour, and it is therefore said to be "hepatised." This is a consequence of inflammation; and induration of this kind is a very common consequence (as we shall see) of the same morbid process in various other parts and organs. Another instance of induration of the pulmonary substance we have in what is badly called pulmonary apoplexy. This is independent of inflammation. Blood is collected and coagulates in a part of the lung which should contain air—in the vesicles of one or more of its lobules: the lobules thus gorged with blood become even harder and firmer than when hepatized—but by a different process.

In the instances last mentioned, fluids, after escaping from their proper vessels—*i. e.* in technical phrase, after being *extravasated*, pass into the solid form, and thereby render the parts which they pervade harder and more firm. But fluids may concrete and harden *within* their proper vessels, and so lead to another form of induration. Thus the blood, under certain circumstances, coagulates in the living veins—nay, sometimes even in the heart itself: and we may hereafter have to consider the conditions under which this coagulation is liable to occur, and the serious consequences that it involves. The bile again, as you probably know, sometimes concretes, by a rude kind of crystallisation, into what are called gall *stones:* and the passage of these calculi through the narrow ducts that connect the gall-bladder with the bowel is apt to be attended with pain the most intense. The formation of *urinary* calculi is not exactly of the same kind.

Numerous specimens of all the changes I have been describing are upon the table before you. You may examine them at leisure after lecture, or in the museum.

I have yet to notice another source of unnatural induration, in the deposition or growth of unorganised masses of matter within the body, differing remarkably from any of the solids or fluids that enter into its healthy composition. These unnatural formations vary considerably in their nature and appearance, and in their consistence at different periods. Sometimes they exist in distinct and separate masses, and whether hard or soft in themselves, cause induration by their pressure upon surrounding textures: sometimes they are diffused through or among the natural tissues of a part, which thus they indurate. All the varieties of tubercle, and of carcinoma, and many other forms of disease which have been styled malignant, fall under this head.

These new and morbid products play a fearful part in disorganizing the bodily frame, and in embittering and shortening life. They will necessarily occupy much of our attention in the progress of the course. At present I merely point them out as illustrations of the manner in which the consistence of parts may be *increased*.

LECTURE III.

SOFTENING—ITS CAUSES AND VARIETIES. TRANSFORMATIONS OF TISSUE—LAWS OF THEIR OCCURRENCE. TRANSFORMATION INTO CARTILAGE, SKIN, FIBROUS TISSUE, SYNOVIAL MEMBRANE, ADIPOUS TISSUE, MUCOUS TISSUE, BONE. NEW TISSUES. CHANGES OF SITUATION—IN THE CHEST, OF THE LUNG, OF THE HEART—IN THE ABDOMEN AND PELVIS, HERNIA, INTUSSUSCEPTION, PROLAPSUS.

Softening.—We were occupied with that branch of pathological inquiry which relates to the various ways in which the several parts and organs of the living body are liable to be altered by disease.

We considered the changes to which the solid parts are subject in *bulk* and *form;* and that alteration of their consistence which constitutes hardening or *induration.*

The opposite condition to this is *softening*, diminished consistence, a less degree of cohesion of parts and tissues than is natural.

This also is a state of which it is important that you should comprehend the nature, and causes, and varieties; and the share that it often has in breaking down the structure of organs, and in destroying life.

There is scarcely any tissue of the living body in which softening may not take place. I shall here, however, as before, mention a few illustrations only of its occurrence, taking those instances in which the phenomenon is most evident, or is best understood.

Softening is perhaps never more strikingly obvious to our senses than when it affects the brain or spinal cord. We find portions of these organs manifestly softer than the rest. You are familiar with the usual consistence of the adult brain: you will find it sometimes reduced, in places, to the consistence of cream: a gentle stream of water suffered to fall upon the softened pulp suffices to wash it away, and a cavity is left in its place.

The cellular tissue is another part in which softening is exceedingly common, although the change is not so readily perceived. The cellular tissue is the great connecting tissue of the body; and we are made sensible of its diminished consistence when parts which it unites become separable with unusual ease. Thus you may sometimes, by exerting a very slight degree of force, strip off a serous membrane from the parts that it invests, or a mucous membrane from the surface lined by it. This ready separation is a consequence of the diminished consistence of the subserous, or the submucous, cellular tissue. The membranes themselves, in such cases, may be in a perfectly natural state.

Muscles, again, are often palpably softer than they should be: the muscular substance of the heart, for example. Here the muscular fibre may itself have undergone a change of consistence; or the muscle may simply appear to be softened, in consequence of the softening of the threads of cellular tissue by which its fibres are tied together.

The mucous membranes very frequently present the phenomenon of softening. This is more commonly seen in the stomach than elsewhere. Instead of being raised from the subjacent tissues in large flakes, the mucous membrane, when seized between the forceps, breaks off in small fragments, or it may be crushed and mashed by the pressure of the finger, or washed away in shapeless pulp by a little current of water. This condition of its lining membrane is usually limited to parts of the stomach; but occasionally it is general.

Even the bones are liable to this change of consistence. There is a disease called *mollities ossium*, in which the bones even of adults become soft and pliant, and capable of bending in any direction. There is a deficiency in their earthy constituents; atrophy of that particular tissue. Indeed, softening is often a concomitant of atrophy.

The accidental products to which I adverted when speaking of induration—especially some of the forms of carcinoma—are sometimes remarkably soft, resembling brain in

consistence and appearance, or cream, or jelly. But in these cases we can scarcely consider the change as an example of softening of the textures of the body; it rather consists in the addition of parts that are themselves soft and half fluid.

Now softening may occur under very different circumstances. One very general cause of softening is inflammation. Every part, I believe, that is inflamed, undergoes, in the first instance, a diminution of its consistence. This appears to be almost the necessary consequence of the stagnation of the blood, the effusion of serosity, and the suspension of healthy nutrition. These are circumstances to which I shall recur. I cannot avoid alluding occasionally to things with which you are supposed to be as yet but little acquainted, and which will engage our particular attention as the course advances.

It would be a great mistake, however, to suppose that all softening results from previous inflammation. Doubtless it often proceeds from the direct and simple diminution of nutrition, and is then closely allied, as I said before, to atrophy. Thus, softening of the brain is, sometimes, due to inflammation: we meet with it where the inflammation has been unequivocal, and has been caused by external injury; but sometimes also it is quite independent of inflammation, and is owing to disease of the cerebral arteries, whereby the brain, or a portion of it, is deprived of its full supply of arterial blood, and ceases to be properly renovated: hence a loosening of its texture—a separation of its component particles—an approach to the fluid state. I shall, of course, hereafter endeavour to point out to you more particularly the means we possess of distinguishing these two forms of cerebral softening; they constitute morbid conditions of the highest interest.

I may observe, that we have an illustration of the principle now laid down, in that general softness, flaccidity, and slight cohesion of parts, noticeable in children, or others, who are imperfectly nourished. We find this general absence of the natural firmness coincident with paleness, and a thin watery condition of the blood. Magendie kept animals upon food unsuitable to them, containing no azote, and incapable of supplying sufficient nourishment; and one curious consequence that followed was a loss of substance in the cornea, which melted down and disappeared.

There is another source of softening which requires to be mentioned—I mean the gastric juice, which has the power of dissolving not only food which is submitted to its action, but the mucous membrane of the stomach itself, and even all its tissues and coats. This cause of softening operates, however, in the dead body only; but its effects have often been mistaken for the consequences of disease; and therefore it will be necessary for me hereafter to call your attention to the circumstances under which they may be expected, and to the means we possess of discriminating them from similar changes, which are more properly called morbid.

Upon the whole, it may be said that every form and kind of softening in the living body—whether it proceed from inflammation, from disease of the arteries, from insufficient sustenance, or from altered qualities of the blood—may ultimately be resolved into suspended or defective nutrition.

Furthermore, as there is a hardness of parts resulting from repletion and distension, so there is a *softness* rather than a *softening*, from their emptiness and flaccidity: as of the breast immediately after the child has sucked—of the integuments in those who, having been fat, have wasted, either from disease or from advancing age, and so on.

Transformations of tissue.—There are a curious set of changes in the living body constituting, or arising from, what has been called, by modern pathologists, the *transformation* of tissues. In the proper place of one natural tissue we sometimes find another, which last is thus *un*natural in regard to its situation, but natural in all other respects. The new tissue is such as we meet with elsewhere in the body, but it is not such as properly belongs to the place it occupies. Either the original tissue has been gradually converted into the new, or the original tissue has disappeared, and the new tissue has been substituted for it; that, for example, which should be cartilage we sometimes find to be bone, or *vice versa*. The new tissue may not be, and perhaps seldom is, so perfect as that which is natural, but it very closely approximates to it in sensible qualities and physical structure.

This mode of alteration, incidental to parts of the body, has not been much attended to till of late years; yet it is peculiarly interesting, inasmuch as some of the laws, according to which the transformations take place, seem to have been satisfactorily ascertained.

In most cases the tissue that has been changed or displaced is in one of the two following predicaments:—

Either its natural function has been for a long time suspended;

Or, it has been accidentally called upon to fulfil a purpose for which it was not originally destined.

In the former case, it gradually approximates towards cellular tissue, and is at length converted into it.

In the latter, it assumes the characters of that other tissue, of which it has taken up the office.

Now all this is curiously in accordance with what we know of the laws that govern the progressive development of the human body. In the embryo, all the tissues commence by being cellular tissue, and they only assume other forms and characters, each on the condition (so to speak) of its fulfilling some special purpose. Certain of the embryonic tissues do not attain their ultimate and perfect state until they have passed through the forms of two or three other tissues in succession. This being so, we can the more easily understand how there should be a tendency in each tissue to revert to its primitive state—to the state, *i. e.* of cellular tissue—when the exercise of its peculiar function ceases. The law coincides with that by which the hypertrophy and atrophy of parts are governed, these changes being but plus and minus degrees of the same process. A muscle, put for a long time in a state of complete inaction, gradually passes from the muscular into adipous tissue, and at length into cellular.

Again, if the nature of the original function determines in the first instance the nature of the tissue, we have the less difficulty in conceiving how the nature of a new and accidental function imposed upon a tissue may determine the kind of transformation it shall suffer. Thus, to take again the instance of muscle. if a muscle comes accidentally to lie round and invest an unreduced joint after a dislocation, it assumes the characters, together with the uses, of those tissues which naturally enclose the joint—it is converted into fibrous or ligamentous tissue. We find the very same law prevailing in the vegetable kingdom: the cut *branch* of a willow tree, for example, planted in the earth, takes up the office, and gradually acquires the form and properties of a *root*.

You are not to suppose however that this transformation of tissues is indiscriminate; while all tissues are convertible into tissues different from themselves, there is a limit to the number into which they can be transformed. Nerve, muscle, and gland, are convertible into other tissues, but other tissues are not converted into them. The cellular tissue being the original web upon which all the natural tissues are constructed, is accordingly capable of numerous transformations. The other tissues admit of one, or two, or three forms only of conversion. There are some transformations much more common also than others; the change, for example, into adipous tissue, and into osseous tissue.

Not only do conversions of tissue take place during the growth of the fœtus, but the several tissues (as we learn from the study of comparative anatomy) are substituted, the one for the other, in different species of adult animals, according to the needs of these creatures; what is cartilaginous or fibrous tissue in one animal, is bone, perhaps, in another. For example, the *fibrous* sclerotic of quadrupeds becomes *bony* in fishes.

Now the following laws on this subject have been stated by Andral:—

1. Those tissues alone in the human body are susceptible of morbid or accidental transformation, which, in the progress of uterine life, or in the ascending series of adult animals, undergo regular or normal transformations. In this category are included the muscular, fibrous. cartilaginous, mucous, and cutaneous tissues.

2. The accidental transformations to which these tissues are liable are of the same kind with the normal transformations that occur in the fœtus, or in the scale of adult animals. For instance, cartilage may be converted into bone, but it is never changed into the mucous tissue.

3. Those tissues which in the embryo, or in the ascending series of animals, undergo no known transformation, undergo none of these morbid conversions. If they ever appear to do so, it is, in truth, from the conversion of parts immediately contiguous to them.

4. Every tissue that becomes atrophied tends towards a conversion into cellular tissue; retrogrades, as it were, towards its primitive organization.

In strictness of language this change ought not perhaps to be called a *transformation:* the proper substance of the tissue or organ vanishes, and the cellular tissue that sustained it being left behind, becomes more apparent.

These transformations then are not the offspring of mere chance, but obey laws originally impressed upon the living body, and impressed for wise and benevolent ends. They exemplify the working of what the older pathologists discerned, and called the *vis medicatrix naturæ*. This is a phrase that has been much sneered at; but (as I conceive) very unjustly, and sometimes ignorantly. It is simply a short formulary expressive of a great general truth, viz., that the animal frame is so constituted that, while it is necessarily liable to injury and disorder, it contains within itself the elements of repair, and of conservative adaptation. To a certain extent it is a *self-mending* machine. Surely this is an admirable provision, and strongly indicative both of wise design and of beneficent intention.

Special transformations take place then, when a tissue falls into disuse, or when a new function devolves upon it. But may all transformations be included within these limits? What is called ossification of the coats of an artery is of very frequent occurrence. Here there is neither any cessation of natural function, nor any assumption of a new function. The same may be said of ossification of the cartilages of the ribs, and of the larynx. It would seem, therefore, that there must be other agencies or conditions under which the transformation of tissues is brought about. One of the circumstances assigned as an occasional cause of such changes is what is rather vaguely called irritation. It is said, for example, that chronic inflammation and ulceration of the mucous membrane of the larynx favours and accelerates the conversion of the laryngeal cartilages into bone. A joint that becomes anchylosed under slow inflammation is a case still more in point. Another circumstance which certainly has some connection with the same change is age. In old persons we find, almost always, more or less ossification of the cartilaginous tissue in various parts of the body.

It has been urged, by those who desire to establish the universality of the rules now laid down, that the cases adduced as exceptions to those rules are not cases of true transformation, but of morbid deposit. The earthy patches which constitute the ossification of arteries differ from bone in their chemical composition—are sometimes very small and scattered—while the intervening parts of the tissue in which they are found remain quite healthy. These circumstances, they argue, distinguish such changes from real conversions of tissue.

Bearing in mind what has already been said, let us briefly run over a few of the most common and remarkable transformations presented to us in the human body.

Cartilage.—Of the conversion of cellular or other tissues into *cartilage* we have every-day instances in the parietes of arteries—in the valves of the heart—in the walls of morbid cysts—sometimes even in the walls of a vomica.

Of transformations into cutaneous tissue, or *skin*, by far the best examples we have are furnished by the mucous membranes. When a portion of the mucous membrane of the rectum, for instance, or of the vagina, protrudes externally, is permanently exposed to the air, and subject to the friction of clothes, or of neighbouring parts—that is to say, when it is placed under the same conditions with the skin—it assumes the characters of the skin: it gradually loses its red colour and approaches the tint of the skin, ceases to secrete mucus, becomes dry, obtains even a permanent cuticle, acquires firmness and density, and is less sensible to the contact and pressure of foreign substances. It is impossible not to perceive the beneficial nature of this transformation.

Fibrous tissue and synovial membrane.—A new formation of *fibrous* tissue may result from the conversion of cellular tissue, or of cartilage, or of synovial membrane, or of muscle. I have already spoken of this change as it is seen in unreduced dislocations: it is common wherever false joints are formed. And almost under the same head we may place the conversion of cellular tissue into *synovial* membrane. Synovial membrane seems, indeed, to be little more than condensed cellular tissue. Sir B. Brodie, in his book on Diseases of the Joints, gives instances of synovial membranes being formed, where none before existed. "In a young lady who had attained the age of ten or twelve years, labouring under the inconvenience of a club-foot, a large bursa was distinctly to be felt on that part of the instep which came in contact with the ground in walking. In

another young lady, who had apparently recovered of a caries of the spine, attended with a considerable angular curvature, a bursa appeared to have been formed between the projecting spinous process and the skin."

We cannot look upon any of the last mentioned changes as being morbid: they are clearly the result of natural and spontaneous methods of cure, or of accommodation.

The conversion of other tissues into *adipous tissue*, or fat, is a very curious and a very common change. Generally, I believe, this occurs in connection with suspended function. Sometimes there is an apparent transformation of this kind in cases of hypertrophy of the adipous tissue, or general obesity, such as I have previously described. But probably these are not true transformations. If you examine a fat heart—such as is not uncommonly met with in very fat persons—you will generally find a combination of hypertrophy of the adipous tissue with atrophy of the muscular, rather than any conversion of the one into the other. The fat is deposited around, or even between the wasted muscular fibres, and the two may usually be separated from each other. It would seem, therefore, that here the converse of one of the propositions which I lately mentioned holds good—of the proposition, namely, that a suspension of the function of a tissue leads to its degeneration into adipous or cellular tissue: in the instance before us, the accumulation of adipous tissue tends to diminish, or at length to abolish, the function of the part.

Dr. Carswell, however, believes that a real conversion of the muscular tissue into the adipous takes place: and he states that the change is most conspicuous in the muscles of the lower extremities, when the limb has long remained motionless through palsy. The muscular fibres remain distinct, but are of a pale white, or straw colour, and when they are pressed between the fingers a clear oily fluid oozes out. If a portion of the muscle be steeped for a time in alcohol, this oil collects, in considerable quantity, on the surface of the spirit. What is called the fatty liver affords an instance of a morbid conversion, not at all unfrequent. The altered liver is of a light tawny colour, of diminished specific gravity, retains the impression of one's finger, is tender, and tears easily: it greases the knife that cuts it, or bibulous paper in which it is wrapped. By boiling it you may obtain a concrete oil, which has all the characters of fat.

Now what is very curious in respect to this morbid condition of the liver is, that we can produce it, at will, in some at least of the lower animals. You know that the "*foie gras*," procured from certain birds, is an article of great luxury among epicures. It is obtained by a very cruel process. Geese, or ducks, are confined in baskets just large enough to contain them, but not large enough to allow them any motion: they are kept continually in the dark also—sometimes even, I am afraid, their eyes are put out, but this I should imagine to be a useless and superfluous piece of cruelty, it being the absence of light, and not the absence of the power of vision, which helps to bring about the desired effect. At the same time the birds are sedulously crammed with food. Under this discipline their livers acquire the requisite size, and greasiness, and the true flavour.

An ingenious theory has been constructed, in reference to the *modus operandi* of this process. The excessive supply of nutriment tends, no doubt, of itself, as it always does, to the production of fat: the constrained state of inaction interferes probably with the right and healthful formation of blood, a matter in which the liver is believed to be a good deal concerned: then we know, by experience, that the privation of light has the effect of blanching animals as well as vegetables, and thus farther interferes with the due renovation of the blood. In this way the transformation of the liver into a fatty mass has been sometimes attempted to be explained. To what extent the explanation is correct, I will not pretend to say: but it is worth remarking that the fatty liver is very frequently met with in persons who die of consumption; and in that disease there are various causes in operation tending to modify the constitution of the blood.

The history of these unfortunate fowls is not barren of instruction in respect to the more limited bad effects of full diet, want of exercise, and a short allowance of day-light, upon the "*featherless biped*" man.

The conversion of certain other tissues, and particularly of the cellular, into *mucous* tissue, is by no means a rare thing. It is held that the skin is capable of this transformation—that the skin and mucous membrane are thus naturally convertible into each other. Unquestionably they are at all times closely related, forming a continuous surface even in

the sound state, and exhibiting different characters only, perhaps, in virtue of the different conditions in which they are placed, and of the different uses they serve in the animal system. When the cuticle happens to be removed by friction, or by any other means, from a portion of the cutis vera, the exposed surface, as you all know, reddens, becomes preternaturally sensible, is constantly moistened by an albuminous fluid, and approximates very manifestly to the nature of a mucous membrane.

It is the *cellular tissue*, however, that most commonly and most decidedly assumes the properties of a mucous membrane. We find that sinuses, fistulous openings, and tubes in various parts, clothe themselves, at the expense of the cellular tissue, with a surface of mucous membrane. This new tissue is not distinguishable, anatomically, from portions of original mucous tissue. It is true that we do not find it provided with mucous follicles, or with ordinary villi; but these are no essential parts of true mucous membranes. The membrane by which the surface of a fistulous channel is lined resembles very precisely, in character and qualities, the simpler forms of true mucous membrane; such, for example, as are seen in the urethra, in the biliary passages, and in excretory ducts generally. Like them it is with difficulty made to take on the adhesive inflammation: and it is for this reason that sinuses of this kind, and chronic abscesses, are so troublesome to the surgeon, and require to be laid open before they can be abolished.

I have already given you instances of the conversion of other tissues into *bone*. At first sight it would appear that nearly all the tissues are susceptible of this change. The brain and spinal marrow, the lungs, the heart, have been found more or less completely encased in bony matter: so that it is common to speak of, and to exhibit specimens, of ossification of the pleura, of the pericardium, of the dura mater, and so on. But the truth seems to be that, in these cases, the ossification has taken place in the cellular tissue immediately subjacent to these membranes; bony or earthy matter being laid down in that tissue in granular deposits, or in ossific plates. And we may say, with Dr. Carswell, that the osseous transformation is exclusively confined to the cellular, fibrous, fibro-cartilaginous, and cartilaginous tissues.

We have been considering the conversion of natural tissues into other natural tissues. But tissues exactly like the natural are liable to be formed *de novo*, and afterwards to undergo many or most of the transformations that have been mentioned. You may probably have seen, for example, the pleura pulmonalis connected to the pleura costalis by very perfect cellular tissue. Now this new tissue may be converted into other tissues; into cartilage, or into bone. These are ultimate consequences of inflammation, and as such, I shall have occasion to revert to them hereafter. You will in the mean time bear in mind that this is one mode in which very remarkable changes in the bodily structure may be effected.

The intimate texture of parts may be farther altered—not simply by conversion into other known forms of structure—but by an absolute disappearance or confusion of all regular structure. This is usually a consequence, either of the effusion in the natural interstices of the parts, of fluids, which afterwards pass into the solid state, or it is a consequence of the growth of solids which do not belong to the healthy body. In this sketch of general pathology I must content myself with thus briefly alluding to this source of morbid change.

I may as well observe, here, that the alterations, with which we have hitherto been occupied, of the solids of the body, fall, almost all of them, under the head of *lesions of nutrition*, as the French pathologists speak. That is to say, they commence, and have their primitive seat, in that process and place, where the blood, having reached the capillary system of vessels, performs its special purposes. It is in the capillaries that the fluids and solids accomplish their vital union. Each solid receives from the blood, and assimilates with its proper substance, material particles, identical in their nature with those of which it already consists. Each solid gives up also to the blood, and so dismisses, other particles, which before formed a portion of itself, but which have become unfit, or superfluous. Now any departure from this continual building up and pulling down—any excess, or defect, of the particles added, or of the particles subtracted—any irregularity in the manner in which they are deposited—any variation from their right consistence, or in their kind

and quality—in short, any deviation from the regular process, as I have briefly described it—is called a *lesion of nutrition*.

The few changes already spoken of, and *not* included among the lesions of nutrition, are:—

The distension of the hollow organs by an undue accumulation of fluids within them;

The coagulation of the fluids in their proper vessels, excluding however the capillaries;

The escape of the fluids, as such, out of and beyond their containing vessels; and

The solution of tissues, after death, by the chemical agency of the gastric juice.

None of these, properly speaking, constitute lesions of nutrition, although they sometimes lead to them.

Lastly, let us take a glance at the changes of *situation* to which the solid parts of the body are liable. They are sometimes of very serious import.

These changes of place—sometimes the consequence of disease, sometimes its cause, and not unfrequently the cause of death—respect chiefly the viscera—and most especially the viscera of the chest, abdomen, and pelvis. I omit dislocations of joints, as belonging exclusively to surgery.

In the chest, a whole lung may be displaced, and compressed against the vertebral column, by blood, or serum, or air, effused into the cavity of the pleura. An alteration of this kind, whereby one half of the respiratory apparatus is rendered incapable of its special function, cannot be otherwise than full of peril.

The very same causes operating on the left side of the thorax will dislocate the heart, thrust it over to the right of the sternum, where it may be felt, and heard, and seen, to pulsate. This again cannot happen without greatly disturbing the vital function of circulation, and putting life in jeopardy.

Yet neither of these serious displacements are necessarily fatal. Both admit, under certain circumstances, of remedial treatment: as I hope to prove to you hereafter.

In the abdomen and pelvis, the various forms of hernia may be adduced as involving very dangerous changes in the place and relative position of parts. Portions of the intestinal tube are apt to pass through accidental openings in the diaphragm—or between the edges of the linea alba surrounding the navel—or out at the abdominal ring—or through some other natural or accidental aperture. I need not tell you how fearfully life is compromised when, in consequence of such faulty position, the bowel becomes constricted—when its contents can no longer pass onwards, and inflammation, or gangrene, are present or impending. Even when there is no strangulation, the mere displacements to which the escape of the contents of the abdomen and pelvis from their natural limits gives rise, may be productive of much discomfort, deformity, and danger. Of this the historian Gibbon presented a remarkable example. He had an immense scrotal hernia; so large it was, that it hung down very nearly as low as his knees. After his death it was found that almost the whole of the omentum, and the greater part of the colon, had descended into the scrotum, and had dragged the stomach after them; so that its pyloric orifice lay close to the abdominal ring.

Akin to hernia is that partial displacement of the bowel in which a portion of it passes, not through any natural or accidental opening, but into the bowel itself: just as one portion of the finger of a glove is sometimes pulled into the remaining part, by the withdrawal of one's hand. The contain*ed* portion of intestine is liable to be nipped and strangulated by the contain*ing* portion—and all the peril of hernia results, with much less chance of relief by art. This state of things is called *intus-susception*.

Exactly of the same nature, though less alarming, is *prolapsus* of the rectum, or of the vagina. Here also a portion of the tube passes into the contiguous portion; but being near the extremity of the canal, the inverted part protrudes externally, and becomes, in most cases, a source of distress and suffering, rather than of danger. Inversion of the uterus is another example.

Thus much, then, of the changes to which the *solid* parts of the body are subject, in *bulk*, in *form*, in *consistence*, in *texture*, in *situation*.

You cannot fail to perceive the injurious effects which many of these changes in the

various solids are calculated to produce upon the movements and working of the living machine: how some of them must impede or derange its natural action; some stop that action altogether.

Now the fluid parts of the body are liable also to alterations, which, if they are not always so obvious as those of the solids, are certainly not of less moment.

You are probably aware that, for many centuries, the fluids were supposed to be the primary agents in every form of disease: that all maladies were attributed to some acrimony or peccant state of the humours; and that however else the theories of medicine might vary and fluctuate, the *humoral* pathology, till a comparatively recent period, ran through almost all of them. At length, the absurdity of the hypothesis, and still more the dangerous practice which this doctrine involved, began to be manifest, and led to its total abandonment. Rather more than a century and half ago, the foundation of the opposite doctrine appears to have been laid, by the writings of Glisson in this country, and by those of Baglivi in Italy; and presently the notion came to prevail throughout the schools, that all the morbid conditions of the body had their exclusive origin in the solids. The pendulum of opinion swung at once, as is usual, into the opposite extreme of error. It promises, in our time, to settle at the proper medium. The humoral doctrine still indeed finds favour among the ignorant, and is commonly adopted by the quack: a circumstance which illustrates the fact that the mischievous influence of unsound theories survives the duration of the theories themselves. The scientific physician of the present day can only wonder how exclusive solidism, or exclusive humoralism, should ever have found advocates.

LECTURE IV.

MORBID ALTERATIONS OF THE FLUIDS, ESPECIALLY OF THE BLOOD. CHANGES IN ITS QUANTITY AND DISTRIBUTION. GENERAL AND LOCAL PLETHORA. POVERTY OF BLOOD. ACTIVE CONGESTION—ITS PHENOMENA—STATE OF THE VESSELS AS SEEN BY THE MICROSCOPE. MECHANICAL CONGESTION. PASSIVE CONGESTION. RELATIONS OF THESE FORMS OF CONGESTION TO INFLAMMATIONS—TO HEMORRHAGES—TO DROPSIES.

AFTER running over the principal alterations to which the solid parts of the body are liable, we were beginning to inquire into those no less important morbid changes which are apt to take place in its fluid constituents. I reminded you that, respecting the whole of this subject, pathologists had passed from one extreme of opinion to another: that for a very long period the *humoral pathology* prevailed in the schools, and that in times not very remote from our own it was entirely superseded by the opposite doctrine of exclusive *solidism*. It is strange that either misconception should have so long maintained its ground.

If we consider the definite relation subsisting between the solids and the fluids of the body, and the unceasing agencies which they mutually exercise on each other—how, for instance, on the one hand, all the solids are originally built up, and are afterwards perpetually nourished and sustained by materials furnished from the blood—how, again, on the other hand, some of the solids are continually employed in the reciprocal office of feeding and renewing the blood, while others are as constantly busy in decompounding it by the various secretions—we cannot avoid perceiving that distinctions of the kind I have mentioned, founded upon mere differences of consistence, are futile; that no notable alteration can take place in the solids of the body which will not soon affect in some way its fluids; and that every important change in its fluids must lead to a corresponding and proportionate modification of its solids: in fine, that the dispute between the solidists and the humoralists was altogether baseless and unprofitable.

The animal fluids are—the blood, the fluids that enter the blood, and the fluids that proceed from the blood.

The fluids that enter the blood are of two kinds.

1. Those by which it is renewed and enriched.

2. Those which enter it in order that they may be conveyed out of the body.

Now although we cannot doubt that any considerable modification of the fluids that feed and renovate the blood, and particularly of the chyle, must have a direct influence upon its composition and quality, we really know but little about them, except in their effects. We seldom have any means of procuring these the first products of nutrition, so as to examine them, or to test their qualities. Yet we can perceive causes that are likely to deteriorate or deprave those fluids (unfit aliment, impure air), and we know that under the continued operation of such causes, the blood, replenished by these fluids, *is* actually and sensibly modified.

Again, we cannot doubt that some of the matters derived from the body itself, and taken into the blood in order to be conveyed away, may, and often do, directly alter and contaminate the blood, and act as poisons upon the system: matters, for instance, absorbed from parts of the body that are diseased, or dead and putrefying; in this way, doubtless, disorders which were at first strictly local may come to affect the whole economy:—matters, again, which though harmless in minute quantity, become noxious when retained and accumulated in the blood, in consequence of faulty or deficient action of the organs destined to eliminate them from the circulating fluid. The injurious effects of some of the substances which thus become deleterious—as urea, of which the blood, during health, is continually purified by the kidneys, and bile, which is naturally separated therefrom by the liver, and carbonic acid, which it is the office of the lungs to excrete—will furnish topics of interesting inquiry hereafter.

The fluids that *leave* the blood may be considered under a threefold division.

1. Those which are directly expended in the growth or maintenance of parts, some of them becoming fixed and solid, and others retaining their fluid condition. Of these the principal alterations have been briefly pointed out among the *lesions of nutrition*.

2. Those that are employed in aid of some definite function of the body: as the saliva, the gastric juice, the bile, the pancreatic secretion, the tears, the synovia of the joints, and so on. Now these may be secreted in excessive abundance, or in too scanty quantity, or of imperfect quality, or not at all:—and all, or any, of these deviations from the healthy standard may be the result of very serious disease, or may cause very serious disease: and they will be spoken of hereafter when the disorders of the parts or functions connected with each shall be discussed.

3. Those which are separated from the blood merely to be excreted—as the urine, certain secretions from the bowels, and from the bronchi and skin. Some of these are extremely worthy of study, as indications of disease; but they require no particular consideration in this part of the course.

Dismissing, therefore, for the present, all further account, as well of the fluids that concur to form the blood, as of the fluids that issue from the blood, let us inquire what morbid changes the blood itself is liable to undergo.

The blood, then, is subject, first, to remarkable variations in its *quantity*, both in respect to the whole system, and in respect to particular organs and tissues.

2. Closely connected with these differences of quantity is the variety which is observable in regard to the proportions between the several constituents of the blood. The changes that occur of this kind are sometimes strikingly evident to our senses. For example, we not unfrequently perceive that the blood drawn from a vein is thinner, manifestly more watery, less rich in fibrin and in colouring matter, than blood of the standard quality.

3. Again, independently of mere alterations in the relative proportions of its constituent parts, the blood is liable to great change in its chemical composition, and, therefore, in its physical quality. This appears to be the case in sea-scurvy, and in the analogous disease called purpura, and it is doubtless so in many other complaints.

Diseases of the alimentary canal, interfering with the process of chylification, must affect the blood: diseases of the organs of respiration, interfering with the arterialization of the blood, will alter it: diseases of other channels of excretion—the bowels, the biliary apparatus, the kidneys, the skin—will (as I have already hinted) indirectly contaminate it: so will various foreign matters, gaining entrance, as they may when in solution or in a

gaseous state, through artery, vein, or any membranous structure, such as bladder and intestine: and so, also, there is good reason to believe, will certain states of the nervous system.

But contenting myself with having indicated these latter changes, or sources of change, I shall defer giving a more particular account of any except those that relate to the *quantity* and the *distribution* of the blood.

I say the blood may undergo important alterations in its *quantity*. It may exist in too great abundance throughout the body; and it may exist in too great abundance in certain parts only of the body. These states have been recognised for ages. Sometimes they are called respectively general and partial *plethora;* sometimes general and local *congestions* of blood; people speak also of irregular *determinations* of blood to different organs; and, of late, the term *hyperæmia*, first invented by M. Andral, in France, has been imported into this country, and much adopted here: all these words and phrases mean, in truth, the same thing; and their frequent recurrence in medical works, is, of itself, sufficient evidence of the frequency and importance of the conditions which they express.

If we comprehend rightly this subject of plethora or congestion, we shall be prepared to understand some most important morbid states, of which it seems to be in many, if not in all cases, the earliest approach—the initial step. Inflammation, hemorrhage, dropsy, all acknowledge and imply a previous condition of congestion. "There is probably," says Dr. Alison, "no kind of diseased action of which any part of the living body is susceptible, which is not connected, sooner or later, with increased affiux of blood towards that part, either as its cause or its effect; and the immediate object of all our most powerful remedies is to act on these irregularities of the circulation."

That the blood *may be* differently distributed in the capillaries at different times, we know by the variable colour of the surface, which depends upon the varying degrees of fulness of the cutaneous blood-vessels. The phenomenon of blushing, the red cheek of anger, the heightened colour of the skin under brisk exercise, are familiar facts illustrative of partial plethora of the capillaries, consistent with health.

There are reasons (which I shall hereafter lay before you), for believing that a similar sudden accumulation of blood, taking place in internal parts, may sensibly disturb their functions; causing transient fits of giddiness, insensibility, and sometimes death itself, when the congestion affects the *cerebral* blood-vessels; and attacks of difficult breathing when the capillaries of the *pulmonary* tissue are concerned; and even *these* attacks, for aught that I know, may end fatally.

It often happens that when certain portions of the surface, as the cheeks, are visibly redder and fuller of blood than usual, or when such symptoms as I have just referred to denote the probability of some internal congestion, other parts of the surface, as in the extremities, are visibly paler: and there are, at the same time, corresponding and palpable differences of temperature.

Perhaps it may not be so obvious that the whole quantity of blood, throughout the body, is sometimes in excess.

That in the adult state, when the growth or increase of the body has been completed, blood may be made in greater abundance, and more rich in the materials of nutrition than the wants of the body require, is not only conceivable, but true. We are able to assign circumstances in which this is likely to happen, and we find that under such circumstances it actually does happen. Full living, and a sedentary life, are causes likely to occasion general plethora—and they do occasion it. The full diet, so long as the digestive powers are perfect, provides more chyle, conducts into the blood a larger quantity of its proper pabulum. The sedentary life precludes that freer circulation of the blood, and that more liberal expenditure of it through the skin, and by means of the other organs of secretion, which would occur under more active habits. Persons thus circumstanced are apt to grow fat; the adipous tissue seeming, in these cases, to form a kind of safety valve for the diversion of the superfluous blood. Such persons have turgid and florid cheeks, red lips, red mucous membranes, and (not uncommonly) ferrety eyes. Their entire vascular system is preternaturally distended. If you open a vein, you find that they bear a large abstraction of blood without fainting, and are even refreshed by it; and the blood drawn separates

into a large and firm mass of coagulum, with but little serum. Keeping to the nomenclature we have already employed, we might say that there is here *hypertrophy* of the blood.

When inflammation arises in the subjects of this general plethora, it runs high, and requires active treatment. But they are not, as you might naturally expect them to be, and as many writers state them to be, peculiarly prone to suffer inflammatory complaints. There is general fulness of the vascular system, but no irregularity, nor any necessary tendency to irregularity, in the distribution of the blood.

You will observe that the relative proportion of the more solid to the more fluid constituents of the blood is increased in these cases of general plethora: the blood is not only more abundant, but it is richer also in fibrin, and in red particles.

The means to be adopted for redressing this unnatural and unsafe condition of the circulation, are those which common sense would suggest. The abstraction of a part of the superfluous blood, a more restricted diet, a larger allowance of active exercise.

It will be worth our while to contrast this state of general plethora with its opposite—that in which the blood is scanty and poor—what Andral calls (though with questionable propriety) *anæmia*. *Oligæmia* is the cacophonous but more exact name assigned to it by Gendrin; but *poverty of blood* is the ordinary English phrase for it, and the best. This is a state which we can produce at will, by abstracting blood from the body in moderate quantity, but repeatedly, and at short intervals. It occurs, also, frequently, in spontaneous disease, and from various causes; from a privation of the materials destined to replenish the blood; and in cases in which these materials appear to be turned to little account, as in chlorotic girls. We see it in those who habitually and frequently lose a certain quantity of blood, in disease; in persons, for example, who are subject to piles, and who bleed daily from the rectum; still oftener in women who suffer repeated hemorrhages from the uterus. When the drain has long been continued, these persons become very pale; even those parts which are naturally most red, as the lips and tongue, become almost white; their faces look like wax; and if still you draw blood from a vein, and allow it to coagulate, you will have a small clot floating in an abundance of serum, and that small clot will be of a light rosy colour; showing a great diminution in the proportion of fibrin, and a still greater deficiency of the red particles. The blood, as they say, is "turned into water." It is a curious pathological fact, that the red particles require more time for their restoration than the other constituents of the blood. And I may mention to you now, what I shall have to repeat, that in conjunction with the obvious curative measures comprised in arresting the habitual loss of the vital fluid, and in affording sufficient nutriment to the system, the preparations of iron, and the respiration of pure air, have signal efficacy in renewing the red particles, and giving back again their native hue of redness to the cheek and lips.

In general plethora every part is preternaturally full of blood, and the blood itself is full of the elements of nutrition. General plethora therefore implies, *in one sense*, local plethora of every organ and tissue. In strictness, however, local plethora is only predicable of a part that contains more than its share of red blood.

Now the converse of this is not true, as it might be expected to be, of the opposite condition. A deficiency in the whole mass of blood contained in, and circulating through the body, does not protect the *parts* of the body from congestion—from having an undue quantity of blood sent to them. Far from it. Local determinations of blood are *very common* in persons in whom the mass of that fluid, and the proportion of its nutritive materials, have been considerably diminished by disease, or by hemorrhage.

This remarkable tendency, under such circumstances, to an unequal distribution of the blood in the capillaries, admits (I think) of this explanation. A due supply of healthy blood is requisite for the steady and equable performance of the functions of the brain and nerves. When this supply is defective, or uncertain, those functions become disordered and irregular, and, in their turn, influence the various solids, disturb their action, and derange the balance of the circulation. That the capillary blood-vessels may be filled to excess, or completely emptied, by causes operating *through the nervous system*—by moral emotions, for example—we are sure from the phenomena just now adverted to, the blush of shame or anger, the paleness of fear; and there can be no doubt that *morbid* congestions,

which sometimes are separated from those that are consistent with health by very slight shades of difference, are often determined through the agency of the same nervous system. And persons endowed with great sensibility or irritability of the nervous system are very liable to partial or irregular congestions of blood.

But this is not the only way in which local congestion may arise.

We can produce it, upon the surface of the body at least, at pleasure, and that in various ways: by friction, by exposing the part to a high temperature, by certain stimulating applications, mechanical (as a cupping-glass), or chemical (as a mustard poultice): we produce an injection of the small cutaneous blood-vessels; there is, evidently, more than the usual quantity of blood attracted to the part, or detained in the part—a degree of redness, which soon subsides if the cause of it be withdrawn in time.

Congestion thus occasioned is not inflammation, but it is the first step towards that complex process; and for that reason it deserves all your attention. Apply the exciting cause a little longer, or increase, in a slight degree, its intensity, and the phenomena of inflammation begin to manifest themselves.

I said we can produce local congestion, when we please, upon the *surface* of the body: but there can be no doubt that a similar state may be produced by analogous causes, in internal parts. Look at this representation of the stomach of a dog (*one of Dr. Roupell's plates*). You see one portion of it of a bright red colour, actively and vividly congested. This was the consequence of a dose of alchohol. We may be certain that something of the same kind is the result of every visit to the gin shop.

Local congestion thus produced, or of this kind, is said to be *active*. M. Andral, whose nomenclature has come much into fashion of late years, calls it sthenic, or active hyperæmia. The arteries, perhaps, have more to do with it, in the first instance, than the veins. But it is in the capillary vessels, which are distinct from, and interposed between the minute arteries and veins, that further changes are wrought, when the process advances a stage beyond mere local plethora. What has been observed, by the aid of the microscope, with respect to the blood-vessels, I will endeavour to describe to you.

I take the account I am about to give you chiefly from Kaltenbrunner, a German pathologist, who has recently investigated the subject experimentally, and whose observations are believed to have been most carefully and skilfully conducted, and their results no less faithfully narrated. His observations were made upon the circulation as it appeared in the web of a frog's foot, under a powerful microscope. It would be idle, and something like committing a fraud upon you, were I to lay any stress upon my own knowledge or experience in this matter, for I cannot pretend to any great skill in the use of the microscope, and my opportunities of noticing, by its help, the phenomena of the circulation, have been too few to render their results of much value. Yet it may be in some degree satisfactory to you to know that I am not blindly repeating the remarks of others, and that what I *have* witnessed is perfectly in accordance with the statements of Kaltenbrunner, and affords me a strong assurance of his accuracy and fidelity. There is another reason, too, why I consider him the more trustworthy—he has no theories to which he might be disposed to bend or accommodate his facts.

Before I detail to you his account of the phenomena of congestion, I may briefly describe the scene which presents itself when the web of a frog's foot is looked at through a good microscope. It is a most beautiful and wonderful spectacle, and particularly interesting to those who, like ourselves, are desirous of gaining some insight into the healthy and diseased states of the circulation. It is a sight which I hope and believe you also will have many opportunities of seeing in this place. You perceive, then, occupying the circular field of the instrument, a number of blood-vessels, through which the blood, with its globules, is in active motion; and you see at once that there are three different kinds of vessels before you. First you notice the blood shooting with great velocity along tubes which divide and subdivide into smaller and smaller branches, each branch (speaking generally), going off at an obtuse angle: these are plainly arteries. Then, in another part of the field of view, you see the blood moving in the contrary direction, more slowly, in larger trunks, which are formed by the continual union and accession of smaller and tributary vessels of the same kind, that meet, for the most part, at acute angles: these you know to be veins; and all the intermediate and surrounding surface in view is occupied

with other vessels or channels, which connect themselves with the ultimate ramifications of the arteries on the one hand, and with the primary radicles of the veins on the other, but which differ from both arteries and veins in these particulars—that they interlace and anastomose in all parts, in a very irregular manner, and at all angles, and that they retain everywhere the same uniform size; they neither collect into larger and larger trunks, nor separate into smaller and smaller branches, but are disposed like the threads forming the meshes of a net, except that the interstices are irregular in size and shape. These are the true capillaries, intermediate between the arteries and the veins, and perfectly distinct in character from each, but communicating and continuous with both.

If now you press upon the animal's leg, so as to obstruct the circulation a little, the motion of the blood is retarded, especially in the capillaries. You see the globules slowly following one another. These so-called globules are not spherical, but have an oblong form; and you may sometimes see one of them sticking in a capillary channel, its long diameter having got at right angles to the direction of the current: other globules accumulate behind it, till at last they all pass on again together.

Now Kaltenbrunner irritates the web by pricking it, and soon afterwards the following appearances present themselves:—There is an increased afflux of blood to the part, so that arteries, veins, and capillaries, receive a column of blood two or three times as great as usual; the velocity of the blood is accelerated; the distended sides of the vessels seem to tighten around the stream of blood which they contain. With this alteration of the circulation, the natural functions of the part begin to be modified. The change of the blood from arterial to venous is interrupted. The globules, passing with great rapidity through all the vessels, retain the characters of arterial globules even when they arrive at the veins; they present a bright colour, show a tendency to stick together, and often form little clots, which pass through the capillaries and become visible in the veins.

One of the natural functions of the web is the secretion of a kind of lymph; but this secretion is now suspended. The parenchyma itself begins to be slightly tumid, and assumes a brighter tint than common.

All these phenomena begin from a circumscribed spot, of which the circumference gradually expands as the affection increases; and they cease insensibly at that circumference.

This is active congestion.

A certain period always intervenes between the first action of the irritant cause, and the commencement of true congestion. This period, the occurrence of which you will be good enough to bear in mind, Kaltenbrunner calls the period of *incubation;* the period in which the congestion is *hatching*.

Active congestion, as such, does not continue long. It either passes on into inflammation, as I shall hereafter explain, or it begins to decrease. When it has been very slight, the quantity of blood, and the rapidity of its movement, diminish gradually from the circumference towards the centre; and in this way the congestion insensibly vanishes.

But in other cases, when it has not been so slight, the congestion terminates by an evident crisis, which Kaltenbrunner thus describes:—The blood, receding from the circumference of the congested part towards the centre, gives out, by exhalation, a liquid. The exhalation takes place by fits, and here and there, through the sides of the capillary tubes, and generally on the surface of the organ. The moment of exhalation is very transient; but it is repeated often, and in different spots, until the congestion has disappeared. It is evidently critical, for the congestion is relieved and extinguished in proportion as the exhalation is repeated.

I shall follow these consequences of active and continued local congestion no further at present; but merely remind you again that the changes I have last mentioned constitute the earliest appreciable modification of structure leading or belonging to inflammation. What we thus may see (and it is what I myself have had some opportunities of seeing) in the transparent textures of animals, we reasonably infer to take place, under analogous circumstances, in those parts of the body which are internal and opaque, and consequently hidden from our view.

I will just observe, also, that as active congestion is the parent of inflammation, so it sometimes causes hemorrhage, and is relieved by it. But, comparing this form of con-

gestion with another which I am about to mention, the connection of hemorrhage with it is, relatively, unfrequent.

One obvious mode of remedying this congestion is the mechanical abstraction of blood from the loaded part. But it is seldom that this measure alone suffices; and sometimes it would be ultimately hurtful to adopt it. The state of the constitution may be such, that the disposition to local plethora would be increased by the loss of blood. Disordered action and undue susceptibility of the nervous system are apt to be aggravated by bleeding; and in proportion as the nervous functions are irregularly performed, does the tendency to unequal distribution of blood in the capillary vessels augment: we have daily examples of this in hysterical young women. It is not, therefore, the mere congestion that we have to consider; we must look deeper, for its cause. Leave a small thorn in the finger—the blood will be collected there in consequence of its irritation—and will continue to collect in spite of depletion. But extract the thorn, and your remedial measure of taking away blood is at once successful. So it is also with internal congestions of blood—of which the exciting and sustaining cause is not always so well known.

Contrasted, in some important particulars, with active congestion such as I have been describing, is that morbid fulness of the capillary vessels which arises when the return of the blood from them towards the heart through the veins, is impeded by some *mechanical* obstacle. With this *mechanical congestion* the veins are exclusively concerned.

Congestion of this kind may be strictly local. It may be confined to a single limb, when the principal venous trunk belonging to that limb is compressed, or otherwise diminished in size; and when no collateral and compensatory channels for the returning blood have been established. If there be disease of the liver, of such a nature as to prevent a free passage of the blood through that organ, congestion will ensue in all those parts of the capillary system from which the blood is conveyed by the veins that ultimately concur to form the vena portæ. The force of gravity alone is sufficient to produce venous congestion, and consequently congestion of the capillaries, in parts of the body in which, under ordinary circumstances, the circulation through the veins is aided, instead of being opposed by that force. If, for instance, the head be suffered to hang downwards for a certain time, we see the unequivocal signs of such congestion in the tumid condition and the purplish red colour of the lips, cheeks, eyelids, and ears. When an impediment to the free transmission of blood exists in the heart itself, a tendency to stagnation is produced, first in the venæ cavæ, then in the smaller ramifications by which these veins are fed, and at length in the general system of capillary vessels: and thus even general congestion may proceed from a physical cause; the parts that are the most vascular being also the most readily and the most completely gorged.

There is yet a third form of local congestion, differing, in some respects, both from active and from mechanical congestion. The capillaries become loaded, and the course of the blood in them is languid and sluggish, without any previous increased velocity of the blood in the arteries, and independently of any mechanical obstacle in the veins. To this form of congestion the term *passive* is applied. Andral denominates it passive or asthenic hyperæmia. I will tell you the class of facts from the observation of which the real existence of this passive plethora has been ascertained.

In persons enfeebled by age, or by disease, the lower parts of the legs, the insteps and ankles, and the skin which forms the surface of old scars, are often habitually purplish, or violet coloured. There is congestion of dark blood in those parts. You may, perhaps, be ready to ascribe this to the mere influence of gravity upon the blood, but this cannot be the whole explanation, because the force of gravity is the same with all persons, and at all ages. A horizontal position of the limb will perhaps diminish the livid redness, or may even sometimes entirely remove it. But the depending position ought not to cause it, and would not cause it, if the blood-vessels were in a healthy condition. Neither can the difference of posture be any source of *irritation* to the congested part. The capillaries themselves appear to have lost, in a great degree, their natural elasticity; they easily dilate under the pressure of the blood, which, being thus retarded, accumulates in the part. The employment of friction, or some stimulating application, will often remove this congestion.

I say all this is often to be noticed when there has been no cause of irritation operating

upon the part, and no preceding state of active congestion. But it is important to mark the very frequent connection that exists between these contrasted conditions. The one very often succeeds the other: the vessels become dilated under the force of the active hyperæmia, and, the irritation ceasing, they do not at once recover their tone, but remain passively loaded and distended. They are frequently left in the same state upon the subsidence of inflammation.

Take another illustration from what you may any day witness in respect to indolent ulcers. You will find that the large, flabby, and livid granulations which they present, may be made to contract, and to assume a more healthy and florid hue, by local stimulants; these evidently act by quickening the previously languid circulation, and unloading the congested capillaries.

Observe, again, what not unfrequently happens in regard to the eye; a little organ, indeed, but one that supplies us with more striking lessons in pathology and therapeutics than any other portion of the body. You know that the conjunctiva and sclerotica, through which, while healthy, colourless fluids alone circulate, are traversed, under various forms of disease, by innumerable vessels bearing red blood. Now it is notorious that, in certain cases, the application of any stimulating substance to the surface of the organ will increase the existing redness, multiply the number of visible vessels, and aggravate the complaint. These are cases of active congestion, dependent upon irritation that is still subsisting. But it is equally well known to practical men that the blood-vessels of the eye are liable to congestion of a very different kind. They are seen to be distended, somewhat tortuous, almost varicose, and the redness has a browner tinge, and is less vivid, than in the former case. In this kind of vascular fulness—or in this *stage* of it, for it sometimes succeeds to active congestion—emollient applications do harm rather than good, while strongly astringent and even irritant substances will often promptly dissipate the vascularity. These, again, are cases illustrative of congestion of the asthenic or passive character. The strong topical irritants restore the feeble and relaxed vessels to their natural elasticity, stimulate them to contract upon their contents, and to force onwards the red blood, which they cease to admit from the arteries; and the redness vanishes.

In the production of *active* congestion the arteries appear to be principally concerned; in the production of *mechanical* congestion, the veins: in *passive* congestion, the capillaries—which, strictly, are neither arteries nor veins, but lie between the arteries and the veins—are the vessels chiefly in fault.

If we turn our thoughts from the visible textures of the body to those which are hidden internally, we shall find reason to believe that these also are equally liable to similar conditions of passive congestion. Take those exceedingly vascular organs, the lungs, through which the whole of the blood circulating in the living body has to to pass. The lungs, as might be expected, are *very* liable to congestion and engorgement of their capillary vessels. Oft times this is clearly active, and the result of some irritating cause. But it is not always so. Many of you recollect the epidemic disorder called the influenza, which was so prevalent here in the spring of 1831, and again in the early part of 1837. Among the most constant and striking characters of the disease were the symptoms of pulmonary catarrh; and it was remarkable how long, in some persons, these symptoms persisted. After the pulse had regained its natural frequency of beat, and when all fever had ceased, the patient would continue to breathe with constraint and some labour, to wheeze a little, to cough, and to expectorate mucus. As all febrile disturbance had subsided, and no further benefit seemed to flow from adhering to what is called the antiphlogistic system, it was a reasonable conjecture that this disappointing obstinacy of some of the symptoms might depend upon a lingering but passive congestion of the pulmonary mucous membrane. And the nature of the *juvantia* confirmed the truth of this conjecture. Tonics and stimulants, so far from aggravating the pectoral symptoms, speedily abated or removed them.

You cannot fail, I think, to perceive the important bearing of these distinctions between active and passive congestion upon our notions of disease and our choice of remedies. These distinctions are not to be discovered by the knife of the anatomist. You must take care not to confound a knowledge of pathology, in the proper sense of that word, with a knowledge of morbid anatomy. Pathology comprehends not only the visible changes of

structure which accompany disease, and are disclosed by death, but the processes by which those changes are effected in the living body, and the laws which govern those processes.

There is one important law ascertained in respect to both active and passive congestion —viz: that it is apt to *recur;* that those parts are most liable to suffer it, or inflammation, which implies it, that have suffered it before. We may often turn our knowledge of this general fact to good account, in what is termed the *prophylaxis* of disease—in devising means for warding off disorders.

I have stated that *active* and *passive* congestion sometimes occur in succession, the latter being a sequela of the former. So, also, it may be said of *passive* and of *mechanical* congestion, that they often exist *together*. If the capillaries of a part or organ be much enfeebled, the mechanical effect of the gravity of the blood may suffice to bring them into a state of congestion. It is thus that Andral explains the occurrence of a gorged condition of the posterior portions of the lungs (evinced by symptoms during life, as well as by inspection of those parts after death), in persons who, having laboured under no previous pulmonary affection, have been confined to a supine position by long-continued disease or debility. This state of the capillaries is called by Lerminier the "engorgement of position;" and by Laennec, "the pneumonia of the dying." It neither proceeds from irritation, nor has it the essential characters of inflammation; although it is apt to be considered an evidence of inflammation by the mere morbid anatomist.

Again, as active congestion, when continued or intense, is antecedent and conducive to inflammation; so is mechanical congestion, when it reaches a certain point, the prolific source of hemorrhage, and the almost constant precursor and immediate cause of a large class of dropsical effusions.

I spoke a little while ago of general plethora, as a state in which the whole mass of blood circulating in the body is excessive in quantity, and rich in quality—full of fibrin and of colouring matter, thick with globules. But the blood, as a mass, may be in excessive quantity, yet poor in its materials, serous, deficient in globules, and fibrin, and colour; and in this condition of the blood also, as we shall hereafter see, dropsies are apt to arise.

We have now, therefore, laid the foundation for the better understanding of those three great classes of disease—*Inflammations*, *Hemorrhages*, and *Dropsies*.

There is no region or organ of the body exempt from these diseased conditions and their consequences; and of each of them some general account must be given, before we come to consider the special diseases incident to the several parts and organs.

But previously to entering upon this general account of inflammation, of hemorrhage, and of dropsy, we have still some other preliminary matters of importance to discuss:—The causes and modes of death. The causes of disease. A sketch of the nature, classification, and import of symptoms.

Our inquiries hitherto have related to the manner in which the physical conditions of the various parts of the body are capable of being altered, and their functions disturbed or suspended, in disease. But how it happens that some of these alterations of structure, or interruptions of function, are incompatible with the further continuance of life, and put a stop to the working of the whole machine, is an inquiry of no less interest, though of a somewhat different kind.

LECTURE V.

DIFFERENT MODES OF DYING. PATHOLOGY OF SUDDEN DEATH. DEATH BY ANÆMIA, ITS COURSE, PHENOMENA, AND ANATOMICAL CHARACTERS. DEATH BY ASTHENIA, ITS COURSE, PHENOMENA, AND ANATOMICAL CHARACTERS. SYNCOPE. DEATH BY INANITION. DEATH BY APNŒA: DEATH BY COMA: THEIR COURSE, AND PHENOMENA, AND THE ANATOMICAL CHARACTERS COMMON TO BOTH. APPLICATION OF THE PRINCIPLES OBTAINED FROM THE INVESTIGATION OF THE PHENOMENA OF SUDDEN DEATH, IN ELUCIDATING THE SYMPTOMS AND TENDENCIES OF DISEASE.

I PROPOSE to devote the present lecture to the following inquiry:—wherefore it is, and how it is, that some of the corporeal changes which we have been considering, or the diseased conditions connected with those changes, come to be incompatible with the further continuance of life? how is it that they put an end to the working of the living animal machine? why the machine should not continue to work, though, perhaps, imperfectly, *notwithstanding* such changes?

When our watches stop, we take them to a watchmaker to ascertain why they have stopped. The watchmaker knows that there are various ways in which the movements of the instrument may have been arrested. The mainspring may have broken: or the little chain that connects the barrel and the fusee may have snapped: or the teeth of some of the wheels may have become inextricably entangled: or the watch may have ceased to go (as the saying is) simply because it has not been wound up. Now the examination which the watchmaker undertakes in respect to the watch, I am desirous of making in respect to the human body. I am going to inquire into the several processes and modes of dying—the steps, or ways, by which the vital functions of the body are extinguished. A very little experience in the sick chamber, or in the wards of a hospital, will suffice to teach you that, although all men must die, all do not die in the same manner. In one instance the thread of existence is suddenly snapped; the passage from life, and apparent health perhaps, to the condition of a corpse, is made in a moment: in another the process of dissolution is slow and tedious, and we scarcely know the precise instant in which the solemn change is completed. One man retains possession of his intellect up to his latest breath: another lies unconscious, and insensible to all outward impressions, for hours or days before the struggle is over.

We seek to ascertain the laws and mechanism of these mysterious differences.

The inquiry is not one of merely curious interest, but has a direct bearing upon the proper treatment of disease. It will teach us what we have to guard against, what we must strive to avert, in different cases. In speaking of particular diseases, I shall constantly refer to the facts and reasonings which I am now about to lay before you.

Conditions of life.—In pursuing this inquiry, we need not go into any deep physiological disquisition respecting the conditions that are essential to life. It is sufficient for our purpose to remark that life is inseparably connected with the continued circulation of the blood. So long as the circulation goes on, life, organic life at least remains. When the blood no longer circulates, life is presently extinct: and our investigation of the different modes of dying resolves itself into an investigation of the different ways in which the circulation of the blood may be brought to a stand.

Observe the ample provision that is made, in the construction of the body, for carrying on and maintaining this essential function. First, there is an extensive hydraulic apparatus distributed throughout the frame, and consisting of the heart and other blood-vessels. Next, there is a large pneumatic machine, forming a considerable part of the whole body, and composed of the lungs, and the case in which they are lodged. Lastly, the power by which this machine is to be worked and regulated is vested in the nervous system. Each

of these systems must continue in action, or the circulation will stop, and life will come to an end. The functions they respectively perform are, consequently, called vital functions: and their main organs—the heart, the lungs, the brain (by which I understand the intercranial nervous mass)—are denominated vital organs. The functions of any one of the three being arrested, the functions of the other two are also speedily extinguished. But the phenomena of dying vary remarkably according as the interruption begins in the one or the other organ. Hence Bichat, who, in his *Recherches sur la vie et la mort*, laid the foundation of the distinctions I am about to describe, spoke of death beginning at the *head*, death beginning at the *heart*, and death beginning at the *lungs*. This nomenclature is, however, unsatisfactory and insufficient, as you will presently perceive.

That the heart may continue to propel the current of the blood, two things are necessary: first, a certain power or faculty of contraction; and, secondly, a sufficient quantity of blood in its chambers, to be moved, and also to stimulate them to contract. If this, the proper stimulus to the internal surfaces of the heart, be withheld, or much deficient, it will soon cease to beat. There are plainly, therefore, two ways in which death might be said to begin at the heart; and these require to be distinguished.

The respiration is entirely subservient to the circulation of the blood. The two organs, the heart and lungs, respond intimately to each other. The whole of the blood is sent by the right heart to the lungs, simply that it may there be submitted to the chemical action of the atmosphere. The respiratory apparatus is added to the body for the sole purpose of thus repeatedly ventilating the blood.

To this purpose also (setting aside all accidental impediments) two things are requisite: first, circumfused air to enter and depart at short intervals; and, secondly, alternating movements of the chest to cause its entrance and exit.

Now these movements, although they admit of being regulated by the will, are essentially involuntary. The ordinary acts of respiration depend upon a certain condition of the medulla oblongata. If this condition fails, the mechanical part of the respiratory process, and, consequently, the chemical part also, ceases.

The respiration hangs, therefore, directly upon the nervous system.

On the other hand, the action of the heart is not directly or necessarily dependent upon any constant nervous influence conveyed to it. The circulation goes on in an acephalous fœtus; it may be kept up, by maintaining artificial respiration, in a decapitated animal: nay, even when both brain and spinal cord have been abstracted from the body.

But though the nervous influence is not necessary to the movements of the heart—further than as it is necessary to the respiration, and to the introduction of nutriment—it has been clearly ascertained that very sudden and extensive injury or shock to the nervous system may instantly paralyse the heart, and so stop its action.

There are certain states, then, of the brain and nerves, which, without directly affecting the heart, bring the motions of respiration to a pause: and there are certain states of the brain and nerves which act directly on the heart and arrest its play. That is, there are two different ways in which death might be said to begin at the head.

Hence, I say, the nomenclature employed by Bichat is defective and inaccurate.

Pathology of sudden death.—In order to see clearly the steps by which the circulation, and with it life, finally terminates, in the various forms of dying, we must study the problem under its simplest forms. We must examine the cases in which the vital functions are, each in their turn, *suddenly* stopped, by some known cause, operating upon this or that vital organ. We must take advantage of the experiment (if I may so call it) which is performed before our eyes whenever a healthy man is cut off at once by external violence, or by poison, acting directly upon a particular organ or system of organs. The inquiry might be assisted, and, indeed, it has been mainly carried on, by experiments made upon living animals of a similar conformation with man. But the pathology of sudden death is happily now too well understood to require any further recurrence to that painful mode of "interrogating nature."

Death, as it takes place in disease, is usually complicated. Many parts are affected, and different functions languish, and various disturbing causes are in operation, at the same time. Occasionally, however, the process of dissolution is as simple and obvious as in death produced by violence; and in most cases some primary and predominant derange-

ment may be traced of this or that vital function; and a tendency is more or less clearly manifest to one or the other of the modes of dying, which we may now proceed to consider in succession.

Death by anæmia.—And first let us examine that form of death which is caused by a want of the due supply of blood to the heart. This is called, with much propriety, *death by anæmia.*

The best examples of death taking place in this way are those in which it is the consequence of sudden and profuse hemorrhage. The circulation fails, not because the heart has lost its power of contraction, but because blood does not arrive in its chambers in sufficient quantity.

We assure ourselves of this in two ways. In the first place, when the body of an animal is examined immediately after death from sudden and copious loss of blood, the heart is not found dilated, and full of blood, as it would be if it had ceased to act from a want of power to contract upon its contents; but it is found empty, or nearly so, and *contracted.* Secondly, this conclusion is confirmed by the reverse experiment: by the effect, I mean, of the *transfusion* of blood. It is a fact well ascertained, first by experiments made upon animals, and afterwards by most happy trials upon the human subject, that in cases of apparent death from violent hemorrhage the suspended functions may be restored by conveying a timely supply of blood into the vessels of the seemingly dead animal from the veins of a living animal of the same species. Now it is quite clear that this introduction of fresh blood could be of no avail in a case where the heart was unable to act upon the blood which had reached it.

The phenomena which attend this mode of dying are paleness of the countenance and lips, cold sweats, dimness of vision, dilated pupils, vertigo, a slow and weak pulse, and speedy insensibility. With these symptoms are frequently conjoined nausea, and even vomiting, restlessness and tossing of the limbs, transient delirium: the breathing is irregular, sighing, and, at last, gasping; and convulsions generally occur, and are once or twice repeated, before the scene closes.

It is thus that women often die, in whom "flooding" happens after childbirth. Sometimes the sudden bursting of an aneurism occasions this form of death. It is common on the field of battle, and in accidental injuries whereby large blood-vessels are wounded. Internal hemorrhage, depending upon diseases to be hereafter described, may also prove fatal in the same manner.

Death by asthenia.—This, then, is one form of death beginning at the heart. Another form, the converse of this, but spoken of also as death beginning at the heart, is that in which there is no deficiency of the proper stimulus to the heart's action, but a total failure of contractile power in that organ. This is well denominated death by *asthenia.*

Death occurring in this way is not uncommon. The effects of some kinds of poison furnish a good illustration of it. There are certain substances which, applied to some part or other of the body, speedily extinguish life: and when, after their fatal operation, the thorax is opened, each chamber of the heart is found to be filled with its proper stimulus, upon which it has been unable to contract.

This was distinctly made out by Sir Benjamin Brodie, in his able and scientific investigation of the effects of different poisons. You may read with advantage his papers on this subject in the Philosophical Transactions for 1811 and 1812. He ascertained, upon examining the chest after death occasioned by the *upas antiar*, that the heart was not empty, but full, there being purple blood in its right, and scarlet blood in its left cavities. These are the anatomical characters of this kind of death; and they prove that the action of the heart does not cease from a defect of stimulus, but from a loss of its contractile power.

Syncope.—The state of suspended animation common to both these forms of dying—(the ultimate external phenomena being nearly the same in each, and the result in each being the simple failure of the circulation)—is often expressed by the term *syncope.*

Besides the essential distinctions between them already mentioned, there is this further point of difference. In death by anæmia, the suspension of the functions of the nervous system arises from a lack of the blood which should be sent to the brain from the *heart.* Hence the well known effect of mere position. Syncope is sooner produced by venesection

when the person bled is sitting up than when he is recumbent: and the first remedy for the fainting state is to lay the patient flat upon the ground, or even to place his head a little lower than the trunk of his body. In the one posture the current of the blood towards the head is impeded by the force of gravity; in the other it is not. In sudden death by asthenia this order is reversed; the *nervous system* is the part first affected, and through it, consecutively, the heart. This appears from the fact that sudden death by asthenia is sometimes produced by causes which we know to act primarily upon and through the nervous system; by strong mental emotion—as intense grief, joy, terror. Cases of fatal concussion, where the brain is jarred by some bodily shock—and death occurring almost instantly from blows on the epigastrium—are of this kind. Lightning and electricity kill too, when they kill at all, in the same way. And we shall hereafter see that certain varieties of apoplexy, and several other diseased conditions, destroy life by suddenly arresting the contractile power of the heart.

When death by asthenia occurs more slowly, from disease, the phenomena are somewhat different. The pulse becomes very feeble and frequent, and the muscular debility extreme; but the senses are perfect, the hearing is sometimes even painfully acute, and the intellect remains clear to the last. The tendency to death of this form is remarkably manifest in acute inflammation of the peritoneum, in what is called malignant cholera, and in cases of extensive mortification.

Death by inanition.—Akin to this form of dying is that in which the living powers are slowly exhausted by lingering and wasting disorders, as in many cases of phthisis, in diabetes, and in dysentery; or by hemorrhages moderate in amount, but frequently repeated; or by any other long-continued drain upon the system; the death is partly, however, to be ascribed to a deficient supply of the natural stimulus to the heart's action. The type of these mixed modes of dying is seen in death by *starvation*, which may be considered intermediate between death by anæmia and death by asthenia. Death from inanition can never be sudden. The blood, renewed no longer from without, and fed only by absorption from the system itself, diminishes gradually in quantity, while its quality is probably deteriorated. Gradually also the contractile power of the heart, as well as of the muscles generally, is weakened; and from these combined causes its movements at length cease. Accordingly after death by starvation the heart is not found to be so much contracted, nor so nearly empty, as after death by sudden and copious hemorrhage.

Certain diseases of the throat or the œsophagus, prohibiting the introduction of food; of the stomach, preventing its retention; of the digestive organs generally, hindering its assimilation, are fatal in this manner.

We have yet to consider how death is produced by the suspension of the respiratory function—in other words, by a want of the due arterialization of the blood.

There are two perfectly distinct modes in which this cause of death may happen; distinct, *i. e.* in regard to the steps of the process, although identical in regard to the ultimate result.

1. When the access of air to the lungs is suddenly denied by some direct obstacle to its entrance;

2. When the muscular actions required for breathing cease in consequence of *insensibility*, caused by disease or injury of the brain.

The first of these two forms of dying is commonly called death by *asphyxia*. The second is conveniently termed death by *coma*. Bichat spoke of them respectively as death beginning at the lungs, and at the head.

Death by apnœa.—It is of much importance to get rid, when we can, of improper names. They are very apt to warp our notions concerning the real nature of the things they are intended to express. This term *asphyxia*, which is in every body's mouth, is very inappropriate, if we look to its etymology, to the kind of death which it has come to denote. It signifies, you know, literally, pulselessness, the want of pulse; and therefore it *might* express any kind of death whatever; or if applied to any particular *mode of dying*, it would seem to belong to that we have just been considering, namely, death beginning at the heart. And you will presently see that it is *peculiarly* inapplicable to all those cases where death results from the nonarterialization of the venous blood. Its current signification has, I am afraid, been too long established by custom, to allow of its being restored to

its proper meaning without much confusion. But, at any rate, I can and shall avoid its use, and adopt in preference the generic term *apnœa* (privation of breath) as justly expressive of the mode of death to which the word asphyxia is commonly given by authors. The generic English term is *suffocation*.

The entrance of air into the lungs may be prevented in various ways: by stoppage of the mouth and nostrils (*smothering*):—by submersion of the same inlets in some liquid (*drowning*); or in gases which, though not in themselves poisonous, contain no oxygen; such are hydrogen, and azote: by mechanical obstruction of the larynx or trachea from within, as by a morsel of food (*choking*), or from without, as by the bowstring, (*strangulation*; both these varieties are included in the term *throttling*):—by forcible pressure made at once upon the chest and abdomen, preventing all movement of the ribs and of the diaphragm; this happens sometimes to workmen employed in excavating, who are buried, their heads excepted, by the falling of a mass of earth; it was near happening, Dr. Roget tells us, to an athletic black man, of whose body a cast was attempted to be taken by one operation, and in one entire piece, as an academic model. "As soon as the plaster began to set, he felt on a sudden deprived of the power of respiration, and to add to his misfortune, was cut off from the means of expressing his distress. His situation was just perceived in time to save his life;" in this way the victims of Burke and Hare were stifled; and the same immovable state of the lung-cases is sometimes produced in tetanus, or by the poisonous influence of strychnine, all the respiratory muscles being fixed in rigid spasm:—by paralysis of the same muscles, from injury or disease of the spinal cord above the origin of the nerves that give off the phrenic nerve, and therefore above the origin of the intercostal nerves also; or from sections of the phrenic and intercostal nerves:—lastly, by such breaches in the walls of the thorax as admit air freely to the surface of both lungs, and spoil the pneumatic machine, as a pair of bellows is spoiled when deprived of its valve. Of course the same consequences ensue when both pleuræ become filled with liquid of any kind.

Whenever the privation of air is sudden and complete, the following external phenomena present themselves:—Strong but vain contractions occur of all the muscles concerned in breathing, and struggling efforts to respire are made, prompted by that uneasy sensation which every one has experienced who has tried how long he can hold his breath, and which, when unappeased, soon rises to agony: this extreme distress is transient, being almost immediately succeeded by sensations, not unpleasant, of vertigo, and then by loss of consciousness, and convulsions: at length all effort ceases, a few irregular twitchings or tremors of the limbs alone perhaps remaining; the muscles relax, and the sphincters yield: but still the movements of the heart, and even the pulse at the wrist, continue for a short time after all other signs of life are over; there is no *asphyxia* (properly so called) till the very last.

During this process, which does not occupy more than two or three minutes, the face at first becomes flushed and turgid, then livid and purplish, the veins of the head and neck swell, and the eyeballs seem to protrude from their sockets; at length the heart ceases to palpitate, and life is extinct.

The internal changes, which correspond with and cause these outward symptoms, have been carefully studied, and accurately, though slowly and lately, ascertained. They all proceed from the prevention of the chemical alteration naturally produced in the blood, in the capillary vessels of the lungs. The blood, continuing venous, passes at first in considerable quantity through the pulmonary veins, into the left side of the heart, and thence, through the arteries, to all parts of the body. This venous blood is, however, inadequate to sustain, or sufficiently excite, the functions of the parts it thus reaches. In the brain the effect of the unnatural circulation is felt at once; and shown by the convulsions and insensibility that ensue. The motion of the blood in the pulmonary capillaries is also, from the first, impeded, and its current gradually retarded, until it stagnates altogether; the lungs remaining full, the right chambers of the heart distended, and therefore less capable of contracting. Meanwhile the black blood, flowing more and more tardily and scantily into the left chambers, leads by its unnatural quality, as well as its deficient supply, to feeble contractions; and this side of the heart is comparatively empty.

In this state, even after the heart has ceased to beat, but not *long* after, if the cause which

has excluded the air be withdrawn, and fresh air readmitted—in other words, if artificial respiration be instituted—the blood in the pulmonary capillaries undergoes the required change, becomes arterial, begins again to pass onwards, and by degrees the circulation is restored, and the patient saved.

In this mode of death, the circulation is first arrested, and death truly begins, *in the lungs*.

When the carcass of an animal that has thus perished of apnœa is immediately afterwards examined, (so speedy an inspection of the human body being, for obvious reasons, seldom practicable or proper,) the left side of the heart is found to contain a small quantity of dark blood, while its right cavities are greatly distended, and the lungs, the cavæ, and the whole venous system, are gorged with blood of the same character. These are, in few words, the anatomical characters of sudden death by apnœa.

The pathology of this mode of dying has, I say, been thoroughly understood only of late. It will not be uninteresting, and may, I think, be useful, to trace briefly the successive steps by which the true explanation has been attained. Haller was of opinion that the quiescence of the lungs, consequent upon the cessation of the alternate movements of the thorax, formed a mechanical impediment to the further transit of blood through them; and that death resulted from obstruction of the circulation *in the lungs*. He was partly right; but he erred in supposing that the stream of blood was arrested absolutely, and at once, and by a mechanical obstacle. Apnœa, with all its peculiar phenomena, occurs, when atmospheric air is excluded, although the lungs continue to play; as in persons who breathe azote or hydrogen gas. It was clearly shown by Dr. Goodwyn, in his Essay upon the Connection of Life with Respiration, that the unaerated blood passed through the lungs, and entered the left auricle and ventricle of the heart; but he thought that it went no further. His notion was that arterial blood is the only stimulus which can excite the contraction of the left cavities of the heart, and that when venous blood arrives in them, the organ becomes motionless; and no blood being sent to the brain, the person dies. Had this theory been true, the left chambers would be found full of blood after death (which they are not,) and the mode of dying would not have differed essentially from that which we have already considered as death from asthenia. The well-devised experiments of Bichat carried the investigation a step further, and proved that the unaerated blood not only reached the heart, but was propelled by the contractions of that organ to every part of the body, through the arteries. Having applied a ligature upon the trachea of a living animal, he made a small opening in one of its carotid arteries. Presently the slender stream of blood that issued began to lose its florid tint, and to assume the dark colour of venous blood, but *it continued to flow*, and the afflux of this dark blood upon the brain was marked by convulsions and insensibility. Bichat conceived, therefore, the erroneous belief that the blood underwent *no* obstruction in its passage through the lungs, but that, remaining unpurified and venous, it acted *as a poison* upon every part to which it was carried by the arteries—first upon the nervous system, and ultimately (passing through the coronary arteries) upon the muscular substance of the heart itself. There are, however, two well-known facts, which upon this theory would be inexplicable—the comparative emptiness of the left chambers of the heart, and the restoration of the suspended functions by the timely performance of artificial respiration. The air could never reach and revivify or depurate the venous blood, stagnating in the capillaries of the heart. It was reserved for Dr. Kay to correct the unsound parts of Bichat's doctrine, and to show that the blood begins to stagnate in the capillaries of the lungs, in consequence of its failing to undergo the change from venous to arterial; and that the movements of the left heart are brought to an end, principally by the deficient supply of blood from the lungs. His experiments tend moreover to prove that venous blood circulating through the arteries has no directly poisonous operation, but is capable, though much less effectually than arterial blood, to support in some degree the irritability of the muscles. A muscle will continue to contract longer when supplied with venous blood by its arteries, than when supplied with no blood at all. Doubtless, in death by apnœa, the movements of the heart are weakened, partly in consequence of the imperfect stimulus afforded by the venous blood that penetrates its substance; but the main cause of the failure of the circulation is the difficulty with which the nonarterialized blood finds its way through the capillaries of the lungs. This theory is consistent with all the phenomena observed.

For a detailed account of the experiments and reasonings upon which Dr. Kay's conclusions are founded, I must refer you to his work on Asphyxia.

Sudden death by apnœa is not very often witnessed as the result of disease. It sometimes is caused by a spasmodic closure of the rima glottidis. It is no uncommon consequence of accidents, in which the upper cervical vertebræ are broken or displaced. I have seen several instances of death rapidly produced, with all the symptoms of sudden suffocation, generally in intoxicated persons, in whom the chink of the glottis has been found closely plugged by a fragment of meat, which "had gone the wrong way."

But there are numerous forms of more chronic disease, in which the tendency to death by apnœa is plainly discernible, sometimes for a long while before their fatal termination arrives. And the phenomena are similar in character to those which are noticed when the struggle is short. We hear the patients complain of the "want of breath;" we see how they labour to satisfy this want, when it becomes urgent, by the elevated shoulders, the dilating nostrils, the energetic action of all the muscles that are auxiliary to the respiration; we perceive by the dusky and loaded countenance, the livid lips and ears, and eyelids, that the blood is but imperfectly arterialized. The diminished capability of such blood to support the functions of the brain is made evident by the vertiginous sensations, and the delirious thoughts of the gasping sufferers; and after death we find the same distension of the right chambers of the heart, while the left are nearly empty—the same gorged condition of the pulmonary arteries and venous system generally, which constitute the anatomical characters of this mode of dying. These appearances are even more constantly visible in the dead body, when apnœa has been gradually produced, than after sudden suffocation; simply, I believe, because they are more *permanent*. After sudden death, however caused, the blood seldom coagulates; and the venous turgescence consequent upon rapid apnœa, although great at first, has time to subside and disappear before the body is examined.

In protracted cases, death does not take place purely in the way of apnœa; the heart is weakened, and the nervous influence impaired by the continued circulation of imperfectly arterialized blood; but the symptoms belonging to apnœa are plainly predominant.

When (as is most common) the privation of air is incomplete, and a scanty and insufficient supply is admitted, morbid changes take place in the lungs themselves; the air-tubes and cells become charged with serous fluid, which operates as an additional cause of suffocation. The same phenomenon is observed when the par vagum is divided on both sides.

Death by apnœa in disease is extremely common. It may be produced by any thing which narrows the chink of the glottis, by warts that sometimes grow there, by œdema of the submucous tissue of the larynx, by inflammatory tumefaction of its lining membrane: it may result from the presence of what are called false membranes in the windpipe and bronchi, such as are formed in the croup: it may be the consequence of disease situate in the substance of the lungs themselves, rendering them incapable of receiving the requisite quantity of air; of this we have examples in pneumonia, and in pulmonary apoplexy:—or it may proceed from disorders of the pulmonary mucous membrane, the air passages becoming blocked up with excessive and unnatural secretions, as in bronchitis. Phthisis is sometimes fatal in the way of apnœa; more commonly it tends to death by asthenia. Diseases of the pleuræ attended with effusion, and causing pressure upon the lungs; diseases of the heart and great thoracic blood-vessels, affecting the quantity of blood in those organs; even certain abdominal maladies, accompanied by swelling, and thrusting the diaphragm upwards—terminate by the same mode of dissolution.

Death by coma.—Death by *coma*, although common enough, and of much importance to be understood, need not detain us long. Certain morbid states of the brain (it is unnecessary at present to inquire into their nature and origin) produce stupor, more or less profound; the sensibility to outward impressions is destroyed, sometimes wholly and at once, much oftener gradually; the respiration becomes slow, irregular, stertorous; all voluntary attention to the act of breathing is lost, but the instinctive motions continue; the stimulus conveyed by the pulmonary branches of the eighth pair of nerves still excites, though perhaps imperfectly, the reflex power of the medulla oblongata, which sustains the involuntary movements of the thorax. At length this function fails also—the chest ceases to expand—the blood is no longer aerated—and thenceforwards precisely the same internal changes occur as in death by apnœa.

You will observe that the extinction of *organic* life takes place in exactly the same manner in both cases; the difference between the two forms of dying being this—that in the death by apnœa, the chemical functions of the lungs cease first, and then the circulation of venous blood through the arteries suspends the sensibility; whereas, in death by coma, the sensibility ceases first, and in consequence of this the movements of the thorax, and the chemical functions of the lungs, cease also. So that the circulation of venous blood through the arteries is in the one case the cause, and in the other the effect, of the cessation of *animal* life.

The causes that destroy the sensibility leave no constant or necessary traces of their operation. The essential anatomical characters of death by coma, and of death by apnœa, are therefore the same.

Death occurring in the way of coma has this peculiar kind of interest belonging to it, that it may sometimes be effectually obviated by a mechanical expedient. The circulation ceases because the actions of respiration cease—and the failure of the acts of respiration arises from a suspension of the nervous power. If it be merely a suspension—if the nervous functions are within the verge of recovery—organic life may be sustained by the performance of artificial respiration, until the insensibility has passed away; and thus the danger to life, which depended on that insensible state, may be escaped.

Many years ago, in the course of those researches to which I have already referred, Sir Benjamin, then Mr. Brodie, was led to think that by continuing the respiration artificially in animals labouring under the influence of narcotic poisons, the heart might be kept in action until the stupefying but transitory effect of the poison upon the brain had gone off. This idea he soon brought to the test of experiment, and the result was such as to justify his ingenious reasoning. He inserted some woorara into a wound which he had made in a young cat. After a certain time the respiration had entirely ceased, and the animal *appeared to be dead*, but the *heart* could be *still felt beating*. The lungs were then artificially inflated about forty times in a minute. The heart continued to beat regularly. When the artificial breathing had been kept up for forty minutes, the pupils of the cat's eyes were observed to contract and dilate upon the increase or diminution of light, but the animal remained perfectly motionless and insensible. At the end of an hour and forty minutes there were slight involuntary contractions of the muscles, and every now and then there was an effort to breathe. At the end of another hour the animal, for the first time, showed some signs of sensibility when roused, and made spontaneous efforts to breathe twenty-two times in a minute. The artificial breathing was therefore now discontinued. She lay, as if in a state of profound sleep, for forty minutes longer, when she suddenly awoke, and began to walk about.

Sir Benjamin Brodie had indeed been anticipated in this happy proposition for recovering persons apparently dead after taking narcotic poison, after submersion, and the like,—although he does not seem to have been aware of it. The experiment had once been tried before, and on the human subject, and with success, though not upon such scientific principles. The case is given by Mr. Whately, in the Medical Observations and Inquiries, vol. vi. A man who had swallowed an immense quantity of solid opium, and who seemed to be dead, was restored by the patient continuance, on the part of his medical attendants, in a process of artificial breathing.

It is seldom that we can hope for success from this expedient in the treatment of disease; simply because, in most cases, the injury of the nervous system, which has produced the insensibility, is irretrievable.

In most forms of apoplexy, and of hydrocephalus, death occurs in the way of coma. Sometimes, however, as I mentioned before, the lesion of the nervous substance is so extensive and sudden, as to operate like a shock, and cause death by asthenia. The tendency to death by coma is also strongly pronounced in sundry affections of the brain, both acute and chronic. These will form subjects for our consideration hereafter.

The several modes of dying, then, in cases of sudden death, are clearly enough made out. Let me briefly sum up the conclusions at which we have arrived. Life cannot be maintained without the circulation of arterial blood: and whenever a person dies, he dies either because *no* blood circulates through his arteries, or because *venous* blood circulates through them.

When it comes to pass that no blood is circulated through the arteries, we say that death occurs in the way of syncope; and this is of two kinds. In the one there is not blood enough received by the left side of the heart to stimulate its chambers to contract, or to be sent onwards by their contraction; in the other, there may be blood enough, but the heart has not sufficient power to contract upon it.

Also there are two ways in which death may be brought about in consequence of the circulation of venous blood through the arteries. In one of these, the first step is the sudden shutting out of air from the lungs; the blood which arrives in those organs is not aerated, or rendered arterial, but circulates again as venous blood, producing a failure of the animal functions, and weakening the muscles, till it finally stagnates in the capillaries of the lungs themselves. In the other, the animal functions are the first to suffer—insensibility occurs—the power which governs the actions of respiration is withheld—the breathing ceases—and organic life is extinguished as in the former case.

I trust you even already perceive that a right understanding of these matters is calculated to throw both light and interest upon our study of the symptoms, and of the tendencies of disease. It will enable us to aim with more precision at fulfilling the indication so often inculcated by Cullen, of "obviating the tendency to death." In this sketch I have merely been able to hint at the important bearings of such views upon our practice. My attention was first called to them by the lectures of my respected instructor, Dr. Alison, who was accustomed to illustrate them by reference to the phenomena of one large class of disorders. All the modes of dying that I have described are apt to take place in *fevers*. Sometimes we have to combat the one, sometimes the other tendency. The disease often proves fatal in the way of coma; this happens principally when the brain has been a good deal affected, when there has been much headache, delirium, and stupor: sometimes, when the lungs have been seriously implicated, life is extinguished in the way of suffocation or apnœa; and occasionally fever seems to terminate fatally in the way of syncope, especially when the stress of the disorder has fallen upon the bowels, when there has been continued diarrhœa, and ulceration of the intestinal glands. Or if death does not occur precisely in these ways, at least it resembles more, in different cases, sometimes one form of dying, sometimes another.

It is notorious that very different remedies, and even different plans of treatment, have been strongly recommended, in fever, by different practitioners. One probable reason of this is that one plan has been found the most proper to avert the fatal event in one form of the disease, and one in another. The tendency to a particular mode of death will prevail in, and characterize a whole epidemic. We shall resume these considerations hereafter: in the mean time the facts we have been reviewing may teach us the danger of applying, with too much confidence, the experience we may have gained of one epidemic to the treatment of another; and the risk we are sometimes liable to, of misjudging, and criticising unjustly, the practice recommended by other physicians, because it does not appear to accord with the results of our own observation.

LECTURE VI.

CAUSES OF DISEASE: DISTINCTION BETWEEN PREDISPOSING AND EXCITING CAUSES. ENUMERATION OF CAUSES, AS CONNECTED WITH THE ATMOSPHERE—FOOD AND DRINK—POISONS—EXERCISE—SLEEP—MENTAL AND MORAL CONDITIONS—HEREDITARY TENDENCIES—MALFORMATIONS. TEMPERATURE. EFFECTS OF HEAT AND OF COLD.

THE *causes of disease* are commonly arranged under three heads—as predisposing, exciting, or proximate.

Of these three, the last mentioned, or the *proximate* cause, is nothing else than the

actual disease itself—the actual condition of that part, or those parts of the body, from which the whole train of morbid phenomena essentially flows. When we know those parts, and that condition, we name the disease accordingly. When we do not, we call the complaint after the group of symptoms by which it is characterized. The term proximate cause is therefore an unnecessary term:—it is moreover, to learners, a puzzling term, and tends to give to the consideration of disease a scholastic and repulsive aspect. I wish you to get into the habit of contemplating the whole science of medicine under its simplest and plainest form. I am sure we may very well abolish the term proximate cause altogether; and having now given an explanation of its meaning, for your guidance when you meet with it in books, I shall never employ it, in these lectures, except perhaps in a quotation, again.

In strictness of language, one event is held to be the cause of another event which follows it, when the first being absent, the second never occurs—and the first being present, the second never fails to occur, unless some other event intervene to prevent it. But the causes of disease will not bear to be spoken of after so strict a fashion. We perceive that certain external circumstances (*quæ nos circumstant*) often precede such and such diseases; and that the diseases seldom happen when these circumstances are not previously observable; and we begin to regard those circumstances as exciting causes of those diseases. We find that the diseases are much more frequent among persons known to have been exposed to the agency of the presumed causes, than among persons who are not known to have been so exposed. The evidence at first is presumptive only. But the more uniform their conjunction, and the more rare their disjunction, the more confidently do we ascribe to the two consecutive events the relation of cause and effect. By this kind of observation a number of exciting causes of disease have been clearly established to be such.

But recollect, certain circumstances being present, such and such diseases do *often*, not *always*, follow. Some persons are more liable to be affected by the operation of many of these ascertained causes than others are; and the same persons more liable to be influenced by the same cause at one time than at another. And special circumstances, existing in particular cases, will be found to account for this variable operation of known exciting causes upon the bodily health. These special circumstances may properly be called *predisposing causes*. Thus, of a score of persons exposed to the same noxious influence—to the combined influence of wet and cold during a shipwreck for example—one shall have catarrh, another rheumatism, a third pleurisy, a fourth ophthalmia, a fifth inflammation of the bowels, and fifteen shall escape without any illness at all. A man shall do that with impunity to-day, which shall put his life in jeopardy when he repeats it next week. It is not therefore the exciting cause *alone* that in all cases determines the disease. Something —nay much—or all—will frequently depend upon the *condition of the body* at the time when the exciting cause is applied:—and this condition of the body, which we call *predisposition*, will depend upon circumstances then or previously in operation—and these circumstances are, in our language, *predisposing causes*.

Do not confound, as many seem to do, the predisposition with the circumstances creating it. The predisposition is a certain state of the body—the predisposing cause is what produces that state. The *cause* of the predisposition is the *predisposing cause* of the disease. A predisposing cause may therefore be *defined* to be any thing whatever which has had such a previous influence on the body as to have put it in a condition of greater susceptibility to the exciting cause of the particular disease.

It is sometimes difficult, or impossible, to say of a given cause whether it ought to be ranked among the exciting or the predisposing causes: whether it has prepared the system for being affected by some other agent, or whether it has itself produced the disease; but for the most part the distinction is real—and sufficiently well marked—and of great importance to be attended to.

Disease may sometimes be averted, even in despite of strong and fixed predisposition to it, if we know and can guard against the agencies by which it is capable of being excited. A man may inherit a proclivity to consumption, yet fortunately escape that fatal complaint by timely removal to a warm and equable climate, and by other suitable precautions; that is, by avoiding whatever tends to rouse the dormant tendency into action. On the other

hand, disease may often be warded off, notwithstanding the presence and application of its exciting cause, when its predisposing causes are ascertained and can be prevented. In proportion as the body is weakened or exhausted, it yields more readily to the pernicious influence of contagion, or malaria; but by obviating all causes of debility, and fortifying the system, we walk with comparative security amid surrounding pestilence.

Diseases sometimes occur, when no exciting cause, when no cause at all, has been apparent. All that we can say of such cases (which are not, however very frequent) is, that the causes have not hitherto been discovered.

Now the ascertained causes of disease are many and various. Whatever ministers to life, health, or enjoyment, may become the medium, under changing circumstances, of pain, disease, or death. The atmosphere, in which we are constantly immersed, is full of dangers. Both the organic and the inorganic world of matter around us abound in poisons; they lurk in our very food, which becomes pernicious when taken in excess, or when it consists of certain substances, or certain admixtures of substances: so that there really was much truth, as well as some humour, in the startling motto to Mr. Accum's book on adulterations—"*There is death in the pot.*" Our passions and emotions also, nay even some of our better impulses, when strained or perverted, tend to our physical destruction. The seeds of our decay are within as well as around us.

Let us enumerate, however, a little more particularly, the various known sources of disease, with the view of making, afterwards, a few practical comments upon some of them.

Chemical and mechanical injuries.—I shall pass over, in this enumeration, nearly all chemical and mechanical injuries—inasmuch as these belong to surgery.

Atmospheric influences.—Under the head of atmospherical causes, we shall find that those varieties in the state of the air which proceed from mere differences of degree in its natural qualities may be productive of disease. Such are—extremes of heat and cold; sudden variations of temperature; excessive moisture or excessive dryness; different electric conditions; differences of pressure, as measured by the barometer; a deficiency of light.

Again, the atmosphere may be a source of disease in consequence of its being loaded with impurities. Malaria—contagions of various kinds—and noxious gases in general, may be considered as so many poisons.

Nutriment.—Under the head of *nutriment* we may place the use of food of which the quality is bad and hurtful; this cause also strictly belongs to the class of poisons:—again, an insufficient supply of healthy food:—a still more common cause is *excess* in *eating* and *intemperance* in *drinking*.

Poisons.—The numerous *poisons* which are not comprehended under either of the foregoing heads are also prolific sources of disease.

Employments.—Another great class among the causes of disease might be formed by considering together the influence of various *trades and avocations* which are directly injurious to the health of those who pursue them.

Exercise.—We know, by ample experience, that a certain amount of bodily *exercise* is essential to good health. We see the evil consequences of much overstepping that amount, in the deformities and disorders that result from too severe and continued labour. But a much more numerous train of complaints follows the opposite state—that in which, from indolence, or from necessity, but *little* exercise is used.

Sleep.—Excessive indulgence in *sleep* on the one hand, and long continued want or interruption of repose on the other, are apt to give rise to serious maladies.

Mental and moral causes.—Very many diseases have a mental origin. Excessive intellectual toil—the domination of violent passions—the frequent recurrence of strong mental emotions—vicious and exhausting indulgences,—each and all will sap the strength, and grievously impair the health of the body: and perhaps there is no cause of corporeal disease more clearly made out, or more certainly effective, than a continuance of mental anxiety and distress.

Hereditary tendencies to disease, and malformations.—When we add to this catalogue of the sources of disease all those morbid tendencies which are *hereditary*—and those which flow from original *malformation*, and are congenital—we shall have a tolerably complete list of the manifold dangers to which our mortal frames are continually liable.

There are several points of view under which the considerable of these causes of disease might be shown to be interesting. We might inquire, for example, which of them are commonly predisposing, which exciting causes; and what are the circumstances which are found to render the same agent at one time merely a predisposing, and at another time an exciting cause.

We might also separate, with some advantage, those causes of disease to which the human body is often and necessarily exposed, from those which consist in agencies that are of local or temporary existence only. But without multiplying these artificial distinctions, I shall take occasion to advert to them either when speaking more in detail of particular causes, or when speaking of the disorders they have produced.

In our investigations into the causes of disease, great caution is necessary in order to avoid being misled by individual cases. The circumstances capable of influencing the bodily health are so various—so many of them are apt to be in operation at the same time—and so little power have we of excluding them, one after the other, so as to ascertain the exact efficacy of each—that our observations respecting their relative or their actual effects are open to much fallacy. We endeavour to escape this source of mistake by repeating and multiplying our observations. But it is by tracing diseases as they affect considerable masses of men, placed as nearly as possible under the same external circumstances, that we gain the surest and most satisfactory evidence in respect to the causes of disease. And hence it is that the experience of those medical men who are employed in our fleets and armies is so valuable. Dr. Alison has well remarked that all the circumstances of the whole number of men whose diseases fall under the notice of military and naval practitioners are, in many respects, exactly alike: the men are generally healthy adults in the first instance—the circumstances in which they are placed are thoroughly known to the observer—and indeed are often to a certain degree at his disposal; they are often suddenly changed also—and changed sometimes as to one portion of the whole mass of individuals, while they remain unchanged as to another portion: so that his opportunities of observation partake in some measure of the nature of experiments, and being made upon a large scale, they are especially interesting and conclusive. In point of fact a great deal has been learned, with absolute certainty, upon this subject.

Hitherto I have simply *enumerated* the principal causes of disease:—but conceiving a bare enumeration of this kind to be of but little use, I shall inquire somewhat more nearly into the nature and mode of operation of several of them *now:* of others I prefer to speak in connection with the particular diseases to which they give rise.

You will not consider the inquiry superfluous. To know the cause of a disease is sometimes to be able to *cure*, often to be able to *prevent* it. In some cases the cause is beyond our power, but an acquaintance with its nature may teach us how to moderate or to remedy its consequences. There are many diseases also over which medicine has very little control, but the causes of which, when ascertained, may be avoided, or extinguished. Such causes, when they do not happen to be removable by individual efforts, are often susceptible of extinction by the united measures of a community. And for this reason it is very desirable that correct opinions respecting the causes of disease should be widely diffused among the public;—and there is no way in which information of this kind is so likely to be made generally known, as by communicating it to medical students who are about to scatter themselves in all directions over the face of the land.

I shall proceed, then, in the first place, to the consideration of *heat and cold*, as external agencies capable of producing disease.

Temperature.—The range of atmospheric temperature compatible with human life is very considerable. Its limits are probably just those extremes of heat and cold that belong to the lower strata of the air in the different parts of the planet on which man is destined to exist. Under the burning sunshine of the tropics, and amid the profound frost of the polar regions, we alike find human dwellers. These different degrees of external temperature impress indeed peculiar physical characters upon those who are subjected to them, but they do not of necessity extinguish life, nor even cause disease. It requires more care, however, to preserve life under intense cold than under intense heat. In some parts of India the temperature ranges for a long time together from 80 to 100, and even 110° of Fahrenheit's thermometer: I believe it sometimes reaches 120°. We can form

some estimate of this heat by remembering the oppressive effect of the lowest of these temperatures—that of 80°—to which the thermometer sometimes rises in this country in the hot weather of summer. But these tropical climates are very thickly peopled. In the arctic countries, on the other hand—in the northernmost parts of America for example, where the sun appears above the horizon for a short part of the year only, and where the thermometer sinks to 40 or 50° degrees below zero—we still find inhabitants indeed, but they are few, and thinly scattered. This mainly depends, no doubt, upon the scanty supply of human food in those parts of the world; but something also is to be ascribed to the depressing influence of extreme cold upon the vital powers. Indeed the deficiency of human food is itself owing to the restraining effect of a low temperature upon organic life. Under a degree of temperature a little greater than that at the equator —or a little less than the lowest around the poles—it seems probable that man would soon perish. And in this fact we have one striking example of the adaptation of external nature to the physical constitution of the human race.

But, *for a short time*—and *under certain circumstances*—man is capable of enduring a very much higher degree of heat than the open and general atmosphere ever attains even in the hottest portions of the earth. Whether he could continue to exist, even for a little while, under a much more intense cold than ever occurs naturally on the surface of the globe, is more questionable.

External effects of heat.—It was long believed that the human body could not be safely exposed, even for a short time, to a degree of heat much exceeding that which is met with in hot climates. This belief, which we now know to have been erroneous, was strengthened by the result of some experiments made by the celebrated Fahrenheit himself, and related by Boerhaave in his Chemistry. Some animals were shut up in a sugar-baker's stove, where the mercury stood at 146°. A sparrow died in less than seven minutes, a cat in rather more than a quarter of an hour, and a dog in about twenty-eight minutes. The *noxious air* of the stove had probably more to do with the speedy deaths of these animals, than the heat. The truth, upon this subject, may be said to have been discovered by accident. In the years 1760 and 1761, MM. Duhamel and Tillet were appointed to devise some means of destroying an insect which consumed the grain in the province of Angoumais in France. They found that this could be done by subjecting the corn, and the insects contained in it, in an oven, to a degree of heat great enough to kill the insect, but not so great as to hurt the grain. In order to ascertain the precise heat of the oven, they introduced into it a thermometer placed upon the end of a long shovel. The mercury, when the thermometer was withdrawn, was found to indicate a degree of heat considerably above that of boiling water. But M. Tillet was aware that the thermometer had sunk several degrees as it was drawn towards the mouth of the oven. While he was puzzled to invent some way of determining more exactly the actual degree of heat, a girl, who was one of the attendants on the oven, offered to go in, and to mark with a pencil the height at which the mercury stood. And she did enter the oven, and remained there two or three minutes, and then marked the thermometer at 100° of Reaumur, which nearly equals 260° of Fahrenheit. M. Tillet then began to express some anxiety for the safety of the girl, but she assured him that she felt no inconvenience, and remained in the oven ten minutes longer, during which time the mercury reached the 288th degree of Fahrenheit's scale—which denotes 76° of heat above that of water when it boils. When she came out her complexion was considerably heightened, but her respiration was by no means quick or laborious. This experiment was afterwards repeated. Another girl remained in the oven as long as the former had done, at the same temperature, and with the same impunity. Nay, she even breathed, for the space of five minutes, air heated to about 325° of Fahrenheit—or 113° above that of boiling water.

The publication of these facts naturally excited the curiosity of scientific men, and other experiments were soon instituted. Dr. Dobson, of Liverpool, and several other persons with him, shut themselves up in the sweating room of the public hospital there, the air having been heated till the quicksilver stood at 224° of Fahrenheit. They did not experience any oppressive or painful sensation of heat. Dr. Fordyce and Dr. Blagden made some remarkable trials of the same kind. They entered rooms artificially heated to a very high degree, sometimes naked, and sometimes with their clothes on, and bore

the extraordinary temperature of 240°, and even 260°, for a considerable time, with very little inconvenience. In all these experiments it was found that the animal heat, as ascertained by thermometers placed under the tongue, or grasped in the hand, was scarcely increased at all; and the respiration but little affected: but the pulse was very much quickened. The frequency of Dr. Blagden's pulse in one instance was doubled. You may read a detailed account of these experiments in the Philosophical Transactions; but to give you a more lively notion of the degree of heat to which the bodies of these gentlemen were exposed, I may tell you that their watch-chains, and other pieces of metal about them, became so hot that they could scarcely be touched; when they breathed upon the thermometer, the mercury immediately *sunk* several degrees; each act of expiration produced a pleasant feeling of coolness in the nostrils, and they cooled their fingers by breathing upon them. In and by the same heated air which they respired, eggs were roasted quite hard in twenty minutes, and beef-steaks were dressed in thirty-three minutes; and when the air was blown upon the meat by means of bellows, it was sufficiently cooked in thirteen minutes.

It is ascertained, then, beyond all doubt, that the human body is capable of sustaining these very high degrees of temperature, *for a short time*, without detriment.

Facts of this kind may, perhaps, appear to you rather curious than useful. Man is never submitted to any natural heat of the air even approaching towards that to which the authors of the experiments I have been describing voluntarily exposed themselves. But a knowledge of extreme cases always tends to throw light upon those that lie *between* the extremes; and the direct results arrived at in these philosophic inquiries are not barren of practical utility to members of our profession. It is not long ago that a man was found almost dead in an oven: he expired a quarter of an hour after he was taken to one of the borough hospitals; and an inquest was held upon his body. The newspaper report of the case (which is the only one I have seen) states the temperature of the oven to have been about 120°—a candle was melted by it in half a minute. Now prior to the trials just mentioned, exposure to such a degree of heat would have been held a sufficient cause of death. We now know (and it would be discreditable if we could not support our opinion in a court of law, or before a coroner, by a reference to authentic facts) that something else must have concurred in extinguishing life; and, in fact, it turned out that the man was *drunk* when he went into the oven.

But what are the effects upon the human frame of a high, yet less excessive, temperature of the air?

One very constant effect of heat is that of stimulating the *organic functions* of the body. We have seen that the temporary application of great heat accelerates remarkably the action of the heart; the pulse was uniformly found to be much increased in frequency in the persons who made trials of their powers of endurance in heated rooms. We have evidence to the same purpose in the annual changes that take place in the vegetable kingdom at a given place, the summer renewing its foliage, the winter checking and repressing it; and still more in the superior luxuriance of vegetation in warm climates as compared with cold. And the same observation applies to those functions which animals possess in common with plants. Towards the poles both man and the lower animals are smaller than at the equator. Linnæus remarks that the hares, partridges, and other animals which inhabit the northern climes, are considerably smaller in size than the same species in more southern countries. And Mr. Tooke, in his View of Russia, observes, "As we approach nearer to the north pole, both the animal and vegetable productions of nature become more and more stunted. The ordinary stature of the Samoyedes seldom exceeds four or five feet, and their whole exterior corresponds with their dwarfish size." The bodily stature in hot climates does not, I believe, exceed that which is proper to the temperate zone: but it is notorious that the body grows more rapidly, and that the sexual functions are more early developed, in proportion as we approach the equator: and this is to be attributed to the stimulus of heat acting upon the organic functions, and upon the circulating system in particular.

On the other hand, and in some sort as a contrast with this, we may observe that considerable heat, when applied for some time together, has a sedative or depressing influence

upon the *animal functions, i. e.*, upon the nervous system, causing languor and lassitude —want of energy—a disinclination to exertion, both bodily and mental.

Under favourable circumstances, and where due precaution is exercised, it is probable that a very high degree of natural temperature of the atmosphere may be borne with impunity. Sir James M'Grigor informs us (in his account of the passage of the army in 1801 from India to Egypt) that during the march over the sandy desert of Thebes, where the heat was *uniform*, though the thermometer in the soldiers' tents was as high as 118°, the health of the troops was equal to that which they had enjoyed in any former period in India.

But there are some forms of disease which are distinctly traceable to heat as their cause.

The effect of hot weather in promoting the cutaneous perspiration is notorious. By the same influence the hepatic function is rendered more active. Dr. James Johnson first, I think, distinctly pointed out the sympathy or consent that obtains between the liver and the skin, under varying conditions of external warmth. Whatever may be the explanation of the fact, experience has taught us that a high atmospheric temperature, when its operation is continued for some time, has a marked influence upon the liver, increasing the quantity of bile that is secreted, and altering its sensible qualities; and this disturbance of function is not unfrequently followed by inflammation of the gland itself. In this country we witness, almost annually, the effects of a succession of sultry weather, in those attacks of vomiting and diarrhœa which are so common towards the latter end of summer, and in the autumn, especially when the season happens to have been unusually hot, and which result, apparently, from the excessive quantity or the morbid state of the bile. The English cholera (a total different disorder from that which has of late years been called, most improperly, *the* cholera) is, as you know, so frequent and general in some years, as to be fairly considered and termed an epidemic disease. In tropical climates the same morbific operation of external heat is still more conspicuous; leading not only to violent disorder of the stomach and intestines, with the evacuation of large quantities of vitiated and acrid bile, but also to acute inflammation of the liver going on to suppuration, and the formation of large abscesses. These last conditions are extremely rare in this latitude. The yellow complexions of those who return to England after a long residence in India are to be attributed to that disordered state of the liver, and of its functions, to which persons are proverbially subject, and which has, in them, been brought on by the influence of a hot atmosphere, operating for a long space of time together. Hepatic affections, acute or chronic, are among the chief diseases to which Europeans, at least, are liable in that climate.

We have here an example of the distinction I wish you to notice between predisposing and exciting causes. The heated atmosphere stimulates unduly the secreting function of the liver. Now a secreting organ is never so apt to be affected by any exciting cause of inflammation as when the process of secretion is going on. This law, which I mention by anticipation, depends, no doubt, on the increased afflux of blood that accompanies the act of secretion. The excessive activity of the hepatic function constitutes thus a predisposition to inflammation of the liver. The hot atmosphere, which creates this predisposition, holds the place of a predisposing cause in respect to the inflammation that ensues; but the exciting cause is exposure to cold: one of the most common and best ascertained exciting causes of inflammation in general. You are not to imagine that there can be no such thing as exposure to cold in a climate where the temperature of the air is habitually above 80°. Dr. J. Johnson, in his book on Tropical Climates, observes that on the coast of Coromandel the temperature is steady by day, and the nights are hot; but yet, he says, nothing is more common than *exposure to cold* in this place. The European soldier or sailor, after the heat occasioned by his employments in the day, strips off his clothes, and lies opposite a window or port, his shirt wet with perspiration, to enjoy the sea breeze at night. And the same author tells us that the application of cold after or during perspiration commonly produces an attack of hepatitis in some one of its various forms. Now the effect of that kind of exposure here described does not depend upon the *actual* temperature, but upon the *sensation* that is produced, and the sensation depends upon the *relative* temperature; and there can be no doubt that, under the circumstances mentioned by Dr. Johnson, a strong sensation of cold would be occasioned, even by a sea

breeze as warm as 80°, or warmer. Changes of temperature seem to be as readily felt at one part of the thermometric scale as at another, and in which ever direction they take place. Dr. Walsh states that while sailing along the coast of Brazil, after having been long accustomed to a temperature of 72°, a strong breeze set in from the sea, and the thermometer fell to 61°; (*i. e.*, to what we should here call temperate;) "but," he says, "the sense of cold from the sudden transition of temperature was quite painful. After bearing it for some time shivering on deck, it became intolerable, and we all went below, put on warm clothing, and dreadnoughts,—and again appeared with thick woollen jackets and trowsers, as if we had been entering Baffin's Bay, and not a harbour under one of the tropics."

It is interesting to compare this statement with Dr. Parry's account of a change of temperature at the opposite extreme of the scale, and in the other direction. Having previously said that the thermometer had fallen to 13° below zero in the night of the 21st of October, he goes on thus:—"The wind veering to the southeast on the 24th and 25th, the thermometer gradually rose to 23°. I may possibly incur the charge of affectation in stating that this temperature was much too high to be agreeable to us; but it was nevertheless the fact, that every body felt and complained of the change. We had often before remarked that considerable alternations of the temperature of the atmosphere are as sensibly felt by the human frame at a very low part of the scale as in the higher. The difference consists only in this, that a change from —40 upwards to about zero is usually a very welcome one; while from zero upwards to the freezing point, as in the instance just alluded to, it becomes, to persons in our situation, rather an inconvenience than otherwise."

Besides the more gradual effects of great heat, direct or indirect, upon the human body, it sometimes operates distinctly as an *exciting* cause, and gives rise to more sudden attacks of illness. Persons who are exposed to the direct beams of a hot sun, especially during any labour or active exercise, are apt to be affected with what is called the sun-stroke, the *coup de soleil*, insolation: they fall down insensible, and often die in a very short time. This disorder is common among troops in long marches in India. It is a complaint of which the cause has long been known by the inhabitants of hot climates. There is a case of it related in the Bible. "And Menasses was her husband, of her tribe and kindred, who died in the barley harvest. For as he stood, overseeing them, and bound sheaves in the field, *the heat came upon his head, and he fell on his bed, and died* in the city of Bethulia."

Pathologists are not agreed respecting the intimate nature of this distemper; nor about the manner in which it destroys life. Some regard it as a sort of apoplexy; and hold that death takes place in the way of coma. But the most approved remedies of apoplexy—bleeding and other evacuations—have not proved successful in relieving it. The natives of India prefer the pouring of cold water upon the head to every other curative measure. Our army surgeons also found that stimulants—rum and water, for instance—answered better than depletion. I have never seen this affection, but I should conjecture that it is more akin to the state we call concussion than to true apoplexy. It would appear that the sun's rays act upon the brain like a shock. The nervous system is suddenly and extensively influenced, and the heart's movements arrested, as in syncope. One of Sir B. Brodie's experiments is in favour of this opinion. He placed a rabbit in a basket in an oven the temperature of which was not more than 150°, and it died in a few minutes without any apparent suffering. The heart was afterwards found distended with blood, on both sides, as after death by asthenia.

Great heat tends also to the production of certain cutaneous diseases: it is said that few Europeans escape, on their first settling in tropical climates, an eruption of pimples, attended with almost intolerable itching and prickling, and lasting for some weeks. It is called in India *the prickly heat*.

Effects of external cold.—Before considering that most prolific source of disease which is familiar to the commonest observation in sudden *transitions* of temperature, let us inquire what are the ascertained effects of extreme cold upon the human frame. Of course I use the term cold in its popular acceptation, as if it were something positive, instead of signi-

fying the mere privation of heat. It is much more convenient to speak of it in this way, and there is no risk of your being misled by my doing so.

Now this inquiry is of more practical interest to us than that which is concerned with the immediate effects of extreme heat. Even in this climate medical men are not unfrequently called upon, in cases of injury or death produced by intense cold, either to remedy the morbid conditions it has caused, or to explain the mode and probability of its operation in extinguishing life.

The effects of cold, as might well be imagined, are in many respects the direct opposites of the effects of heat. When its application is continued, it acts as a sedative upon the organic functions both of animals and of plants. This appears from the shrinking of the external parts: the superficial arteries become unable to transmit the blood in the usual quantity through the integuments. Hence the skin becomes pale, and contracting round the miliary glands and roots of the hair, exhibits a roughness which is compared to that of the skin of a plucked goose, and is technically called *cutis anserina*. By the same contraction of the smaller vessels, and diminished circulation, the extreme and projecting parts are diminished in size. Thus, rings which are tight on the fingers while the body is warm, drop off in cold weather—and even the shoes fall from the feet during extreme exposure. The heart and the whole arterial system becomes weak. I have already, when speaking of the contrasted operation of heat, illustrated the depressing influence of a low atmospheric temperature upon the organic functions, by referring to the dwarfish size of both men and the lower animals, as well as of plants, in cold regions. We have evidence of the same fact in the slow development of the functions, and particularly of the sexual functions, in cold climates as compared with hot; and in the winter torpor of certain animals, which is very analogous with the state of trees and shrubs in that season.

I need not tell you that to judge of the effects of mere coldness of the atmosphere we must take the case of the atmosphere *at rest*. The air is a bad conductor of caloric, and for that reason, a much lower, as well as a much higher degree of temperature, can be borne when it is in a state of quiescence, than when fresh portions of it are perpetually brought into contact with the surface of the body by currents of air. "With the thermometer," says Captain Parry, "at —55°," (a most fearful degree of cold you will observe, 55° below zero, *i. e.* 87° below the freezing point) "with the thermometer at —55°, and no wind stirring, the hands may remain uncovered for ten minutes or a quarter of an hour without inconvenience; while with a fresh breeze, and the thermometer nearly as high as zero, few people can keep them exposed so long without considerable pain." And speaking in another place of the cold, when the thermometer was 49° below zero; 9 or 10°, that is, below the point at which mercury freezes; he says, "The weather being quite calm, we walked on shore for an hour without inconvenience, the *sensation* of cold depending much more on the degree of wind at the time, than on the absolute temperature of the atmosphere as indicated by the thermometer. In several of the accounts given of those countries, in which an intense degree of natural cold is experienced, some effects are attributed to it which certainly did not come under *our* observation in the course of this winter. The first of these is the dreadful sensations *said to be produced in the lungs*, causing them to feel as if torn asunder when the air is inhaled at a very low temperature. No such sensation was ever experienced by us, though in going from the cabin into the open air, and vice versâ, we were constantly in the habit, for some months, of undergoing a change of from 80° to 100°, and in several instances 120° of temperature, in less than one minute: and, what is still more extraordinary, not a single inflammatory complaint (beyond a slight cold, which was cured by common care in a day or two) occurred during this particular period."

But when the cold air is in motion, in other words, when there is wind, so that fresh portions of cold air are brought, in succession, in contact with the surface; or when it is accompanied with moisture, or occurs under other circumstances favourable to its operation, and to be spoken of more particularly presently, then cold of a much inferior degree of intensity may very speedily occasion partial or total death. By partial death I mean the loss of vitality in certain parts of the body only—the ears, nose, fingers, toes, and feet. The parts thus affected are said to be frost-bitten: and the mode of managing such accidents falling within the province of surgery, I shall confine my remarks almost entirely

to the case where general death—death in its full and ordinary meaning, is either brought about, or impending, in consequence of exposure to cold.

One of the earliest effects of extreme cold upon the system at large has been said to be a remarkable and overpowering drowsiness. But I believe you will find that most or all of the persons in whom this torpor has been noticed had not only been exposed to severe cold, but had been using also a great deal of exercise; and perhaps the drowsiness ought to be ascribed, in some measure at least, to that exercise. They who attribute it to the cold alone, explain the comatose state in this way. They say that the chilling of the surface and extremities drives the blood inwards, causes it to accumulate internally, and increases the flow of blood towards the head. One thing, however, is certain, viz: that drowsiness is not a necessary consequence of exposure to severe cold, although it is a very common consequence. Dr. Currie, in his Medical Reports, gives a very interesting account of the shipwreck of an American vessel on the coast of Ireland, by which the greater part of the crew, fourteen in all, were kept for twenty-three hours in a great measure under water, the temperature of the water probably not exceeding 33 or 34° of Fahrenheit: and he states expressly that none of the men were drowsy, and that in no one of the three that perished was death preceded by sleep.

The overpowering tendency of cold when combined with fatigue, and perhaps under certain circumstances of intense cold alone, to induce sleep, was strikingly exemplified in what befell Dr. Solander among the hills of Terra del Fuego. The story, as given in Captain Cook's Voyages, is well known. Sir Joseph Banks and Dr. Solander had been out botanizing. On their return towards the ship, after various hardships, and after having travelled through swamps for a considerable way, the weather, which had been very fine, became gloomy and cold, with sudden blasts of piercing wind, accompanied by snow. Finding it impossible to get back to the ship before the morning, they resolved to push on through another swamp that lay in their way, into the shelter of a wood, where they might build a wigwam and kindle a fire. Mr. Banks (as he was then) undertook to bring up the rear. Dr. Solander, who had more than once crossed the mountains that divide Sweden from Norway, and who well knew that extreme cold, especially when joined with fatigue, produces a torpor and sleepiness that are almost irresistible, conjured the company to keep moving, whatever pain it might cost them, and whatever relief they might be promised by an inclination to rest. "Whoever *sits down*," said he, "will *sleep*, and whoever *sleeps*, will *wake no more*." Thus at once admonished and alarmed, they set forwards; but they had not gone far before the cold became suddenly so intense as to produce the effects that had been most dreaded. Dr. Solander was the *first* who found the inclination, against which he had warned others, irresistible, and he insisted on being suffered to lie down. Mr. Banks entreated and remonstrated with him in vain: down he lay upon the ground, although it was covered with snow, and it was with much difficulty that his friend kept him from sleeping. Richmond, also, one of the black servants, began to linger in the same manner. When he was told that if he did not go on he would in a short time be frozen to death, his answer was that he desired nothing but to lie down and die. The Doctor said he was willing to go on, but that he must first take some sleep; although he had but a short time before told the company that to sleep was to perish. Mr. Banks and the rest found it impossible to carry them, and there being no remedy, they were both suffered to lie down, being partly supported by some bushes, and in a few minutes they fell into a profound sleep. Soon after some of the people who had been sent forward returned with the welcome news, that a fire was kindled about a quarter of a mile on the way. Mr. Banks then endeavoured to wake Dr. Solander, and happily succeeded; but, though he had not slept five minutes, he had almost lost the use of his limbs, and the muscles were so shrunk, that his shoes fell from his feet. He consented to go forward with such assistance as could be given him; but no attempts to relieve poor Richmond were successful. He, together with another black left with him, died.

In many instances, before this complete torpor comes on, intense cold has a curious effect upon the nervous system, blunting the sensations, and confusing the intellect, and giving to the person exposed to it the appearance of one intoxicated. It is very necessary that you should be aware of this, for there is too much reason to believe that poor wretches who have been picked up by the constables in the streets at night, during periods of hard

frost, have been supposed to be drunk, when, in truth, they were only stupefied by the cold. Such a mistake is most likely to be fatal to them: instead of receiving the attention and treatment proper for persons in their condition, they are liable to be laid aside, by themselves, to sleep off their supposed debauch, and the morning finds them corpses. It is not at all improbable that some of you may be called upon to investigate such cases: and as actual instances are more readily impressed upon the memory than any general description, I will read you a short history illustrative of what I have just been saying, from Captain Parry's Journal.

"John Pearson*** had his hands severely frostbitten, having unfortunately gone without mittens, and a musket in his hand. A party of our people, most providentially, found him, although the night was very dark, just as he had fallen down a steep bank of snow, and was beginning to feel that degree of torpor and drowsiness which, if indulged, inevitably proves fatal. When he was brought on board his fingers were quite stiff, and bent into the shape of that part of the musket which he had been carrying: and the frost had so far destroyed the animation in his fingers on one hand, that it was necessary to amputate three of them a short time after."

It is what immediately follows this, that I was desirous of pointing out to your attention.

"The effect which exposure to severe frost has in benumbing the mental as well as the corporeal faculties, was very striking in this man, as well as in two of the young gentlemen who returned after dark, and of whom we were anxious to make inquiries respecting Pearson. When I sent for them into my cabin, they looked wild, and spoke thick and indistinctly, and it was impossible to draw from them a rational answer to any of our questions. After being on board for a short time, the mental faculties appeared gradually to return with the returning circulation; and it was not till then that a looker-on could easily persuade himself that they had not been drinking too freely. To those who have been much accustomed to cold countries, this will be no new remark, but I cannot help thinking (and it is with this view that I speak of it) that many a man may have been punished for intoxication who was only suffering from the benumbing effects of frost; for I have more than once seen our people in a state so exactly resembling that of the most stupid intoxication, that I should certainly have charged them with that offence, had I not been quite sure that no possible means were afforded them on Melville Island to procure any thing stronger than snow-water."

When persons in this state are suffered to sleep, and the operation of the cold continues, they become less and less sensible to external impressions, until death closes the scene.

LECTURE VII.

CAUSES OF DISEASE, CONTINUED. LAWS BY WHICH THE OPERATION OF COLD UPON THE BODILY HEALTH IS REGULATED. CIRCUMSTANCES THAT FAVOUR ITS INJURIOUS EFFECTS, AND RESPECT, FIRST, THE BODY ITSELF; SECONDLY, THE MANNER IN WHICH THE COLD IS APPLIED. MODIFYING INFLUENCE OF CERTAIN STATES OF THE MIND—OF SLEEP—OF HABIT. MEANS OF PROTECTION. INFLUENCE OF THE DIFFERENT SEASONS. IMPURITY OF THE AIR. HEREDITARY TENDENCIES TO DISEASE.

IN the last lecture I commenced the consideration of some of the *causes* of disease.

We learned, by the evidence of authentic facts, that the human body is capable of bearing a very *high* degree of external temperature, for a short time, without detriment—and even without much inconvenience: and we learned—also by the testimony of facts—that the body is equally well calculated to endure, under favourable circumstances, a very *low* degree of atmospheric temperature—or, to speak in popular language, a very intense degree of *cold*.

It appears also that a high, but not extreme, atmospheric temperature, when long continued, has a stimulating effect upon the *organic* functions, and a depressing or sedative effect upon the *animal* functions of the body. Long-continued heat predisposes the body to be injuriously influenced by exposure to cold: the diseases apt to follow such exposure, under such circumstances, being derangement of the hepatic functions—violent disturbances of the stomach and bowels, with a copious discharge of vitiated and acrid bile—and acute inflammation of the liver itself. As more direct consequences of exposure to extreme heat—in other words, as examples of disorders of which extreme heat sometimes proves an *exciting* cause—I mentioned the *coup de soleil*, and the eruption called *the prickly heat*.

With respect to external cold, I pointed out to you its depressing effects upon the organic functions of the body—and, when it becomes very intense, indeed, its directly sedative influence upon the animal functions also—producing a state resembling intoxication, overpowering drowsiness, and coma, especially when the cold has had an auxiliary in fatigue; and, ultimately, death itself.

I hardly need say that the effect of external cold upon the body, within certain limits of intensity and duration, is totally different from all this. When it is not intense—or when, though intense, it is applied for a short time only—or when its refrigerating and sedative properties can be sufficiently counteracted by exercise and warm clothing—cold becomes a *tonic*, stimulating, refreshing, and invigorating both mind and body. Instead of benumbing, it heightens the sensibility; instead of stupefying, it clears and sharpens the faculties, and inspires alacrity and cheerfulness of spirit; and in this way, among others, cold becomes a very important curative agent.

Here also, therefore, the contrast obtains; a high external temperature relaxes and depresses—a low one, under the circumstances just mentioned, braces and enlivens.

Nevertheless, exposure to cold is one of the most common causes of various complaints. Many or most of the internal inflammations acknowledge cold as their ordinary exciting cause. Acute rheumatism has, perhaps, no other origin. Apoplexy and palsy, and dropsy, are its frequent consequences. "With the exception," says Dr. Bateman, in his *Observations on the Diseases of London*, "with the exception of a small number of diseases occasioned by unwholesome occupations, and by the contagions, the great mass of human malady in this metropolis is referable to the climate or state of the seasons, and to intemperance; but, of these two causes, the vicissitudes of the weather, especially its cold, are by far the most prolific sources of mischief."

It must, therefore, to every one who is engaged, or likely to be engaged, in the practice of physic, be a matter of first-rate importance, and of great interest, to ascertain the circumstances under which the application of cold is the most prejudicial, or has the greatest influence upon the body—as well as the means by which the bad effects of exposure to cold may often, in a great degree, be prevented.

There are some short but valuable hints upon this subject in Cullen's First Lines. The late Dr. Currie, of Liverpool, was, however, the first person who distinctly pointed out the laws that regulate the operation of cold as a cause of health and disease.

Of the circumstances which favour the morbific effects of cold, some relate to the condition of the body itself, some to the particular manner in which the cold is applied. The former are predisposing circumstances; the latter accessary. We will glance at these in succession.

Predisposing circumstances.—It has long been a popular, as well as a professional axiom, that *sudden vicissitudes* of temperature are dangerous: that a *previous hot state of the body* augments the hurtful effect of the application of cold, either externally or internally. But the proposition thus broadly stated is not universally true. It is well known that the inhabitants of Russia are in the habit, while reeking from their vapour baths, of rolling immediately in the snow, or plunging into cold water, without suffering from the change. Sir Charles Blagden, describing some of the experiments which I mentioned in the last lecture, says, "During the whole day we passed out of the heated room (of which the temperature ranged from 240° to 260°) after every experiment, immediately into the cold air without any precaution. After exposing our naked bodies to the heat, and sweating most violently, we instantly went into a cold room, and staid

there even some minutes before we began to dress, yet no one received the least injury." And Capt. Scorsby, speaking of the arctic regions, tells us that he has often gone from the breakfast-room of the vessel, where the temperature was 50° or 60°, to the mast head, where it was only 10°, without any additional clothing, except a cap—"yet," says he, "I never received any injury, and seldom much inconvenience from the uncommon transition."

It is plain, therefore, that the proposition which assigns danger to sudden vicissitudes of temperature requires limitation. The effects of a sudden descent from one point to another in the scale of atmospheric temperature varies according to the state of the body at the time. Without going into any physiological discussion respecting the source of animal heat, I may just remind you of the faculty of evolving heat possessed by man and the warm-blooded animals; by which faculty very nearly the same degree of inward temperature is steadily maintained under very different degrees of outward temperature. If the external temperature be lower than that of the body, the caloric thereby carried off is speedily replaced, in a healthy adult, by this evolution of heat from within, aided by clothing, or exercise. When the external temperature approaches the standard heat of the body, sweat soon breaks forth, and the superfluous heat is removed by evaporation: for so constant is the internal evolution of caloric, that an atmosphere which does not as constantly abstract any of it is excessively incommoding: an external temperature of 98°, which is about the average heat of the blood in man, is, as you know, extremely oppressive. The terms hot, warm, cool, cold, as applied to the surrounding air, are regulated by the sensations that it produces upon the average of persons. If the heat be carried off as fast as it is generated, and no faster, no particular sensation is felt, and the bodily powers are neither stimulated nor exhausted. This equilibrium is maintained, (supposing that no extraordinary exertions are made) when the thermometer stands at 62°, or thereabouts. We call that point in the scale *temperate*. All degrees above that point, up to 70, are reckoned *warm:* all above 70, *hot*. Descending in the scale, we speak of the temperature denoted by any degree between the 60th and the 50th as being *cool;* and every lower degree of temperature is *cold*. I am speaking of the average of healthy men: for remarkable diversities occur among individuals in respect to the names which they assign, under the guidance of their sensations, to particular degrees of the thermometric scale; their sensations differing according to the power which their constitutions respectively possess of evolving heat. Now if this power of evolving heat, thus inherent in the system, be entire, and, active, and persistent—if it have not been weakened by any of those circumstances which are known to have the effect of weakening it —no peril need attend even violent alternations of external temperature. Unusual heat of the body at the time when the cold is applied, so far from implying danger, is really the condition of safety, provided the heat is steady and permanent. You may read, in Dr. Currie's book, numerous instances of the cold affusion being employed in the hot stage of fever, and particularly in cases of scarlet fever, not only with impunity, but with great benefit to the patient. The same holds true of the application of cold when the body has been heated by exercise—and, indeed, whatever may have been the cause of the increased heat—provided always that that cause remains steadily in action, that there is no local disease, and that the body is not fatigued and rapidly losing its heat. But if a person be already exhausted and weakened by exercise—if he be sweating and rapidly parting with his heat—and especially if the exercise is remitted, and he remains at rest immediately after the application of the cold—then it becomes highly perilous, and likely to produce internal mischief.

The more correct statement, therefore, respecting the application of cold is, that it is dangerous—not when the body is *hot*—but when the body is *cooling after having been heated*.

This principle applies alike, I say, whether the cold be applied externally or internally; to the surface of the body, or to the mucous membrane of the stomach. Very many instances are recorded of death taking place immediately after a copious draught of cold water. I believe it will be found that in all these cases, the body, after having been much heated and enfeebled by severe exertion, was losing its preternatural heat from profuse perspiration, and, in general, from the cessation also of the exertion by which

this heat was accumulated. Celsus was aware of the danger: "ex labore sudanti frigida potio perniciosissima est." The fatal influence of cold water thus applied was experienced, on a large scale, among the troops of Alexander the Great, upon their reaching the banks of the river Oxus, thirsty, fatigued, and perspiring from their toilsome march of forty-six miles across the scorching sands of the desert. According to Quintus Curtius, Alexander lost more of his soldiers on that occasion than in any one of his battles. "Sed qui intemperantius hauserant intercluso spiritu extincti sunt; multoque major horum numerus fuit, quam ullo amiserat prœlio." Dr. Currie relates a striking example, which fell under his own observation, of sudden death thus produced. A young man had been playing a severe match at fives, and had violently heated himself. When it was over he sat down on the ground panting for breath, and covered with profuse perspiration. In this state he called to a servant to bring him a pitcher of cold water just drawn from a neighbouring pump. After holding it in his hand a little while, till he recovered his breath, he put it to his head, and drank a large quantity at once. He laid his head on his shoulder, and bent forwards; his countenance became pale, his breath laborious, and in a few minutes he expired.

I may take the opportunity of telling you that the remedies to be administered, when life is in jeopardy from this cause, are warmth to the epigastrium; and laudanum, in free doses.

If death does not speedily follow the external or internal application of cold to the body under the untoward circumstances I have described, inflammation of some internal part is very apt to arise.

By attending to the principles now laid down, you will be enabled to furnish those whom it may be your business to advise with many useful suggestions, and to caution them against some common mistakes: mistakes which have had their origin in the unqualified credit that has been given to the maxim, that sudden vicissitudes of external temperature, and exposure to cold while the body is hot, are dangerous; whereas, these things are dangerous only under certain circumstances. Thus, you may tell the sportsman that wet feet, or a wet skin, need cause him no apprehension, so that he continues in active exercise; and changes his clothes, and avoids all further application of cold, as soon as the exercise is over. You may admonish the bather, that after walking in a hot day to the river's side, he had better *not* wait to cool himself a little, before he plunges into the stream; and in like manner you may venture to advise the young lady who has heated herself with dancing, not to linger in the entrance hall till the glow has somewhat subsided, but to make the best of her way to her carriage, and thence to her bed; and you may tell your male friends, who happen to be similarly circumstanced, that the best thing they can do is to walk briskly home in their great coats. The main points to be remembered are, that the "heat which is preternaturally accumulated by exercise is held with little tenacity, is dissipated by profuse perspiration, and is speedily lost when to this perspiration is added a state of rest after fatigue;" and that, in these circumstances, the application of cold is most apt to be prejudicial.

Among the circumstances which favour the morbific effects of cold, and relate to the condition of the body itself, is to be included—for reasons that will now be obvious to you—whatever has the effect of weakening the system, and so diminishing its capability of evolving heat. The most common of these debilitating circumstances are enumerated by Cullen—"fasting, evacuations, fatigue, a last night's debauch, excess in venery, long watching, much study, rest immediately after great exercise, sleep, and preceding disease." All these, you will observe, tend to lessen the vigour of the circulation, and to depress the power of generating heat. Consistent with the same principle is the fact ascertained by M. Edwards, that the faculty of evolving heat is very feeble in old persons, and in the newly born; it being in these classes that we find the greatest number of victims to the power of cold.

The bad effects of cold upon the system depend partly upon the intensity of the sensation it produces—but still more upon the duration of that sensation. We are seldom the worse for a momentary sensation of cold, however lively it may have been; whereas even slight feelings of chilliness, if long protracted, are apt to terminate in some form of disease.

Accessary circumstances.—By the help of this principle we may explain most of the circumstances which, relating to the manner in which the cold is applied, have been found by experience to aggravate its hurtful influence.

Cold is more likely, *cæteris paribus*, to prove injurious when it is applied by a wind, or current of air. The sensation of cold is sustained by the continual accession of fresh particles of frigid air to the surface of the body. Some striking facts in illustration of the refrigerating and depressing effects of a stream of cold air were mentioned in the last lecture.

Again, the injurious operation of cold is augmented, when it is accompanied with moisture. Wetness is notoriously the worst way in which cold can be applied. The contact of wet or damp clothes with the skin both increases and prolongs the sensation of cold. For the same reason a cold foggy atmosphere is more prejudicial than a clear, and therefore drier one, of the same temperature. The heat of the body is abstracted more rapidly than it is generated from within, and if it be not replaced by exercise, or cordials, the balance of the circulation is deranged, and internal mischief often follows.

Counteracting circumstances.—The same principles serve to illustrate the effect of certain other circumstances, adverted to by Cullen, as being counteractive of the morbific tendency of exposure to cold: "passions engaging a close attention to one object"—"that state of the body in which sensibility is greatly diminished, as in maniacs,"—and "the power of habit." These circumstances are worthy of a moment's notice.

Abstraction of mind.—Impressions that are unheeded are unfelt and inoperative. As it is scarcely possible, when the attention is engrossed by bodily pain, to carry on any connected train of thought—so on the other hand the senses become impassive in proportion as the mind is fixed upon some absorbing subject of reflection, or enchained by some powerful emotion: impressions made upon the organs of sense are no longer taken notice of; the corresponding sensations, if they are excited at all, are not remembered, and the effect of such impressions is as if they had never been; they are not followed by the usual consequences. Persons gasping for breath in spasmodic asthma will remain for hours at an open window, with scarcely any clothing, during severe frost, and without suffering from the cold: their attention is so anxiously and exclusively bestowed upon the distress in their breathing, that the coldness of the air is unnoticed and unperceived, and has no sensible effect. "For where the greater malady is fixed, the lesser is scarce felt." The morbific effect of cold upon the system is certainly modified by the degree of attention that is paid to the sensation it excites.

Insanity.—Upon the very same principle may be explained the impunity with which some maniacs undergo exposure to cold—even when suffering no fever which might regenerate the lost heat. "I have seen," says Dr. Currie, "a young woman, once of the greatest delicacy of frame, struck with madness, lie all night on a cold floor, with hardly the covering that decency requires, when the water was frozen on the table by her, and the milk that she was to feed on was a mass of ice."

Sleep is enumerated by Dr. Cullen among those conditions of the body which diminish its power of resisting cold. And certainly cold is very readily caught, (as the phrase is,) when its causes are present, during sleep. But while we sleep sensation is in a great measure suspended. This would seem, therefore, to furnish a contradiction to the principle that the effect of cold upon the bodily health depends upon the strength and the duration of the sensation excited by it. Dr. Alison—I speak from recollection of his observations heard many years ago—disposes of this difficulty by affirming that the sleeper who thus suffers does really feel, and is conscious of, the sensation of cold, and that it mingles with and probably suggests his dreams. Lord Brougham, in his *Discourse of Natural Theology*, gives a very lively picture of dreams so excited—drawn, as I should guess, from his own experience. Probably something of the same kind has occurred to most of us. "Every one knows (he says) the effect of a bottle of hot water, applied during sleep, to the soles of the feet: you instantly dream of walking over hot mould, or ashes, or a stream of lava, or having your feet burnt by coming too near the fire. But the effect of falling asleep in a stream of cold air, as in an open carriage, varies this experiment in a very interesting, and indeed instructive manner: you will, instantly that the wind begins to blow, dream

of being upon some exposed point, and anxious for shelter, but unable to reach it; then you are on the deck of a ship, suffering from the gale—you run behind a sail for shelter, and the wind changes, so that it still blows upon you; you are driven to the cabin, but the ladder is removed, or the door locked. Presently you are on shore in a house with all the windows open, and endeavour to shut them in vain;—or, seeing a smith's forge, you are attracted by the fire, and suddenly a hundred bellows play upon it, and extinguish it in an instant, but fill the whole smithy with their blast, till you are as cold as on the road."

Certain it is, that though while sleeping we are not sensible of, or (what perhaps is the same thing) do not remember, ordinary impressions, we are nevertheless conscious of unusual sensations; so that the facility with which we take cold during sleep is no real exception to the general law that the sensation produced by cold is concerned in its injurious effects.

Habit.—The last of the accessary circumstances mentioned by Cullen is "the power of habit." No one can doubt the effect of custom in enabling the body to resist the operation of cold, who has had opportunities of observing how differently an inclement temperature is borne by persons whose employments oblige them to live much under the open sky, as shepherds, sailors, stage-coachmen—and by such as pursue in-door occupations—mechanics, tailors, shopmen, and the like. Probably the sensibility of the surface is blunted by habitual exposure. We may believe too that the purer air breathed by the out-door labourer, and his more active life, confer a more vigorous state of health, and endow him with an ampler faculty of evolving animal heat. The fact is unquestionable; and we may sometimes turn our knowledge of it to good account, in gradually fortifying the system against the influence of cold that cannot be avoided. An ill-directed application of this principle has led, however, to grave errors, and cost many lives. You will now and then hear parents talk of *hardening* their children, by causing them to brave all sorts of weather, by teaching them to be indifferent about variations of temperature, to sit in winter-time without a fire in the room, and to despise great coats, flannel, and other additions to their usual dress. Fearing to render them effeminate by over care and cockering, they run into the opposite and more dangerous extreme.

This process is often attempted with children originally delicate, and to such it is doubly hazardous. During the early periods of life the inherent protective power of evolving heat is comparatively feeble; and in this climate it requires to be carefully cherished.

The experiment of hardening should never be tried on any child or person that is ailing or unsound; that shows any signs of present disease; or any marked disposition to future, and especially the scrofulous, disease. Whenever it is tried it must be conducted in conformity with the principles already laid down. The subject of the experiment must be sufficiently clothed, and he must not fail to use such exercise during the exposure as may be requisite to excite and sustain the adequate generation of animal heat. An *abiding* sense even of chilliness must never be aimed at nor permitted.

The most direct and certain mode of fortifying the body against injury from accidental exposure to cold, is afforded by the use of the cold bath, and especially of the shower bath. When this is regularly taken in the morning, the surface of the body becomes enured to a degree of cold greater than it is likely to encounter during the remainder of the day. It is fortunate that we have an easy criterion of the propriety of continuing this expedient. When the sense of cold does not remain long, and is followed by a glow of warmth, the cold shower bath is sure to do good. If, however, after the bath, the person suffers headache, and continues long chilly, languid, and uncomfortable, it should at once be given up, as useless, and even hazardous.

By observing these simple rules, a healthy child may be made hardy also, without the risk, which their neglect would impose, of damage to his bodily fabric, and of abbreviating, by what was meant to prolong, his mortal span.

Influence of the seasons.—Closely connected with the effects of temperature upon the health is the influence of the different seasons of the year. A few remarks upon that influence, as it is witnessed in our own climate and country, may properly conclude our present subject.

It is open to the commonest observation that the general health of the community fluctuates with the changing seasons. Catarrhs, and coughs, and pectoral complaints of all kinds, are most apt to commence, or to grow worse, in the winter and spring months, while bowel complaints are more numerous amd distressing in the summer and autumn. The mucous membranes of the air-passages sympathize with the skin under the agency of external cold—those of the stomach and intestines under that of continued atmospheric heat.

The thoracic disorders are more serious and fatal than the abdominal. Various other maladies are likewise aggravated by cold, or by vicissitudes of temperature. Hence the mortality of the winter is always larger than that of the summer: unless indeed, this rule happens to be disturbed by the intervention of some widely-spread epidemic. I am not sure that the superior salubrity of the hotter over the colder portion of the year is generally acknowledged, even in this age of enlightenment.

It is the cold that, more than any other element of the weather, occasions the difference.

There are two small publications by the present Dr. Heberden, to which I would direct your attention, as being singularly instructive upon these points. One you will find in the eighty-sixth volume of the Philosophical Transactions: *Of the influence of cold upon the health of the inhabitants of London.* The other is a separate pamphlet: *Observations on the increase and decrease of different diseases.*

From the number of tables, framed chiefly upon the weekly bills of mortality, Dr. Heberden draws (in the last-named paper) the conclusion that the whole number of deaths is greatest in January, February, and March, and least in June, July, and August. This is contrary to the notions of the ancients—and perhaps of many of the moderns also. Celsus says, "Igitur saluberrimum ves est; proxime deinde ab hoc, hiems; periculosior æstas; autumnus longe periculosissimus."

In his paper in the Philosopical Transactions, Dr. Heberden compares the number of deaths that took place in London in January 1795, which was an unusually severe month, with the number that occurred in January 1796, which was an uncommonly mild month. Of those two successive winters one was the coldest, and the other the warmest, of which any regular account had been kept in this country. In the month of January 1795, the thermometer, upon an average, stood at 23° in the morning, and at 29°.4 in the afternoon; always, you will observe, below the freezing point. In the same month in 1796, it stood at 43°.5 in the morning, and at 50° in the afternoon; always much above the freezing point. The average difference in the two months was more than 20°.

In the five weeks beginning with January 1st, 1795, there were 2823 deaths: in the five weeks beginning with January 1st, 1796, there were only 1471. The difference, 1352, is enormous. The mortality of the former year was nearly double of that in the latter.

One object which Dr. Heberden had in view in making this comparison was to disabuse his countrymen of the notion that cold frosty weather is more favourable to health than mild weather in winter; a notion which has been embodied in the proverb, that "a green Christmas makes a fat churchyard."

It is very instructive to remark in what classes of persons the injurious effects of the severe weather of winter is most felt. The increased mortality was found to be chiefly among the very young, and the very old: in other words, among those in whom the recuperative power of generating heat is the feeblest.

In January 1795 there were in London 717 deaths of persons above sixty years old, while in January 1796 there were only 153 such deaths; or scarcely more than one-fifth of the former number.

I have often been struck by the unusual length of the newspaper obituaries during periods of hard frost; and by observing how many of the individuals whose deaths they record were far advanced in years. Dr. Heberden remarks that among persons older than sixty, the tide of mortality, as measured by the weekly bills, follows regularly the degree of coldness of the weather—so that any one accustomed to examine these lists may form a tolerably accurate judgment of the severity of any of our winter months, by noting the ratio of the mortality in persons above sixty.

The deaths from asthma (under which vague term all kinds of pectoral disorder attended with shortness of breath appear to have been included) were 249 in January 1795—only 29 in January 1796. In the former of these months there were 825 deaths attributed to consumption—in the latter 342.

All this accords with what I mentioned before of the effect of cold weather in producing or exasperating diseases of the respiratory organs.

One of the conclusions deduced by Dr. Heberden from the examination of the bills of mortality is, that "the number of deaths by palsies and apoplexies is in this country always greatest in winter." There are intelligible reasons for this. When the surface is chilled, and the blood driven out of the superficial vessels by the cold, it must accumulate in internal parts, and so press with increased force towards the head. And there is another reason for the frequency of these affections in the winter season: it is, as we have seen, the season of pectoral complaints, and of embarrassed respiration. Dyspnœa, and fits of coughing, greatly impede the return of the blood from the head through the veins: and cerebral congestion tends to the production of cerebral hemorrhage, especially when the arteries of the brain are diseased; and they often are so. Accordingly we find that in January 1795 there were fifty-two deaths from apoplexy and palsy —while in January 1796 the number was only thirty-one.

You may trace the influence of the seasons—not only in the prevalence of particular diseases in certain portions of the year—but also in the character of other disorders that are liable to occur in all periods of the year alike;—in the character, for example, of fevers. In the majority of cases of continued fever you will find that the pectoral symptoms are most troublesome in the spring, and the abdominal symptoms in the autumn. It is said also, but I do not know that this is so generally true, that affections of the head, in continued fever, are more frequent and severe in the winter, than at other periods of the year.

Impurity of the air.—Mere impurity of the air—by which I mean impurity that does not result from the admixture of any *specific* poison, such as the marsh poison, and the various contagions—is a powerful predisposing cause of disease. The prejudicial effect of impure air is seen, on a large scale, by comparing the inhabitants of great towns, in respect to health and longevity, with those who live in the country. In a valuable letter by Mr. Farr, appended to the *First Annual Report of the Registrar General*, a comparison is made of the mortality of about seven millions of persons, one half of whom dwell in towns, the other half in counties. "The concentration of the population in cities doubles the deaths from epidemic diseases, and disorders of the nervous system." In cities, as compared with counties, the deaths from consumption are increased 30 per cent, those from childbirth 71 per cent, those from typhus 221 per cent.

These differences we can explain only by attributing them to the weakening influence of impure air, and the want of sufficient exercise: for, as Dr. Alison has remarked, "it is hardly possible to observe separately the effect on the animal economy of the deficiency of exercise, and deficiency of fresh air, these two causes being very generally applied together. But it is perfectly ascertained on an extensive scale, in regard to the inhabitants of large and crowded cities as compared with the rural population of the same climate, that their mortality is very much greater, especially in early life—and the probability of life very much less." There is one circumstance which shows that impure air is the more noxious agent of the two, namely, the very great mortality, in towns, of children under two years of age, even although they get as much exercise as their time of life would allow of anywhere.

The noxious and depressing influence of vitiated air is made strikingly manifest by the effect of a removal to a purer atmosphere. We are continually obliged to recommend "change of air" to our patients. We advise them to go out of London, that their recovery from acute disorders may be accelerated, and that they may regain the degree of general strength which is necessary to the cure of many chronic complaints: of all those especially that require the use of *tonic* medicines, among which class of remedies no one is so effectual, in constitutions that have been weakened by a town life, as removal to the clear and pure air of the country.

It is necessary, however, to remember that although impure air has most unquestionably a very hurtful effect upon the general health, there is no specific disease which can be distinctly traced to it as an *exciting* cause. It is as a predisposing influence that the impurity operates. For instance, it never *generates* (as I believe) continued fever, yet it will most certainly aggravate the symptoms, and favour the propagation, and augment the mortality, of that, and of other diseases, in a great degree. If there be any diseased condition that is strictly the product of impure air, it is scrofula. Scrofula (as I shall presently show you) depends in part upon hereditary constitution; it partly arises also from exposure to cold and wet; but there is much reason for believing that impure air is a very powerful agent in calling scrofula into action, and in aggravating the strumous diathesis.

I have entered the more fully into the consideration of certain states of the atmosphere, its extremes and its variations of temperature, and its impurity, as causes of disease, because there is no part of the course in which I could more conveniently introduce them. Most of the other causes of disease, enumerated in my last lecture, will be discussed in connection with the disorders to which they give birth: malaria, for example, when I speak of ague; contagions, when we come to the exanthemata and to continued fever; improper or insufficient diet, and intemperance generally, with indigestion, and the disorders of the alimentary canal: and so on. There is, however, one remarkable predisposing cause of disease, a few observations upon which may serve to fill up the little that remains of the present hour. I mean, that disposition to certain diseases which is apt to descend from parents to children: *hereditary tendencies* to disease.

Hereditary predisposition.—We must distinguish between *susceptibility* of disease, and a *tendency* to disease. In one sense all persons are born with a predisposition to most forms of disorder. No one is protected by nature against inflammation, when the causes of inflammation come into play. Poisons of various kinds, and specific contagions, which indeed are poisons, operate with tolerable uniformity upon *all men* alike.

But there are certain complaints which we may separate in this respect from the others: which complaints some persons have a tendency to, and some have not. The tendency is sometimes strong and evident; sometimes feeble and faintly marked; sometimes it displays itself in the midst of circumstances the most favourable to health; sometimes it requires for its development conditions the most adverse and trying. To mention some of these diseases: scrofula, which I shall soon describe more particularly, gout, mania, and (I believe I may add) spasmodic asthma.

Not only is a disposition to these complaints strikingly pronounced in some persons, but other persons appear wholly free from such a tendency, nay even devoid of the susceptibility of them. Gout, in those *capable* of it, may be acquired by habits, as it may be repressed and prevented by the opposite habits. The habits that, in certain persons, bring it on, are the intemperate use of the luxuries of the table, and an indolent or sedentary manner of life: but there are many people in whom no amount of rich living or idleness will generate gout. So there are some in whom no exposure to impure air, cold and wet, and no privations--in other words, no appliance of the circumstances calculated to bring the strumous diathesis into play, will ever produce any form of scrofula—will ever render them consumptive, for instance; consumption being one of the most common and fatal shapes of scrofulous disease. There are many who never become afflicted with asthma, although surrounded by the most powerful exciting causes of that complaint.

Now with respect to these diseases, and perhaps a few others, it is matter of fact that they occur much more frequently in persons, some one or more of whose ancestors have suffered from them, than in other persons: the tendency is transmitted, is hereditary.

That the circumstances of the parents *do* influence the physical character of the children, no one can doubt: it is matter of daily observation; and one of the best possible illustrations of the fact is to be found in what are called family-likenesses. We see children resembling their father or their mother; or both parents at once, as mulattoes.

It has been suggested that the similarity in features and expression, and even in moral character, which cannot be denied to exist, may be ascribed to education and imitation.

But allowing something to that cause, it cannot be all. It was, I remember, a common remark when I was at Cambridge, that the followers and admirers of a very good man, the late Mr. Simeon, appeared to come at last to resemble him. So man and wife are sometimes fancied to grow like each other. That is, the same prevailing cast of thought and feeling—the idem sentire et idem velle—may give such an habitual expression and character to the countenance, as to superficial observers may constitute a likeness. But there are family-likenesses which will not admit of such an explanation as this: similarities in the shape or size, or disposition of peculiar features. Every one has heard of, or may remark in portraits, the hereditary thick lip of the Imperial House of Austria. Many persons now living have had the opportunity of tracing the lineaments of our own Royal Family through at least three generations. The sisters of one of our English Dukes are remarkably handsome young women, and bear, to this day, a striking resemblance to the portraits of their beautiful ancestress, the celebrated Nell Gwyn. And independently of the general cast of features, we trace these family likenesses in minute or unequivocal particulars—the colour of the hair and eyes, the shape of the limbs, the stature of the body, and so on: nay, in more decided peculiarities than these, in points of unusual formation. You have heard, probably, of the American calculating boy, Zerah Colburn. A great number of individuals of his family, descended from a common ancestor, had six fingers and six toes instead of five. The peculiarity was transmitted through four successive generations; and probably, could his pedigree have been farther traced, through many more. I am myself acquinted with a gentleman who had the misfortune some years ago to have a bastard child laid to his charge. At first he had some misgivings on the subject, and suspected that he might have no real title to the credit (or I should rather say to the discredit) of the imputed paternity; but all his scruples were satisfied when he found that the child had six fingers on each hand, for he had himself possessed two small supernumerary fingers, which had been amputated when he was an infant. Haller gives an account of a web-footed family, descended from a mother in whom that configuration existed.

Now there is one very curious circumstance observable in regard to these family-likenesses, namely, that they may fail to appear in the child, and yet appear in the grandchild; may skip over a generation or two; may, after lying dormant, break out, as it were, in some collateral branch of the family tree.

This not only proves that certain physical peculiarities may be transmitted, but it discloses this curious property, that peculiarities *not possessed* by the parent, may nevertheless be *transmitted* by him. And this evidently opens a wide field for the operation of hereditary tendencies. A person is not to consider himself as necessarily free from a disposition to consumption or gout, because his parents have never shown any symptoms of those disorders.

When one parent only bears the transmissible tendency, the disease appears to be most apt to break out in the children who most resemble that parent in their physical conformation and appearance: yet this is not a universal rule. I am acquainted with a gentleman who has lost several brothers or sisters by phthisis. The fatal disposition is known to exist on his mother's side, while his father's pedigree is believed to be quite free from it. All the children that have hitherto become consumptive have resembled the mother in bodily configuration and features, except this gentleman, who is like his father's family, but who, nevertheless, labours under unequivocal consumption.

It becomes a very interesting, and a very important question, whether *acquired* peculiarities can be transmitted. I have been told, by a gentleman attending the class, that he knew a man, who having been accidentally deprived of sight, afterwards propagated blind children. I believe, however, such an event is uncommon. Dr. Prichard is of opinion that all original or connate bodily peculiarities tend to become heriditary, while changes in the organic structure of the individual from external causes during life, end with him, and have no obvious influence on his progeny. Although this general law is probably true, I doubt whether it be yet sufficiently established by a reference to actual facts.

I need scarcely say a word respecting the importance to medical men, and indeed to all men, of a knowledge of these hereditary dispositions. Such knowledge ought to regulate, in some degree, the choice of persons wishing to marry. Where both parties have a de-

cided tendency to any complaint, there will be a double probability of a diseased progeny. Lawful intermarriages between members of the same family are often highly objectionable on the same score. Any inherent defect or morbid propensity is aggravated by what cattle-dealers call "breeding in and in."

Again, if it be known that in any family an hereditary proclivity exists; to gout and gravel, for instance, or to consumption; this knowledge ought to warn every individual of that family sedulously to avoid the causes that foster and develope these diseases; and medical men possessed of the requisite information may give most valuable advice and instruction on these points.*

LECTURE VIII.

SYMPTOMS. THEIR USES IN RELATION TO THE DIAGNOSIS, THE PROGNOSIS, AND THE TREATMENT OF DISEASES. SIGNS, AS DISTINGUISHED FROM SYMPTOMS. PATHOGNOMONIC, COMMEMORATIVE, DIRECT AND INDIRECT, SYMPTOMS. EXAMPLES OF SYMPTOMS AS THEY CONSIST OF UNEASY SENSATIONS, DISORDERED FUNCTIONS, OR CHANGES OF SENSIBLE QUALITIES.

WE are perpetually reading and talking about *symptoms:* and no wonder, for symptoms are the signals by which we learn that disease is present; the evidence upon which all our art proceeds. We are always, therefore, observing symptoms, analyzing them, striving to interpret their meaning, to ascertain what they signify. Without a knowledge of symptoms we can have no knowledge of the art of physic. Sagacity in penetrating the import of symptoms constitutes a great part of the skill of an able physician. We shall find it useful to take a cursory view of symptomatology, and to familiarize our thoughts with some of the cardinal symptoms themselves, before we speak of them in connection with particular diseases.

What do we mean by a symptom? συμπτωμα—"Something that happens concurrently with something else." Symptoms, they say, are *coincidences*, but this is merely translating the word συμπτωματα into English through the Latin. Symptoms are sometimes defined to be *morbid phenomena*—"any thing observed in a patient out of the course of health." But in forming our estimate of disease we must often take into account the functions that are natural: these have been said to furnish *negative* symptoms. For my own part, if I were called upon to define a symptom, I should say, "Every thing or circumstance happening in the body of a sick person, and capable of being perceived by himself or others, which can be made to assist our judgment concerning the seat or nature of his disease, its probable course and termination, or its proper treatment: every such thing or circumstance is a *symptom*."

And it is well that you should take notice at once, that it is for the three purposes just adverted to, that we cultivate the study of symptoms, viz.

First, To ascertain the seat and the nature of the disease under which our patient is labouring: in technical language to form the *diagnosis*. I am no great friend to technical phrases where they can be avoided without inconvenience; but in some cases short terms of art save us a great deal of tiresome periphrasis and circumlocution.

* During the passage of this lecture through the press, the *Second Annual Report of the Registrar General* has been circulated, by the obliging attention of Mr. Lister, among the members of the profession. It contains much that illustrates and confirms several of the positions here advanced. This immense yearly addition to the science of vital statistics is of the highest value. It cannot fail to throw light upon the sources, and to point towards the prevention, of many very fatal disorders. To ascertain the causes of any disease, and to display them before the public mind, are large steps towards their removal, so far as that may be practicable. The report itself, and Mr. Farr's able analysis of the registered facts, are replete with interesting and useful information.—T. W.

A *second* object of the study of symptoms is to enable us to foresee and foretell the probable course and issue of the disease; in other words, to frame the *prognosis*.

And a *third*, and most important use of a knowledge of symptoms, is to direct our *treatment* of the disease.

I suspect that the immense importance of the first mentioned of these three objects—the diagnosis or *recognition* of disease, is not always clearly seen, either by students or practitioners of medicine. Sometimes we are obliged to prescribe for a malady, although we are in great uncertainty, or in total ignorance, in respect to its nature or situation. But this is always unsatisfactory. On the other hand, when we have ascertained where and what the disease is, we apply with much more confidence, and precision, and comfort, those rules for its relief which we have picked up by our own observation, or have been taught by others. But this is a very limited view of the importance of an exact and true diagnosis. Diagnosis forms the indispensable basis of all *advances* in physic as a practical art. There is a vulgar proverb, that the knowledge of what a disease is, is half its cure. In one sense this may sometimes be true, but in another sense it is not so. Almost all that we know concerning the proper treatment of the sick is *originally* derived from observation, *not* of the *nature of diseases*, but of the *effects of remedies*. That rhubarb will purge, and opium lull to sleep, and loss of blood occasion faintness, are truths which experience alone could suggest, and successive trials alone confirm. They are purely empirical truths. No one could guess them beforehand. No skill in the discrimination of disease has even a tendency to teach them. In some few cases, indeed, we *see* that certain *mechanical* derangements exist, which are manifestly capable of *mechanical* relief. When parts of the body are displaced, as in herniæ and dislocations; or when distension and pressure are evidently produced by accumulated fluids; the mechanical remedies are at once suggested by the physical and obvious faults. But with such exceptions, diagnosis does not, of itself, afford us *any* direct information as to the cure of diseases; but it does *this*—it defines and fixes the objects about which observation is to be exercised, and experience collected. When we can once identify a given diseased condition, we obtain the privilege of watching the behaviour of that diseased condition, again and again, under the operation of therapeutic measures; and from that time the increase of our knowledge concerning the appropriate management of that particular disease becomes progressive and sure. The term experience is obviously misapplied, and the results of all observation are vitiated, when any doubt exists about the sameness of the objects contemplated. It is mainly to this imperfection in the diagnostic part of medicine that we must attribute the uncertainty and variation, both of doctrine and practice, which have brought so much suspicion, and reproach, and ridicule, upon the science we profess. *False* experience, if I may use such a term, has greatly retarded the progress of the healing art: and *false experience* springs from *false diagnosis*. A man will tell you that he has cured a score of cases of advanced phthisis; but he has deceived himself: they were not cases of true phthisis, but simply cases of chronic inflammation, with puriform discharge, of the mucous membrane of the bronchi. He publishes an account of his success, and of his plan of treatment; and thus he deceives others also: and thus he retards the science which he fondly and conscientiously believes he is promoting. Accuracy of diagnosis, therefore, cannot be too highly estimated, or too diligently sought after. It has been wonderfully extended within the last twenty years.

The *prognosis*, or foreknowledge of the course and event of diseases, has but little connection with the promotion of medicine as a science; but it is not on that account less worthy of our attention. Both the physician and his patient find their advantage in the capability of the former to determine whether a disease be remediable—to foresee the changes that may be expected in its progress—to predict the manner in which it will terminate. Knowledge of this kind opens to us a fair and honourable source of credit and reputation; and it begets a degree of confidence towards us, which is beneficial, not merely to ourselves, but to our patients. Our influence over a sick person, and the efficacy of many of our remedial measures, are remarkably increased by the reliance he places on our skill, and by our apparent acquaintance with the nature of his complaint. It is often of material consequence, in another point of view, that the fatal character of a disease should be plainly perceived. A sick man, aware of his danger, is furnished with a motive

and an opportunity for arranging his worldly affairs, in the settlement of which the future comfort and happiness of his family may be very deeply concerned; for making his will; and also for more solemn preparation for the awful change that awaits him. For these reasons medical men have, in all periods, endeavoured to read, in the phenomena presented to them by diseases, the event to which those diseases tend. To *form* an accurate opinion on this head is, however, one thing—to *divulge* it another. There is always some risk of losing, as well as of gaining credit, by strong statements or predictions of the death or the recovery of a patient. If you give an unfavourable prognosis, you have a good chance of losing your patient altogether; his friends argue very naturally, that you are not infallible, that you may be wrong, that if *you* know of no means of safety for him, some other practitioner may, and they *will* grasp at whatever straw comes near them. Do not suppose that this is merely a selfish view of the matter: it is often of much moment to the patient himself, that he should not be tempted to put his life under the charge of impostors, who will feed his hopes, and promise largely, and torture him perhaps with their discipline, and have no mercy upon his pocket. Many an instance have I known of persons dying of consumption, who, when given over by their regular attendants, have been brought to London at considerable expense, to exchange the many comforts of home for the inconveniences of a hired house lodging, that they might be *cured* by that ignorant and cruel and rapacious quack, Mr. St. John Long. There are other reasons, too, why we must sometimes conceal the truth from our patients. It often happens that a person is extremely ill, and in great danger, but yet may recover if he is not informed of his peril. To tell a person in these circumstances that he is likely to die, is to destroy his *chance* of recovery. You kill him if you take away his hope of living. It must be confessed that the duty of the medical man in these cases is very painful and embarrassing: the patient and the patient's friends are urgently inquisitive to know whether there is any danger; or whether he is not yet out of danger. The rule which I have always adopted in circumstances of this distressing kind, when I see clearly that the case is hopeless of cure, is to fix as well as I can upon that person among the family or friends of the patient to whose prudence the real state of the matter may be the most safely confided. If I think that there is a possible chance of recovery, and that a knowledge of his danger by the patient would diminish that chance, of course I urge the necessity of speaking to *him* with assumed cheerfulness and confidence: if I see that the case is absolutely and inevitably mortal, either soon or at some little distance of time, I leave it to the discretion of the person with whom I communicate to disclose or conceal my opinion, as he or she may think best. There are, I believe, practitioners, who make it a point, on principles of worldly policy, *never* to speak despairingly of a patient; but I cannot regard such a rule of conduct as honest, or justifiable, or consistent with one's Christian duty.

Now I wish you to observe that symptoms do not serve equally or indifferently the three several purposes that I have spoken of. The same symptom or set of symptoms may indeed at once reveal the nature of the disease, and foreshow its result, and indicate its treatment. When we have discovered what the disease is, we may want no further information to tell us how it will terminate, or how we are to prescribe for it. A man previously sound and well, shivers, then becomes hot, and afterwards sweats, and then reverts to his natural state of comfort and good health: and the same series of phenomena occur every other day. We pronounce the disease to be ague; we predict that, in this climate at least, the patient will recover; and we give him quina; all upon the strength of the same set of symptoms. But this is not necessarily the case: certain symptoms may disclose to us what the malady is, and where it is situated; other symptoms teach us whether our patient is likely to survive or not; and a still different set instruct us what is the proper method of cure to be attempted. We see a number of little pustules scattered over his skin, and we know that our patient is labouring under smallpox. His chance of recovery will be singularly different, according as the spots upon his face run together, or remain separate and distinct from each other. And we investigate the state of his pulse, and his breathing, of his bowels and his brain, before we can venture to prescribe for him. Those symptoms, or combinations of symptoms, which distinguish the place and nature of the disease, we call *signs of disease;* those which teach us what to do, we call *indications of treatment*. We speak also of prognostic *signs*. By keeping these distinct ends of the study of symp-

toms in mind, we shall be enabled to group them to advantage, and not to huddle confusedly together symptoms that speak, not a different language, but upon a different topic. The ancients, who knew but little of the intimate nature of diseases, but who paid great attention to symptoms, have laid down most admirable rules in respect to prognosis; which shows not only that the prognostic signs are more easily made out, in many cases, than the diagnostic, but also that they may be independent of them.

I have just spoken of *symptoms* as being *signs*. These words are not, however, exactly synonymous, although they are frequently employed as if they were so. Even those medical writers who admit a distinction between them, have not always succeeded in clearly pointing out the difference. Signs are deduced from symptoms, by arranging and comparing them, and noticing the circumstances under which they occur. Symptoms are obvious to all persons alike—to the nurse as well as to the physician: signs, for the most part, are such to medical eyes alone. Let me try to make this plainer by the help of an illustration. Symptoms may be considered as resembling so many *words*. When taken separately, or when put together at random, the words have no force or signification. Arrange them in due order, reduce them into a sentence, and they convey a meaning. The sentence is a *sign* or expression of something which is thus revealed. Symptoms become signs when their import can be interpreted.

A certain crackling sound, of which I shall have much to say hereafter, is heard (we will suppose) in some part of a patient's lung, by the ear applied outside his thorax. This sound is a symptom; any one who listens may perceive it. It is even so far a *sign*, that it denotes the unnatural presence of fluid in the lung, and the passage of air through that fluid. But the fluid may be one of several—mucus, or serum, or pus, or blood: we cannot tell by the sound alone which of these it is. But if we learn that the person in whose lung the sound is audible has been ill for a day or two only, that he has pain in his chest, cough, embarrassed breathing, and fever, we conclude that he is labouring under that serious disease, *inflammation* of the lung. The crackling sound alone could not assure us of this; nor without the addition of this sound could the pain, the laboured breathing, the cough, or the fever. Taken altogether, the symptoms constitute a *diagnostic sign*, and bespeak the existence of pneumonia.

Sometimes a symptom, or set of symptoms, becomes a sign, by its relation to what has gone before and what has followed it. To adhere to our illustration, the meaning becomes evident from the context. By comparing, at short intervals, in the supposed case of pneumonia, the extent and character of the sounds heard during respiration, we ascertain whether the disease be advancing or receding; and thus convert the sounds, or their variations from day to day, into a *prognostic sign*.

We always strive, then, to penetrate beyond the symptoms to the disease of which they are significant. But we do not always succeed in this, and when we do not, (as in the case of ague,) we are driven to the necessity of regarding the combination of symptoms as the disease.

You will often hear of *pathognomonic* symptoms. A pathognomonic symptom is one which, when it occurs, settles infallibly the nature of the malady; becomes a positive *sign* or token of a particular morbid condition. But there are very few symptoms, if there be any, which, taken singly, can ever be said to be strictly pathognomonic signs. But a symptom which in itself possesses little or no value may become very significant when conjoined with others.

Much light is often thrown upon symptoms by what the French call *commemorative circumstances;* that is, by a knowledge of the previous history and condition of the patient. For example, a person may have palpitation and other marks of disordered action of the heart, and doubts may exist whether these symptoms depend or not upon organic disease of that organ. The question is often determined in the affirmative, by our learning that the patient has had one or more attacks of acute rheumatism of the joints.

There are some other general divisions of symptoms, which it is useful to attend to. Thus some symptoms are said to be *direct*, and others to be *indirect* symptoms. Direct symptoms relate to the very part which is affected; indirect symptoms are such as "declare themselves through the medium of some other parts, or through the medium of the constitution at large." There are some cases in which the direct symptoms are of much

more value than the indirect; and there are other cases in which those which are indirect are the most important; and there are yet many more which require for their elucidation a knowledge of both the direct and the indirect symptoms.

Again, there are many symptoms of which we receive no information, except from the statements made by the patient himself; and there are many others of which we learn the existence by means of our own observation, by the exercise of our several senses. The relative importance of *these* varies too in different cases. Of course those symptoms which we are able to ascertain for ourselves are the most *trustworthy;* but both sorts of symptoms shed a mutual light upon each other: we should constantly be making mistakes if we relied solely upon what our patients tell us; on the other hand, the value of the information we derive from their statements is made apparent by the difficulty we are apt to experience in investigating the diseases of children; of those who are dumb; or what is much the same thing, who speak no language that we can understand.

Now, setting aside that notice of the healthy functions which is sometimes necessary in order to determine the relative value and meaning of other symptoms, and regarding those symptoms only which consist of *morbid* changes, they may all be classed under three heads: 1. Uneasy, unnatural, or impaired *sensations:* 2. Disordered or impeded *functions:* and 3. Alterations of structure or of appearance; changes of sensible qualities. When these last come within the direct cognizance of our senses, they are called, usually, *physical signs.*

Uneasy or altered sensations we can only be aware of through the testimony of the patients themselves. The symptoms belonging to the other classes fall, generally, under our own observation.

Uneasy or altered sensations comprehend a large class of morbid symptoms. By their occurrence persons sometimes become conscious that they are unwell before any other symptoms are observable. Of all the uneasy sensations pain is the most common and the most important. It is very rarely absent in one stage or another of inflammatory disorders; and it very often occurs, and is very acute too, when there is no inflammation at all. I shall have occasion, in a subsequent lecture, to lay before you the criteria between pains that accompany inflammation and pains that are independent of it. Upon the decision of that point the whole question of treatment commonly depends; and it is often a most difficult point to decide.

There are many different kinds and degrees of pain. Different kinds of morbid action are accompanied by different kinds of pain; and the same kind of morbid action, inflammation for example, produces different modifications of pain, according as it affects different parts. The pain that belongs to inflammation of the lungs differs from that which is felt in inflammation of the bowels. Bones, muscles, tendons, ligaments—the bladder, the kidney, the uterus—all modify, in a manner peculiar to themselves, the pain that is produced in them by injury or disease. Different epithets are given to the different varieties of pain—*i. e.* persons endeavour to explain how they feel, by likening their sensations to something which they have felt before, or fancy they have felt. Thus we hear of *sharp* pain—*shooting* pain—*dull* pain—*gnawing* pain—*burning* pain—*tearing* pain—and so on.

When pain is felt in a part, only when it is touched, *i. e.* when *pressure* is made upon it, it is called *tenderness:*—the part is said to be tender. This is a very important kind of pain, as we shall see hereafter. A part may be both painful and tender: or painful without being tender; or tender without being otherwise painful.

Pain often takes place, not in the part really affected by disease, but in some distant part. Inflammation of the liver or diaphragm will cause pain of the right shoulder: the mechanical irritation of a stone in the bladder produces pain at the extremity of the urethra: inflammation of the hip-joint occasions pain in the knee: disease of the heart is often attended with pain running down the left arm: many headaches result from irritation of the stomach. We call these instances of indirect, or sympathetic pain. Some of them admit of no very obvious explanation:—others have been ascribed to connections of the sentient nerves of the two parts—"especially when the part really injured is internal, and that to which the feeling is referred is external, and both derive their sentient nerves from

the same larger branches." You will perceive that a due estimation of these sympathetic pains is of no small importance.

I may observe of pain in general, that it is differently felt—or at any rate differently complained of—by persons of different constitutions and temperaments. There are even, I fancy, *national* differences in this respect. I have been present, as you may believe, at a great number of surgical operations, and I have been struck with the different degrees of patience with which the same operation has been borne by Irishmen and by Scotchmen. The Irishman, speaking generally, either feels more acutely, or gives more free vent to his feelings in cries and exclamations: the Scotchman, on the contrary, most commonly preserves a resolute silence. In complaints that are attended with low spirits, and hypochondriacal symptoms, there is reason to believe that the pain spoken of often depends, in a great degree, upon the eager attention that is paid to it. The accounts given by such patients of their sufferings are always to be received with a grain of allowance; and this is often an embarrassing circumstance in practice. Patients take it ill if they do not seem to be implicitly credited; and yet if they are not convinced that much of what they suffer depends on their great attention to it, they will never get well. You will often find that they cease to feel pain—*i. e.* they forget to attend to their complaints—when their attention is otherwise strongly arrested—as by conversation or music. I adverted to this principle in my last lecture.

Besides pain, in all its modifications, there are many other, and very interesting, uneasy sensations. *Itching* is an uneasy sensation nearly allied to pain. A severe mechanical irritation will cause pain, so a slighter degree of it will cause itching. Itching occurs in many cutaneous diseases, and it gives a name to one of them, which is emphatically called *the itch*. And the Latin word signifying the same sensation, *prurigo*, is made use of to denote other forms of disease of which itching is the most prominent symptom. It often affects some one of the natural outlets of the body; it occurs about the rectum, from the motions of little worms that nestle in the lower part of that gut. This prurigo podicis, which does not always depend on the cause just mentioned—and the prurigo pudendi in the female—are sometimes most distressing complaints; harassing the patients continually, preventing sleep, driving them from society, and requiring medical treatment. Acrid matters in the intestines will sometimes produce a kind of itching there; and the call to void the fæces is perhaps more akin to itching than to any other sensation: sometimes, indeed, it amounts to pain. The tickling often felt in the windpipe, and provoking the person to cough, appears to be of the same nature. Tingling and pricking are sensations which have also some analogy with itching.

Nausea is another uneasy sensation. It is sometimes a *direct* symptom of disease or disorder of the stomach, to which the sensation is referred. Sometimes it is a very important *indirect* symptom, taken in conjunction with others, of disease in some part at a distance from the stomach—in the kidney for example, or in the brain. The nausea which is so troublesome to pregnant women is another instance of a morbid sensation sympathetic of irritation in a distant organ.

Giddiness—vertigo—is another example of an uneasy sensation. It sometimes results from disease within the head; sometimes it is an indirect consequence of disorder of the stomach; or of mere debility and an approach to syncope.

Patients will also complain of an undefinable sensation which they usually call *sinking*—a sensation which is referred to the epigastric region. This is frequently a source of much distress to hysterical women; and it is occasionally the forerunner of death at the close of severe diseases which have a tendency to end fatally in the way of syncope.

Many other symptoms might be mentioned which belong to this class of uneasy sensations, and for our knowledge of the existence of which we must depend upon the accounts given us by the patients themselves. Sensations of weight; and of tightness and fulness; drowsiness, tenesmus, strangury, heartburn; and various depraved conditions of the special senses. In the majority of diseases the appetite is lost or impaired; but sometimes excessive hunger accompanies and denotes disease. We sometimes form the first suspicion of the existence of diabetes from the preternatural keenness of the appetite. Thirst is a very constant and striking symptom in all febrile and inflammatory disorders; and in the disease just now mentioned, diabetes, it frequently constitutes the whole distress of which the

patient is sensible. The appetite may be perverted, as well as deficient or excessive. Chlorotic girls will eat cinders, sealing-wax, slate-pencil, and such trash. So women who are pregnant either have or pretend to have inordinate longings for particular kinds of food—longings which are evidently fostered by encouragement. They are not, I believe, common at present in this country; and among the poor, who have not the means of gratifying them, they are less frequently heard of than in the higher classes of society.

The class of uneasy sensations you see then is a very large one; and some of the morbid feelings are of very great moment. However, there are not many diseases which consist altogether of uneasy sensations; and when we find that pain or uneasiness is complained of in any part or organ, we next proceed to inquire whether the *functions* of that part or organ are disturbed or suspended. If we discover any interruption or derangement of function, we have additional reason for concluding that the part so affected in its sensations and in its functions is actually the seat of disease. This is an inquiry which we can prosecute with much less assistance from the patient himself; and mostly with no assistance at all; and even in spite of any erroneous notions which he may have formed, and is anxious to state upon the subject. The study of disordered functions is of great practical value.

The functions of the brain and nerves—of the heart and blood-vessels—of the respiratory apparatus—and of the digestive organs—are all of vital consequence.

Some of the impeded or disordered functions which relate to the brain and nerves are in fact identical with the last class of symptoms, and consist of altered or morbid *sensations:* sensation being one of the natural functions of those parts. Depravations, for instance, of the sense of touch; numbness; the total absence of sensation, which we call anæsthesia. Symptoms of this kind do not constitute primary diseases, but they often portend or accompany very serious alterations in the brain, or in some part of the nervous system; and it is from that circumstance that they derive the great interest and importance which belongs to them. The same may be said of perverted conditions of the other senses. The sense of vision is often impaired, and in various ways and degrees, from mere dimness or imperfection of sight, to total blindness. And this total blindness may occur without any other *apparent* disease, the humours and fabric of the eye itself being in all evident respects healthy and right: it may come on, too, so gradually, and increase so slowly, as not to be discovered for a long time, by the patient himself. Mr. Day, the great blacking man, of the firm of Day and Martin, who died not long since, was almost entirely blind. He told me that he first discovered that the sight of one eye was gone, one day when he attempted to look at a distant object through a telescope: he could see nothing, and he imagined that the little brass plate which slides over the eye-glass had not been withdrawn. There was, however, no such obstacle; and he too soon found that when the other eye alone was closed, he was in total darkness. This state of blindness is called amaurosis, and it may depend upon pressure made upon the retina, or upon the optic nerves, or upon the brain at the origin of those nerves. Its approach is sometimes marked by the fallacious appearance of black spots upon the objects the patient is looking at, or floating before him in the air—*muscæ volitantes.* Some of the other depravations of sight are still more extraordinary, and, except that they are not uncommon, might almost be considered fabulous. Thus persons sometimes see things around them apparently in motion, when in truth they are not so. This is, in fact, a symptom I have mentioned before—vertigo: if the patient shuts his eyes, and consequently can see nothing, he feels as if he were himself turning round, while in reality he is at rest: persons in this state fancy sometimes that the bed on which they lie is sinking rapidly down with them into some abyss. A still stranger depravation of the sense of vision is that in which a person sees only one half of an object at which he is steadily looking. One man, in passing along the street, fancied that every body he met had only one eye. The late Dr. Wollaston was subject to this optical delusion; he frequently found that only one half of the object he looked at was visible: and he wrote an ingenious paper in the Philosophical Transactions to explain this. After his death a tumor was found in his brain, interfering with the optic nerves. The celebrated Mr. Abernethy had once a temporary affection of the same kind, dependent no doubt upon some slight and transient injury of the brain. He was thrown, I believe, from his horse—at any rate he received a violent blow on his head, which stunned him;

and when he had recovered a little, he was taken home in a hackney-coach. On his way he amused himself with reading the names of the trades-people placed in front of the shops, and he was greatly surprised to find that one half of each name—the last half—seemed blotted out: he described this in his lectures, in his whimsical way, by taking his own name as an example: "I could see as far as the *ne* (said he), but I could not see a bit of the *thy*."

Those very wonderful cases of spectral illusion which sometimes occur, come within the class of symptoms we are now considering: they throw a strong light upon many of the well-authenticated ghost stories—which were in fact mere examples of disease or disorder in the brain of the ghost-seers. It would be out of place to go into any detail upon this interesting subject here. You will find some excellent examples of these spectral illusions in Dr. Hibbert's book on Apparitions, and in Sir David Brewster's Natural Magic, and in Sir W. Scott's Letters on Demonology.

The sense of hearing is liable to analogous disorders. Sometimes it becomes preternaturally acute; and this is a bad symptom when it does occur. I was called a year or two ago to see a gentlaman in the Temple; he had been taken ill only a few hours before, but I found him dying: the pulse was gone from his wrist, and his skin was cold, but his intellect was entire, and he complained of nothing but of the distress he felt from the loud noises that were made by those around him, in moving about or speaking, although, in fact, all noise was as much as possible avoided, and conversation was carried on in a whisper: but his hearing was painfully acute: he died the same evening; I believe of an irregular form of cholera. It is always right that patients should be protected from the irritation which might arise from this source; for that degree of noise which would not interfere with the sleep of a healthy person will often not only prevent it in a sick man, but bring on delirium, and aggravate greatly the disease under which he labours. The custom of strewing the streets with straw before the houses of those who are seriously ill is, in many cases, a very proper precautionary measure.

The opposite fault, *obtuseness* of hearing, is much more common. It often occurs in fever, and it is not thought a bad symptom: it certainly is a much less unfavourable circumstance than morbid acuteness of hearing, and it probably depends upon a disordered state of the brain which is not in itself very dangerous.

What is called *tinnitus aurium* is an instance of the depravation of the sense of hearing. It seems sometimes to result from the too strong throbbing of the arteries. It occurs in many diseases, and is not unfrequently a symptom of diseased cerebral vessels, and a precursor of apoplexy or palsy. It is sometimes in itself extremely annoying. Curious and undefinable sounds are heard by some patients—sounds like a rushing wind, like the falling of a cataract, the noise of a bell, or of a drum. A female patient of mine in the Middlesex Hospital last year, who had disease of the bones of the ear, with symptoms that threatened some implication with the brain; this patient heard a perpetual noise in her ear like the singing of a tea-kettle. I have lately been consulted by a gentleman from the country, who had no other complaint than a constant hissing, which worried him greatly, in one ear. In a case of spectral illusion related by Sir David Brewster, voices and sentences are said to have been repeatedly heard, when none were uttered.

Affections of the intellect—of what are sometimes called the internal senses, are very common, and very important, symptoms of disease. They are sometimes considered as primary diseases themselves; they very frequently accompany certain febrile diseases; and they are not uncommon in diseases that are not attended with fever. There is more or less derangement of the internal senses from the very beginning of continued fever. The power of attention is impaired. That kind and degree of mental exertion which would afford gratification and amusement when we are well, becomes laborious and irksome when we are ill; and to compel, or to urge the attention, under such circumstances, is injurious. This state is probably only the first degree of delirium, and therefore these slight approaches to the derangement of the internal senses are by no means to be disregarded. It is curious that the delirium of fever is always most marked during the night: this seems to be owing to the circumstance that the erroneous notions and wandering thoughts of the patient are not corrected by external impressions made upon his senses.

You will find, conformably with the same principle, that your patient sometimes ceases to be delirious upon your visiting him: the sight of a new face rouses him for a time, but he soon relapses.

Voluntary motion is another function connected with the nervous system, and one which affords a great variety of important symptoms. Like the senses it may be excessive, or deficient, or perverted. Excess of voluntary motion is not common, nor very important. In mania patients sometimes exhibit an extraordinary degree of muscular strength; indeed, in the delirium of fever something of the same kind may be observed.

But the opposite state, that in which the power of voluntary motion is deficient, muscular debility, is exceedingly common. Debility is an original and essential part of fevers. It appears before there has been time for it to be produced by the exhaustion of disease. It is not always proportional to the other symptoms, and does not necessarily imply any great degree of danger. This sudden and early weakness has been a very striking symptom in our two recent visitations of influenza. Persons previously in apparent good health would be seized as they walked along the street, and be glad to sit down in a shop, or a carriage, and to get home and go to bed. Young and strong persons would be thus prostrated.

In some instances debility does not appear till late in the disease, of which it then forms an important prognostic symptom, and an important guide for our treatment: it shows us that there is a tendency to death by asthenia, and we have to endeavour to keep the patient alive by supporting his strength as well as we can,—this being the chief or the only indication.

Debility is occasionally the principal symptom of the whole disease—as in hemiplegia, paraplegia, or in more partial palsy, palsy of one limb, even of a finger, or of a single muscle, as of the levator palpebrarum. This, though it may seem trifling in itself, is far from being so in reality; it often forms a fragment only of a most serious disease; and from such partial symptoms of palsy we may prognosticate a more general and alarming attack. A slight degree of paralysis affecting some of the muscles of the eye will produce a squint, and consequent double vision; and this occurs not only in hydrocephalus, when it is a most significant phenomenon, but also as a prelude to more extensive palsy. General palsy is sometimes prefaced by an affection of the tongue of the same nature, producing a faltering and indistinctness of speech.

Spasm is an instance of disturbance and the power of perversion of voluntary motion. It consists in an irregular and violent contraction of muscular parts—involuntary, even when the voluntary muscles are concerned. Cramp is a familiar example of it; and we have been taught, since the cholera has made its appearance among us, to regard cramp as sometimes a very formidable symptom: not formidable in itself, but formidable in respect to the condition that gives rise to it. Tonic spasm is the principal symptom also of that frightful disease—frightful in its phenomena and in its frequent fatality—tetanus. The convulsions of epilepsy and hysteria, and the jactitation of chorea, are ordinary examples of the perversion of the function of voluntary motion. Sometimes convulsions bode great danger, sometimes none at all.

So also *tremor*, which is nearly akin to spasm, is a sign, frequently, of a morbid state of the greatest peril; and sometimes it is violent without being attended with the smallest hazard.

If we turn now to the great function of respiration, we shall find that it affords a very great number of morbid symptoms—and those of the highest importance.

Dyspnœa, difficulty of breathing, is one of the most prominent of these symptoms. It may depend upon various causes. In inflammation of the lungs and pleuræ there are several circumstances in operation to render the respiration difficult; for example, pain, which would be enough of itself; the effusion of lymph into the texture of the lung, or of serum into the cavity of the pleuræ, mechanically impeding the entrance of the air. In dyspnœa the breathing is almost always most difficult when the patient is lying on his back. One reason for this is plain. In the supine horizontal posture the action of the diaphragm is obstructed by the weight and pressure of the abdominal viscera; and the erect

position obviates this. Upright breathing, *orthopnœa*, has come to be considered as a distinct modification of dyspnœa. The patient cannot lie down.

Sometimes, as in asthma, the difficulty of breathing comes on in separate paroxysms; the respiration becomes all at once loud, sonorous, and wheezing. A person who had never seen any cases of this kind would imagine that the patient was at the point of death —that it was all over with him; but the most frightful of these attacks are seldom attended with any *immediate* danger. They depend frequently upon organic disease of the lungs, heart, or aorta: sometimes they seem to be purely spasmodic: sometimes to result from transient congestion of blood in the lungs.

Cough is a violent spasmodic action. A full inspiration is taken; then the glottis is closed pretty firmly—and in expiration the air is forced suddenly out, and with it, frequently, mucus, or other matters which irritated the lungs or bronchi. It seems to be one of the efforts of nature to expel from the lungs things which ought not to be there. There are several varieties of cough: it is a symptom belonging to so many dangerous complaints—pneumonia, pulmonary consumption, and diseases of the heart—that it always demands strict attention. No one who has once heard it can ever mistake the *hooping* cough. There is also a startling obstreperous sort of cough, shattering one's ears almost, like the noise of a person coughing through a brass trumpet—which depends upon some peculiar state of the nervous system, implies no danger, and is more distressing to the bystanders than to the person who utters it. I believe you may often distinguish the cough of inflammation of the lungs from that of phthisis, and each from the cough of hysteria, by their respective sounds: but we have much better methods of distinguishing them—viz. the concurrence or the absence of certain other sounds belonging to the respiration, and ascertained by auscultation.

Sneezing is another morbid symptom, which, though it may appear trifling, is not to be overlooked. It is a very common symptom in catarrhal affections. When sneezing occurs in combination with cough, it affords a *presumption* that the cough is not phthisical. Sneezing may even happen as a primary disease, occurring in long-continued paroxysms. I have at present under my care a young lady of an hysterical disposition, whose main distress consists in violent and protracted attacks of sternutation, which have harassed her almost daily for many months. One of our bishops is subject to very inconvenient fits of this kind. He will begin to sneeze and go on sneezing incessantly for a long time together. I believe that he finds an effectual remedy for these attacks in plunging his head into cold water.

I say nothing here of those direct symptoms of pulmonary disease which are ascertained by the sense of hearing—by percussion and auscultation. I shall enter fully into that subject hereafter. A systematic account of symptoms, if this were the fitting place for it, which it is not, would require a dozen or twenty lectures. To perceive the relation of symptoms, taken one by one or in diverse combinations, to the various known forms of disease, you must have some prior knowledge of diseases. But I am obliged to suppose (however incorrect the supposition may be in respect to some among you) that you are mere beginners, and have still to learn even the rudiments of such knowledge. Different diseases may have many symptoms in common. The same symptom may bear a very different import according as it is combined with other symptoms; or connected with this or that disorder. The proper place for a comprehensive and complete review of symptoms would, therefore, be at the *end* of a course of lectures on the practice of physic. When the various forms of disease had been gone through, in reference to the symptoms belonging to them, then would be the time to take the converse aspect of the case, and to consider the long list of symptoms in reference to the diseases they denote or accompany. All that I am at present attempting is to give you some general notion of what symptoms are; to put before you, as samples, a few of the most prominent; and to show you, even by this cursory and imperfect view of them, of how great importance it is that we should make their relations to each other and to different diseases, and their signification, diagnostic, prognostic, and therapeutic, the objects of our most diligent attention.

I might find matter for two or three lectures—if my present object would admit of them —in the symptoms that are drawn from functions belonging to the *circulation*. Every body knows how much importance is attributed to the state of the pulse. A patient would

think us careless and negligent if we did not feel his pulse; and really the information obtained by that little operation is often of the most interesting and instructive kind. But it requires practice and intelligence to appreciate that information. The qualities that we most attend to in the pulse are its frequency, its regularity, its fulness, and its force. It is necessary that we should know the number of beats which the heart habitually makes in health; for it varies much in different persons. Its average number of pulsations in a healthy adult is from 70 to 75; but there are persons who, when they are quite well have always a pulse of 80 or 90; and there are others in whom the pulse seldom rises beyond 60. If we do not inform ourselves of these peculiarities, we may fall into great mistakes. In disease the pulse may acquire a degree of frequency which is scarcely calculable; and the less so because, when it is extremely frequent, it is also extremely feeble: it will reach 150, 160, or even 200 beats in a minute. In other cases—as in apoplexy sometimes, and in some organic affections of the heart—the pulse will become extremely slow. The slowest pulse I ever felt was that of a man sixty-eight years old, who was for some time a patient of mine with diseased heart, and dropsy. His pulse was often as low as 25 in the minute. He died suddenly in his chair, and I was very desirous of examining his body, but his widow would not allow it. In the 17th volume of Duncan's Medical Commentaries, there is a case related in which the pulse was as low as nine beats in the minute. We learn a good deal in certain disorders from the variations and fluctuations of the pulse in respect to frequency—in hydrocephalus, for example, and in continued fever.

Irregularity of the pulse is another condition which is often full of meaning, and of interest. I hope, as the lectures proceed, to be able to point out the bearings of these several qualities of the pulse upon our views of disease, and especially upon its treatment. At present I must repeat that I pretend to do no more than furnish you with a few samples of the phenomena that characterise disease. Irregularity of the pulse is natural to some persons. I have a brother who enjoys very good health, and whose pulse is always extremely irregular: I have been told that when he was ill with a fever at school, it became regular. I have heard of several precisely similar cases. There are two varieties of irregular pulse—in one the motions of the artery are unequal in number and force, a few beats being from time to time more rapid and feeble than the rest: in the other variety a pulsation is from time to time entirely left out—the pulse is said to intermit. These two varieties may coincide in the same person, or they may exist independently of each other.

Irregularity of the pulse may be caused by disease within the head—by organic disease of the heart—by simple disorder of the stomach—or it may be merely the result of debility and the prelude of the entire stoppage of the heart's action from asthenia. How important must it be to ascertain and construe each of these meanings of the same symptom. It may indicate mortal disease—it may imply no danger at all: it may afford no clue to any available treatment; or it may teach us how to ward off impending dissolution.

Another most important quality of the pulse is what is called its hardness, or incompressibility. You find that you can scarcely abolish the pulsation by any degree of pressure; the blood still forces its way through the artery beneath your finger. Sometimes it is felt to strike a large portion also of the finger, and then we say that the pulse is both hard and full, or large. When it strikes a very small portion of the surface of the finger, it is compared to a thread; it is a small pulse: and if at the same time it be hard—such a pulse is often described as a wiry pulse. It requires some education of the finger to appreciate with exactness the several varieties of the pulse, even those which are practically important; for many have been mentioned by authors which are purely fanciful, or useless and unnecessary refinements.

Now this hard pulse I shall soon have to speak of again, in connection with the treatment required in inflammation. It is one of the best warrants we have, in many cases, of the propriety of bleeding our patient. It does not occur, however, in *all* inflammations, and it may occur when there is *no* inflammation. It may depend upon hypertrophy of the left ventricle of the heart, and then it is beyond the reach of blood-letting as a remedy. It often seems to be dependent upon a morbid condition of the artery itself, brought on,

as Dr. Latham has suggested, by the pernicious habit of dram-drinking. It is, however, at all times considered so much a guide to our practice, that whenever it occurs it is very necessary that the exact cause of it should be carefully inquired into.

Before I conclude this rough review of symptoms, I must point out one or two that belong to the third class mentioned; viz. changes of sensible qualities. These include variations in the temperature of the body: in the colour of the surface, and especially of the face: the diminution or increase of bulk; the latter, when general, we call corpulence; when partial, swelling: and various other symptoms, especially those which are detected by auscultation.

Wasting, or emaciation, is sometimes the first observable symptom of disease. It occurs in complaints that are not commonly dangerous—as in dyspepsia, and in hypochondriasis, which is often connected with dyspepsia: and when it does appear it marks the reality of the disease. This wasting also happens in many fatal diseases—in phthisis pulmonalis, for example—and in dropsy, although the dropsical enlargement sometimes masks it. It accompanies many acute diseases, and is reckoned an unfavourable symptom; for it shows that the body is not properly nourished. Sometimes the emaciation is so extreme, that the integuments give way—the bones of the patient are said to come through the skin.

We have examples of symptoms that consist in changes of colour, in the flushed cheek of fever; in the pallor belonging to many diseases; in the combination of those colours presented by the red spot surrounded by paleness of the cheek so characteristic of hectic fever; in the yellowness of the skin and conjunctiva in jaundice; in the dusky hue of the countenance and the lividity of the lips noticeable whenever the due arterialization of the blood in the lungs is interfered with; and in a long catalogue of cutaneous disorders.

Let me once more remind you of the peculiar importance of accustoming yourselves to take notice of the symptoms comprised in the last two classes, and especially the last class, that you may attain to a quick perception of them. Changes of sensible qualities speak for themselves, and speak the truth. They cannot deceive us, as the verbal statements of even conscientious patients respecting their uneasy feelings might. They direct us in the choice and order of our questions: nay, they frequently spare us the necessity of putting many questions; questions which might be irksome or fatiguing to our patients, or offensive to their natural delicacy, or even hurtful by letting them know our thoughts respecting their disorders. Of the change in sensible qualities we judge by our own eyes, and ears, and fingers, and often by our noses also; and the change is sometimes, of itself, perfectly characteristic of the complaint.

Many more morbid phenomena, or symptoms, or tokens of disease, might have been mentioned; but I have said enough, I hope, to rouse your attention to the extent and the fertility of this field of study. When we next meet I shall begin to consider one of the special forms of disease to which all parts of the body are liable—a disease that meets us at every turn—I mean *inflammation*.

LECTURE IX.

INFLAMMATION. ITS MORBID AND ITS SALUTARY EFFECTS. SKETCH OF THE LOCAL AND CONSTITUTIONAL PHENOMENA OF INFLAMMATION AS IT OCCURS IN EXTERNAL PARTS. EXAMINATION OF THE SYMPTOMS OF INFLAMMATION; PAIN; HEAT; REDNESS; SWELLING. STATE OF THE CAPILLARY BLOOD-VESSELS, AND OF THE BLOOD IN A PART INFLAMED.

INFLAMMATION must needs occupy a large share of the attention both of the surgeon and the physician. In nine cases out of ten the first question he asks himself upon being

summoned to a patient is, "have we inflammation here?" It is continually the object of his treatment and watchful care. It affects all parts that are furnished with blood-vessels, and it affects different parts very variously. It is more easily excited by many external causes, and therefore it is more *common* than any other special disease. A great majority of all the diseases to which the human frame is liable begin with inflammation, or end in inflammation, or are accompanied by inflammation during some part of their course, or resemble inflammation in their symptoms. Most of the organic changes of different parts of the body recognise inflammation as their cause, or lead to it as their effect. In short, a very large share of the premature extinction of human life in general is more or less attributable to inflammation.

Again, inflammation is highly interesting not only in its morbid phenomena and destructive consequences, but in its healing tendencies also. It is by inflammation that wounds are closed, and fractures repaired—that parts adhere together when their adhesion is essential to the preservation of the individual—and that foreign and hurtful matters are conveyed safely out of the body. A cut finger, a deep sabre wound, alike require inflammation to re-unite the divided parts. Does ulceration occur in the stomach or intestines, and threaten to penetrate through them? Inflammation will often anticipate and provide against the danger—glue the threatened membrane to whatever surface may be next it—and so prevent that worse and universal inflammation of the peritoneum, and the almost certain death, which the escape of the contents of the alimentary canal into that serous bag would infallibly occasion. The foot *mortifies;* is killed by injury or by exposure to cold:—inflammation, if it be not anticipated by the knife of the surgeon, will cut off the dead and useless part. An abscess forms in the liver—or a large calculus concretes in the gall-bladder: how is the pus or the stone to be got rid of? If they make their way to the external surface of the organ, as they always tend to do, they enter the cavity of the abdomen, and excite fatal peritonitis. But a natural safeguard arises; partial inflammation precedes and prepares for the expulsion; the liver or the gall-bladder, as the case may be, becomes adherent to the walls of the abdomen on the one hand, or to the intestinal canal on the other; and then the surgeon may plunge his lancet into the collection of pus—or the abscess or the calculus may eat their own way safely out of the body—through the skin, or into the bowel. Inflammation, limited in extent and moderate in degree, becomes conservative by preventing inflammation more severe and more widely spread, which would be fatal. This is what I mean when I speak of the curative properties of inflammation; and surely this process, which may save life or destroy it, deserves and demands our most careful study.

But inflammation has a still farther and peculiar claim upon our attention. The salutary acts of restoration and prevention just adverted to, are such as *nature* conducts and originates. But we are ourselves able, in many instances, to direct and control the effects of inflammation—nay, we can excite it at our pleasure; and having excited it, we are able, in a great degree, to regulate its course. And for this reason it becomes in skilful hands an *instrument* of *cure*. This instrument the surgeon employs when, after letting out the water of a hydrocele, he wilfully excites inflammation of the tunica vaginalis, which obliterates its cavity, and renders the reaccumulation of the fluid impossible. It is by availing himself of the same agent that he is enabled to remedy many afflicting derformities:—to unite the cleft lip; to close up the fissured palate; to restore the dilapidated nose. There is no other special disease which is thus at our command; we cannot, if we would, produce a tubercle or a cancer. For all these reasons inflammation possesses a very high degree of interest for us—and for every one who would inquire, with any prospect of success, into either the pathology or the treatment of diseases.

Of the amount of our knowledge respecting the *intimate nature* of inflammation, I shall have occasion to say a few words by and by. We first become acquainted with inflammation in its symptoms, and as it displays itself externally. After we know what they are, it may be right, and cannot but be interesting, to inquire how they come about. Now the symptoms which, when they exist together in an external or visible part, betoken or denote inflammation of that part, are four in number: pain—redness—heat—swelling; *preternatural* redness, and *preternatural* heat. These, from the earliest ages, have been

recognised as the signals of outward inflammation. "Notæ inflammationis (says Celsus) sunt quatuor—rubor et tumor cum calore et dolore."

No definition, however, or general description, can be made to embrace all the forms in which inflammation presents itself. We can give no useful account of it in the abstract; and therefore I shall first sketch the phenomena of inflammation under one of its most common external forms; and taking this as a type of the disease, proceed afterwards to trace its modifications and varieties, and to fill up the picture.

Let us suppose, then, that a healthy man receives some local mechanical injury—that he falls, for instance, against a window, and gets a piece of glass stuck into his arm. In a short time he begins to have a pain in that part of his arm, and this is soon succeeded by redness, and increased heat, and swelling. The skin becomes of a bright red colour; the swelling increases. In the immediate place of the injury the swelling is firm and hard, and exquisitely tender: at some distance from that centre, although there is still swelling, the parts are softer and more yielding. In the seat of the redness and swelling the patient experiences a sense of heat, a burning pain: the part is sensibly hotter than natural to the touch of a by-stander; and if its actual temperature be measured by means of a thermometer, it will be found to exceed the temperature of the neighbouring surface. *The part is inflamed.* This is what is called phlegmonous inflammation. φλεγμονη is a Greek word, and *inflammatio* is a Latin word, and they both mean the same thing, viz. a burning, or a flame. Phlegmonous inflammation is therefore, in truth, a tautological phrase. But custom has assigned a particular signification to the epithet phlegmonous;—it denotes that kind of violent inflammation in which the inflamed part seems all on fire.

If the inflammation reach a certain degree of intensity, other signs of disorder present themselves at a distance from the injured spot. The patient usually at first feels chilly and feeble; but soon the temperature of the whole of the surface rises, the skin becomes hot and dry, the pulse more frequent and fuller and harder than is usual; lassitude comes on, and headache, and wandering pains in the limbs. The patient is unable or unwilling to exert himself, and finds that he is unapt for any mental effort; he cannot command his attention, gets confused and restless, and sleeps ill; he loses his appetite, his tongue becomes white, his mouth is parched, he is unusually thirsty, and the various secretions of the body are deranged and diminished.

This is *inflammatory fever.* This is an *indirect symptom* of inflammation, manifesting itself *through the medium of the system at large.* Various names have been given to this general disturbance of the vascular and nervous systems; constitutional disturbance—sympathetic fever—symptomatic fever. It matters little what term is used, provided that we affix always the same meaning to it: but inasmuch as the word *fever*, in this and in other languages, is taken to express a specific disease, it would perhaps be better to employ the term *pyrexia*, as Cullen and others have done, to denote that secondary febrile state which grows out of, or is associated with, primary local inflammation.

Now what is the end of this remarkable state of things? Why, it may end in one of two or three different ways. Supposing the piece of glass to be extracted, and proper measures to be taken for subduing the inflammation, or even supposing that no other measure is adopted except removing the bit of glass, then it will often happen that the phenomena just described will gradually recede and disappear; the pain will abate, the redness fade, the swelling diminish, the heat decline, the pyrexia cease; until the part at length regains its usual sensations and its natural appearance. When inflammation subsides in this way it is said to be *resolved*, to terminete by *resolution;* and this is its most favourable and desirable mode of terminating, whenever inflammation occurs as a morbid process.

But in many instances the inflammation does not thus subside. The irritant cause still remains in action—or the original intensity of the inflammation has been too great to admit of resolution—or the means proper to abate it have not been used—or have not succeeded. The symptoms already described continue, and are aggravated in degree: at length the swelling begins to assume a more projecting and pointed form, and the skin in its centre to look white; the central part of the swelling, formerly so hard, becomes softer

—the pain is of a throbbing kind: a pulsative sensation, keeping time with the beats of the heart, is experienced in the part, and often a feeling occurs as if something had given way within it: at last (if art does not interpose) the cuticle breaks, and a yellow cream-like fluid is poured out, which we call pus, and upon its escape there generally ensues a considerable and speedy abatement of all the local symptoms of inflammation—of the pain, the heat, the redness, the tumour.

This is *suppuration.*

Meanwhile, especially if the suppuration be long continued, and the discharge of pus profuse, the character of the general febrile excitement undergoes a change. Slight but frequent shiverings, or feelings of chilliness, take place, followed by flushes of heat, which end in perspiration.

This is *hectic fever.*

If the injury has been still more serious, and the inflammation more intense, the part that it has invaded perishes by the violence of the disease; there is partial death. In that case the vivid red colour alters to a purplish or livid, or even a black, or greenish-black hue, the tension of the part exists no longer, the cuticle is elevated by a sanious fluid, the pain ceases, the part is devoid of all sensation—is dead and putrid, and exhales a peculiar and offensive odour.

This is *mortification.*

When the injury has been extensive, a corresponding and characteristic change is again observable, sometimes in the constitutional febrile disturbance. The patient grows more and more feeble, and delirious; he has involuntary startings of the tendons of the voluntary muscles—his pulse is weak and very frequent—his tongue becomes dry, brown, tremulous—his lips are black with accumulated sordes—his countenance is shrunk, haggard, ghastly—his stools and urine escape from him without his appearing to be conscious that they do so.

This is *typhoid fever.*

Under more favourable circumstances the dead or mortified part, which is called a slough, separates from the living parts, and leaves a breach of surface. The separation is effected by a vital process which is denominated *ulceration,* but which I need not now describe. The cavity thus formed gradually fills up, and heals in a peculiar way.

There is one other circumstance, not to be omitted in this rough outline of the local and general phenomena and effects of inflammation. If during its progress blood be drawn from a vein, it exhibits, after standing and coagulating, the peculiar appearance known by the name of the *buffy coat; i. e.* on the surface of the coagulum, and to a certain depth in its substance, the colouring matter of the blood leaves the fibrin, which is therefore seen of a yellowish hue, or buff colour.

Taking the preceding statement as a groundwork, let us look back upon it, and trace its particulars a little more in full. The four characteristic signs of inflammation being pain, heat, redness, and swelling, it will be useful to examine each of these symptoms more closely, in its turn.

The *pain* varies much in different cases of inflammation, both in degree and in kind. It is differently felt, *cæteris paribus,* by different persons, according to their natural susceptibilities. It varies from the slightest degree of sensibility to the utmost agony and torment. Parts which, when sound, are endowed with little or no sensation (as tendons, ligaments, cartilage, bone), become often exquisitely sensible under inflammation. The organs of sense are variously affected in this respect: thus the specific sensibilities of the mouth and nose are blunted by inflammation—those of the eye and ear are often rendered painfully acute.

There are great diversities also in the kinds of pain. Sometimes it is of a dull aching character, as in toothache; sometimes it is a pricking, tingling, smarting sensation: this is the case in some forms of inflammation of the skin, as in erysipelas for example, and in herpes; sometimes it is sharp and piercing, as if the part were stabbed or cut with a knife—such is frequently the case in inflammation of the serous membranes, as in pleurisy for instance; sometimes the pain is tensive or stretching; and sometimes there is scarcely any pain at all. This last chiefly happens in the mucous membranes and in the parenchymatous texture of organs. Very often the pain is a "bulking" or throbbing

pain—every beat of the heart makes itself felt in the tender part. The pain of inflammation results, no doubt, from the implication of the nerves in the diseased process. The stretching of the vessels and textures adds to the pain. Every body who has been plagued by boils (and few escape them) has had proof of this: the pain is most harassing a short time before the ripening little tumour gives way, or is laid open by means of a scalpel; but as soon as the distension is thus relieved, perfect ease and comfort ensue. It is the same in common ear-ache. It is upon this principle, I believe, that the differences in regard to pain, which occur in different structures under inflammation, are partly to be explained. Speaking generally, there is more pain felt in external inflammations, and in the inflammations of *investing* membranes, than in inflammation of the substance of the viscera, or of the *lining* membranes: and it has been conjectured that this may be because, in the latter cases, the parts affected have fewer nerves of common sensation. But I do not think this explanation satisfactory. If it were well founded we should not have such exquisite pain in some of the textures already mentioned, which appear to be furnished with very few nerves of common sensation, and scarcely feel at all in their healthy state: tendons, ligaments, and cartilages, I mean. I think it will be found that most pain is felt in those parts which are least capable of yielding—in which the tension produced by the swelling, or the tendency to swell, is the greatest. The substance of the liver, spleen, and viscera generally, is soft and yielding—the mucous membranes are spongy in their texture, and often attached to the subjacent parts in loose folds, and they allow of an accumulation of blood within them without becoming much stretched, or very tense. The investing serous and fibrous membranes are more tightly applied, and much less capable of yielding: and their inflammation is usually attended with severe pain.

The pain that belongs to inflammation sometimes precedes any other apparent change. This is especially observable in respect to internal parts. Sometimes the pain is continued and uniform. Sometimes it is continued, but irregular in severity, having periods of great exacerbation: sometimes again it is intermittent, or even periodic.

It is an unsettled question that has often been mooted, whether, in inflammation, the state of the blood-vessels is determined by that of the nerves, or the reverse. Mere nervous pains are known sometimes to be followed by congestion of the part in which they are felt. Whatever may be the true state of this question of priority, it is certain that the disordered condition of the blood-vessels, when produced, greatly augments the sensibility of the part. We may suppose that this depends, partly on over-distension and stretching of the vessels and fibres, partly on pressure made upon the nerves by the swelling.

It is important to remark of the pain belonging to inflammation, that it is usually *aggravated by pressure:* frequently it is not felt at all, except when pressure is some how made upon the affected part—intentionally by the medical man—or accidentally, from the movements or position of the patient. This is *tenderness.*

And this is a point which requires a little farther notice. I say the aggravation of the pain by pressure is an important circumstance, because it continually helps us to distinguish pain that is inflammatory from pain that is not inflammatory. Thus pain of the abdomen may result from colic, or spasm—from a distension of the intestines by air, and a stretching of the textures and nerves belonging to them: and this sort of pain will mostly be relieved by pressure; you will find patients lying upon their bellies across the back of a chair for the sake of obtaining ease: but if the pain proceed, as it may, from inflammation of the peritoneum—oh! then the gentlest pressure, even that of the superincumbent bed-clothes, causes intolerable torture. The *suddenness* with which the pressure is made —and its being made on a *part* only of the suffering organ—these circumstances have much to do with the augmentation of the pain; and it is curious, and instructive too, to know that *gradual* pressure, applied *uniformly* to the *whole* organ or part under inflammation, is sometimes so far from enhancing the pain, that it relieves or removes it. Dr. Elliotson puts a very good case in illustration of this. "If (he says) you have a blister upon the sole of the foot, or at the ball of the great toe, and you rest gradually upon the part, the pain becomes mitigated, till at last it seems to be almost entirely removed; but the moment you take off the pressure, and raise the foot from the ground, you feel the part begin to throb—to throb with violent pain."

Now all this exemplifies what I said just now—that though a deranged condition of the nerves, marked by pain, may, for aught I know, first lead to the vascular fulness—yet that fulness, and the distension which it implies, will greatly increase the pain. In fact, the expulsion of the blood by means of well-regulated pressure is made the foundation of certain proposed methods of cure. This has lately been recommended in hernia humoralis, or swelled testicle—what is now more scientifically called orchitis. It gives one a sort of horror even to think of, pressure being made on the healthy testicle—much more when it is rendered preternaturally sensible by inflammation: yet when properly managed it is said (by Dr. Fricke, of Hamburgh, and others) not to increase the pain, but entirely to remove it, so that the patient can at once walk about the room; and the disease is thus ultimately cured. In the same way it has been proposed to cure erysipelas, and gout, and rheumatism. I don't enter here into the expediency of such measures; but mention them in illustration of the various effects of pressure in various circumstances, upon inflamed parts. Certainly it must be dexterously and fortunately managed not to do harm; not to make matters worse; and if there be already much *extravasation* of any kind, I should think that pressure, however regulated, would be peculiarly likely to increase the pain and the mischief.

It is sometimes necessary to recollect, especially when the existence of internal inflammation is suspected, that all expression of the sense of pain, and probably all sensation of pain, may be prevented or abolished by the presence of stupor or *coma*. So also, if the nervous connection between the inflamed part and the sensorium be cut off, no pain is felt. Limbs in a state of palsy are often (though not always) destitute of sensibility also; and inflammation readily occurs in them, but is attended with no pain.

That *mere* pain will not constitute inflammation must I think be plain to you. Spasmodic contractions of the muscles, stretching and tension of the tissues, a particular state of the nerves, and sundry causes that do not imply inflammation, may nevertheless be attended with severe pain.

Let us next consider the *heat*.

Of course, as I hinted before, this means *preternatural* heat: the temperature of the part exceeds that which belongs to it in health; but in truth, the heat is not in general so much increased as the sensations of the patient, or his heightened sensibility, would persuade him it is, or even so much as a by-stander might suppose. The heat of inflammation does not rise above the maximum heat of the blood in the central parts of the body. The natural heat of the blood is about 98 or 100°, but in fevers and inflammatory diseases it has been known to reach 107°, and the maximum heat of the blood in fever is probably the limit of the temperature as it exists in inflamed parts. The surface of the body, in its natural state, is not quite so warm as the internal parts, and the extremities are generally less warm than the trunk; so that the contrast between an inflamed and a healthy part, in respect to heat, is greater in the extremities than on the trunk. Thus if a blister be placed upon the chest, the heat of the part inflamed by its application will not exceed that of the neighbouring healthy surface by more than a degree or two; while a blister applied upon the leg may occasion a difference of 5 or 6°. John Hunter took great pains to ascertain the degree of heat produced in inflammation. He excited inflammation in the cavity of the thorax of a dog, and in the vagina and rectum of an ass, and he could not find that the temperature of the parts thus inflamed ever exceeded that of the blood at the centre of the circulation. He did not neglect the opportunities that came before him of making similar observations on the human body. He had occasion to tap a patient in St. George's Hospital for hydrocele: as soon as he had let the fluid out, he introduced a thermometer through the puncture made by the trocar, and placed it in contact with the testicle. He found the temperature to be 92°. He repeated this experiment the next day, when inflammation had set in, and then the thermometer rose to 98¼°. So that here an increase of 6¼° had taken place in consequence of the inflammation; but even this, you see, did not go beyond the natural warmth of the blood.

It may be presumed that the increase of heat depends mainly upon the increased influx of blood into the part. Possibly some vital actions taking place under inflammation tend to develope it, but it is a curious fact, a fact worth remembering, that the

heat of inflammation does not transgress or surpass that of the blood in the central parts of the body.

Heat alone does not constitute inflammation, for parts of the body may be made preternaturally hot by holding them before the fire, by friction, by exercise, while there is no inflammation.

I apprehend that increased heat is essential to inflammation, in some stage or other of its progress, although there are cases in which the augmented temperature is not perceived or appreciated. Sometimes the increase of heat is very slight, and may be easily overlooked, there being nevertheless unequivocal inflammation; redness and swelling, which go slowly into suppuration. The heat is often concealed from the observation of the physician or the surgeon, by the situation of the part affected, and it escapes the notice of the sufferer, because the sensibility to heat is less generally diffused through the body than the susceptibility of common sensation. The heat of inflammation is usually less felt and less complained of by the patient than the pain. A vivid sensation of heat *is* pain.

The *redness* of inflammation must also be *preternatural* in degree, for many parts of the body are by nature, and in health, more or less red. This phenomenon depends upon the greater quantity of blood contained in the vessels of the part, and sometimes also upon the extravasation of a portion of the blood into the affected texture. There is more blood than usual in those vessels which naturally carry red blood; red blood enters too into vessels which in the healthy state are destined to receive and convey colourless fluids only, or which naturally admit *so few* of the red particles, that from their paucity, and the quickness of their motion, they cannot be seen. We are sure of this from what takes place in ophthalmia. Doubtless also the redness is sometimes increased by the formation of *new* vessels that admit the colouring particles of the blood in visible numbers.

That the vessels which naturally circulate red blood are actually distended and enlarged in inflammation, there can be no doubt. John Hunter (whose treatise on Inflammation is a mine in which all succeeding writers have dug) excited inflammation in one of the ears of a rabbit, and then killed the animal. He next injected the head and ears from the aorta, so that the fluid injected passing through both the carotids, was driven in equal quantities and with equal force towards each ear. The arteries of the inflamed ear were enlarged one-third beyond their natural size, and arteries in it were injected that were not visible in the sound ear. That the apparent increase in the number of blood-vessels is often owing to the circumstance that red blood enters tubes which already existed, but did not previously admit the colouring matter or did not admit it in sufficient quantity to be visible, is evident from the *rapidity* with which the redness may be produced in many textures: in the eye, for example, it may be effected in a few seconds; and many of the vessels which become suddenly apparent are evidently *continuations* of the trunks that could be seen before.

There is much variety in the tint of the redness of inflammation, depending on the kind and degree of the inflammation, and on the nature of the part affected. Sometimes the redness is bright and vivid, as if the part were full of arterial blood; this generally happens in the acuter forms and the earlier stages of inflammation. Sometimes the redness is dark, or livid, or purplish, more as if the part were gorged with venous blood: this occurs in some of the chronic and sluggish forms of inflammation, and it is often the case when there is a tendency to gangrene. Sometimes the redness is distinctly circumscribed, or in patches; and sometimes it is diffused in a general blush over a large space.

The redness may, and often does, remain for some time after the inflammation has ceased.

Now seeing that redness accompanies inflammation of the external parts, we presume that it exists also in internal inflammation: indeed we may convince ourselves that it is so. If a portion of intestine be drawn out through a slit in the parietes of the belly of a dog, and suffered to remain exposed to the air, it will soon inflame, and, inflaming, it grows red. We see also that internal parts are *left* red after death, which parts we have other reasons for knowing had been inflamed during life; and we infer that

redness may have been present during life, although we find none remaining when the body is examined. That when it has been owing to mere fulness of the natural blood-vessels, it may disappear with parting life we know, because the same thing happens externally, as in erysipelas and scarlet fever: but in such cases the inflammation has not gone to any great height.

It is proper to remark, that as the absence of redness is no proof that there has not been inflammation, so its presence is no proof of the contrary. There are many kinds of redness, both within the body and on its surface, that have nothing to do with inflammation: yet some of these are very apt to be mistaken for traces of inflammation. I shall endeavour to instruct you how to avoid such a mistake, when we come to examine the morbid anatomy of particular forms of disease.

While inflammation actually exists, redness, of some shade or degree, is seldom absent, even though the other symptoms may be scarcely apparent.

Lastly, let us take a glance at the *swelling*. This also depends, in part, upon the distension of the blood-vessels; but no great amount of swelling can be attributed to this cause; and as much as does proceed from it occurs *early* in the disease. A part also, and usually by far the greater part, of the swelling, results from the presence of matters poured out into the interstices of the affected part. These effused matters are of very different kinds, although they are modifications of the same liquid, the blood. I mentioned, in describing the condition of the part inflamed, that the central portion of the swelling is, at first, hard and resisting, while at a greater distance from the centre the swelling is softer, and yields more readily when pressed by the point of the finger, and sometimes even pits a little under that pressure. Now the central hardness is to be ascribed to an effusion into the cellular texture of the part, of a fluid, which, transparent at first, speedily becomes opaque and more consistent, and at last assumes a solid form: this is what is commonly called, in this country, coagulable lymph. The softer swelling at the circumference of the tumid part proceeds from the effusion of a thinner fluid, of serum, into the cellular tissue. Under very violent inflammation, blood in substance is poured out into the same parts. When the central portion of the swelling softens and becomes pointed, this part of the whole enlargement is owing to the presence of a quantity of pus. The different liquids that I have now been mentioning are of great importance, and play a conspicuous but diversified part in altering textures. Blood: serum: albuminous fluid or coagulable lymph: pus. They are called the *products* of inflammation. We are sure that inflammation has been at work if we meet with certain of these products. We are not sure there has been inflammation if we perceive mere redness:—we are not always sure if we find serum only:—we are not sure if we find blood alone:—we are tolerably certain if we discover pus; we are certain at least that there has been inflammation *somewhere*, though doubts have been started whether the pus is not sometimes conveyed from an inflamed part to other parts of the body. We are *quite* sure that there has been inflammation in a part if we find coagulable lymph in that part. This often remains, as a monument of the inflammation, during life: it frequently becomes organized, furnished with blood-vessels, and a great number of changes, some reparative, some morbid, depend upon its presence. I shall have to recur to these *products* of inflammation hereafter.

The degree of swelling in different cases depends partly on the intensity of the inflammation, partly on the nature and texture of the structures affected.

I need scarcely observe that swelling may exist without any inflammation. Hernia, simple anasarcous enlargements, dislocations, will occur to you as every-day examples of swellings that have no necessary connection with inflammation.

On the other hand, inflammation may exist without any *appreciable* swelling. Inflammation of the sclerotic coat of the eye, for instance, may be present, without any swelling cognizable by our senses.

We have seen, in this review of the symptoms of inflammation, how much they severally depend, the pain, the swelling, the redness, and the heat, upon the increased influx of blood *into* the part.

It may not be uninteresting to pause here for a moment to inquire what has been ascertained in respect to the actual condition of the capillaries of an inflamed part, and of the

blood they contain. Much has been learned on these points by patient and minute observation with the microscope, and by reasoning upon the facts thus brought to light. Kaltenbrunner, Gendrin, Müller, and others, have corrected many erroneous notions which formerly prevailed upon this subject.

In order to comprehend the minute phenomena of inflammation, you must have a clear conception of the constituent elements of the blood, and of the main changes it is liable to undergo. The rough anatomy, rather than the chemistry of the blood, is what I allude to.

The blood consists of red particles, or globules, and of a transparent colourless fluid called lymph, or liquor sanguinis. Müller succeeded in separating these two constituents of the blood by filtering through paper that of a frog, which contains very large red globules. The liquor sanguinis thus obtained separates spontaneously, by coagulation, into two parts, into serum and fibrin, the last having previously existed in solution in the liquor sanguinis.

When the coagulation is suffered to take place without any attempt to remove the red particles, these are entangled and enclosed in the fibrin as it becomes solid; and the common well-known appearance of clot and serum occurs. You may even then wash out the red particles from the clot, and leave the fibrin.

I must now recur to the experiments and observations of Kaltenbrunner. I should have told you, on a previous occasion, that various stimulant substances, mechanical or chemical, when applied to the web of a frog's foot, will produce irregular disturbances in the circulation, which irregular disturbances you are not to confound with true congestion: in like manner you must avoid confounding them with the phenomena of *inflammation*, which are always preceded by those of true congestion. Kaltenbrunner found likewise, that (just as in congestion) a certain interval of time generally happened between the application of the exciting cause, and the apparent development of the inflammation. This accords with what we observe to be the case in respect to local injuries, and to those local internal inflammations that are apt to be produced by exposure to cold. There is a pause before the mischief lights up: or (to take the metaphor from the eggs of birds) there is a period during which the inflammation seems to be hatching, and it is called accordingly a period of *incubation*. Kaltenbrunner describes inflammation to be a regular process—as he had also described congestion to be.

On looking then at the web, to which some violence had been done, he observed, after the first irregular disturbances were over, and when the period of incubation had elapsed —he found (I say) that an afflux of blood took place to the part about to be inflamed: the velocity of the blood in the vessels was greatly accelerated; the vessels themselves were distended and tense, and therefore disposed to tighten upon the blood they contained —the functions of the part, that is to say, the secretion and absorption of lymph, were interrupted; the blood underwent an evident change—or it failed to undergo the proper changes: its globules stuck together, and the parenchyma of the web became tumefied. Now all this is just what I represented to you in a former lecture as constituting the state of the blood-vessels under *active congestion;* and I also told you, at that time, that such congestion was just one step short of inflammation. The congestion now described increases, until, at length, this remarkable alteration happens: the capillary tubes, instead of tightening upon their contents, dilate, or grow larger; the circulation, at first so rapid, begins to be *delayed* in some of the capillaries; the direction of its motion becomes uncertain; it oscillates, as it were, irregularly in those vessels, and at last stops altogether, the globules cohering in irregular masses, and thus *points of stagnation* are formed; and these points of stagnation, if the affection goes on increasing, augment in size, and multiply in number. Around them, beyond their circumference, the circulation remains still very rapid, and the congestion persists. This is *inflammation*—of which the characteristic or pathognomonic feature is the formation of these *points of stagnation.*

Now one early consequence of the stagnation of the blood is, that a portion of it transudes through the sides of the vessels containing it: the serum; or the liquor sanguinis; or even sometimes the blood itself, red particles and all. The effused serum remains, or is absorbed, as serum. The fibrin, when it has so transuded, concretes, and thus the interstices of tissues are filled up, and layers of coagulable or coagulated lymph are

formed upon the surfaces of inflamed parts, constituting false membranes; and under certain circumstances, already adverted to, other, or farther changes take place; the red particles which have passed into the inflamed tissue, or which still remain stagnant in the capillaries, undergo a remarkable transformation, lose their coloured capsules, become yellow, grow larger, and are converted into globules of *pus*. So that pus is nothing else than altered blood.

But pus appears also to be formed in a more rapid manner, by a sort of secretion: it almost streams, sometimes, from the surfaces of mucous membranes. It has lately been affirmed that certain globules, visible in blood drawn during health, and larger that the rest, are pus-globules; and that under inflammation these pus-globules multiply some how. The question must be regarded as *adhuc sub judice*.

Certainly much which used to be thought mysterious in the process of inflammation has been rendered more simple and intelligible by modern research. Most of the events or consequences of that process are traceable to the stagnation of the blood in the capillaries, and to the changes which the stagnant blood subsequently undergoes.

I must not omit to tell you what Kaltenbrunner says about the direct absorption that takes place in the inflamed part. He found that the *colouring* matter, and the *adipous* matter, were thus taken away. The web of a frog's foot is speckled over with little stars of five rays, caused by a black pigment. The extremities of these rays gradually disappear until mere black points are left in the places of the stars. He says that he has been lucky enough to catch the exact moment when the blood, circulating rapidly in the canals, has detached a particle from one of the rays, and carried it into the torrent of the circulation. In the sound state, the mesenteric vessels of the rabbit are surrounded with much fat. When the mesentery is inflamed, the adipous cells soon empty themselves: a number of capillary canals develope themselves on the walls of those cells, and it is probable that the fat is carried off by the blood circulating in these canals.

Another curious and interesting sight witnessed by Kaltenbrunner was the formation of new blood-canals. He says that in an organ recently inflamed, when the circulation is accelerated, globules of blood may be seen to sally, all of a sudden, from some capillary, pass into the surrounding parenchyma, force themselves a channel, and reach another capillary canal. Thus a new capillary channel is formed; the blood circulates through it; its formation is often the work of a few seconds only. As the same thing is repeated in different parts, a rich net-work of new capillary canals is added to the original set, whereby it happens that organs which in the sound state are but slenderly furnished with capillary vessels (as the mesentery of the rabbit) present an astonishing number under inflammation.

While new capillary vessels form, the old ones dilate, and assume the appearance of small arteries or veins according as they are continuous with the arteries, or border on the veins.

The fact has long been known that when coagulable lymph has been poured out in inflammation, blood-vessels gradually form in it, whereby it obtains a vascular connection with the surrounding textures, and becomes a living portion of the body: and these microscopic disclosures of the manner in which they form appear to me to possess a peculiar interest.

I should be making a very wasteful use of your time and my own, if I entered into the undecided and unprofitable disputes that have been raised respecting the vital conditions of the vessels engaged in inflammation. While some have pretended that the action of the small vessels is increased, others assert that it is diminished; that the vessels are in a state of atony. For my own part I have never yet seen any conclusive evidence that the small vessels possess any vital contractile power distinct from their elasticity. And granting them such a power, it is extremely difficult to conceive how any increase in their vital contraction should produce the changes that are observed in inflammation. Certainly we have no warrant that any such contraction takes place, in the results of microscopical examination of the vessels of an inflamed part. The inquiry might be more properly directed, I think, into the vital conditions of the *nerves* of the part: but here we are wholly in the dark.

I do not think it so evident as some have supposed it to be that a greater quantity of

blood than is natural passes through an inflamed part in a given time. It is quite true—and it is proper that you should be aware of it—that the arterial trunks leading to an inflamed part often pulsate with more than ordinary force, and, if opened, project a jet of blood farther than they would naturally project it. It is true also that a venous trunk leading from an inflamed part will discharge a greater quantity of blood in a given time than a corresponding vein leading from a sound part. Mr. Lawrence declares that he has frequently tried this experiment, and always with similar results. He has found it necessary to bleed a patient whose hand and forearm were inflamed; and he has opened a vein in each arm, and has found that within the same space of time, the two veins being opened at the same moment, about three times more blood flowed from the vein of the inflamed limb than from that of the sound. But it scarcely follows from this that more blood circulates through the part actually inflamed: the activity of the circulation in the vessels that remain pervious, and are merely congested, around the focus of inflammation, is greatly increased, and more blood circulates through the *limb:* and yet the blood may be stagnant, or scarcely circulate at all, in the very part that is strictly and truly inflamed. However, the fact of this increased afflux of blood towards the parts concerned in the inflammatory process is an important one.

LECTURE X.

Inflammation continued. Buffy coat of the blood. Terminations or events of inflammation. Resolution; delitescence; metastasis. Effusion of serum. Effusion of coagulable lymph, or fibrin. Organization of this lymph. Suppuration. Ulceration.

In the last lecture, after giving a very general sketch of the phenomena of inflammation, I particularly considered its four characteristic symptoms—pain, heat, redness, and swelling: and endeavoured to describe the changes that take place in an inflamed part, as they are seen through a microscope.

Buffy coat of the blood.—There is one very remarkable and important circumstance which is not often absent in cases of inflammation, but which hitherto I have barely mentioned: I mean a peculiar appearance of the blood itself after it has been drawn from a vein. A portion of the fibrin at the upper surface of the coagulum parts with its colouring matter; so that upon the deep red clot there is to be seen a layer of a yellowish, or sometimes of a bluish white colour, varying in thickness from a line or two to perhaps three-fourths of an inch. This uppermost whitish layer of the coagulum is called in this country the *buffy coat* of the blood. Sometimes the surface of the buffy coat is flat and wide; but often it is contracted and concave; *i. e.* the diameter of the buffy surface is less than the diameter of the lower portion of the clot, and it is hollowed out into a cup-like form. Accordingly the blood is said, in these circumstances, to be both buffed and cupped. The formation of this buffy coat appears to be favoured by many circumstances which have nothing to do with the disease under which the person may be labouring; such as the size of the aperture in the vein, the manner in which the blood flows, the form and size of the vessel that receives it: but it does not occur at all except in certain conditions of the system; and it belongs so especially to the state of *inflammation*, that blood having the buffy coat upon it is often spoken of as *inflammatory blood*, or, with less propriety, as *inflamed blood*. Both these expressions indeed are incorrect, for inflammation sometimes exists without buffy blood; and buffy blood sometimes occurs without inflammation. The phenomenon is, however, upon the whole, a very valuable index of the nature of many cases of disease, and an important guide in their treatment.

Now this crust, or upper layer, or buffy coat, consists of pure fibrin, mixed with a certain quantity of serum, which M. Gendrin says is fuller of albumen than the

rest of the serum. You will not fail to notice the great analogy that subsists between the buffy coat and the coagulable lymph poured out in inflammation, either into the texture of the part; or (as I shall show you more particularly by and by) upon its surface, forming what are called false membranes. Both in appearance, and in chemical composition, the two seem to be identical; and no doubt exists in my mind of their being actually the same substance: the separation in the one case taking place from the blood while contained in its proper vessels; in the other case from the blood after it has been removed from the body.

There has been a great deal of speculation among pathologists as to the cause of this buffy coat. From its situation it is plain that gravity has something to do with its formation: that the red particles, leaving the colourless fibrin before it coagulates, sink downwards by their own weight. But though the *subsidence* of the red particles is occasioned by their greater specific gravity, their *separation from the fibrin* is not to be explained upon that principle alone. If it were, then it would follow that the slower the coagulation of the blood, the more time would there be for the sinking of the red particles, and the thicker and more decided would be the buffy crust: and it used to be supposed that this was the true explanation of the phenomenon. Careful observations, however, have shown that the formation of the buffy coat often takes place when the coagulation of the blood is unusually rapid. Dr. Davy and M. Gendrin both state, as the result of much attention to the subject, that the coagulation of blood drawn from a vein during inflammation begins sooner, and is more quickly completed, than that of healthy blood. But certain observations made and published by Dr. Stokes have settled this question. He noted the appearance of the blood in 27 cases. In 15 of these the buffy coat presented itself; in 12 it did not. Now in 3 of these 12, the coagulation of the blood did not begin till from 20 to 40 minutes after it was drawn; and in 4 others there was no coagulation for 8 minutes. So that there was plenty of time for the red particles to have left the fibrin, and subsided; but they did not do so. On the other hand, in 12 out of the 15 cases in which the blood *was* buffed, the coagulation took place in 5 minutes; and in the remaining three it was only delayed 14 minutes.

The slowness of the coagulation, therefore, although it may and doubtless does *favour* the subsidence of the red particles when they have a tendency to subside, cannot be regarded as the cause of the buffy coat. The red particles very soon begin to subside when they subside at all: you may tell, immediately after it has been drawn, and prior to any coagulation, that blood is about to buff, by a peculiar bluish hue on its surface. A German writer, Schroeder Van der Kolk, has stated observations to the same purpose, showing that in the blood abstracted by venesection during inflammation there is an unusual disposition to a separation of the fibrin from the red particles: a sort of *repulsion* between them. This separation takes place in mere films of blood, so thin as not to admit a buffy stratum to be laid above a red stratum. The fibrin and red particles then separate from each other laterally by horizontal movements, and the films acquire a speckled or mottled appearance, quite as characteristic of the state of the blood as the buffy coat itself.

That the formation of the buffy coat depends, however, upon some *vital* change in the blood appears probable from this—that it will sometimes vary greatly in different portions of blood abstracted at the same bleeding. Thus if the blood be received into four different cups in succession, it will perhaps be buffy in the first, and in none of the others; or it will be buffy in the last only; or in the second and third only, the first and fourth cups being free from buff. Attempts have been made to explain these rapid variations. Some have fancied that the inflammatory state having been remedied by the removal of a certain quantity of blood, the blood that flows subsequently is *therefore* without the usual index of the presence of inflammation; but this explanation will not apply at all to those cases in which the portions last drawn are the only portions that exhibit the buffy crust. Others have suggested that the state of the nervous system is principally concerned in these sudden changes—that the depression caused in the outset of the bleeding by fear, and the faintishness produced towards its termination by the loss of blood, may prevent the appearance of the buffy coat on the first and last cups, when it shows itself only in those that are intermediate between the first and last. We cannot rely much on these hypothetical

explanations: I mention them to impress upon your memory the facts which they are intended to explain.

There are two or three different forms presented by buffy blood; and with these you ought to be familiar.

In one form the buffy coat is thick, tough, contracted, puckered at its circumference, and its surface is cupped. There is a complete separation of the red particles, and a strong aggregation of the particles of the fibrin among themselves. The red portion of the coagulum is also, in these cases, round and contracted, of a globular shape, firm, detached from the sides of the vessel, and floating, generally, in transparent serum.

This is usually seen when the inflammation is violent; when it occurs in strong and vigorous constitutions; and more I believe when it has its seat in certain tissues, in fibrous and serous parts especially.

In another form, the whole coagulum is large, like a cake, or of the shape of the vessel containing it, not so much collected into a spherical form: and the buffy coat is thin and flat, and easily broken. Here there is an imperfect separation of the colouring matter from the fibrin, and no great aggregation of the particles of the latter. This kind of buffy blood is apt to accompany slight or partial inflammation.

In a third form, the buffy coat is thick and abundant, but it is flat and soft, loose and flabby, like paste, and the coagulum often adheres by its edges to the vessel in which the blood has been received: there has been considerable separation, and but slight aggregation. The serum is apt to contain a few red particles distinct from the clot. Such blood is commonly said to be *sizy*. Dr. Alison states that when it is observed, some other cause of general disorder of the system (continued fever, for example) usually coexists with the local inflammation.

It is a curious fact that blood drawn by leeches never exhibits the buffy coat. It seldom appears (yet I have seen it) upon blood that has been removed by means of cupping glasses.

Arterial blood is liable to the buffy coat. I have myself twice at least seen the buffy coat on blood drawn from the temporal artery. One of the patients was violently maniacal when the blood was taken. The other was labouring under acute inflammation of the membranes of the spinal cord, of which inflammation he died.

Blood is more likely to buff, *cæteris paribus*, when it is made to flow in a full stream, and when it is received in a deep and narrow vessel.

On the other hand, the formation of the buffy coat appears to be hindered, when the blood trickles from a small opening in the vein, and when it is caught in a large flat vessel. It is said also that the buffy coat may be prevented by adding to the blood a solution of caustic potass; by keeping it for some time in a state of agitation; by receiving it in a very cold vessel; or by suffering it to fall from a height of three or four feet. In this last case M. Gendrin supposes that the prevention is owing partly to the agitation which the descent of the stream produces in the blood already in the vessel, and partly to the circumstance that the blood is cooled as it passes through the air.

It is seldom, I believe, that the buffy coat appears on blood drawn at the very outset of inflammation; generally a day or two elapses before it shows itself.

I have dwelt the longer upon this peculiar appearance of the blood, because it really is of very great importance in determining the nature of various complaints, and in directing our treatment of them. Speaking generally, when a given organ is inflamed, the buffy coat is more marked in proportion to the intensity of the inflammation: when the organ is not known, it is more likely to be of a fibrous or a serous texture, in proportion as the blood is more decidedly buffed. The appearance of the buffy coat is especially valuable as an indication of treatment in cases concerning which we are in doubt, whether they are inflammatory or not. On the other hand, if we have good evidence, in other symptoms, of the existence of inflammation, we are not to be shaken in our opinion by the absence of the buffy coat. Inflammation may certainly exist without it. I am not speaking now of slight cases of inflammation, which do not disturb the general system. You would not look for buffy blood in the inflammation that supervenes on a cut finger, or in a small boil; but in serious inflammation, attended with pyrexia, the buffy coat may be wanting. It is not un-

frequently absent in inflammation of the mucous membranes, especially in inflammation of the mucous lining of the bronchi.

I stated before—what it is quite necessary to remember—that buffy blood is not confined to cases of inflammation. The blood of persons affected with general plethora is often found to present a buffy coat: and the same thing is true in respect to pregnant women.

Buffy blood is no necessary measure of the *danger* of the disease. The blood drawn in acute rheumatism is always very much buffed and cupped: yet so long as the disease is confined to the joints it is quite free from danger.

Neither is the appearance of buff on the blood, taken by itself, a sufficient warrant for abstracting more blood: for the blood will sometimes, in common inflammation, continue to be buffy, long after it has ceased to be useful, or safe, to bleed the patient.

Events of inflammation.—Some pathologists enumerate several *terminations* of inflammation. Others quarrel with that word, as inappropriate; alleging, with great truth, that the inflammation does not necessarily cease or terminate whenever these so-called "terminations" happen. Some of them are in fact "coexistent states, or successive stages in the progress of the same inflammatory disease." It has been proposed to speak rather of the *local effects* of inflammation; but even this phrase is not free from objection, for sometimes (though rarely) there are no local effects produced, beyond the four symptoms which characterize the inflammation itself. I think the *events of inflammation* is an expression not open to similar cavils. I have no ambition to introduce new modes of speech, unless when those that are already in use are inexact or inapplicable. It is enough if you clearly comprehend the meaning of the terms I employ. Among the events of inflammation I include only the *local* changes observed in its course. To those which are constitutional I must afterwards revert.

You will recollect that I did allude, in the last lecture, though in a very brief manner, to those local events of inflammation. Their frequency and importance renders it necessary that we should consider them somewhat more particularly.

Resolution.—One of these events is the simple subsidence or *resolution* of the inflammation: this may strictly be called a *termination* also. The congestion of the blood-vessels increases till the blood stagnates in some of the capillary canals towards the centre of the affected part, which is then said to be *inflamed;* but the disease goes no farther; there is no escape of the blood, or of any part of the blood, or of any of the constituents of the blood, beyond its natural channels; or, at any rate, there is no *sensible* evacuation into the inflamed tissue, or next to none. The inflammation begins to recede; the stagnant blood is again set in motion; if there have been some slight effusion, it is reabsorbed; the rapidity of the circulation in the surrounding vessels diminishes; and the part returns, in all respects, to its former condition and integrity. This may be considered the spontaneous cure of inflammation; and to this event there seems to be always a natural tendency. It may be promoted sometimes by art.

Delitescence and metastasis.—When the process of resolution is unusually *sudden* and *rapid*, (as it occasionally is, the well-marked phenomena of inflammation completely disappearing in a few hours,) it is called by our neighbours, the French, *delitescence*. And when the symptoms of inflammation thus suddenly desert one part, and show themselves immediately afterwards in another, (as not unfrequently happens in respect to the joints in acute rheumatism, and between the parotid gland and the testicle or mamma in the mumps,) *metastasis* is said to take place.

This transference, as it were, of morbid action, from one part to another, is a very curious circumstance. It is one which we sometimes endeavour to imitate. We excite inflammation upon the surface, where we know its effects will be of comparatively little consequence, in the hope of *diverting* it from some internal organ in which it threatens to work serious or even fatal changes. We follow the same principle perhaps where we apply purgative medicines to the mucous membrane of the alimentary canal. To denote this mode of cure, by stimulating distant parts, the terms *counter-irritation*, *derivation*, and *revulsion*, are employed.

Most commonly, even under moderate inflammation, *some* amount of extravasation takes place into the texture or from the surface of the part.

Serous effusion.—The first effect or event of that kind which we notice, is the pouring out or *effusion* of *serous* liquid. The liquid is so like the serum of the blood, that it is called serosity or serous liquid, and there can be no doubt, I conceive, that it consists of the serum of the blood, slightly modified perhaps. You will recollect my stating that the swelling which accompanies phlegmonous inflammation is not equally firm throughout the whole extent of the inflamed part; that it is hard in the centre, softer towards the circumference; and that in the latter situation it sometimes retains for a few seconds the impression of one's finger;—*pits*. Now this results from the effusion of serous fluid in the cellular tissue immediately surrounding the part inflamed: it is neither more nor less than *œdema:* œdema, (which may exist also quite independently of inflammation) being a filling or infiltration of the cellular tissue with serous fluid. Anasarca is an example of the same state, on a larger scale. Now anasarca is very constantly produced by some impediment to the passage of the blood along the veins; the serous portion of the stagnating blood transudes through the coats of the vessels: and I apprehend that the same explanation may be given of the less extensive œdema which takes place around a phlegmon; the blood being stagnant in the neighbouring capillaries.

But whatever may be the intimate cause of serous effusion, it is one of the earliest events of inflammation; and in some cases it is its most important event, producing, mechanically, new symptoms, and giving rise to conditions of the most perilous kind. The quantity of serous fluid poured out in a short time is often immense. One of the pleuræ may be thus filled in a few hours, and the whole of one lung strongly compressed, and the respiratory apparatus reduced to one-half of its customary efficiency. And if inflammation and effusion should take place on both sides of the chest at once, if *double pleurisy* should occur, as it sometimes does, the patient must presently perish by apnœa, unless his condition is recognised, and free vent is given to the fluid. More than once or twice have I seen persons snatched from the brink of suffocation by what is called *tapping* the chest. Fatal coma is no uncommon result of the effusion of serosity, as an event of inflammation, into the ventricles of the brain.

Even in the cellular tissue, where it is properly enough called œdema, a very trifling amount of this serous effusion may be sufficient to destroy life: when, for instance, it takes place into the submucous cellular tissue of the glottis, closing up by its pressure the little chink, the rima glottidis, and suffocating the patient after another fashion. Here also art may come to the rescue: an *artificial* chink or hole is made for the entrance and exit of air, below that part of the larynx in which the disease is situated, and the patient is delivered from imminent death. I have had two cases under my own care, and have seen several others, in which life *was* so preserved. I allude to such cases now, merely to convince you of the importance of attending to this event of inflammation, and of studying the indications of its existence.

Sometimes some of the small vessels give way, and *hemorrhage* into the part becomes an event of inflammation. Some slight degree of this occurs probably in most cases; and we frequently find that the colouring matter of the blood is mixed with the other effusions, giving to the serous liquid a deep tinge of red.

I hinted beforet that we must not infer inflammation from the presence of serous effusion *alone*. Serum will exude, I believe, from loaded veins, even after death; but this never can be much in amount. It is certain that dropsical effusions may be, and very often are, the result of congestion of a purely mechanical kind.

Effusion of coagulable lymph.—A third *event* of inflammation is the effusion of what is called *coagulable lymph*, which, as I explained to you before, appears to be nothing else than the fibrin separated from the other constituents of the blood, and concreted. It is poured forth, at first, in a state of solution, or in a soft semi-fluid condition, and mixed with more or less serosity; but the more fluid parts of the effusion are either soon reabsorbed, or soon separate themselves from the lymph, which becomes firmer, and at length solid. The hard central portion of a phlegmon, in its earlier stages, owes its hardness to the presence of coagulable lymph in the natural interstices of the inflamed part; and a similar interstitial deposit of the same substance is common in various parts of the body, as a result or concomitant of inflammation. What is called hepatization of the lung is one instance—the spongy texture of the lung is blocked up and solidified by this lymph.

In certain cases of erysipelas, as well as in phlegmonous inflammation, the subcutaneous cellular tissue is rendered dense and hard in the same way. The white opaque spots which are often seen upon the cornea are produced by lymph interposed between the layers of that naturally transparent structure. But the most striking examples of the effusion of coagulable lymph are to be seen upon the surfaces of inflamed membranes: it forms a web or layer which by degrees assumes, itself, a membranous appearance; and is accordingly called by morbid anatomists a false or an adventitious membrane. Sometimes several layers of this kind are spread over each other, forming adventitious membranes of great thickness. When coagulable lymph is thus poured out between membranes that are habitually in contact with each other, it often causes them to cohere together—just as two leaves of a book may be made to stick together by a layer of paste or glue put between them. This is very common indeed with serous membranes, especially the pleuræ, the pericardium, and the peritoneum. Lymph is said also to be poured out, under violent inflammation, from mucous surfaces. In croup, the interior of the trachea is inflamed, and a substance exudes which assumes a membranous form, and adheres more or less firmly to the sides of that tube, or is coughed up in ragged fragments. A similar effusion takes place occasionally from the mucous lining of the alimentary canal, and is expelled, with the other contents of the bowels, in shreds, or in tubular portions, which are, in fact, casts of the interior of the gut. I say *coagulable lymph* is *said* to be thus poured out; but it is more than doubtful whether the false membranes in these cases are really composed of fibrin. Similar films form within the uterus, and are moulded to the exact shape of its cavity, and marked with indentations that correspond with its rugæ; and these membrane-like casts are at length separated and extruded. These last are not very common; but I show you one which came from a young woman who was a patient of mine in the Middlesex Hospital not very long ago. You may see lymph deposited like beads, upon the anterior surface of the iris under inflammation; or gluing its posterior surface to the crystalline lens behind it, and rendering the pupil irregular, and sometimes immovable. The internal surface of the heart, and especially its valves, are often studded with portions of lymph much resembling warts. When the opposite sides of an artery are brought together by a ligature, they inflame, and become united by the same medium. Coagulable lymph is effused, in the course of a few hours, upon the edges of a cut wound; and they adhere, under favourable circumstances, when placed in mutual apposition. This surgeons call union by *the first intention:* and the inflammation which is accompanied by this kind of exudation of lymph, or fibrin, is called *adhesive* inflammation; or the *adhesive stage* of inflammation.

It is seldom that coagulable lymph *alone* is thus poured out. Sometimes it is mixed with the colouring matter of the blood. Oftener it is mingled with, or at first dissolved in a large quantity of serous fluid. When this happens in serous bags—as in the pericardium or pleura—the thinner fluid may keep the opposite membranes apart; and for some time, or entirely, prevent their agglutination. Sometimes the agglutination is partial, and the uniting portions of lymph are stretched out, by the distending effect of the fluid effusion, or by the natural movements of the parts, into strips or bridles of adhesion.

I must call upon you to notice, in passing, that although this event of inflammation may sometimes perhaps have a detrimental or destructive consequence, yet that in a vast majority of instances it is distinctly a salutary and conservative event. Vision may, no doubt, be destroyed by a plug of lymph which shuts up the pupil of the eye. A portion of intestine may become strangulated by a band of adhesion. Of this, which is a mere accident of the adhesion, I have seen some fatal examples. The closure of the trachea by the membrane of croup is not, in my opinion, a fair case in point. There are, at any rate, but few exceptions to the rule, that the effusion of coagulable lymph proves beneficial by preventing some worse event of the inflammation. It is better that inflammation of the cellular tissue should be limited and hemmed in by a barrier of lymph, than that it should extensively diffuse itself. It is better that the bag surrounding the heart, when it happens to be inflamed, should become adherent to that organ, than that the inflammation should run on into suppuration, and fill the pericardium, and oppress the heart, with pus. In the one case life may continue for several years—in the other it seldom lasts

many days. It is clearly more desirable, and more consistent with the safety and comfort of the patient, that his lungs should be fastened to his ribs, than that they should be compressed and flattened against his vertebral column. I shall have occasion so frequently to speak of this protecting and reparative tendency of adhesive inflammation, that I do no more than point it out to you at present.

When lymph has been effused upon an inflamed surface, it very readily becomes vascular and *organized*. Red streaks begin to be visible in it. These are incipient blood-vessels, which may soon be seen to communicate freely, and to be continuous with the blood-vessels of the inflamed part. The plastic lymph is fashioned into a definite structure, and made a living constituent portion of the body. It is in truth this remarkable *plastic* property belonging to the effused lymph, this aptitude for being organized, which invests the adhesive inflammation with its guardian and reparative character. None of the other fluids poured out under inflammation are capable of this change. It is in this way that the lips of recent wounds, and the surfaces of inflamed membranes in contact with each other, are permanently stitched together (if I may use so homely a metaphor) by living vascular threads. By this needle-work of nature parts recently severed from the body may sometimes be replaced; or even transferred and affixed to other situations, as in the Talicotian operation, whereby a new nose is engrafted in the place of that which had been lost. It is thus that ulcers fill up; successive layers of lymph exude, and are in succession attached to the ulcerated surface, and incorporated by this organizing process, until the breach of texture is repaired. The lymph thus organized comes at last to resemble, very exactly, cellular tissue, more or less condensed. And thus, as I pointed out to you in a former lecture, a texture, identical in its nature with an original texture of the body, is formed as it were *de novo*, and is capable of undergoing the same transformations with the natural texture.

The length of time required for the pouring out of coagulable lymph in inflammation—and for its organization after it has been poured out—is variable under different circumstances. It is often effused very early. Dr. John Thomson found a distinct layer of it covering wounds he had made in an animal, in less than four hours after they had been inflicted; and cases are related, in which vascular organization of the effused lymph has appeared to have been effected within the space of twenty-four or thirty-six hours. Sometimes, on the other hand, many days seem to elapse before any such organization is observable.

When serous fluid and coagulable lymph have been poured out in considerable quantity, and simultaneously, the serous fluid is often rendered turbid by the admixture of small portions of lymph, or of albumen, diffused through it; and *flakes* of lymph sometimes float in it, or settle, in virtue of their specific gravity, at the lowest part of the cavity containing the effused matters.

Several conditions seem necessary to ensure this adhesive form, or adhesive stage, of inflammation. The inflammation must reach a certain degree of intensity, or no lymph will be effused; it must not go beyond a certain degree of intensity, or the next *event* I have to mention, the formation of pus, will interfere with the adhesive process. We learn also from what takes place in recent wounds, that seclusion from the air, and the absence of all other causes of irritation, are necessary for adhesion, or union by the first intention.

Suppuration.—The formation of *pus—suppuration*, is a fourth *event of inflammation*, to which brief allusion has already been made.

Many opinions have been broached respecting the nature of pus, and the manner in which it is formed. I shall not trouble you with discussing, or even with narrating them, but limit myself to stating what are the notions held on these points by the most able and the most recent observers.

Pus, then, as I told you before, is altered blood. It is an opaque, smooth, yellowish fluid, of the consistence of cream, and having no smell. I speak now of well-formed, or what is called good, or healthy pus—what the old writers spoke of as *pus laudabile*. This has been thought an absurd epithet: but it serves as well as any other to express what was meant, viz. that kind of pus which accompanies benign forms of inflammation, and indicates that all is going on regularly, and promises a fortunate ending: pus, in short,

the appearance of which was *to be commended*. It is certainly not more absurd than the term *healthy* pus. This pus laudabile was described as being *album, lævе, et æquale*—light-coloured, smooth, and homogeneous. This description of good pus has descended from the time of Hippocrates, who says Το δε πιον, αριζον λευκον τε ειναι, και ομαλον, και λειον, και ως ηκιστα δυσωδες. It consists of yellowish globules, diffused through a thinner fluid, that resembles in some respects the serum of the blood. "If six or eight ounces of good pus be suffered to stand in a phial, it will separate into two portions: a yellowish matter will sink to the bottom, and there will be a slightly yellow, clear, supernatant fluid, like oil in appearance, but not greasy to the touch." The sediment consists of the globules: there is good reason to believe, with Gendrin, that they are the globules of the blood, altered—deprived of their coloured envelopes, and swollen or enlarged. The changes which they are apt speedily to undergo, from mere exposure to the air, may accouut for a contrary opinion held by some modern authors.

But there are various modifications of pus; and its qualities are liable to rapid alteration by various circumstances. Sometimes the globules are few in proportion to the more watery part; and then the pus is said to be *ichorous*. It is *sanious* when some of the colouring matter of the blood is poured out with it. It is sometimes viscid and slimy, from an admixture of mucus; or flaky and curdled, which is common in scrofulous persons. Sometimes, also, instead of being inodorous, it is horribly fetid. All abscesses that form in or about the alimentary canal are apt to contain pus of an offensive odour; as those which occur in the tonsils, and near the rectum. A patient of mine, in the hospital, had a fluctuating tumor in the epigastrium, which Mr. Arnott opened. There came out the collapsed bags of two or three hydatids, and a quantity of stinking pus. The liver, no doubt, was the seat of the suppuration in this case—and perhaps the stench might be owing to the death and decomposition of the hydatids.

Great pains have been taken by many persons to discover some sure criterion between pus and mucus. Healthy pus and healthy mucous are so totally unlike each other, that they never can be confounded together. But sometimes we can scarcely say whether we are looking at mucus so altered as to resemble pus, puriform mucus—or at genuine, though not perhaps laudable, pus. I shall tell you what has been made out upon this point hereafter, when I treat of phthisis; and I shall show you at the same time that the distinction is not of that great importance which has been sometimes supposed.

Pus may be poured out into one of the natural cavities of the body, and then it is called purulent *effusion*. It may be contained in a closed cavity, which is not natural, but formed by lymph and condensed celluar tissue; and then the collection of pus is called an *abscess*. It may also proceed from a free surface of the body—as the skin, or a mucous membrane, or a superficial ulcer or sore.

In the natural cavities of the body pus seems, sometimes, to mingle gradually with the serous effusion, which grows turbid and whitish, and at length distinctly assumes a puriform character. But in much the greater number of cases the formation of pus is preceded by the effusion of coagulable lymph, with or without the effusion of serous fluid: the pus in these cases appears to be poured forth or secreted by the coagulable lymph after it has become organized. Its formation seems to characterize a more advanced stage of inflammation—to denote that the inflammation has been pressed a little beyond the adhesive stage. This was the opinion of John Hunter, who was the first to teach us any thing worth knowing about the process of inflammation. It is also the opinion of Gendrin, one of the latest and most successful investigators of that process. Hunter thus expresses himself on this subject: "the new formed matter peculiar to suppuration is a remove farther from the nature of the blood than the matter formed by adhesive inflammation." And Gendrin says, "Between the *purulent fluid* of inflamed tissues, and the *organizable coagulable* fluid (*i. e.* between pus and coagulable lymph) there is but *one* degree of *more*."

Even the preventing or the allowing the access of air to the surface of a recent cut will make all the difference between adhesion and suppuration. And the same influence of the air in promoting the suppurative process in preference to the adhesive is remarkably seen in various other cases. In simple pleurisy—from exposure to cold—we seldom have

any liquids effused, except coagulable lymph, and serous fluid. But if the inflammation has been caused by a punctured wound from without, or by laceration of the *pulmonary* pleura by the sharp end of a fractured rib, or by a perforation of the pulmonary pleura by the extension of a vomica in the lung—in all which cases air finds its way into the cavity of the pleura—then true *empyema* results—pus is formed. So also in pneumonia: at first the inflamed lung is rendered solid by the effusion of coagulable lymph into the air cells; but if the inflammation persists, the next thing that happens is what is called by Laennec *grey* hepatization—a puriform infiltration takes the place of the lymph. The same principle is exemplified in the case of the urethra; inflammation of the *free* surface of its mucous membrane leads rapidly to the formation of pus; inflammation of its *attached* surface occasions the pouring out of lymph, which produces stricture. And in general I think it may be said of surfaces that are open to the air, of tegumentary membranes, that either pus is formed upon them, under inflammation, without any previous effusion of plastic lymph, or the lymph is slight in amount, and transient in duration, and presently superseded by a puriform discharge. We have every-day examples of this, in inflammation of the conjunctiva, of the bronchi, and of the bladder. Perhaps it is in this principle that we may find an explanation of the fact that whereas in the inflammation of cellular tissue, of glandular organs, and of the parenchyma of the viscera generally, the pus which forms is collected into an abscess; circumscribed abscess in the substance of the lung, from common inflammation, such as we are now considering, is very rare indeed. This is a point which will of course come under our consideration again.

There is however, manifestly, a close connection in many cases between the effusion of lymph and the effusion of pus; although the progress and effects of adhesion and suppuration are very different. When suppuration takes place, the pain belonging to the inflammation usually abates, or ceases, except when the pus is imprisoned so as to keep up the pre-existing tension. Certain remarkable constitutional phenomena also declare themselves, which I shall notice again hereafter.

The effusion is longer continued in the case of suppuration—and the quantity of pus is more copious generally than of lymph—especially in the serous and tegumentary membranes. When pus is diffused through the natural textures it tends to soften and separate them—to break them down: whereas the direct effect of the deposition of lymph in the same parts is to consolidate and harden.

The time required for the formation of pus is extremely variable. It sometimes very quickly follows the commencement of the inflammation; within a few hours, as in gonorrhœa. Sometimes it is postponed to a very distant period, even for weeks.

The duration of the suppurative process is also uncertain, and seems to have no fixed relation to the intensity of the inflammation by which it has been preceded or accompanied.

Ulceration.—A *fifth event* of *inflammation* is *Ulceration*. You may remember my telling you that Kaltenbrunner observed the progress of absorption in the inflamed tissues which he examined by the help of the microscope: how the stellated spots gradually vanished from the web of a frog's foot, and the fat from the mesentery of the rabbit.

Independently of these microscopical observations, it is quite evident that absorption goes on, often very actively, during the continuance of inflammation. The effused fluids, or products of inflammation, the serum, the lymph, the pus, are partly taken up again: and not only are these products of inflammation liable to be so removed, but the original textures of the body are carried off by absorption. We cannot have a better proof of this than the progress that an abscess makes to the nearest surface at which the pus it contains may be discharged; the intervening textures are gradually absorbed. Perhaps a great part of the principle concerned in this progressive approach to the surface is *pressure*. The harder tissues of the body, the bones themselves, yield and disappear before the increasing pressure of an aneurismal tumour. In this case the absorption appears to be independent of inflammation.

But taking the process as one of the events of inflammation, we may say with Dr. Alison that, whenever the absorption of the effused lymph, and of the surrounding textures, takes

place in excess—in a greater degree, that is, and more irregularly than seems to be required for any useful purpose—the result is *ulceration*. This term is, however, commonly restricted to those cases in which the loss of substance occurs upon some *surface* internal or external.

Many circumstances influence the occurrence and progress of ulceration; and great differences are observable in the different tissues, in respect to the facility with which they severally ulcerate. Ulceration is most common in the tegumentary membranes. It is frequently met with also in the inner coats of the arteries, in cartilages, and in bones. But we are not always sure that it is in these cases an event of inflammation. Ulceration is rare in fibrous tissues of all kinds, in serous membranes, and in the outer coat of arteries. These differences have important pathological bearings. But I may not stop to consider these at present: they will be particularly noticed as the course proceeds. When I state that ulceration may lead to perforations of the alimentary canal—of the air-tubes—of the gall and urinary bladders—of the blood-vessels; and to the fatal escape of the natural contents of these organs; I have said enough to convince you that ulceration, so frequently the object of the surgeon's care, requires no less attention on the part of the physician.

There are certain forms of ulceration that are specific in their nature: with these I do not at present meddle. The process of ulceration is very clearly explained in Dr. Alison's admirable *Outlines of Pathology*.

There are three things generally going on at the same time in an ulcerated surface. First, there is an effusion of plastic lymph, by which what are called granulations are formed. Granulations consist of coagulable lymph which has become organized; furnished with numerous delicate blood-vessels. Secondly, there is suppuration; and, thirdly, there is absorption or the removal of parts.

Sometimes, apparently, there is no *suppuration*—we see no pus in ulcers of the cornea, nor in certain cases of absorption of articular cartilages.

When the first of these three processes gets the better, if I may so speak, of the others, the lymph overspreads the surface of the ulcer, fills up the cavity, and the ulcer heals; cicatrization takes place.

When, on the other hand, the absorbing process predominates, the ulcer extends itself—the excavation grows larger, or deeper—or both larger and deeper: and when this excess of absorption is great, and the extension of the ulceration rapid, it is called *phagedenic* ulceration. When a part of the textures perishes during the process of the ulceration, and is separated in entire and sensible masses, the ulcer is said to be a *sloughing* ulcer. "When the process is slow, the lymph effused at the base and round the edge of the ulcer is hardened, and the granulations on its surface are deficient, the ulcer is then said to be *callous* or indolent: and when the granulations are larger and softer, and more flabby than usual, and require to be repressed before the ulcer will heal; to this variety of ulcer the name of *fungous* ulcer is given:" and the coarse and too luxuriant granulations are called, by the vulgar, *proud flesh*. These several terms, in the senses now assigned to them, you will please to remember.

It is by regulating the three processes now described—so far as they are capable of being regulated by art—that the surgeon and the physician endeavour to obviate the threatened ill consequences of ulceration, and to promote the repair of the textures which have been destroyed.

I explained to you, in a former lecture, that inflammation may lead to a wasting of parts, although there is no suppuration or ulceration. The testis sometimes withers as a consequence of inflammation: *interstitial* absorption takes place. *Atrophy* in short.

LECTURE XI.

MORTIFICATION, AS AN EVENT OF INFLAMMATION. INFLAMMATORY FEVER. HECTIC FEVER. TYPHOID FEVER. MODIFICATION OF INFLAMMATION BY DIFFERENCES OF TISSUE: CELLULAR TISSUE; SUBSTANCE OF GLANDS AND SOLID VISCERA; SEROUS MEMBRANES; SYNOVIAL MEMBRANES; TEGUMENTARY MEMBRANES—SKIN—MUCOUS MEMBRANES; MUSCULAR TISSUE; ARTERIES; VEINS: SUBSTANCE OF THE BRAIN.

WE were occupied, when last we met, with what may properly be called the *events of inflammation*. We passed in review, 1st, *resolution* as an event of inflammation; 2ndly, *serous effusion;* 3rdly, the pouring out of *coagulable lymph*, constituting the adhesive form or stage of inflammation; 4thly, the formation of *pus*, or *suppuration;* and 5thly, *ulceration*. The pathology of these several events, so far as it is understood, and the change of symptoms to which they may respectively lead, were also treated of as fully as the limits of my course allow. At the close of the lecture I was about to speak of the sixth and last event of inflammation that requires to be noticed; viz. *grangrene, sphacelus, mortification*.

Mortification.—When mortification thus succeeds to inflammation, the part dies; it becomes cold; all circulation through it is at an end; all sensation in it is over. If it be an external part, its colour changes; from being red it becomes mottled, purplish, green, or black; decomposition takes place; vesications appear, filled with dark-coloured fluids; air is extricated also. If there be a great accumulation of fluid in the part there will still remain tension; but usually the mortified part is flaccid and boggy; and it emits a cadaverous smell.

When *internal* parts mortify under inflammation, they do not always assume this black appearance: often they are yellowish; or the soft tissue of the dead part readily imbibes fluid, and takes the colour of the substances with which it has been in contact. We see sloughs of the mucous membrane of the intestines presenting the ochery hue of the fæcal matters which had rested upon them.

What I have described as mortification, occurring externally, and succeeding to inflammation, is such as the surgeon witnesses. Sometimes it spreads, and loses and confounds itself, insensibly, with the adjoining parts, which still retain life; and which may continue actively inflamed; and subsequently perish also. Under more favourable circumstances, a distinct boundary line is formed between the dead and the living parts; and nature proceeds to amputate the portion which has lost its vitality. The process by which this is effected is extremely interesting: adhesive inflammation constructs a barrier of lymph against any farther advance of the mortification; a furrow of ulceration marks out upon the surface the commencing separation, and (supposing a part of one limb to have become gangrenous, the foot for example) the furrow gradually deepens, until the dead part is completely cut off. This very fact shows that all the textures of the body, skin, muscle, nerve, blood-vessel, and bone, are capable of being removed by the ulcerative process. Meanwhile very interesting changes occur in the part that lives: the large vessels are plugged up, to a certain distance, by the coagulation of the blood contained in them; the coagulation of the blood following its stagnation. They are farther sealed up, and the smaller vessels also are closed, by coagulable lymph. Were it not for these changes, fatal hemorrhage would follow the separation of the dead part. Now this is just what a surgeon rudely imitates when he amputates a limb; he cuts through the parts with knife and

saw, and he ties the larger blood-vessels as he goes along. He follows the path which the natural processes pointed out; and in truth, a great part of both physic and surgery consists in learning what are the expedients of repair and preservation for which provision has been made in the living body; in exciting, or repressing, or directing, or imitating, those natural actions which generally tend, and often suffice, to restore health, and to save life.

Mortification is more common in some internal parts than in others. It is frequent in the cellular tissue; and in the mucous and submucous tissues of the alimentary canal; in the throat, for example, in cynanche maligna; and in the glandular parts of the intestines in fever. It seldom affects the other mucous systems—those which belong to the air-passages and the urinary organs. It occurs sometimes—but not very often—in the substance of the lungs. It is seldom met with in serous and fibrous tissues. It is not at all uncommon in bone; producing *exfoliation* when it is slight and superficial; *necrosis* when the entire shaft of a long bone dies. In these cases the process is slow, and we can watch its repair; and a beautiful process it is: but I must not stop even to admire it.

Now mortification is no certain or constant event of inflammation. It depends, more or less, upon various causes and conditions. Sometimes upon the mere intensity of the inflammation, as in sloughing inflammation of the genitals; the progress of the mortification being fast checked by those measures which are calculated to abate the violence of the inflammation. The sloughing of the cornea in gonorrhœal ophthalmia is another example to the same purpose.

Again, whatever tends to weaken the circulation in the part affected—or in the system at large—tends also to promote the perishing of the textures that are inflamed. In persons who are debilitated by fever, the mere pressure of the body against the bed is enough to produce sloughing of the integuments of the sacrum, hips, and elbows. The same phenomena are apt to occur in parts that are palsied. In dropsical patients, with feeble and impeded circulation, we find that a blister on the extremities, where the circulation is the *most* feeble, will sometimes cause mortification; while it might be applied to the chest without any risk of that event. Probably, in each of these instances, the unhealthy condition of the blood conduces to the sloughing process. Inflammation of the stomach and intestines is marked by a strong disposition to run into gangrene—and this again is consistent with what I formerly mentioned, of the depressing influence of inflammation of these organs upon the heart.

It is necessary to remember that mortification is capable of being produced by other causes as well as by inflammation. The death of frost-bitten parts is perhaps scarcely an exception—the phenomena of mortification occur in them after the reapplication of a certain degree of heat—sufficient, probably, to give rise to more action than the frozen parts can bear without perishing. But the mere cutting off the supply of arterial blood, independently of any inflammation, will cause mortification. Ossification of the arterial trunks, and consequent stagnation and coagulation of the blood in them, is the commonest cause of the dry gangrene of old persons—the *gangrena senilis;* which, by the way, is not always dry. In the majority, however, of these cases, the gangrenous part, not being preternaturally loaded with fluid, does not so rapidly putrefy; but remains dry, and shrinks up. Again, whatever tends to prevent the return of the venous blood from a part (as a firm ligature placed round a limb—or the constriction of the gut in strangulated hernia) is favourable also to the production of mortification. Probably here too the direct or indirect pressure made upon the arteries is chiefly concerned in occasioning the death of the part. We see limbs mortify sometimes after their principal artery has been tied for the cure of aneurism, when the collateral arterial circulation fails sufficiently to re-establish itself: we see the same thing when the passage of the blood through the main artery is stopped by external injuries.

There is also a very curious form of chronic and dry gangrene produced by the continued use of diseased grain as food—and particularly of the spurred rye: and to this, as a distinct disease, I may perhaps have occasion to direct your attention hereafter.

The different stages and events of inflammation that have now been described, are accompanied by corresponding disturbances of the system at large. These were touched

upon (barely mentioned, however) in that rough and general outline which I attempted to sketch in the outset, of the various phenomena of inflammation; and to which I have since referred as a type. They require, for many reasons, to be considered somewhat more minutely.

When, as *surgeons*, you have to deal with *external* inflammation, you have no difficulty, in the first place, in ascertaining its actual existence: you see it; and you know besides, merely by looking at the part, and perhaps handling it, what changes it has undergone. You may perceive that the opposite lips of a wound have adhered; or that a phlegmon, in which you can also distingush fluctuation, has assumed a pyramidal form, and begins to look white upon its summit: or you observe that the abscess has broken, and left an ulcer behind it, which pours out pus, and which shows a tendency to contract, or to enlarge itself: or you may remark the alteration of colour and of temperature which denotes the approach of mortification, or the actual death of the part. The mere exercise of your external senses apprises you, not merely that there is inflammation, but also whether it is of the *adhesive* kind; or has reached the degree of *suppuration;* or has produced *gangrene*. At the same time you do not fail to notice the nature of the *constitutional* disturbance that may be present; and the knowledge thus obtained of the local and of the general symptoms determines your plan of treatment.

But when, as *physicians*, we have to do with inflammations of *internal* parts of the body, and when the changes attendant upon that process are concealed from our view, the case is very different. We should often be unable to make out the nature of the disease at all, if the presence of pyrexia did not instruct us. Sometimes the constitutional disturbance is all that is apparent, until after death. And, as the disease proceeds, we frequently are able to judge that this or that *event* of inflammation has taken place, only by observing the indirect symptoms which declare themselves through the medium of the system at large. Yet it is in many cases of the greatest importance to mark the transition from one stage or event of inflammation to another; and to learn whether, and in what degree, the more urgent of the symptoms depend upon the inflammation itself; or upon the effects which it has produced. I do not mean to say that we have not, sometimes, as sure indications afforded us by *direct* symptoms, cognizable by the sense of hearing or of touch, of the state of internal organs, as we *could* have if they were exposed to our view. To these direct symptoms I am not now about to refer; they must be spoken of in connection with the diseases to which they belong. But the information which the physician gains from what may be called *constitutional* symptoms is *always* highly valuable; and it is *sometimes* the *only* information that offers any guidance to the remedial measures he ought to adopt.

Inflammatory fever.—Inflammation, sufficiently extensive or intense to disturb the general system at all, is attended with pyrexia: and the presence of pyrexia, when the part affected is unseen, marks the *nature* of the disease. The most prominent of the symptoms that denote the existence of inflammatory fever are debility and chilliness; followed by, or alternating with, increased heat of skin; and increased frequency and force, and often *hardness* of the pulse; with considerable derangement of most of the natural functions of the body. Commonly there is headache and confusion of thought, languor, thirst, loss of appetite, a furred or white tongue.

Among these leading symptoms, the chilliness, often amounting to shivering, has this particular importance attached to it, that it marks the *date* of the febrile disturbance. And it is worth observing, that rigors more commonly attend the commencement of spontaneous inflammation than of inflammation caused by external injury.

Now, without going more into detail—of this febrile condition belonging to the early stage of inflammation, I make the following remarks.

1. That it generally *succeeds* the manifestation of the local symptoms of the inflammation: and that we cannot, therefore, help considering the fever as the natural *effect* of the inflammation.

Kaltenbrunner describes an experiment of this kind. He says, if a drop of alcohol be applied to the web of a frog's foot, the blood presently flows towards the part irritated, and the circulation in it is accelerated; *congestion* takes place, and follows its known march.

If the dose of alcohol be augmented, the phenomena of congestion increase considerably, and extend over a larger space: at length points of stagnation appearing in the focus of the affected part announce the establishment of *inflammation*.

If the dose of alcohol be still farther increased, we observe that, on the one hand, the inflammatory points of stagnation become larger and more numerous; and that, on the other, the circumferential disturbances of the circulation extend themselves, so as at length to implicate the whole of the circulating system: they give rise to a *fever*, which is *added* to the *inflammation*. The circulation of the web in the opposite foot is as much accelerated as in the vessels surrounding the inflamed part in the first foot. If the word congestion had not a local meaning, we might call fever (he says) a general congestion.

Perhaps the fever may be owing to the circulation of altered blood throughout the body. We know that the blood *is* altered in these cases, inasmuch as it is found to contain an unusually large proportion of fibrin; and as it has acquired the unnatural quality whereby, when withdrawn from the body, and allowed to coagulate, it exhibits the buffy coat.

That the febrile state follows the local inflammation in point of time, is then the rule: but this rule has frequent exceptions. Erysipelas, and all the frebrile exanthemata, afford instances of exception; the fever sets in before any manifestation at least of the local symptoms. These are indeed diseases of a specific kind: but the same is true sometimes of diseases that appear to be simply inflammatory; such as inflammation of the lungs, and cynanche tonsillaris. There are other cases in which the local symptoms and the general febrile disturbance appear to burst forth simultaneously: this is seen in certain instances of pleurisy, and of peritonitis.

2. Again, it is a curious circumstance that the inflammatory fever is not always proportioned, in its degree of violence, to either the size or the importance of the part inflamed. The pyrexia is often very strongly marked in that common complaint, the quincy, inflammatory sore-throat, cynanche tonsillaris—which can scarcely ever be said to imply much danger.

3. The situation, the extent, and the degree of the local inflammation being the same, the fever commonly runs higher in young, and in plethoric persons, and in those of sanguine temperament, than under the opposite conditions.

4. The inflammatory fever may be modified in the outset, or very early indeed, by the nature of the part upon which the inflammation has seized. I have several times mentioned the peculiar depressing effect upon the action of the heart produced by inflammation of the stomach and bowels, and of some other of the abdominal organs: and particularly by inflammation involving their peritoneal covering. This lowering influence (which is analogous to that of certain mechanical injuries to the abdomen) has been supposed to depend upon the subduing and sickening kind of pain which is apt to accompany inflammation of these parts. However it may arise, it gives a peculiar character to the inflammatory fever; lessens the amount of reaction, or abridges its duration: affects especially the quality of the pulse; and carries with it a strong tendency towards death by asthenia.

5. There is no doubt either that the character of the inflammatory fever is liable to be considerably modified, from the first, by the previous habits of the patient. In persons who have been habitually intemperate—or who have been subject to long-continued excitement of the nervous system of any kind—the fever which attends inflammation approaches more or less to the typhoid form, from the very beginning. The febrile reaction is less strongly pronounced. The functions of animal life are sooner and more deeply involved in the train of morbid actions. Stupor and delirium are apt to occur; with extreme debility and irregular movements of the voluntary muscles. Still more conspicuous are these peculiarities in some cases of inflamed veins; and whenever inflammation is produced or accompanied by the introduction into the system of certain animal poisons.

6. The relative duration of the inflammatory fever is subject to some variety. It may persist for a little while, for a few days even, after all the local signs of inflammation have disappeared: this happens chiefly in persons of an irritable habit. We watch such cases narrowly, not without some apprehension of a relapse. On the other hand, a rapid abate-

ment of the febrile symptoms sometimes takes place, while the local changes continue, or even for a time increase in extent. Nevertheless, we hail this change as a favourable augury of the ultimate result.

Hectic fever.—When inflammation, external or internal, has gone on to the formation of pus, *that* event is frequently marked by the supervention of peculiar symptoms, and the character of the fever undergoes, for the most part, a striking alteration.

It is very important to ascertain the time when this event of inflammation takes place, or is at hand: for the measures which might have been proper and necessary while any prospect remained of the *resolution* of the inflammation, may be useless, and even hurtful, if continued after that prospect is at an end.

When the surgeon perceives any indication of the formation of pus in an external part, he mostly despairs of being able to bring about resolution; ceases to abstract blood from the part, or from the system; and applies perhaps warmth and moisture, by means of a linseed poultice, to promote the suppuration. And a corresponding change of plan is required in internal inflammations.

Now the *commencement* of suppuration is often marked by rigors; and its *continuance* by hectic fever.

If, after the symptoms of inflammation have lasted for a certain time, the patient is attacked by cold shiverings, followed by some increase of heat, that circumstance alone is enough to make us suspect that pus is formed, or is about to be formed: and to teach us that the measures employed to effect a resolution of the inflammation have not been successful.

Rigors are very striking symptoms; but they are by no means necessarily connected with suppuration. They usher in, as I presume you know, most forms of fever, appearing at the very outset of the disease. They recur, at regular intervals, in ague. Slight causes will, sometimes, produce them. They often follow the introduction of a bougie into the uretha. But when they occur *after symptoms of internal inflammation have been for some time present*, they denote, in most cases, the production of pus in the part or organ inflamed. Sometimes one such shaking fit only is observed: sometimes several take place. When they recur, it is usually at irregular intervals: but cases do happen in which the shiverings indicative of internal suppuration are so strictly periodic, that unless all the circumstances be carefully taken into the account, they may be mistaken for signs of intermittent fever.

The leading symptoms of *hectic fever* (by which, I say, the *continuance* of suppuration is commonly marked) are, an abiding frequency of pulse; alternations of chilliness with heat and flushing, followed by perspiration; a gradual wasting of the body; and progressive debility.

I shall hereafter have to describe to you a very different kind of disease, in which, however, there is a succession of symptoms resembling more or less closely the series that characterizes hectic; I mean *remittent fever:* the succession of symptoms being chilliness, heat, prespiration. But these two disorders are in most cases discriminated from each other by the circumstances under which they occur.

The symptoms of hectic fever often creep on, at the outset, insidiously, and almost imperceptibly. "A very slight degree of emaciation, a pulse a little quicker than ordinary, with a small increase of heat, especially after meals, are often the first symptoms which can lead us to suspect the formation of hectic." Cullen has described hectic fever as consisting of two exacerbations in the 24 hours—one about noon, the other towards evening; but in many cases the latter alone is distinctly marked. The patient feels shivery and cold towards night: then the skin becomes hot and dry, especially in the palms of the hands and the soles of the feet, and the pulse *more* frequent; and in the middle of the night, or towards morning, he wakes from short and uneasy sleep, in perspiration, which is often profuse. Sometimes, however, there are two or three fits in a day. The paroxysms are shorter, and less regular, than those of intermittent or of remittent fever. Each of the three phenomena constituting the series may, in its turn, be wanting· and even if the paroxysms are regular for two or three times together, they never continue to be so. Many circumstances connected with the paroxysm itself are very distinctive. "The hectic patient," says Dr. Heberden, who has left us a very good account of

this affection in his *Commentaries*, "is very little or not at all relieved by the breaking out of the sweat; but is often as restless and uneasy after he begins to perspire as he was while he shivered or burned. All the signs of fever are sometimes found the same after the perspiration is over; and during their height the chilliness will in some patients return, which is an infallible character of this disorder. Almost all other fevers begin with a sense of cold, but in them it is never known to return and to last twenty minutes or half an hour, while the fever seems at its height, which in hectic will sometimes happen."

Hectic fever is one of the fearful accompaniments, and sometimes the most strongly marked symptom, of pulmonary consumption: and where the existence of that complaint is suspected, yet a matter of doubt, we look for indications of hectic fever with the greatest anxiety.

With relation to hectic fever, considered as an indirect symptom that suppuration has succeeded to inflammation, and is still going on, it will be worth your while to notice the strong contrast it offers in many particulars, to the *inflammatory* fever that attends the earlier stages of inflammation.

The pulse loses much of all its *hardness* and strength, but it remains permanently more frequent than the pulse of health: the appetite returns in great measure; the thirst abates; the tongue, instead, of being covered with a white fur, becomes clean and moist, and towards the end is sometimes unnaturally red, or speckled with aphthæ: there is no longer headache or confusion of thought.

A few more touches will suffice to fill up the picture of hectic fever.

The face is usually pale; but during the exacerbations it is partially flushed, and very often a characteristic circumscribed red spot appears upon either cheek. Besides the evident emaciation, various minor changes mark the want of proper nourishment: the skin, when not perspiring, is harsh and scurfy; little branny scales may be rubbed from the legs, merely by the friction produced in drawing off the stockings; the hairs become fine and fall off; the finger-nails are incurvated into an adunque form; and the sclerotic coat of the eye, as seen through the conjunctiva, becomes of a pearly white. As the disease advances œdematous swellings of the ankles are very apt to come on.

The connection between hectic fever and the formation of pus in some part or other of the body is so frequent, that it has been deemed, by persons of great experience and sagacity, a *universal* fact. Dr. Cullen tells us, in his *First Lines*, that he had never seen hectic in any case, when there was not evidently, or when he had not ground to suppose there was, a permanent purulency or ulceration in some external or internal part. And Dr. J. Thomson, speaking of the opinion that hectic might occur independently of suppuration, uses these words: "But till facts more decisive, and cases more accurately described than any which have yet appeared are produced in proof of that opinion, I shall think myself justified in adopting the *common* opinion; and in believing that hectic fever is in every instance connected, if not with the *absorption*, at least with the *formation* of pus."

The notion alluded to in the latter part of this quotation was at one time very commonly entertained, viz. that hectic fever resulted from the reabsorption of pus into the blood; but there are many facts decidedly opposed to this belief. Considerable collections of matter not unfrequently disappear, *i. e.* are taken up again into the blood, without occasioning the slightest approach to hectic. Again, hectic will accompany, and be kept up by, a scrofulous joint with an open sore, and it will sometimes cease at once, and completely, upon the removal of the diseased limb by amputation; although a greater quantity of pus is secreted by the stump, than had been secreted in the diseased part previously to the operation. Facts like these prove, I think, that hectic is not simply a consequence of the absorption of pus into the blood: and they seem to have suggested to Mr. Abernethy the notion (which was also held indeed by John Hunter) that sympathetic hectic fever is a *teased* action of the system, endeavouring to throw off what annoys it: the cause of irritation being removed, it ceases forthwith.

And there is another conclusive circumstance to be mentioned. Notwithstanding the opinions I just now quoted from Cullen and Thomson, I believe few persons who have attended to the subject, doubt, now, that there is such a thing as *idiopathic* hectic; hectic

unconnected at least with suppuration anywhere. We often see hectic, or a general state of the system not to be destinguished from hectic, in mothers who have suckled their infants too long: we see it too, sometimes, if I mistake not, in newly married husbands: and it may be noticed as occurring more or less distinctly in those who labour under diabetes. What is common to all these cases is, that there is an habitual drain upon the system beyond what the nutriment taken into it can supply and counterbalance. It is certain, too, that hectic fever sometimes happens in phthisis, not only before there has been any expectoration of puriform matter, but prior even to the softening and suppuration of a single tubercle. I call to mind one instance in particular of this. The hectic was distinctly marked, and continued long. The patient died, at last, comatose, after two attacks of convulsion. Two or three large scrofulous tumors were found imbedded in the substance of his brain. Various other organs were infested with tubercles; but the tubercles were all of them still hard and crude.

However, setting aside these rarer cases of exception, there can be no doubt that hectic fever, considered as a constitutional symptom of mischief that *may* reveal itself by scarcely any other token, and especially as a sign of suppuration, deserves all the attention we can give it; and for that reason have I spoken of it rather at large. Whenever I mention hectic fever in the farther progress of these lectures, you will know all that I wish to express by that term.

Typhoid fever.—I have very little to say at present respecting that modification of the general febrile disturbance, which sometimes attends *mortification* as an event of inflammation. I stated before that the fever in these cases was apt to assume the typhoid form; and to be characterized by sinking of the pulse, shrunken features, coldness and clamminess of the skin, a dry and black tongue, low muttering delirium or stupor, tremors of the voluntary muscles, with spasmodic startings of their tendons, and insensibility to the passage of fæces and of urine. I must, however, now inform you that these typhoid symptoms are no constant or necessary concomitants of mortification. The natural mode of death, under gangrene, is death by asthenia. But the typhoid state involves the nervous functions, and tends to death by coma. Whenever, therefore, typhoid symptoms supervene upon inflammation which ends in sphacelus, they may with much probability be attributed to some contamination of the blood by an animal poison; and such contamination may have taken place previously to the mortification, and have even helped to produce it, as when inflammation arises during the progress of the contagious febrile disorders; or it may occur as a consequence of the mortification itself, by direct absorption into the system of some of the putrefying and poisonous elements, into which the dead part has been resolved.

One circumstance, worth bearing in mind, as sometimes indicating the supervention of internal mortification, is the sudden cessation of pain: giving hope to the patient and his friends that the danger is over; but not deceiving the experienced physician.

Modification of inflammation by differences of tissue.—So much, then, for the local and constitutional events of inflammation, considered generally.

It remains for me to make some observations upon the modifications of inflammation, according as it affects the different tissues of which the body is composed. Many of these observations I have, indeed, already anticipated; but it will be useful to bring together, under one view, the most material facts ascertained on this matter.

Cellular tissue.—When inflammation affects the *cellular tissue*, all the events of inflammation which I have taken some pains to describe are apt to occur; and for that reason, inflammation of this tissue, as it exists beneath the skin, was chosen by me as a convenient type, or general representation, of the inflammatory process. There is the less necessity, therefore, that I should take up much of your time in speaking of the characters of inflammation exhibited in cellular tissue. There is a strong tendency to form circumscribed abscesses: the extension of the suppuration is prevented by a wall of lymph thrown out around it. The adhesive inflammation sets bounds to the suppurative. There is a good deal of pain when the cellular tissue is so situated that tension is occasioned by its swelling.

But sometimes no such boundary wall is erected, and the inflammation spreads and diffuses itself, and becomes a very terrible disease, destroying the cellular tissue over a large

and undefined space, by a process between that of sloughing and that of suppuration. When the skin is implicated also in the inflammation, the disease is usually called *erysipelas phlegmonoides:* when the skin is not involved, it is called *diffused inflammation of the cellular membrane.* This diffused form of inflammation frequently follows the introduction of animal poisons into the system; and accompanies the inflammation of veins and of absorbent vessels. It is this disease which is so often fatal to members of our profession, when it results from wounds or punctures received in opening dead bodies. Dr. Craigie has recently put forth the opinion that in these cases of spreading inflammation it is the *adipous* tissue that is affected.

Substance of glands and solid viscera.—The substance of the *larger glands, and of the solid viscera* of the body, suffers changes analogous to those observed in the cellular tissue: probably because cellular tissue enters largely into their composition. Acute inflammation of the liver, when it does not terminate in resolution, leads to abscess in that organ. Abscess is rare in the lungs, perhaps for the reasons mentioned in the last lecture. Gangrene is also very rare in the pulmonary substance: and quite unknown, I believe, in the liver, and in the kidney. Inflammation of the latter organ is not unfrequently attended by purulent collections. Inflammation of the substance of the viscera is not, in general, attended with much pain.

The cellular tissue is liable to be rendered permanently thick and hard by *chronic* inflammation, as well in the parenchyma of internal organs as where it is spread out beneath the skin, or beneath serous or mucous membranes. Chronic induration and thickening of the cellular tissue which composes Glisson's capsule is no uncommon result of slow inflammation; producing that particular change in the liver which the French pathologists denominate *cirrhose;* but of which the essence is atrophy of its lobules from compression of its nutrient arteries.

Serous membranes.—The inflammation of *serous membranes* is characterized by sharp and severe pain; and by hardness of the pulse; and by buffy blood; by its tendency to spread; by the effusion of serous fluid, and of coagulable lymph; and sometimes, when the inflammation is very violent, or air gets admitted to the inflamed surface, by the effusion of pus. Speaking generally, however, it is *adhesive inflammation* which we expect in inflammation of this tissue. False membranes, consisting of organized lymph, belong to it: and the agglutination of contiguous surfaces. Sometimes the lymph, instead of being deposited in flakes or layers, appears in the form of numerous small granules: this is a phenomenon frequently observable in inflammation of the arachnoid, and of the peritoneum. Sometimes it has a villous or papillary or shaggy arrangement; or is cellular like a honey-comb. This is common in the pericardium. The surface (to use the happy simile of Laennec) resembles that which may be produced by separating two flat plates between which a layer of soft butter had been spread: and it probably depends upon a similar cause; since in health a perpetual sliding motion of the pericardium over the heart is going on. Ulceration of a serous membrane is very uncommon. I mean ulceration *commencing* in that tissue: for these membranes are frequently perforated by ulcers which approach them on their attached side, and which begin in other tissues, especially the mucous. Neither does mortification occur in serous membranes, except sometimes by communication from other parts. The effect of *chronic* inflammation of the serous surfaces is to thicken, harden, and pucker them. We see this effect in the omentum frequently; in the peritoneal covering of the liver; in the serous membrane which forms so large a portion of the valves of the heart.

Synovial membranes.—The *synovial membranes* have a strong analogy to the serous. Gendrin includes the two in the same category: yet their behaviour under inflammation offers, in some respects, a marked distinction between them. They are less *liable* to inflammation than the serous membranes: they rarely throw out coagulable lymph, and, consequently, adhesion of their opposite surfaces is very uncommon. Joints do not become immovable, or what is called anchylosed, in consequence of the agglutination of their synovial surfaces; but, generally, by means of granulations forming upon those surfaces after they have ulcerated. Very seldom indeed does pus form in the synovial sacs, except (again) the inflammation has been caused by mechanical injury, which has laid open the joint, and admitted air. When this is the case, very serious constitutional disturbance is

apt to take place, and the existence of the sufferer is endangered. That this does not depend upon the *mere violence of the exciting cause* is evident from the circumstance that the same acute inflammation, the same general affection of the system, and equal danger, often result from the careful incision made into a joint by the surgeon, for the purpose of removing loose portions of cartilage. I have now at the hospital an out-patient who has, among other ills, a large cartilage floating about in fluid in one of his knee-joints: but I believe that Mr. Arnott, whom I have consulted on the case, will be very slow to recommend its removal, until the inconvenience produced by it is so great as to incapacitate the patient from pursuing his employment, and until other methods of relief have failed. Suppuration of the joints is also one of the occasional consequences of phlebitis. Inflammation of the synovial membrane speedily leads to a *serous effusion* into the joint: which often, especially in rheumatism, is as speedily taken up again.

Tegumentary membranes.—Let us next inquire into the modifications which inflammation undergoes when it affects the tegumentary membranes.

Skin.—Considering the *skin* as *one membrane*, and neglecting its subdivisions into epidermis, rete mucosum, and cutis vera, we find that inflammation assumes a variety of forms in this external covering of the body. Many of these belong to specific diseases, and do not fall within my present purpose, which is that of noting how *common* inflammation varies in the different tissues.

When the inflammation is superficial, it frequently is denoted only by a diffused red blush, which may be banished for a moment by the pressure of the finger, and which after a certain time disappears of its own accord—terminates by resolution; the only consequence of the inflammation being the separation of the cuticle in small branny fragments; in one word, *desquamation*. We call the superficial inflammation, in this case, *erythema*. If the inflammation has been a little more intense—as in some cases of *erysipelas*, in *scalds*, and in that which we are every day exciting by *cantharides*—a serous fluid is poured out, which elevates the cuticle in larger or smaller patches of vesication. Remove the cuticle and admit the air, and the *serous* effusion becomes *purulent* effusion: and if the inflammation be pressed beyond a certain point by any *other* stimulus besides that of air, we may have pus poured out. Erysipelatous (which is also a specific) inflammation of the skin, is characterized by its remarkable tendency to spread: and a most singular circumstance attends several of the other specific inflammations of the skin—viz. that having occurred once, they never occur again: this peculiarity belongs, however, to the great constitutional diseases, of which the skin affection forms merely a part.

Mucous membranes.—Inflammations of the *internal* tegumentary membranes—of the three internal surfaces that communicate with the air, and are clothed with mucous membrane—are very interesting to the physician: and the first thing which strikes our attention in respect to them is the indisposition they manifest to adhesive inflammation: and we are struck at the same time with the *beauty* of this provision. If the mucous membranes were as ready to throw out coagulable lymph, and to adhere to each other, as the serous, almost every occurrence of inflammation in them would prove necessarily fatal; by closing up the *inlets* of the air-passages; or the *outlets* of the urinary passages; or any part of that long mucous canal which, passing through the body, requires a free opening at both of its extremities. But the inflamed mucous membrane pours out serous fluid; or viscid mucus; or pus; or blood. Inflammation of these membranes is, however, sometimes attended with the exudation of something which is very like coagulable lymph, and which has been considered (but, in my opinion, erroneously considered) to be such lymph. The tracheal, bronchial, and pulmonary mucous membrane, the œsophageal, the intestinal, and that which lines the uterus, are all more or less subject to the formation of adventitious membranes under inflammation. Casts of the smaller branches of the air tubes have, in rare instances, been repeatedly coughed up in large quantity; constituting what have very inaptly been called *bronchial polypi*. The membranous exudation of croup is well known: a tubular substance is formed in the trachea, and, sometimes, fortunately expelled: but too often it suffocates the patient. Similar concrete exudations, sometimes in irregular shreds, are occasionally voided by stool. It is said that a long membranous mass of the same kind, and resembling in size and shape an earthworm, has been expelled from the urethra; having formed there in consequence of the injudicious use of stimulating sub-

stances, injected with a view of checking the more innocent effusion of pus. The films, or membrane-like flakes which are thus incidental to inflammation of the mucous surfaces, resemble I say in their general appearance and disposition the strata or layers of coagulable lymph which are the ordinary product of inflammation of the closed serous surfaces. But they differ from these in some remarkable points. They are softer. They never contract permanent or strong adhesions to the subjacent inflamed membrane; but are partially separated from it by the intervention of thinner matters, serous or puriform. Above all, they never become organized. They appear to consist of inspissated and altered mucus; and are composed, in a great measure, of albumen. An opinion has been entertained that the *want of apposition* of the opposite surfaces has a great deal to do with their indisposition to cohere. The mucous *air* tubes are kept open and apart by their *structure:* the stomach and intestines by their *contents*, or by the frequent passage of solids or fluids through them: and, therefore, (it has been supposed) they have no *opportunity* of adhering. But there can be no doubt that these mucous membranes are but little disposed to throw out true lymph at all: and when their opposite surfaces do grow together, I believe it will almost always be found that some abrasion or ulceration of the mucous surface has previously happened.

Inflammation affecting the mucous membranes has sometimes a strong tendency to spread and wander: sometimes, on the contrary, it is strictly confined to a small and definite space. In the former case it commonly restricts itself for a long time, or altogether, to the mucous tissue, leaving the neighbouring tissues untouched. In the latter it is apt to penetrate to the subjacent parts, and to produce obvious and enduring alterations of structure. The membrane becomes fastened to the part which it clothes, and not unfrequently it ulcerates or sloughs.

The spreading form of inflammation is most often met with in the air-passages. Ulceration and sloughing, and circumscribed inflammation, are more common in the alimentary canal.

There is a remarkable contrast between the serous membranes and the mucous, in respect to the *pain* which attends their inflammation. Very little pain is experienced in many cases when inflammation affects the mucous lining in any of the three systems, except towards their openings, where the membranes are about becoming continuous with the external skin: in the mouth and throat, for example, the pharynx, the rectum, the vagina, the extremity of the urethra. And as inflammation of the mucous membranes is attended by less *pain*, so also it is accompanied by less *fever* than when the serous membranes are attacked; and the blood more seldom exhibits the buffy coat.

Muscular tissue.—The *muscular tissue* appears to take on the actions of inflammation very reluctantly: and its vessels seldom, if ever, pour forth any of the products of inflammation. The chief effect of inflammation upon muscle is the destruction of its contractible properties. Serum and lymph, and even pus, are sometimes found diffused through muscular parts; but there is reason to believe that these effusions are rather the consequence of inflammation of the cellular tissue which enters into the composition of the muscle, which ties together its muscular fibres, than of inflammation of those fibres themselves.

Arteries.—I have remarked already that inflammation of an *artery* presently leads to the effusion of lymph, and the coagulation of the blood, in the artery. But arteries do not readily inflame, except under mechanical injury: they do not often suppurate either: and they possess a singular power of resisting mortification. Dr. J. Thomson declares that he has seen cases of phlegmonous erysipelas, in which "several inches of the femoral artery were laid completely bare by the gangrene, ulceration, and sphacelus of the parts covering it, without its giving way before death."

Veins.—Inflammation of the *veins* is much more common than that of the arteries: and it is a disease of fearful interest. In some cases it leads to a deposit of fibrin upon the inside of the vessel, "furring it over," as Mr. Hunter says. The blood soon coagulates, and blocks up the inflamed vein, or leaves, perhaps, a narrow passage in its centre. From this mechanical obstruction to the current of the blood new symptoms arise. The part from which the venous trunk receives its tributary branches becomes œdematous or dropsical. Inflammation of the femoral vein, obliterating its cavity, is the essence of the com-

plaint known to pathologists under the name of *phlegmasia dolens:* a complaint which may happen to persons of any age, and of either sex: but which is most common in women soon after parturition.

This, which may be considered the adhesive form of phlebitis, is also its most innocent form. Too frequently the inflammation runs into suppuration; and then it proves a most terrible and almost hopeless disorder. The vein remains pervious; pus, of an unwholesome and poisonous quality—or some morbid product of the inflammation—is carried into the blood; which thus scatters, in its course, the seeds of inflammation, and determines the rapid formation of purulent collections, in various and distant parts of the body, and especially in the lungs, the liver, and the larger joints. Great constitutional disturbance ensues, and fever of a typhoid type is established. To this, the destructive form of the disease, parturient women are also peculiarly liable. Phlebitis of the uterine veins constitutes the source of the most dangerous and deadly varieties of puerperal fever. It is the same disease which gives to a vast majority of those surgical operations that are followed by death, their fatal character.

Substance of the brain.—We hear continually of inflammation of the *brain;* but what is so called is, most commonly, inflammation of the *membranes* which invest the brain. Inflammation of the cerebral substance itself is, however, not very uncommon; but it is more frequently the result of injury than of spontaneous disease, and it is usually confined to a limited portion of the brain. Softening and suppuration are its ordinary events. Sometimes pus is met with occupying a distinctly circumscribed space: the pus is collected into an abscess. Sometimes, on the other hand, it lies loose, as it were, and surrounded by broken-down cerebral matter, or it is infiltrated into the cerebral pulp. Around the softened portions the inflamed substance of the brain is more dense and firm, sometimes, than is natural. Whether this be owing to the presence of coagulable lymph, has not (so far as I know) been clearly ascertained. Mortification must be very rare in the nervous substance. Dr. Baillie has described it as occurring after violent injury. Once or twice in my life, portions of brain have been shown to me, protruding through an aperture of the skull, dead, of a dark colour, and having an offensive smell. Excepting in these cases of *hernia cerebri,* I have never seen sphacelus of the brain from any cause.

Perhaps, however, I am incorrect in saying this. I formerly told you that portions of the brain often become soft and diffluent, when there has been no inflammation; but simply from atrophy, depending on a diseased state of the nutrient arteries of the brain. Now some persons call *this* mortification of the cerebral substance. They consider it quite analogous to the grangrena senilis, which results from a similar cause, although it happens in another part of the body. The *nature* of the change, they say, is the same, although *the physical characters of it* differ. If this be so, I have seen gangrene of the brain some scores of times: but still I should be able to declare, that with, perhaps, the exception already mentioned, I have never seen unequivocal mortification of the cerebral substance as the result of inflammation; which is what we have now been considering.

This concludes, gentlemen, what I have to say concerning the phenomena of common inflammation, as they are perpetually witnessed in the various textures of the body. I have not, indeed, gone through all the tissues; I have said nothing of the peculiar effects of inflammation, in cartilages for example, and in bones; but I have glanced at all those tissues in the inflammation of which the physician is chiefly concerned; upon such points as I have purposely omitted, you will be amply instructed by my colleague, the professor of surgery.

LECTURE XII.

VARIETIES OF INFLAMMATION; ACUTE AND CHRONIC; LATENT; SPECIFIC. SCROFULOUS INFLAMMATION. TUBERCLES. RELATIVE FREQUENCY OF SCROFULOUS DISEASE IN DIFFERENT ORGANS. SIGNS OF THE STRUMOUS DIATHESIS.

WE have now, gentlemen, considered the phenomena of inflammation, local and general: its symptoms and its events; and the indications of these afforded by the state of the system at large: and we have surveyed the principal tissues of the body, and observed the modifications and peculiarities to which the process of inflammation is liable, according as it is situated in the one or the other of those tissues.

Acute and chronic inflammation.—There are still some varieties of inflammation; and some epithets applied to inflammation; which require to be explained.

Acute, and *chronic*, inflammation: these are words perpetually in our mouths. I have frequently employed them already. What do they mean? Is acute inflammation different from chronic in *kind?* No: they differ only in *degree.*

When the disease runs its course rapidly, and is attended with much general as well as local disturbance, it is said to be acute. When, on the other hand, the local and constitutional symptoms are less violent, and the inflammation runs a longer course, its phenomena following each other in slower succession, it is said to be chronic. The process is the same, but its features are less strongly expressed. The disease passes through similar stages in both cases, but it travels at a different pace. The characters, then, of acute inflammation are intensity of symptoms and rapidity of progress: and the characters of chronic inflammation are mildness of symptoms and slowness of progress. Inflammation can scarcely be very violent, and at the same time of very long duration. When violent it has been likened (by Mr. Lawrence, whose language I have here adopted,) to a fire, which soon burns itself out. It may, however, be mild in its symptoms, and yet quickly over. The two terms acute and chronic are not directly opposed to each other: acute has more relation to the intensity, chronic to the duration of the disease; and some term is wanted—although it is hardly worth seeking for—to denote such a degree of inflammation as exists in a pimple; which is neither severe nor long continued.

Now, in respect to intensity and duration, there are almost infinite shades of difference in different cases of inflammation; and the same difficulty occurs here which always occurs when general terms are employed to express mere differences of degree. We feel no uncertainty or hesitation about those cases which occupy the two extremes of the scale; but with regard to those which lie in the middle we are often at a loss. To meet this difficulty some pathologists have invented a third term, viz. *sub-acute* inflammation, intending thereby to designate cases which hold an equivocal rank; which are neither decidedly acute nor plainly chronic; in which the inflammation may run a brief course, and be attended with a certain degree of fever; but attains no great intensity, effects no profound changes, and does not require very energetic remedies to control it.

You must not suppose that, because chronic inflammation is attended with less violence and disturbance, it is necessarily on that account less dangerous or destructive than acute. The latter is commonly more under the influence of remedies than the former: it is usually soon brought to an end: whereas chronic inflammation is often obstinate and abiding, and leads to very serious changes in the part upon which it fastens. Speaking generally, it tends to thicken and indurate when it is situated in the *interior* of organs, and to the effusion of pus when it affects *membranes.* It is more common in weakly and debilitated

persons than in others: but we must not forget that such persons are also very liable to acute inflammation.

Chronic inflammation is not unfrequently a *sequela* of acute inflammation. And that the two merely differ in degree, and not in kind, is evident from this: that acute inflammation may sink or subside into chronic; and that, on the other hand, chronic inflammation may readily be aggravated into acute.

There is another distinction of inflammation into *active* and *passive*, which is less intelligible. I believe that they who use the term passive inflammation intend to signify by it that languid and sluggish kind of inflammation which is apt to occur under the same circumstances, and in the same conditions, with passive *congestion*. When the granulations of an ulcer are in that state in which they may be made brighter, smaller, and healthier, by the application of a stimulus: when the blood-vessels of the eye, are left, after acute inflammation, turgid and tortuous; and that condition is improved, instead of being worsened, by the use of a stimulating lotion: in such cases as these, some persons would say there was passive inflammation. But I see little difference between this and chronic inflammation; nor do I know any difference between *active* and *acute* inflammation.

Latent inflammation.—The term *latent* inflammation is one of modern introduction. It is applicable to those cases in which internal inflammation runs its course silently, insidiously, and unperceived; without the usual warning tokens of its presence; without its more striking and prominent signs. Pneumonia, going on to disorganization of the lung, may arise, proceed, and even prove fatal, without any of the symptoms which ordinarily announce that disorder: without notable cough, or obvious dyspnœa, or complaint of pain, or the expectoration proper to pneumonia. And the same is true of other inflammations. We discover, with surprise and horror, the traces of their operation, when we come to examine our patient's dead body.

This is a most important form of inflammation; for though it does not declare itself to ordinary observation, neither does it occur absolutely without symptoms: but it requires that the symptoms should be looked for. The *auscultatory* signs of pneumonia, all those symptoms which are furnished by the physical condition of the affected organ, are present, and speak as clearly as in the more flagrant cases.

Latent inflammation is apt to creep on during the progress of certain disorders, whereby it is modified and masked. It belongs to those states of the system in which the sensibility is dull, and the vital powers languid. In continued fever not only have I known the lung pass into suppuration, when the existence of pneumonia had been unsuspected; but I even have seen one case in which that usually torturing accident, perforation of the bowel, took place, with the escape of its contents into the cavity of the abdomen, and extensive peritonitis—yet the patient expressed no sense of pain, and the inflammation was revealed, while he continued to live, by no intelligible symptom.

Inflammation of this insidious and lurking character is most to be apprehended in the aged, in those who are habitually intemperate, and in persons of sluggish temperament. It sometimes occurs during convalescence from acute diseases.

Specific inflammation.—Besides the varieties which have been mentioned in degree, there *are* also differences in *kind* among inflammations. What I have been speaking of during the preceding lectures I have called common inflammation. It is the *most common* form in which that process displays itself. All persons are liable to it; and that again and again. None are at any time privileged from its attacks. But there are several forms of inflammation different from this, which are called *specific*. There are various forms of specific inflammation affecting the skin, discriminated from each other by the local appearances they exhibit, and by the constitutional disorder which attends them. The rash, and the fever, of measles, are very unlike the rash and the fever of scarlatina; and both differ remarkably from those of smallpox, the eruption of which consists of little phlegmons. In each of these cases the application of a specific poison is required for its production: and whereas common inflammation has a tendency, when once it has happened, to happen in the same part again—to *recur*—these forms of specific inflammation never, or almost never, occur more than once.

There is again the gouty inflammation—differing from common inflammation in several signal respects; in the production of chalk-stones; in its attacking those who are descended

from ancestors who have had the disease, and scarcely any others. Then there is rheumatic inflammation, cousin-german to the gouty, yet distinguishable from it. And another variety of inflammation is that which arises from the introduction of the syphilitic poison into the system.

Scrofulous inflammation.—Of the specific forms of inflammation now adverted to I shall speak when I come to consider gout and rheumatism, and the contagious exanthemata, as distinct diseases. But there is one variety of inflammation—I mean the *scrofulous*—which meets us on every side; and is apt to affect so many parts of the body, and so great a number of persons; and has so fatal a tendency in most cases; that it cannot be left out of the account that I have been desirous to give you of inflammation in general.

Scrofulous or strumous inflammation (for struma and scrofula are convertible terms) is a *slow* process; it falls therefore within the class of *chronic* inflammations. It is not attended with much pain, or heat, nor for some time with much change of colour; and the redness which does accompany it has often a livid or purplish tinge.

These, however, are the negative properties of merely chronic inflammation. But *suppuration* at length occurs, which also lasts long: and the pus formed is peculiar and characteristic. It is not homogeneous or smooth, but consists partly of a thin serous whey-like fluid, and partly of fragments of a substance resembling curd: and the ulceration that ensues is marked by corresponding peculiarities. The ulcers are indolent; show but little disposition to heal. Scrofulous inflammation, compared with common, or what is called healthy inflammation, is in general but little influenced by remedies.

Tubercle.—Besides this scrofulous *inflammation*, it is necessary that I should now direct your attention to another form of disease, which is likewise properly denominated scrofulous. It is marked by the appearance, in various parts of the body, of what are called *tubercles*. These tubercles are masses of unorganized matter—also resembling curd or new cheese, more or less; but of various shapes and sizes. They suffer gradual changes; soften or break down; undergo a sort of suppuration; and the softer matter into which they thus (as it were) melt, has the characters that distinguish the pus of a scofulous ulcer or abscess.

Now *tubercles* and scrofulous *inflammation* occur very continually in the *same individuals:* and what is remarkable, although they affect a very large portion of the whole human race, and conduce more often and surely than any one thing else to shorten the natural period of human life, yet they belong, almost exclusively, to certain *classes* of persons. We can tell, beforehand, that such and such persons are likely to become affected with scrofulous inflammation, or with tubercles: and we say of those persons that they have the scrofulous *diathesis*. I will not positively affirm that these forms of disease *cannot* be produced in any or in all persons: but this much is certain —that some persons are particularly prone to them; fall into them as it were spontaneously; on the operation of very slight external causes: while other persons never show *any tendency* to scrofula, even when continually exposed in the same manner: or if they do become scrofulous at all, it is only when the external circumstances most favourable to the production of such disease have been extreme in degree, and protracted in their application.

The occurrence of scrofulous *inflammation* in various parts constitutes distinct diseases; and the occurrence of *tubercles* in various organs constitutes *other* diseases. It will facilitate our future inquiries into these several diseases, if I take this opportunity of stating to you what is known respecting the scrofulous diathesis generally: and of the modifications of inflammation which are determined by its presence.

A good deal of discrepancy, obscuring the whole subject, and puzzling the student, has existed—and I believe I may say still exists—among pathologists, as to the nature, and origin, and precise seat of tubercles, and as to the changes which they undergo.

In general they have been loosely described as being *round* masses of firm friable matter, deposited in various parts of the body. Laennec, who paid great attention to tubercles, states that they are, at first, small, firm, greyish, semitransparent bodies, which enlarge and become opaque. In that condition he calls them *crude* tubercles: at length, after an indefinite period, these crude tubercles begin to grow soft in their centre, and are by de-

grees converted into a liquid mass, having the consistence of cream. There is a great deal of error in this description.

Andral, another great authority, says that tubercles are, in the outset, small round opaque yellowish bodies, unorganized, and of various degrees of consistence. He ascribes their softening (not to any spontaneous changes in their central parts, but) to the admixture of pus, poured out by the textures immediately surrounding the tubercle; which has irritated and inflamed those textures as any *other foreign body* might.

In some respects this statement is nearer the truth than Laennec's.

But in the account which I am about to give you, I shall chiefly follow our countryman, Dr. Carswell, the Professor of Pathological Anatomy in University College; who is one of the latest, and, as I think, most satisfactory writers on the subject. His opinions were formed after a careful examination, for himself, of the parts infested by these tubercles. He devoted several years to the study of morbid anatomy, in Paris, where he made a very large collection of drawings, in which various diseased appearances are beautifully and faithfully delineated. Some of these he has since published. I show you enlarged copies of those which relate to tubercle. They bear out some novel opinions which are stated in the letter-press that accompanies them.

After all, the points in question possess more of curious interest than of practical importance. But as you cannot help forming some notions respecting them, I think myself bound to lay before you those which most recommend themselves to my own judgment. At the same time you are to understand that I do not vouch for their absolute correctness.

Tubercles, then—or rather tubercular matter—is deposited from the blood. It may be considered as a morbid secretion. It is probable that the deposit, in the very beginning, is fluid: but we never see it in that state. It is certainly not so firm at first as it generally becomes afterwards; but its more watery parts are removed by absorption, and then there remains a "pale yellow, or yellowish-grey, opaque unorganized substance." This tubercular matter, so deposited, does not always assume a round form: far from it: the shape in which it appears depends upon the nature of the part wherein it is formed. It used to be held that the tubercular matter was always deposited in the cellular tissue. But Dr. Carswell asserts that its most favourite seat (if one may so speak) is *the free surface of mucous membranes*. In whatever organ it is met with, if mucous tissue enters into the composition of that organ, that particular tissue is either (he says) exclusively affected, or much more extensively affected than any of the other component tissues. These remarks apply to the lungs, the alimentary canal, the liver, the urinary organs, and the organs of generation; but it is much more easy to detect the presence of the tubercular matter in the mucous tissue of some of these organs than in that of others. It is very conspicuous in the fallopian tubes and uterus.

But tubercular matter is often deposited on serous surfaces also; among which Dr. Carswell includes the celluar tissue. It is even to be seen sometimes in the blood itself: not indeed while it is retained in its proper vessels, but when it is collected in the cells of the spleen. You know that the spongy texture of that organ allows the blood to accumulate in it in considerable quantity: and the tubercular matter may be seen forming in the blood at some distance from the walls of the cells in which the blood is contained. In one cell, according to Dr. Carswell, you may perceive simply the blood coagulated: in another, it will be coagulated and deprived of its colouring matter: and in another, converted into a mass of solid fibrin, having in its centre a small nodule of tubercular matter.

Now when a speck or morsel of tubercular matter has been deposited, anywhere, it is liable to increase. It grows larger by continued accretion; by additional deposits upon its surface. This being the case, we see plainly enough how it happens that tubercles assume different shapes, according as they occur in different parts. The round form which is so often observed is purely accidental. When a tubercle is deposited in the substance of the brain—and becomes larger by the continual accession of fresh tubercular matter upon and around it—it naturally takes a spherical form, because there is nothing to limit its enlargement, except the soft cerebral matter itself, which presses it with equal force on every side. For the same reason tubercles deposited in the *cellular* tissue are globular. In

like manner, if tubercular matter be laid down in one of the pulmonary vesicles, so as to fill it up, it exhibits the rounded form of the vesicle. When it fills the cavity of a mucous follicle, it has a similar figure. But in the smaller bronchi it takes a cylindrical arrangement. When (as often happens) it occupies one of these tubes, and also all the air-cells to which that tube leads, then we have a sort of branch of tubercular matter, with a cauliflower termination. You see this depicted in the drawings before you. In the cavity of the uterus, and the fallopian tubes; in the infundibula and pelvis of the kidney, and in the ureters; and in the lacteal and lymphatic vessels; the tubercular matter is moulded to the forms of these parts respectively. We are more in the habit of examining tubercles in the lungs than anywhere else; and you will observe that in making sections of these organs, and looking only at the surfaces of these sections, we may easily overlook the branch-like disposition of the tubercular matter in the smaller bronchial tubes. We see the transverse section only of these tubes, which is necessarily more or less circular. On the surfaces of serous membranes, whether natural or adventitious, the tubercular matter will assume a rounded, or a lamellated form, according as the secretion in which it originates has taken place from separate points, or from a continuous surface.

From what has now been stated you will perceive that no alteration can take place in the tubercular matter after once it has been deposited, except through the agency of the parts around it and in contact with it. It is never organized, or capable of organization; and, consequently, no change in its consistence can originate in the tubercle itself.

It may therefore seem odd, that so accurate an observer as Laennec should have persuaded himself that the softening of tubercles begins in their centre.

Now Dr. Carswell has given what appears to me a very satisfactory explanation of this mistake. Take the lungs; the morbid conditions of which were the most special object of Laennec's investigations. The tubercular matter is effused (principally) upon the mucous surface; upon the inner lining of the air-cells, and of the bronchial tubes communicating with them. Now it need not so accumulate as to *fill* these cavities; and it often does not; there is left a central cavity, which contains mucus, or other secreted fluids: and if the lung be cut across under these circumstances, the divided air-vesicles will look like rings of tubercular matter grouped together; and each divided bronchial tube will present also the appearance of a tubercle, with a central depression, or soft central point. On the other hand, when the tubercular matter has completely filled and blocked up these cavities, both vesicles and bronchial tubes will look, when divided, like the section of round solid tubercles. These Laennec seems, in fact, to have regarded as *crude tubercles:* while he mistook the former appearances for tubercles which were beginning to soften in their centre.

But you sometimes find large masses of tubercular matter in the lungs, or elsewhere: and in these masses you see that the process of softening is going on at several points, within the mass, at the same time. How is this to be explained? Why these large masses are formed, in truth, by the aggregation of many smaller masses, which lying near each other, have been pressed together, as the deposit continued to increase: and the cellular and other tissues originally intervening between them at length suppurate, by reason of the augmenting pressure; and by their suppuration, they soften, and gradually break down the tubercular matter which they enclose, and by which they are also enclosed. This is just the process by which tubercles are frequently expelled from the body. They increase till the surrounding parts take on inflammation, just as they would do if any foreign body exercised the same degree of pressure upon them. The inflammation thus excited is of the scrofulous kind; the thin pus which it throws out pervades and loosens the tubercular matter; a process of ulceration goes on in the surrounding textures; and at length (supposing the lung to have been the seat of disease) the detritus of the tubercle is brought up, gradually, by coughing.

Both Laennec and Louis, the latter also being a very close observer, describe the nascent tubercle as a grey semitransparent corpuscle. I have frequently seen such: but the appearance is rare, in comparison with the more opaque form of tubercle. Of this, too, Dr. Carswell offers what I think a very probable explanation. He says that the

mucous membrane of the air-tubes separates from the blood, not only the matter of tubercle, but also its proper secretion: and that, frequently, when the two have been poured out *together*, a dull yellow opaque point of tubercular matter becomes *set*, as it were, in a portion of grey, semitransparent, and, sometimes, inspissated mucus. The same deceptive appearance is common on serous membranes: in tubercular peritonitis, coagulable lymph exudes, portions of which envelope little globular masses of tubercular matter.

The account which I have now given you, and which I hope I have made intelligible, is, I think, extremely interesting—and much credit is due to Dr. Carswell for having so greatly simplified our views of a subject which had previously been wrapped in profound obscurity. In no other writer, that I know of, is there to be found so complete and credible an explanation of the origin of tubercles; of the form they assume; of the phenomena attending their enlargement, and subsequent softening, and occasional expulsion

These processes of softening, in consequence of surrounding inflammation—and of subsequent expulsion—may be regarded as a natural mode of cure. Such a cure is in truth occasionally effected. A scrofulous abscess will form in the glands of the neck—and pus and tubercular matter will be discharged—and at length the ulcer heals, and no trace of the diseased process remains, beyond a scar: and the same thing takes place also in the lungs; and, if there have been only one or two masses of tubercle deposited, the patient may thus get quite well: but unfortunately, as the scrofulous matter is extirpated from one part of the lung, it is apt to be multiplied in another, till at length we have death by hectic, and all its melancholy accompaniments.

But I am desirous of pointing out to you another way in which tubercular disease may be said to be cured by a natural process And this also has been better described by Dr. Carswell than by any other writer. One form of scrofulous disease, exceedingly common too, especially among children, is what is called "tabes mesenterica." Tabes and phthisis, the one a Latin and the other a Greek word, signify, I need scarcely tell you, the same thing: a wasting away, or consuming: and phthisis is applied to the same disease in the chest, to which tabes is applied in the belly. The common English word is consumption; and we might very well speak of pulmonary consumption, and of abdominal consumption, but the technical name of the latter complaint is tabes mesenterica. This is not only a very common but a very fatal disease in children and young persons. The glands of the mesentery enlarge and become charged with tubercular matter: but they very rarely suppurate. Their enlargement is commonly connected with scrofulous disease and ulceration of the mucous follicles of the intestines: and the little patients die, because the lacteals are no longer able to take up sufficient nourishment from the food: they die starved. But some few do recover from tabes mesenterica. Dr. Carswell relates an interesting case in which such recovery took place, and in which he had an opportunity of examining the glands at a subsequent period: it is the only case of the kind perhaps on record. He says, "The patient who when a child had been affected with tabes mesenterica, and also with swellings of the cervical glands, some of which ulcerated, died at the age of 21, of inflammation of the uterus, seven days after delivery. Several of the mesenteric glands contained a dry cheesy matter, mixed with a chalky-looking substance: others were composed of a cretaceous substance; and a tumour, as large as a hen's egg, included within the folds of the peritoneum, and which appeared to be the remains of a large agglomerated mass of glands, was filled with a substance resembling a mixture of putty and dried mortar, moistened with a small quantity of serosity. In the neck, and immediately beneath an old cicatrix in the skin, there were two glands containing in several points of their substance, (which was otherwise healthy,) small masses of hard cretaceous matter."

Now what Dr. Carswell here saw in the mesentery and the neck, is what sometimes occurs in other parts of the body; in the lungs; and particularly in the bronchial glands at their root, and about the bifurcation of the trachea. From these situations, the hard chalky matter left by the absorption of all the more watery part of the morbid secretion, and by the concretion of its earthy salts, is often coughed up. But it may remain, when the tubercles are few, and there is no tendency to their increase, for years, as an inert, and almost harmless mass.

I mentioned just now that the secretion or separation of the matter of tubercle from the blood, takes place, by preference, upon the free surface of mucous membranes, and very frequently also upon the surface of serous tissues, including the cellular.

It may not be uninteresting to state the relative frequency of scrofulous disease in different organs, or in different parts of the same organ. The facts which we possess of this kind afford us very valuable assistance sometimes in respect of diagnosis.

During the periods of childhood and youth the lymphatic glands are exceedingly prone to scrofulous inflammation: especially the mesenteric and the cervical glands. But in adult age tubercles are, beyond all comparison, most frequent in the *respiratory organs;* and they occupy the summit of the lung much more commonly and thickly than any other part. The superior and posterior portion of the upper lobe is the part in which, if any tubercles at all exist in the lung, they are almost sure to be found: in this situation their softening also commences; and it is supposed that the left lung is more obnoxious to tubercular disease than the right. So constant is this law, with respect to the upper parts of the lung, that Dr. Carswell considers the formation of tubercles in any other part of the lung as a secondary occurrence; and he declares it as the result of his experience (and few persons can have had more opportunities of examining diseased lungs) that there is no deviation from this rule; except when some other portion of the lung may have been the seat of an inflammatory attack, which has determined the priority of tubercular disease in that portion. We shall see hereafter what a very important bearing a knowledge of this law has, in settling the nature of a complaint which might, without it, be doubtful.

Scrofulous ulceration of the larynx and trachea, when they occur, are usually concomitants of tubercular deposits in the lungs.

Next, tuberculous or strumous disease is exceedingly common in the digestive organs: most of all in the mucous follicles of the small intestines; both in those which are separate, and are called glandulæ solitariæ, or Brunner's glands; and in those which are collected into roundish or oblong groups, the glandulæ agminatæ, or glands of Peyer. It is secondarily to these affections, in many cases at least, that the glands of the mesentery become implicated. Tubercular deposits are frequent also in the solitary glands belonging to the cæcum. The ulceration which follows the evacuation of the strumous matter from these parts gives the interior of the bowel an appearance somewhat resembling a moth-eaten garment. Tubercular matter is seldom deposited in any other parts of the intestines, great or small, than those which I have mentioned. Dr. Carswell supposes that it may often be secreted upon the free surface of the membrane, but that, not being entangled or confined in any mucous crypt, it is removed as soon as it forms. It is not often that scrofulous tubercles are formed in the liver of adults: they are not very uncommon in that organ in children, but even then they are few in number and small in size. It is a curious fact that they are much more frequently seen in the spleen also in children, than in grown-up persons. The uterus, the testicle, the prostate gland, are all liable to them: they are common enough upon the surface of the peritoneum.

In the nervous system, tubercles are by no means unfrequent: they are met with oftener in the brain than in any other part of that system. That inflammatory affection, which leads to the effusion of serous fluid in the cerebral ventricles of young children, and is known by the name of hydrocephalus, occurs principally, if not altogether, in connection with the scrofulous diathesis.

Strumous deposits are rare in the organs of circulation. Tubercles have been seen, I believe, in the muscular substance of the heart: but this must be a very uncommon thing. Scrofulous disease is not at all unfrequent in bone, especially in the bodies of the vertebræ, and in the spongy extremities of the long bones.

It is very seldom indeed that scrofulous tubercles occur in one organ only. Almost always they are met with in at least two, and frequently in all the parts at once which are liable to be infested by them. Sometimes the lungs alone are affected: but generally both the lungs and the intestines are occupied by the disease. It has been affirmed, by a great living pathologist, M. Louis, that if you find tubercles in any other organ, you are sure to find them also, and in greater number, and farther advanced, in the lungs. But this, though true as a general rule, is not without exceptions. I have seen the peri-

toneum crowded with myriads of these tubercles, when the most careful examination could not detect a single one in the lungs. And similar examples have fallen under Dr. Carswell's observation.

The question has been much, and eagerly discussed, whether the deposition of tubercular matter be not, what I should call, an *event* of inflammation. Some persons have strenuously argued that the curd-like substance is nothing more than a particular kind of lymph, and that it is never poured out except as a consequence of inflammation; and they cite cases of persons who always had enjoyed good health, until inflammation was accidentally excited in their lungs; immediately after which the well-known signs of phthisis began to display themselves; and after death, the lungs were found full of tubercles. But they forget to take into the account another fact, equally well established, viz. that tubercles are found, in great abundance, in the lungs of persons who were never known, in their lives, to have any functional disturbance of those organs; and whose lungs present, after death, no other traces of having been inflamed. We even find tubercles in the lungs of unborn children. Not that this is conclusive; for inflammation does sometimes attack the fœtus in utero, and leave permanent and unequivocal traces of its action.

Moreover, inflammation continually happens, in all the component textures of the lung, in the form of bronchitis, pneumonia, and pleurisy, without the subsequent development of tubercles. I admit that this fact, to be of weight, should be proved of persons who possess the scrofulous diathesis; and I believe the proof might be found: but the search for it would require much carefulness and candour.

In my own opinion, there is not the shadow of evidence to show that the deposit of tubercular matter is always and necessarily preceded by inflammation. Yet an undoubted and most important *connection* obtains between the occurrence of inflammation and the occurrence of tubercles. Tubercles will cause inflammation, and inflammation will determine the development of tubercles. The enlarging tubercles excite inflammation in the surrounding textures by the pressure they exert upon them; and probably in other ways; by mechanically interfering with the healthy circulation of the blood, for example: and the inflammation lit up is usually of the scrofulous kind; it is slow, and partial, and easily *quieted* by treatment, though scarcely to be *cured*. On the other hand, there are numerous facts to prove that, in a person having the scrofulous diathesis, the occurrence of inflammation within the chest may rouse that previously dormant tendency into action, and become the exciting cause of the secretion of tubercular matter from the blood. The cases in which other parts of the lung than the apex are found exclusively occupied with tubercles, are also cases in which, apparently, the same parts had been the seat of inflammatory action: of which we sometimes see other traces in adhesions of the neighbouring pleura.

The connection between tubercles and inflammation is shown also by their occurrence in the substance of false membranes. And the same phenomenon marks the fact that they are something distinct and different from coagulable lymph.

You must not suppose, from any thing I have said, that persons of the scrofulous habit are not susceptible of *common* inflammation: we know that they are, by the readiness with which slight injuries often heal in such persons: but there is always much reason to apprehend that inflammation occurring in them will take on the scrofulous form; become chronic, if it was not so at first, suppurate tardily, and produce that unhealthy kind of puriform secretion which is characteristic of strumous disease.

Another question relating to turbercular diseases is, whether they are *contagious:* capable, *i. e.* of being communicated from one individual to another. The general belief, in this country, is that they are not. Indeed their very dependence upon a peculiar diathesis would seem to disprove the supposition. Yet some practitioners, even here, have, I know, misgivings on the subject: and in some parts of the continent, in Italy particularly, consumptive patients are shunned, from the persuasion that their complaint is infectious. I shall revive this question when I speak of phthisis hereafter.

Strumous diathesis.--I have stated, that scrofulous disease appears almost exclusively, in certain *classes* of persons, of whom, therefore, we say, that they have the scrofulous diathesis.

It is both interesting and useful to be able to distinguish those in whom the scrofulous habit of body, or the predisposition to strumous disease, exists.

Now there are certain physical and moral characters which teach us to apprehend the existence of a tendency to scrofulous disease, even when there has not, hitherto, been any local manifestation of such disease.

Again, we infer the scrofulous diathesis, in many persons, from knowing that scrofulous disease has existed among their progenitors.

On these two points I have a few observations to make: and first, on what may be considered the external marks of a scrofulous constitution.

The persons, in whom scrofulous disease is *most* apt to declare itself, are marked during childhood, by pale and pasty complexions, large heads, narrow chests, protuberant bellies, soft and flabby muscles, and a languid and feeble circulation. They present many of the features belonging to that pattern of body which is denominated the leucophlegmatic. But the strumous disposition very often indeed accompanies a variety of the sanguine temperament also; and is indicated by light or red hair, grey or blue eyes with large and sluggish pupils and long silky lashes, a fair transparent brilliancy of skin, and rosy cheeks. This red colour, which is well defined in general, is easily changed, however, by cold, to purple or livid; the skin is thin and readily irritated; the sclerotica has often a peculiar pearly lustre; and the extremities are subject to chilblains. Such children are, many of them, extremely clever, and ready of comprehension, of eager tempers, and warm affections, lively, ardent, imaginative, and susceptible. This precocity of mind and intellect, while it delights the fondness of the parent, awakens the fears of the more far-seeing physician.

But the disposition to scrofula is by no means confined to persons of the serous or of the sanguine temperament. It is frequent, though less common, in what has been called the melancholic or bilious temperament; in persons of dark muddy complexion, and harsh skin; in whom the mental and bodily energies are more sluggish and dull. And it is remarked that in persons of this cast, scrofula, when it does occur, is even more than usually obstinate and untractable.

Scrofula does often indeed appear in persons who exhibit none of those signs of a strumous disposition which I have been enumerating: but it is *more likely* to appear, cæteris paribus, where those signs *are* observed.

There are several alleged marks of a scrofulous *diathesis*, which are, in fact, instances of scrofulous *disease*. Such, for example, as that chronic lippitudo, which so frequently disfigures strumous children, rendering them what is called blear-eyed: and chronic inflammation of the conjunctiva, lasting long, without much redness, or heat, and with extreme impatience of light, and a tendency to form little pustules near the edge of the cornea. The tumid and chopped upper lips; the redness and swelling of the columna nasi, and lower parts of the nostrils, so common in children, especially during winter, are early fruits of the strumous taint. Certain maladies of the joints, what are popularly called white swelings, are instances of scrofulous disease. So may perhaps rickets be considered: at any rate, rickety children are very often affected with scrofula also. Moist eruptions behind the ears; chronic enlargement of the glands of the neck; that slow, eating, ulceration of the nares termed lupus; may all be included within the class of strumous disorders.

When any one of these scrofulous affections has once shown itself in any person, we know, by that circumstance, that he possesses the strumous constitution; and we look for the recurrence of his complaint in the same part, or in both parts.

In a former lecture I mentioned scrofula as one of those complaints the hereditary tendency to which is indisputable. The scrofulous *diathesis* is hereditary: and sometimes scrofulous *disease* is so too. I have seen lungs, taken from the body of a fœtus, stuffed with tubercles. There are some fine examples of this in Mr. Langstaff's museum, in the city. We have, therefore, in respect to scrofula, the rare conjunction of congenital disease, and hereditary disposition. I need not repeat here the remarks I made before, respecting hereditary diseases in general. No one, of the least observation, can doubt that the disposition to consumption is very often transmitted from parent to child. We see whole families swept away by its ravages. Like other hereditary tendencies, it may skip over one or

two generations, and reappear in the next, just as family-likenesses are known to do. There are other families in which you can trace no such predisposition: but such families are perhaps few. A little leaven is sufficient, sometimes, effectually to taint a whole pedigree. The tendency, however, exists in very various degrees. It may be so strong that no care, no favourable combination of circumstances, will prevent its local manifestation: and it may be so faint that it would never break out into actual mischief if the exciting causes of scrofulous disease could be warded off. It is important, therefore, to know what these exciting causes are.

"They may all be ranked together (to use the language of Dr. Alison) as causes of *debility*, acting permanently, or habitually, for a length of time, although not so powerfully as to produce sudden or violent effects."

The circumstances to which, acting separately or in combination, we must confidently ascribe the power of developing scrofula, are insufficient nutriment, exposure to wet and cold, impurity of the atmosphere, a want of natural exercise, and mental disquietude. To estimate the separate effect of each of these causes may be difficult; but their combined influence is unquestionable.

There can be no doubt that improper diet, or rather imperfect nourishment, is one main exciting cause of scrofulous disease. Yet it is not an easy thing to obtain evidence of this, which shall be entirely free from fallacy. The disease occurs very often among the poor; but then it very often occurs also in the families of the rich. There is one fact which has always struck me as very instructive and convincing on this point. Infants at the breast, having good milk and plenty of it, seldom show any signs of scrofulous disorder: whereas, as soon as they are weaned, they become subject to various complaints of a strumous kind. When an unweaned child is brought to us with ophthalmia, we expect almost always to discover inflammation of the common and acute kind; the purulent eye. In nine children out of ten who come after weaning, we look for and find some form of scrofulous inflammation, such as pustular ophthalmia.

The greater prevalence of scrofulous disease among the poor may be ascribed, in great measure, to their frequent exposure to wet and cold. Scrofula seldom breaks out in the mild and dry weather of summer. The influence of climate in fostering or repressing the disease is notorious. There is no climate in which it flourishes more than in our own. Consumption is called, in some parts of the continent, the English disease. Persons who migrate from this country to warmer and more equable climates, seldom become scrofulous; nay, it very often happens that the incipient indications of strumous disease are completely arrested or quieted by the change. Phthisical patients, much troubled by symptoms here, are sometimes so thoroughly freed from them soon after their arrival at Madeira, as to be deceived into the belief that their case had been mistaken. They think themselves well. A return to this country undeceives them. The native inhabitants of hot climates are by no means, however, exempt from struma, in any of its forms. When they come to this country they are more subject to scrofula than we are ourselves. And the same effect of climate is very distinctly visible in the lower animals. The physicians in ordinary to the inmates of the Zoological Gardens will tell you that the beasts and birds that are brought hither from warm latitudes perish in great numbers from scrofulous diseases. John Hunter observed this long ago in respect to monkeys.

Of the debilitating influence of impure air I spoke in a previous lecture. That it promotes the evolution of scrofulous disorders we have proof, on a large scale, in the great mortality produced by such disorder among the lower classes in large cities as compared with agricultural districts. The percentage of deaths from consumption, hydrocephalus, and various other diseases that spring from a strumous habit, is much greater in London than in the country. Even in individual cases this influence is too manifest to be overlooked or mistaken. It is impossible to question the beneficial effect, upon children afflicted with scrofula, of a removal from London to the sea-coast.

I said, when I first began to speak to you of inflammation, that it was the only disease which we are able to excite at pleasure: that we could cause inflammation in various ways whenever we desired to do so; but that to make a cancer or a tubercle was beyond our power. Now in strictness of language, and in the practical meaning of these words, this assertion is quite true. But it is not so exact if we extend it to all the *predisposing*

causes of disease. We are able to bring about the formation of tubercles, in the lower animals at least, by so arranging the external influences as to concentrate their prejudicial effects. By shutting rabbits up in a cold, damp, dark, and narrow place—and feeding them on food not natural or suited to them—we can produce or evolve in them tubercular disease. Of course no experiment of that kind can be purposely made upon a healthy man; but accidental opportunities arise of witnessing an approach to a similar trial of the human species: instances are recorded of persons, previously well, (but having probably the strumous diathesis,) becoming affected with scrofula after being confined in the dungeons of a prison, and there scantily fed.

Something of this kind I have, very recently, had the opportunity of seeing.

A number of male prisoners, chiefly young men, became affected with glandular swellings of the neck, after incarceration for some time in the Penitentiary at Milbank. The circumstances of their health led to a relaxation of their punishment. Instead of being kept in solitary confinement, in a coldish cell, and on the prison diet, they were permitted to work, for several hours daily, in each other's company, in the garden of the establishment. Some porter was at the same time given them, and their allowance of meat was increased. The improvement in their condition was rapid and striking. Here we have the disorder germinating under one state of external circumstances, and checked immediately under the opposite state.

If you consider the way of life of the children of the poorer classes in this metropolis, and in the larger manufacturing towns, you will find that they are much exposed (though in a less degree) to the same injurious influences, the combination of which appears to generate tubercles in the rabbit. They live, for the most part, in an atmosphere made stagnant by narrow streets; and in small, crowded, ill-ventilated, and dark rooms in those narrow streets: the stagnant atmosphere is contaminated in a thousand ways; they are very insufficiently protected from transitions of temperature, against cold and wet, by their clothing; they are commonly ill-fed—their diet being frequently scanty, and generally of a kind quite unsuited to their growing years. We need not be surprised therefore at the ravages which scrofula, in its manifold shapes, makes among the children of the poor in large and populous towns. If ever scrofula be generated, in this climate, independently of any hereditary strumous taint in the constitution, it is in them. But in most cases I believe it is the latent disposition that is called into action. Moderate exercise, in pure air, and in the open daylight, with suitable nourishment, sufficient clothing, and attention to the state of the bowels: these circumstances comprise nearly all that we can do for preventing the development of struma: and from each of them many of these poor children are habitually debarred.

LECTURE XIII.

Treatment of Inflammation. Antiphlogistic regimen. Blood-letting.

Treatment of inflammation.—I proceed to speak in a general manner of the measures that are proper to be adopted, when we are called upon to administer to the relief of a person labouring under inflammation: of what is sometimes called the *cure;* but, more correctly, of the *treatment* of inflammation.

In describing the phenomena and progress of inflammation, I took *external* inflammation as a *type*, and I shall keep that type principally in view in what I have to say respecting its *treatment:* making, however, such reference to the inflammation of internal parts as the subject will permit. You will bear in mind that my design at present is merely to explain the *principles* of treatment, generally: I shall point out, by and by, the application of those principles, and the modifications they may require, in respect to particular cases. I speak also, now, of *common* inflammation occurring in a previously

healthy person. There are many observations that concern all inflammations alike, whether external or internal, and by despatching these in the outset, I hope to save much repetition hereafter.

In all cases of inflammation, our first object is, if possible, to obtain *resolution*: and if that be not possible, we next aim at securing that *event* of inflammation which would be the most fortunate in the particular case before us. In external inflammations *good suppuration* will generally be the most desirable event, after resolution; in internal inflammations it will be sometimes *suppuration*, sometimes *adhesion*.

It is necessary to keep in mind the distinction between the treatment proper for the inflammation itself; and the treatment that may be required for the *effects* of the inflammation. At present we are concerned only with the inflammation itself.

I stated to you in a former lecture that a knowledge of the *cause* of a disease might help us in its treatment. Knowing the cause, our first care must be to remove it, if we can. In the case formerly supposed, we should extract from the inflamed arm the fragment of glass. If the inflammation has been excited by the extremity of a *fractured bone*, of a broken rib, for instance, we take measures for bringing the separated bones into their proper places, and for keeping them there: if the mere *displacement* of a part has occasioned the inflammation, as the dislocation of a joint, the protrusion of the bowel in hernia, the first thing to be attended to is the restoration of the part to its natural situation: if there be any *chemical* source of irritation, (in the stomach, for instance, threatening or producing inflammation there,) we eject, neutralize, or dilute it.

I know of but one exception to this rule, and it belongs to surgery; to wit, when a bullet or a splinter is so lodged in the interior of the body, that its extraction would be more hurtful or hazardous than its remaining where it is.

A knowledge of the cause of an inflammatory disease may help us in another way. We do not treat a joint that is inflamed in consequence of external violence, as we should treat the same joint when inflamed in rheumatism.

But it is very seldom, except in internal inflammation, that we can accomplish the removal of the cause. In most internal cases, either it cannot be got at, or it has already ceased to be applied; as when the inflammation has been excited by exposure to cold. But it may be possible, and it is of the utmost importance when possible, to prevent any *re*-application or repetition of the same cause, which would be likely to frustrate our endeavours to bring about resolution.

Antiphlogistic regimen.—Next in importance to the removal and avoidance of the exciting cause, must be placed, in most instances, the observance of what is called the *antiphlogistic regimen*. This may seem an old-fashioned phrase, but it is a very convenient one; being a brief form of expressing the sum of several distinct provisions for the welfare of the sick, and for the conduct of their attendants. The word antiphlogistic is derived, indeed, from an obsolete theory; but we retain it as a useful arbitrary term, without reference to its etymology, or to its original meaning.

The object of the antiphlogistic regimen is to put and keep the patient in that state which is most favourable to the natural subsidence of the disease, or to the sanative influence of remedies. This regimen consists in the avoidance of every stimulus that can be avoided, whether external or internal. Common sense will suggest to you the details. It implies a total abstinence from animal food, and strong drink of all kinds. It prescribes the exclusion of all that may excite or exercise the mind, or produce a strong impression upon the senses: noise; bright light; great heat or cold. The patient should be kept in a temperature of about 62°, and in a well-ventilated apartment. He must not be allowed to converse, or to attend to matters of business; unless, indeed, his mind happens to be disturbed and anxious about some point which a short interview with a friend may effectually settle. All causes of strong emotion, and mental agitation, should be strictly guarded against. Whatever tends to quicken the circulation is to be shunned; and therefore not only those influences which operate through the nervous system, but also all needless bodily effort and exertion, must be prohibited. The patient (in the serious cases I am now contemplating) must remain in bed: and in a position which facilitates, or at least does not impede, the free return of the blood by the veins from the suffering organ. If the inflammation is seated in or about the head, that part should be elevated by pillows.

If one of the lower extremities is affected, even when the disease is not so intense as to require confinement to bed, the limb must be sustained horizontally, or be even still more raised up. On the same principle it is that we suspend an inflamed hand or forearm in a sling. In some cases of internal inflammation—in pleurisy for example—the patient will choose his own position. He is admonished, by the pain and distress they occasion, that certain postures would be hurtful and dangerous, and he carefully avoids them. We often derive much information from this instinctive caution on the part of the patient.

The function of the organ inflamed should also be spared its exercise whenever, and in as great a degree as that can be done. As you would not allow a patient to move an inflamed joint, so you must not permit him to use an inflamed eye; to speak more than may be absolutely necessary with inflamed lungs; to exert by thinking, and by attention to external excitements, an inflamed brain. This last rule is essential, even when the brain is not the seat of the inflammation: it is to be observed in all febrile disorders.

The adoption of this antiphlogistic regimen is not, indeed, necessary, nor even proper, in all cases and stages of inflammation. The inflammation may be so slight as not to require it; particularly in external cases, of which the causes and extent are known; as slight contusions, trifling wounds, and some kinds of eruption. But this exception must always be applied with great caution, to cases of internal inflammation, about the causes, and extent, and tendencies of which we may be less sure. In chronic forms of inflammation again, as in scrofulous inflammation of the lymphatic glands, or of the eyes, attended with but little pain or heat, the antiphlogistic regimen would often fail to be beneficial: the state of the general system being such as to require support and strengthening measures, more than the local symptoms call for an opposite treatment. So also when suppuration or gangrene have supervened, the antiphlogistic regimen must generally be modified, or abandoned.

But in the outset of all cases of serious inflammation, when the strength is entire, and the inflammation intense enough to produce pyrexia, all the particulars of the antiphlogistic regimen may require to be observed.

Blood-letting.—Of all the *direct remedies* of inflammation, the abstraction of blood, bleeding, or blood-*letting*, as it is called, is by much the most effectual and important. We should, I think, be prepared to expect this, prior to any experience of it. Blood being the natural stimulus of the heart, we should deem it probable that the removal of a portion of that fluid would diminish the force with which the heart contracts: and as an inflamed part contains a preternatural quantity of blood, and as (with the exception of resolution and mortification, which really are *terminations* of inflammation—as with these exceptions) all the *events* of inflammation consist of the effusion of certain parts of the blood from its containing blood-vessels, we should be inclined, *a priori*, to believe that the amount of those effusions would be checked and limited by lessening the supply of blood to the inflamed organ, as well as by abating the force with which the blood reaches it. And we find it in fact to be so. The results of experience confirm, in this matter, the suggestions of our reason. Blood forms the pabulum of the whole process. "If," (says Mr. Lawrence) "we may be allowed to use figurative language, the obvious increase of heat in the part is analogous to that of fire; and blood is the fuel by which the flame is kept up: in fact if we could completely take away its blood from a part, we should be able entirely to control or arrest the increased action."

But it is not every case of inflammation that requires or warrants the abstraction of blood: and when blood-letting *is* requisite, the mode of taking away the blood, the quantity proper to be taken, and the propriety of repeating the bleeding, all vary greatly in different cases. It is obviously of vast importance that you should learn so to use this valuable remedy as not to abuse it. Its power is great for evil as well as for good: and in rash or inexperienced hands it too often becomes an instrument of fatal mischief.

There are, as you are all aware, several modes of abstracting blood: phlebotomy, arteriotomy, scarification, cupping (which is merely a variety of scarification), the application of leeches. Bleeding performed in either of the first of these methods is called *general bleeding*. The rest are, in most instances, topical or *local:* but they are not *merely* topi-

cal in all cases. The main object of general bleeding is to diminish the whole quantity of blood in the system, and thus to lessen the force of the heart's action. The object of local bleeding is, in most instances, that of emptying the gorged and loaded capillaries of the inflamed part. Sometimes the blood is thus taken directly from the turgid vessels themselves; more often, I fancy, topical blood-letting produces its effect by diverting the flow of blood from the affected part, and giving it a new direction, and so indirectly relieving the inflammatory congestion. General bleeding has also incidentally a similar tendency to deplete the vessels concerned in the diseased process: and, on the other hand, a dexterous cupper, under favourable circumstances, will take away blood from a part as copiously and rapidly as if it were made to flow from an opened vein: and then the effect upon the *system* will be alike in the one case and in the other. The same may be said of leeches, when they are applied in the enormous numbers which our neighbours, the French, are fond of using. In whatever way the blood is drawn, whether from a vein or from an artery, or by the pressure of a cupping glass upon a surface previously scarified, or by the suction of leeches, the general effect upon the system will be in proportion to the quantity of blood abstracted in a given time. The most convenient and effectual mode of general bleeding, upon the whole, is certainly the common one, from the veins at the bend of the arm. But sometimes those veins are small or deep, especially in fat people; and we fail in our efforts to get the blood to flow from them in a full stream: and then we may open some other vein or an artery, or call in the cupper to our assistance, or cover the neighbouring surface with leeches; according to the situation of the part inflamed, and other circumstances.

Let us now briefly consider what the indications are by which we judge of the expediency of taking away blood. We are guided very much by the degree of pyrexia; by the quality of the pulse; by the importance of the organ affected; by the intensity of the inflammation, in what manner soever that may be measured; by the period or stage of the disease; by the age, and sex, and general condition of the patient; and frequently also by the ordinary character and course of the disease, when inflammation happens to be, or to accompany, an epidemic disorder. It is not one of these circumstances alone, but several of them, that we have to take into the account, in most cases: and what I have now to say in reference to them must needs be very general.

The presence of pyrexia, especially when the febrile disturbance is well marked, admonishes us, indeed, to search after other indications of the propriety of blood-letting, and confirms them if they are found; but is not, of itself, a sufficient reason for resorting to that remedy. There may be high febrile symptoms without any inflammation at all: as in the hot stage of an ague fit: and a smart attack of fever may spring out of local inflammation, and yet the known course of the disease, or the nature of the part affected, may render the abstraction of blood unnecessary, and therefore improper.

Our judgment is more often determined by the quality of the pulse, although we are by no means to be *wholly* directed by this. The quality of the pulse which—other things being the same—bespeaks the necessity of blood-letting, is *hardness*. I described this quality to you in a former lecture: it may coexist with a large or a small, a slow or a frequent pulse. Most commonly (and yet the exceptions are numerous), most commonly in acute inflammations the pulse is full and frequent as well as hard. The hardness is ascertained and measured by the resistance which the throb of the artery makes to the pressure of your finger. The pulse is sometimes said to be incompressible; which means that, although you apply your finger with considerable firmness, the blood still forces its way through the vessel beneath it.

Now this hardness of the pulse is sometimes our best warrant for active depletion by means of the lancet: yet I say we must not trust to this alone: for a hard pulse may habitually exist, where there is no inflammation, Certain chronic diseased conditions of the heart may occasion it; and it probably results also sometimes from some unnatural state, which is not inflammation, of the whole of the circulating system. When you happen to know your patient, and have ascertained what kind of pulse he has when he is well, and are previously aware that his pulse during health is *not* a hard pulse, that circumstance will certify that the new quality it has now acquired denotes the presence of inflammation;

and usually of active inflammation, likely to go on, if not controlled, to the destruction of the part it has seized upon.

Many persons, and young practitioners in particular, are apt to look to the *frequency* of the pulse, when they wish to ascertain the expediency of blood-letting: but really its frequency is very subordinate in importance to its hardness or softness: and this is very unlucky, because any body with his stop-watch in his hand can *count* a pulse: but it is not every one who can tell a hard pulse when he feels it. The finger requires a certain education for that purpose; and there are some persons who seem never to attain the *tactus eruditus*. I should advise you to attend particularly to this quality of the pulse, and to compare your perceptions of the hardness or softness of the pulse in individual cases, with those of other persons.

The frequency of an inflammatory pulse ranges for the most part between 90 and 120. When the hard pulse is much more frequent than this, it commonly occurs either in young children; or in persons who are more than usually nervous and susceptible; or in persons who were previously labouring under some chronic and wasting complaint, in which the pulse was already frequent, though not hard: as, for example, in phthisical patients, when acute pleurisy supervenes upon tubercular disease of the lungs.

As the hardness of the pulse is, with certain exceptions at which I have just glanced, our lawful warrant for general bleeding; so the disappearance of that hardness is a token that the blood-letting has been carried far enough.

Again, the nature and importance of the organ affected will influence our judgment in respect to the question of abstracting blood. If the organ inflamed be a vital organ; or if we are not sure about that, but have any reason to suspect that it may be a vital organ; I need scarcely say that we must act upon the worst supposition, and bleed. But if the part be of less importance in the economy of the body; or if inflammation is known to run its course in that part without producing any abiding damage; it may not be worth while to have recourse to this potent remedy, even though the fever be high and the pulse hard, for the sake of subduing inflammation which is attended with so little danger. In this predicament may be placed many instances of cynanche tonsillaris, and of acute rheumatism. The subsequent debilitating effects of the loss of blood upon the system may be more certain and more hurtful than the effect of the bleeding upon the local inflammation is likely to be beneficial.

The period or stage of the disease forms a most important element in the question before us. It is of inflammation while yet in its early progress, that blood-letting may emphatically be pronounced the cure; while the disease is still within the possibility of resolution; before there is any great amount of effusion, or any serious disorganization of structure. The sooner we bleed, the more surely will the inflammatory process be moderated and limited, even when it cannot be wholly quenched. In no case within the range of medical practice is the maxim "principiis obsta" more imperative. Those among you who happen to be attending the wards of the Middlesex Hospital may wonder indeed, after hearing my estimate of the power of blood-letting over inflammation, that I so seldom prescribe venesection there. The truth is, not that I undervalue the remedy, but that the time for its employment has generally gone by. The poor are unwilling to relinquish the occupations by which they subsist: they struggle on as long as they can, and resort to hospitals only when they are compelled to do so by the exigency of their malady. Many of them, labouring under inflammation, have been freely bled before admission. It is commonly too late, when they present themselves, to expect that the course of the disease can be so arrested. The first effect of blood-letting is to deplete and relieve the labouring circulation. But when it is again and again repeated, it becomes (as the French say) *spoliative;* it robs the vital fluid of its nutrient and plastic materials. Pushed still farther, it produces a peculiar state of the nervous system, marked by great weakness and irritability. Now although blood-letting is the *summum remedium* for inflammation at its commencement; there is a point beyond which it not only does no good, but is positively injurious. And this point it is not always easy to hit. On one side is the danger that the inflammatory action may continue and extend; on the other the danger that the strength of the system may be so reduced as to prove unequal to

the process of restoration: for, to remove the interstitial extravasations, and to repair the damage that has accrued, a certain degree of vital power is requisite, and a sufficient quantity of healthy blood. Bleeding will cure inflammation, but it will not always cure the effects of inflammation; nay, it may render them lingering in their departure—or even determine their fatality. I cannot too often, or too strongly, inculcate the precept, that, in order to check and extinguish acute inflammation, you must, above all, bleed *early*.

We judge that the bleeding has been carried far enough when the inflammatory fever subsides or changes its character; when the pulse regains its softness, or undergoes some marked alteration; when any of the signs (already specified) of suppuration appear. Upon these points I hope to give you more explicit instruction when we come to special instances of inflammation.

Whenever inflammation supervenes on other chronic disease: whenever it arises in the progress of idiopathic fever, or whilst the constitution is contaminated by some specific poison: whenever suppuration is inevitable, or even probable: in all these cases general blood-letting may be necessary, but it must be employed with great caution.

Nor can we, safely, neglect the age, and sex, and general condition, of the sick person, when we are turning in our minds the propriety of bleeding. The very young, the old, and the feeble, do not bear well the loss of much blood. This consideration is not to deter you from bleeding such persons when they are attacked by dangerous inflammation; but it especially enforces, with respect to them, the general rule, that no more blood should be abstracted than is absolutely requisite to control the disease.

It is also very necessary to study the character and tendency of the reigning epidemic: whether that may depend upon some predisposition silently and gradually produced in men's bodies by the agency of causes that are but little understood; or whether it may result from some peculiarity in the exciting cause of a particular epidemic disease. I have been long enough in practice in London to have learned, in common with others, how much the character of continued fever may alter. Since about the time when the virulent form of cholera made its first appearance, among us, continued fever has neither required nor borne the abstraction of blood as it did bear and require it for some years prior to that period. Perhaps some variation in the intensity of the poison may partly explain the comparative malignity—the greater tendency, I mean, to the typhoid type—which marks certain epidemics of scarlet fever, small-pox, and measles. The influenza, or epidemic catarrh, which was almost universal in this town and kingdom in the years 1833 and 1837, afforded a striking illustration of the point I am endeavouring to set before you. The inflammatory symptoms—the bronchitis, and sometimes pneumonia—were in many cases strongly marked, and it was necessary to abstract blood; but persons suffering under influenza bore bleeding exceedingly ill, and where the use of the lancet could not be avoided, it was never resorted to without reluctance and misgiving.

When we bleed in acute inflammation of an important organ, we endeavour, I say, to effect our purpose as speedily as possible, and with as little expenditure of the vital fluid as possible. It would be quite ridiculous to pretend to give any precise direction as to the number of ounces of blood that should be taken. You must stay by the patient, and bleed, in such cases as I am now contemplating, until you produce some distinct impression by the bleeding; and one of the best guides in this matter is the state of the pulse. If you find, as you sometimes will do, that the most pressing symptoms give way while the blood is still flowing—that the pain, for instance, is mitigated—that the respiration (when the lungs are concerned) becomes easier and deeper—that (in affections of the brain) the patient emerges from a state of stupor or delirium—you may be sure that you are doing right in bleeding; but you must keep your finger upon your patient's wrist, and suffer the blood to flow, until the hard pulse is sensibly softer, or until symptoms of impending syncope appear; and then you had better tie up the arm, and wait a few hours, and repeat the bleeding if the symptoms which at first demanded it again become urgent.

As it is desirable to produce the necessary effect upon the system as quickly as may be, the blood should be taken *pleno rivo*, *i. e.* a sufficiently large orifice should be made in the

vein; and sometimes it may be right to open a vein in both arms: and the patient should be bled in the upright position. Faintness and syncope depend upon a defective supply of blood to the brain; and therefore will be likely to occur the sooner when the force of gravity facilitates the descent of the blood from the head through the veins, and retards its ascent towards the head through the arteries. And conversely, the first thing to be done towards remedying syncope is to lay the person flat in a horizontal posture, or even with his head lower than his trunk.

If you neglect these smaller matters, and make an insignificant slit in the vein, and suffer your patient to lie down when you are bleeding him, you will be obliged to take much more blood in the end; or you may drain him of his blood and of his strength by repeated bleedings of this sort, and make no impression after all upon the disease. It is one of the numerous cases in which parsimony is not true economy.

The quantity of blood requisite to be taken in order to produce the due effect is exceedingly various. It is a remarkable circumstance, well worth attending to, and much insisted upon of late years, especially by Dr. Marshall Hall, that a patient under the influence of mere inflammation will bear to lose a far greater quantity of blood without becoming faint, than he could bear in health: that the state of the system produced by the presence of inflammation protects it from the ordinary consequences of loss of blood. The amount of the bleeding necessary to occasion syncope will be in proportion to the exigency of the case. This fact—if it be really a fact, as indeed I believe it is—is evidently one of the highest value and importance, for it furnishes, what is always so desirable, especially in an uncertain art like ours, a simple rule of practice. Yet it is not a rule so firmly established as not to admit of exceptions. If the mere state of syncope was the curative influence required, we should have no difficulty. That the faintness does constitute a part of that influence I fully believe. Dr. M. Solon even relates a case in which it sufficed to the cure of erysipelas of the head and face, attended with high fever. The patient fainted from alarm, before the vein was opened. The inflammatory symptoms thereupon ceased: but with returning animation they presently recurred. Again preparation was made for venesection; and again the young lady lapsed into syncope: and this time the inflammation and fever disappeared, never to return. She is described as having been quite well the next day. I cannot, however, entertain a doubt that the withdrawal of a certain quantity of blood is, in almost every case, essential to the permanent control of common acute inflammation, attended with pyrexia: and it may be advisable to keep persons who, like M. Solon's patient, are of a timid disposition, and liable to syncope from slight causes, in a recumbent posture, in order that the requisite discharge of blood from the system may be obtained.

In equivocal cases (and there are many such), where it is questionable whether the symptoms proceed from inflammation or not, the diagnosis may often be settled by observing the quantity of blood which, taken in the upright posture, suffices to bring on incipient syncope.

Dr. M. Hall's book "*On the Effects of Loss of Blood*," is well worth your attentive perusal. He suggests that a scale of diseases might be formed, representing the protective influence of some diseases against the effects of blood-letting; and the opposite influence of some others in producing preternatural susceptibility of those effects. "It would begin (he says) with congestion of the head, or tendency to apoplexy; inflammation of the serous membranes, and of the parenchymatous substance of various organs, would follow; then acute anasarca; and lastly, inflammation of the mucous membranes. This part of the scale would be divided from the next by the condition of the system in health. Below this would be arranged fever; the effects of intestinal irritation; some cases of delirium; reaction from loss of blood; and disorders of the same class with hysteria; dyspepsia, chlorosis, and cholera morbus."

With respect to the propriety of *repeating* venesection, it is his remark, that if at the first blood-letting much blood flowed before any tendency to syncope manifested itself—an early repetition of that remedy will probably be required—and at any rate an early repetition of our *visit* to the patient will be proper. But this last precept is of universal obligation in all cases of serious inflammation.

I am almost afraid to tell you how much blood I *have* seen taken at one bleeding, lest I

should seem to encourage you to imitate such heroic practice. I once stood by, and saw, not without trembling—although I was quite free from responsibility in the matter—a vein in the arm kept open until seventy-two ounces (four pints and a half) of blood had issued from it: and then, and not till then, did the patient become faint. The event of the case quite justified the bleeding in that instance, for the man got perfectly well. It was a case of general dropsy, which had come on suddenly, in a young and robust man. It occurred in the clinical wards of the Infirmary at Edinburgh: the physician had desired the clinical clerk to bleed the patient in the erect posture, until some sensible effect was produced upon his pulse: and no such effect could be perceived until the enormous quantity I have mentioned had been abstracted. It is very seldom that such large bleedings are required: you will generally find that five-and-twenty or thirty ounces, taken properly, will be sufficient to accomplish the purpose of the measure. Sometimes one such bleeding will extinguish, as it were, the inflammation; sometimes two or three, or half a dozen, may be necessary: and we judge of the propriety of repeating the venesection by the effect of the former bleeding; by the character of the pulse; by the appearance of the blood already drawn. It would be impossible, in a general account like the present, to lay down any minute directions on this head.

I have hitherto been speaking of bleeding, as we perform it for the cure of active inflammation, occurring in a person previously healthy, affecting an important organ, and attended with febrile disturbance of the system. But the abstraction of blood is scarcely less valuable as a remedial measure in *chronic* inflammation, when the system at large scarcely sympathizes at all with the local disease. And here it is that what is properly called local bleeding is so useful—by cupping glasses, or a moderate number of leeches. The object is always the same, viz. to unload and relieve the turgid capillary vessels of the part: and this we could not do by general bleeding without carrying it to an extent which would be dangerous to our patient's existence. These local bleedings for chronic inflammation usually require to be often repeated. Considered as a remedy, blood-letting resembles some other remedies in this, that it must be proportioned and adjusted to the rate of progress, and the duration, of the disease. The remedy must be used chronically when the malady is chronic. A patient may lose, on the whole, much more blood for the cure of a chronic inflammation than for the cure of one that is violent and acute: but then the bleeding must be spread over a larger space of time.

With respect to the relative merits and advantages of cupping and leeches, as topical remedies for local inflammation; it may be said in favour of cupping, that the precise quantity of blood taken away is more accurately determined in that manner, and the operation is sooner over, and is less fatiguing, than the suction of leeches. But on the other hand the leeches seldom bungle in the operation; while the surgeon often does. It requires a good deal of practice to become handy and dexterous in the application of the glasses—to avoid torturing and burning the patient—and on that account it is that in large towns, as in this metropolis, cupping is an art carried on by a distinct class of persons. You may apply leeches also to parts where the cupping glasses could scarcely be used.

General bleeding then is best adapted to acute inflammation; and topical bleeding is most appropriate in that which is chronic and slow. But a combination of the two is often highly proper and useful. You may lessen the force of the general circulation by venesection; but the small vessels of the inflamed part may remain unable to rid themselves of their excess of blood, and continue dilated and full. Such, at least, we may reasonably suppose to be sometimes the case; and certainly we often act successfully upon that theory; that is, we bleed from the arm, and at the same time, or presently after, we empty the capillaries of the labouring organ, or the neighbouring vessels, by the help of leeches, or the scarificator and exhausted cup. The effect of local bleeding, after the general febrile disturbance has abated under venesection, is often very marked in the relief of *pain*.

I have recommended blood-letting to you when, among other circumstances, the pulse is full and hard; and have stated that the blood should be suffered to flow until some distinct impression is made upon the system. But I wish also to apprise you, that you ought not to be deterred from bleeding merely because the pulse is small. It is very apt to be so in dangerous inflammations within the abdomen; and it is a very curious thing that the pulse

will often rise, and the artery develope or expand itself during the time the blood is flowing. Now you must look upon *that* circumstance as a distinct impression made upon the system, although it is one of a rather different kind from what I spoke of before. You had better, in my opinion, pause when this effect is fairly obtained: for so great is the tendency to death by syncope in abdominal inflammation that it would not be prudent to urge the effect of the blood-letting farther, at one time, than the change I have just mentioned. Wait, therefore, and repeat the venesection if the circumstances should again render it necessary.

LECTURE XIV.

TREATMENT OF INFLAMMATION, CONTINUED. RECAPITULATION. BLEEDING: PURGATIVES: MERCURY: ANTIMONY: DIGITALIS: COLCHICUM: OPIUM. LOCAL REMEDIES. EXTERNAL COLD: EXTERNAL WARMTH: COUNTER-IRRITATION.

AFTER pointing out to you, yesterday, the necessity of guarding your patient, as much as possible, from all stimulants or sources of irritation, both internal and external, the avoidance of which constitutes what is called the *antiphlogistic regimen*, I began to speak of the *remedies* of inflammation.

Now the great remedy in acute and dangerous inflammation is blood-letting: and when this remedy is used at all, it should be used freely, and so as to produce a decided impression: and its efficacy will always be the greater, in proportion as it is applied in the earlier stages of the inflammation. The objects of the abstraction of blood are two-fold: to lessen the force of the heart's action is one object; to empty the gorged capillaries of the part inflamed is the other. We effect the first of these objects, or both of them at once it may be, by making an orifice with a lancet, in the trunk of some convenient vein or artery, and allowing the blood to escape; we accomplish the second by making little incisions with a scarifier through the skin as near the inflamed part as we can, and forcing the blood through these little wounds by the pressure of the atmosphere: *i. e.* we take off the pressure from the part scarified, by placing over it a glass cup, from which the air has been in a great measure exhausted, and then the unbalanced weight of the atmosphere upon the surrounding surface forces out the blood: or we suffer leeches to scarify the skin, and to suck out the blood. These two modes of drawing blood, from the trunks of the blood-vessels on the one hand, and from the capillaries on the other, we call, respectively, general bleeding, and topical bleeding.

I say the main point to be achieved in general bleeding is so to manage the operation as to make a decided impression, as quickly as possible, upon the pulse or the heart; and to do this we place our patient in an upright position, and make a free orifice in the vein of one or both arms.

And when the force of the general circulation has been thus abated, it will in many cases be proper and necessary to take away blood from the capillaries also, in the neighbourhood of the suffering organ: this is almost always safe and good practice: there can seldom be any reason for abstaining from it, except when the general bleeding has brought the patient so low that the abstraction of a few more ounces in any way might be hazardous. But the employment of local depletion presently after general is *then* especially indicated, when the *local* symptoms remain unrelieved; when, although the indirect symptoms which manifest themselves through the medium of the system at large have been moderated by the general blood-letting, yet the direct symptoms belonging to the part, and disturbing its functions, the pain, for example, or the labouring breath, or the stupor, have not undergone a proportional improvement. Under such circumstances, the unloading the oppressed capillaries by means of leeches or cupping will often be attended with the happiest effects.

I mentioned that the most common way of performing general blood-letting in this country is by venesection; and that the veins chosen, as the most suitable for that purpose, are the cephalic and basilic veins at the bend of the arm: but that when, from accidental circumstances, blood cannot be obtained easily and abundantly from those veins, any other large and superficial blood-vessel may be opened. It matters little which, in my opinion, so far as regards the effect of the *abstraction of blood* upon the disease. Some persons are fond of opening the temporal artery when the inflammatory disease is situated in or about the head: and certainly, when we see this vessel starting from the surface like a cord, and tortuous from its fulness, and visibly throbbing, we feel tempted to give vent to the blood which is distending it. But arteriotomy is not so easily managed as phlebotomy. It is sometimes difficult to get the blood to flow properly; and it is sometimes difficult to stop its egress when we wish to do so; and sometimes there are after-consequences which are far from being pleasant: little aneurismal tumors are apt to arise. It is, besides, desirable to avoid the necessity of bandaging the head, in order to restrain the farther efflux of blood from the artery. Other practitioners recommend opening the external jugular vein in head cases, especially in children, whose veins in the arm are small. This is a plan which I have very seldom adopted, and which, I am bound to tell you, I do not much like; 1st, because I think it is seldom necessary; 2dly, because I think it is often unsafe.

It is seldom necessary: for in children we can always get as much blood by topical bleeding as will be equivalent to a general blood-letting. And it is unsafe in two ways: 1st. It is not always an easy matter to stop the bleeding from the jugular vein, especially in a struggling and unmanageable child; and the difference of a few ounces of blood may be a *fatal* difference. Here also any compression of the neck, to stay the hæmorrhage, might affect injuriously the cerebral circulation. 2dly. There is a distinct and peculiar danger attending the incision of this vein, that, namely, of admitting *air* into it. You perhaps are aware that if air enters a large vein near the heart, and passes on to that organ, it kills outright. If you open the jugular vein of a horse, and blow forcibly into it towards the heart, the animal drops down dead. The celebrated Dupuytren was performing some operation about the neck, in the course of which he cut across one of the veins there situate: some bubbles of air rushed in at its open mouth, with an audible clucking noise, and, in an instant, his patient expired. The same frightful accident has occurred in operations performed in this country, and in America. I was told very lately that in one of our metropolitan hospitals it was thought right, for some reason or other, to bleed an adult patient by opening his jugular vein: the opening was made very near the clavicle, so that pressure between the orifice and the heart was very hard to effect. Of course the blood soon leaves the portion of the vein nearest the heart; and whether by some suction power of the heart itself upon the veins, or how, one scarcely knows, but air rushed in, and the patent was presently a dead man. Perhaps misadventures of this kind may be capable of being prevented by using great caution in such cases: but as it is the etiquette for physicians to direct but not to perform these manual services towards the sick, and as, therefore, I should incur all the responsibility, and at the same time be able to insure none of the necessary care, I confess that I am shy of recommending venesection to be made in that particular place.

Whether, all other things being the same, the abstraction of arterial blood be more effectual in restraining inflammation than the abstraction of venous, is more than I can tell you.

When topical bleeding is employed with the view of disburdening the turgid capillaries, either in chronic inflammation, or in acute inflammation as an auxiliary to general bleeding, it would seem most expedient to get as near the part affected as we can. To apply, for example, our cupping-glasses or our leeches to the temples, or behind the ears, or just below the occiput, in inflammatory affections of the head; to the chest and præcordia, when the lungs or heart are the seat of the disease; to the surface of the abdomen, in inflammation of the liver, or stomach, or intestines, and so on. And this is the plan which I have almost always adopted; and with such satisfactory results that I have felt little inclination to try any other. But many persons do believe that local bleeding is more useful when it is performed at some distance from the affected part: they would put leeches,

for instance, on the insteps, to relieve an inflamed throat; and they attribute the benefit that ensues to what is called *revulsion:* they suppose that the suction of the leeches solicits the blood, as it were, to that quarter, and diverts it from the vessels of the part that is inflamed. It seems to me that the revulsive influence of *topical* bleeding would be greater in the neighbourhood of the inflamed part than far from it. I know, however, some very practical men who have been much struck with the results of this distant blood-letting, which they had seen practised in the Parisian hospitals. Leeches are also sometimes applied at a distance from the seat of the inflammation, on another principle—that of drawing the blood directly from the veins which communicate with the diseased part. In abdominal affections, in inflammation of the liver or intestines, the French are in the habit of applying leeches in great numbers to the verge of the anus: because, they say, the blood is then abstracted from the very veins through which it is returning towards the already overloaded organs. It is right that you should be aware of these opinions, and of this practice. I can say but little of it from my own knowledge. I can well believe, however, that it is good and useful practice; but in this country we should find it difficult to persuade many of our patients to submit to have leeches planted round the anus: and I have seldom been disappointed of the benefit I expected from topical bleeding, when it has been employed at the surface as near the part inflamed as possible.

Purgatives.—The *evacuatian* next in importance to blood-letting, is purging. This is an expedient which in cases of violent inflammation, or high general fever, should scarcely ever be omitted. To keep the bowels what is called *open*, forms indeed a part of the antiphlogistic *regimen;* but in acute inflammatory diseases, active purging is of very great service. These two points are gained by it. The stomach and intestines are freed from accumulated fæces, or other matters, which, by their bulk or their acrimony, might prove irritating: and at the same time depletion is carried on by means of the serous discharge which is produced from that large extent of mucous membrane. There are some cases of inflammation in which the operation of purgative medicines is of especial service; as in inflammatory affections of the head, either external or internal, of which part these medicines assist or cause the depletion in a very sensible manner. We have an illustration of this in the paleness of the face, which often, during health, accompanies the action of a brisk cathartic. The usefulness of repeated purgatives is less distinctly seen in inflammations situated within the thorax; although in these cases also they are often highly beneficial. They are efficient remedies too in all inflammatory conditions of the liver. But when inflammation has fastened upon the stomach or bowels themselves, although it may be indispensable that they should be unloaded of their contents, which are often composed of irritating ill-digested food, and of morbid secretions, no less irritating and hurtful, the propriety of going beyond this point is extremely questionable. I believe that much harm is often done by pressing the inflamed alimentary canal with active purgatives. But to all these points I shall have occasion to return.

Mercury.—Next to blood-letting, as a *remedy*, and of vastly superior value upon the whole, in serious inflammations of various kinds, to purgation, is *mercury*. This mineral is really a very powerful agent in controlling inflammation; especially acute, phlegmonous, adhesive inflammation; such as glues parts together, and spoils the texture of organs. It is of the greatest importance that you should accurately inform yourselves concerning the various effects of mercury upon the system: the changes it produces; the changes it arrests or prevents; the cases in which it does good; the cases in which it does harm; that you should learn, in short, how to wield a very potent but a two-edged weapon.

If we inquire what mercury does when it is administered to a person in health, we find three very marked effects following its internal use. They vary, indeed, in different cases, and under different circumstances: but we know that the employment of mercury under any of its usual forms of exhibition is often followed by increased watery evacuations from the intestines; or by an increased discharge of bile; or by an increased flow of saliva; that is to say, it determines (as the phrase is) to certain secreting organs—the mucous membrane of the bowels, the liver, the salivary glands; it augments their natural secretion; and in this augmentation of secretion is implied an increased afflux of

blood to the secreting part. It is probable that mercury has a similar influence on most or all the secreting surfaces of the body, altering the condition of the capillary circulation throughout. And an explanation of its curative power in inflammation has been drawn from this fact: it has been supposed that mercury thus tends to *equalize* the circulation; that by causing the blood to be distributed in larger quantity than common upon *several* surfaces at the same time, it obviates, *pro tanto*, its excessive congestion or accumulation in any one organ. Whether this hypothesis in respect to the *modus operandi* of mercury be true or not, I will not pretend to say; but it certainly is not an unreasonable hypothesis.

If you push this remedy in healthy persons, other effects ensue: inflammation is actually *produced:* the gums become tender, and red, and swollen, and at length they ulcerate; and in extreme cases, and in young children especially, the inflamed parts may perish: the cheeks, for example, sometimes slough internally. Not only the gums, but the throat and fauces, become red, and sore, and sloughy.

Now you will do well to observe what is the *character* of the inflammation thus produced. It is superficial, spreading, erysipelatous: it leads to ulceration without any distinct occurrence of suppuration: the ulcers enlarge. Of the three processes which I formerly pointed out as going on in different degrees, at the same time, in an ulcerated surface, that of absorption is vastly predominant; and you will find that persons in whom this local affection, this condition of the parts within the mouth, has been produced, get rapidly thin: their fat disappears: they become emaciated. That is, the absorption of the old materials throughout the body exceeds the deposit of new matter. Patients who are kept under the influence of mercury grow pale as well as thin: and Dr. Farre, who has paid great attention to the effects of this drug, remedial and injurious, holds that it rapidly destroys red blood: as effectually as it may be destroyed by venesection. As an example of this he was in the habit of relating in his lectures the case of a lady who was attacked with hæmatemesis: her gastric system and her liver were gorged with blood. "Her complexion," said the Doctor, "was composed of the rose and the violet. Under a course of mercury she was blanched, in six weeks, as white as a lily."

There are still other, occasional, effects of the continued introduction of mercury into the system: a peculiar eruptive disease; a peculiar condition of the nervous system: but with these I do not now meddle; they will come under our more particular consideration hereafter. At present I am desirous to place such facts before you as may help you to determine in what cases mercury is a fit remedy for inflammation: in what cases it would be improper to give it. The facts I have already mentioned show that it has a loosening effect upon certain textures; that it works by pulling down parts of the building.

But the great *remedial* property of mercury is that of stopping, controlling, or altogether preventing the effusion of coagulable lymph; of *bridling adhesive inflammation:* and if we, in our turn, could always bridle and limit the influence of mercury itself, it would be a still more valuable resource.

From the little I have now said you will readily understand in what description of cases mercury is likely to be useful. In common adhesive inflammation, whether of the serous or the cellular tissues; whenever, in fact, you have reason to suppose that coagulable lymph is effused, or about to be effused, and mischief is likely to result from its presence, then you may expect much benefit from the proper administration of mercury; as an auxiliary, however, to blood-letting, not as a substitute for it.

On the other hand, mercury is likely to be hurtful in those forms of disease "where the morbid action approximates to its own action." In cases of erysipelatous inflammation having a disposition to gangrene; in scrofulous diseases; in inflammatory complaints attended with general debility, and an irritable condition of the nervous system, or a manifest tendency to take on a typhoid character.

When we have to contend with acute inflammation, and desire to prevent or arrest the deposition of coagulable lymph, our object is, after such bleeding as may have been proper, to bring the system as speedily as possible under the specific influence of mercury. How may this best be done? and how are we to know that it has been achieved?

I will answer the last of these questions first. We know that the whole system has

been brought under the specific influence of mercury, as soon as its effects become even slightly perceptible in the gums and breath of the patient; and in adults we cannot be sure of it before. The gums grow red and spongy; the patient complains that his gums are sore; and that he has a metallic taste, a taste like that of copper in his mouth: and an unpleasant and very peculiar fœtor, easily recognized again when it has been once perceived, is smelt in his breath. These symptoms are enough: you need not in general look for any more decided affection of the mouth, such as ulceration of the gums, swelling of the glands beneath the jaw, and of the tongue, and a profuse flow of saliva. Formerly, when it was believed that the material cause of the disease was carried out of the body with the saliva, the mercurial treatment was continued with the view of producing the discharge of many ounces, and even of a pint or two, in the twenty-four hours: but all that is requisite is that the gums should become distinctly tender, and that the mercurial fœtor should be unequivocally manifest, and that these symptoms should be kept up for a certain time.

Now this is best effected, usually, by giving some form of mercury in equal and repeated doses, by the mouth. For urgent cases calomel is the best form in which it can be administered: two or three grains, given every four or six hours, will generally suffice to touch the gums in the course of thirty-six or forty-eight hours. If it acts as a purgative its specific effect upon the whole system will be postponed by that circumstance; and it then becomes expedient to combine it with just so much opium as will prevent its passing off by the bowels. A quarter of a grain of opium with two grains of calomel—or a third of a grain of opium with three or four grains of calomel—will generally be sufficient to restrain the purgative operation of the latter. When a speedier effect is desirable we give larger doses; such as five or ten grains every three, or even every two hours: or we combine mercurial inunction with the exhibition of calomel by the mouth. It is impossible to lay down any precise rule that will fit all cases.

Blue pill, or the hydrargyrum cum cretâ, may, in certain cases, be preferable to calomel; but they must be given in larger doses. Some practitioners believe that a combination of blue pill and calomel acts sooner, and answers better, than a proportional dose of either, given alone.

This mode of giving mercury, so as to affect the system at large, is eminently useful in many instances of acute phlegmonous inflammation, after bleeding has been carried as far as the circumstances of the case will warrant. I repeat that it must not be allowed to supersede blood-letting. Previous bleeding renders the body more readily susceptible of the influence of mercury; and the operation of the mercury comes in aid of the salutary effect of the abstraction of blood. The two remedies accomplish by their joint power what neither of them could accomplish singly.

It is important to know that different persons admit of, or resist, the specific agency of mercury in very different degrees: so that in some patients the remedy becomes unmanageable and hazardous; while in others it is inert and useless. It is most grievously disappointing to watch a patient labouring under inflammation which is likely to spoil some important organ upon which it has seized, and to find, after bleeding has been pushed as far as we dare push it, that no impression is made upon his gums by the freest use of mercury. Such cases are not uncommon; and unfortunately they seem most apt to occur when the controlling agency of mercury is most urgently required. On the other hand, there are persons in whom very small quantities of mercury act as a violent poison; a single dose producing the severest salivation, and bringing the patient's existence into jeopardy. This history was told to Dr. Farre by a medical man, under whose notice it fell. A lady, whom he attended, said to him, at his first professional visit to her, "now, without asking why, or speculating about it, never give me mercury, for it poisons me." Some time afterwards she met with the late Mr. Chevalier, and spoke to him about her complaints; and he prescribed for her, as a purgative, *once*, two grains of calomel, with some cathartic extract. She took the dose; and the next morning showed the prescription to her ordinary attendant. "Why (said he) you have done the very thing you were so anxious to avoid; you have taken mercury." She replied, "I thought as much, from the sensations I have in my mouth." Furious salivation came on in a few hours; and she died, at the end

of two years, worn out by the effects of the mercury, and having lost portions of the jaw-bone by sloughing.

Another medical man informed me that he knew a person so susceptible of the influence of mercury, that when his wife had rubbed a very small quantity of white precipitate ointment upon her neck for some cutaneous affection, after sleeping with her his gums were tender for three or four days, and slight salivation took place. This did not happen once only, but three several times. On one occasion this same man took two blue pills, as preliminary to a common purge, and he was salivated profusely for six weeks. Cases similar to these occur now and then to most medical men; we cannot tell beforehand in whom such effects are to be looked for; but it is never prudent to neglect any warning which the patient gives of his own previous experience on this point. You will generally find that where the affection of the gums and salivary organs goes on to a troublesome or distressing extent, it has supervened upon the employment of a very moderate quantity of mercury.

So distressing are these effects of mercury upon the mouth sometimes, that I may pause a moment to tell you what I know about the mode of remedying them. You will constantly be called upon to do something for the relief of this disease (for so we must call it), which you yourselves, or some of your brethren, have with the best intentions inflicted. I have tried all sorts of expedients, and I have asked a great number of my friends what are the best means to adopt in such cases: but I never could get much satisfactory information from them. Some thought purging was the best thing. Others recommended alum gargles; or gargles made of the chloride of soda; and these last certainly have one good effect, that of correcting the fœtor. Others believed that sulphur, which has long been prescribed in such emergencies, was really of use: and some advised that the patient should be as much as possible in the open air: a few commended iodine. All admitted that they knew of no certain remedy. Neither do I. But there are two expedients which I am confident are often of very great use in checking the violence of the salivation, and in removing the most distressing of its accompaniments. If there be much external swelling, treat the case as being, what it really is, a case of *local inflammation:* apply eight or ten leeches beneath the edges of the jaw-bones, and wrap a soft poultice round the neck, into which the orifices made by the leeches may bleed; and I can promise you that, in nine cases out of ten, you will receive the thanks of your patient for the great comfort this measure has afforded him. When the flow of saliva, and the soreness of the gums, form the chief part of the grievance, I have found nothing *so* generally useful as a gargle made of brandy and water; in the proportion of one part of brandy to four or five of water. This last piece of practice I learned from the present apothecary to the Middlesex Hospital; I have tried it over and over again; and I tell it to you as a thing worth remembering. These little points are by no means to be despised. A very fashionable and successful physician, now dead, used sometimes to say when he met others of his brethren in consultation, "It is all very well to speculate about the exact situation, and the precise nature of the disorder, but the question with me is, 'what is good for this, that, or t'other thing?'" A wise physician will seek to combine with an accurate knowledge of disease, and settled principles of treatment, those practical expedients and minor appliances which are picked up by casual experience; which could never have been reasoned out; and which sometimes constitute nearly all that we can do for the benefit of our patient.

But to return to mercury as a remedy against inflammation. It is of great service in many cases of *chronic* inflammation; and I may repeat here the observation I formerly made when speaking of blood-letting—that the treatment must *keep pace*, as it were, with the disease. When textures have been slowly altered by a gradual deposition of coagulable lymph, we should gain but little by suddenly or speedily salivating our patient. The lymph, if it can be dispersed at all, must be *gradually* taken up again: and mercury, given with the view of promoting its absorption, must be slowly and gradually introduced into the system; and when at length its specific influence is felt, it must be sustained for a considerable length of time.

You must not expect any good, but the contrary, from the exhibition of mercury in scrofulous inflammations; and where the scrofulous diathesis is well marked, you should be cau-

tious in giving mercury at any time. But I am certain that many men are *too* scrupulous on that head; and that, through over tenderness of your patient's constitution, you may risk his life, by withholding mercury because he shows tokens of scrofula. You may recollect my stating that scrofulous persons are not exempt from attacks of *common* inflammation; and in some such cases the probable aggravation of their general ill health is not to be put in competition with the immediate danger from the local inflammation. I have again and again seen scrofulous patients benefited by moderate salivation; which, if it proved injurious at all to their general condition, was certainly less injurious than the unchecked local complaint would have been.

Antimony.—There are some other remedies for acute inflammation, which in this general account of its treatment, I must briefly notice. Antimony is one of them; and a very valuable remedy it is in some forms of inflammatory disease. Antimony, properly administered, subdues the action of the heart and arteries, producing nausea, paleness, and sinking of the pulse, and frequently great relief to the local symptoms. You bring the circulation into that state into which it may be brought by free blood-letting. But when the violence of the inflammatory symptoms recurs again and again, you cannot again and again employ the lancet: and if you do so employ it as at length to extinguish the inflammation, you reduce your patient to a state of pitiable, and even perilous debility. Now you may continue or repeat the depression of the circulation by means of antimony, without any dread of such subsequent weakness. Antimony, as far as my own observation goes, is admirably suited to cases of active inflammation, in which mercury would either be not so useful, or could not be brought to bear. It is in inflammation of the mucous membrane of the air passages that antimony is so signally beneficial. You will see a patient labouring for breath, unable perhaps to lie down, with a turgid and livid countenance from imperfect arterialization of the blood. He has been ill but a short time; it is an acute affection; and uponl istening at his chest, you hear that peculiar wheezing sound which we call *sibilus*, in every part of his lungs. I shall have to describe this sound, and its causes, and its meaning, in a future part of the course. You give such a patient repeated doses of antimony; he becomes sick, vomits perhaps, but he feels nausea; his pulse becomes less forcible, his face grows pale, and he can breathe again. The nausea is not a pleasant sensation; but the want of breath is a far more distressing one; and that is greatly mitigated. Perhaps a free secretion takes place from the congested membrane, and then the patient is easy and safe. Now you could not effect this change so quickly and readily, or so conveniently, by mercury, and perhaps not at all. Bronchitic affections are very common in children, in whom it is usually difficult to induce the specific influence of mercury.

On the other hand, antimony does not appear to be nearly so valuable a remedy as mercury, when serous membranes are inflamed.

The French and Italian physicians place much reliance upon antimony for the cure of inflammation; and they seem to know little or nothing of the remarkable agency of mercury upon that disease. For my own part I do not see how any useful *comparison* can be made between these two substances in respect to inflammation, considered generally, as we are now considering it. There are some particular forms of inflammation to which the one remedy is better suited, and there are others in which the other is most effectual. I must content myself with having adverted to these distinctions for the present.

As to the form in which the antimony should be exhibited, I apprehend that we shall all come at last to freshly dissolved tartar emetic. The antimonial powder is of very uncertain strength; and the antimonial wine contains too much spirit to allow of its being given in large and frequently repeated doses. It is a curious circumstance, that although vomiting and purging are apt to be produced by the first two or three doses, they usually cease when the same quantity is persevered with. *Tolerance* of the remedy is produced. But although these unpleasant primary effects cease, the curative agency of the antimony appears to continue. When you desire to obtain its full influence in a short time, you may dissolve a grain of the tartar emetic in two ounces of hot water; and give a fourth-part of the solution every half hour. If the patient becomes pale and sick, you pause a while, and allow him to recover himself; and if the inflammatory symptoms return, you repeat the

medicine. It sometimes acts violently upon the bowels, and then it will be necessary to add a few drops of laudanum to each dose.

Digitalis.—Digitalis is another powerful medicine, from which much was at one time hoped as a remedy for active inflammation; but those hopes have been in a great measure disappointed. It is not a manageable remedy in such cases. Its singular property of retarding the circulation, of bringing down the number of the heart's pulsations, and abating its force, led to the expectation that it might render the use of the lancet unnecessary; that it might check the inflammatory process without permanently reducing the strength of the patient. But if you give moderate doses of digitalis, its peculiar effect upon the pulse comes on at very uncertain periods, and may be postponed until it is too late to be of any service. If, on the other hand, you give it in such quantity as speedily to affect the heart's action (which is what we want in acute and serious inflammation), then you are never secure against what may be called its poisonous effects: deadly faintness, frightful syncope, and even death itself. Most practitioners can tell of cases in which patients, who were taking full doses of digitalis, have suddenly expired; and when the remedy has appeared to have had more to do with the fatal event than the disease. There are practitioners, however, and I know one of them, who affirm that digitalis may be given, after due depletion, and in acute inflammation, in very large, and I should say startling doses, with the very best effects—doses which range from half a drachm to half an ounce, and even six drachms of the officinal tincture. I confess to you that I should be very unwilling to sanction this mode of using digitalis. I never attempt to employ it with the view of knocking down acute inflammations—to which alone you will observe that my present remarks apply. Digitalis is often of great service in other complaints; but I am not at present discussing the remedial virtues of digitalis, or of any other drug, except so far as they relate to the cure of recent and active inflammation.

Colchicum.—Colchicum is a remedy which is often prescribed in inflammation. It is a most valuable remedy in certain *specific* forms of inflammation. But for repressing common phlegmonous inflammation we have much more certain and better remedies. For this purpose colchicum is, I believe, a very unimportant medicine.

Opium.—I have formerly been asked, by students attending here—and therefore I anticipate the question now—respecting the utility of *opium* as a remedy in inflammation. Certainly opium, like most of our powerful medicines, may do much good, as it may do much harm, in different inflammatory diseases; and it is not very easy to point out clearly, in a general view of the treatment of inflammation, the rules for its administration by which we must be guided in different cases. Yet there are a few general observations which I may make now on this subject.

The administration of a full dose of opium has been strongly recommended after that free and effective bleeding which I have already described. It prevents the rekindling of the inflammation which is apt to result from irritation of the nervous system—a kind of irritation, you will remark, which the copious abstraction of blood is calculated to produce; or to augment, if it finds it already existing. The opium soothes this nervous irritability; and it must be given, when given at all, in doses that will have that effect. It is best adapted to those cases in which a natural irritability is inherent in the constitution of the patient—to those in which such irritability has been acquired by bad habits of life—and to those in which the local disease is attended with much *pain*, which is in all constitutions a source of irritation.

However, this is a remedy which requires to be used, in inflammation, with great caution and discrimination. In cases of active inflammation within the cranium, its propriety is very questionable. It is apt to confuse both the patient and his physician, who is unable to say, after a full dose of opium has been given, how much of the stupor that follows is owing to the disease, and how much to the drug. It is a very ticklish remedy in pectoral inflammations. I believe that by the free use of opium I saved the life of a relation of my own, an old lady, who was in danger of being worn out by the cough and bronchial affection which attended the influenza. On the other hand I have certainly known more than one person, labouring under extensive and severe bronchitis, so effectually quieted by a dose of the same medicine that they never woke again. As a general rule I should say

that you should be very careful how you venture upon opium in inflammatory diseases that tend to produce death by *coma*, or *apnœa*. If there be any unnatural duskiness of the face, if ever so slight a tinge of purple mingles itself with the red colour of the lips, this is an appearance which should warn you against opium. It shows that the blood is imperfectly arterialized; and imperfect arterialization of the blood, as I hope you all know now, either results from, or conduces to, a state of coma.

On the other hand, it is, *cæteris paribus*, in cases where the tendency is towards death by *asthenia*, that the use of opium, as a remedy for inflammation, is most serviceable. It has a capital effect often, after free bleeding, in cases of peritonitis, and of enteritis. It probably does good in various ways: by quieting the nerves—by sustaining the faltering action of the heart—by keeping the inflamed parts at rest. There are some frightful accidents in which we can expect little from blood-letting, but in which the judicious employment of opium affords some glimmering of hope. I allude to those cases of intense and general peritonitis which arise upon the escape of irritating substances into the cavity of the belly; the contents of the intestines, from ulceration, or from external injury; urine from rupture of the bladder; and so on. If there be any hope for such cases, it is to be found in the continued exhibition of opium in considerable doses. But upon all these points I shall go more into detail when we come to consider individual diseases.

External remedies.—A very few remarks, in respect to external remedies, in cases of inflammation, will terminate both this lecture and what I have to say, thus generaliy, of the treatment of inflammatory complaints.

Cold.—The application of external *cold* will aid us very powerfully, in certain serious cases of inflammation; and especially in cases of inflammation within the cranium. It is really wonderful what a sedative and soothing effect this expedient frequently has in allaying delirium, the result of active inflammation of the brain and its membranes. Thin folds of linen, kept constantly moist and cold, by cold water, are placed upon and around the shaven head. We often apply ice in the same way. But I need not go at present into any detail on this subject: I will only observe, that we have a most excellent and simple guide as to the probable usefulness of cold applications to the head, in the *sensations* of our patients. It is very lucky that it is so. As long as the cold cloths, or the bags of ice, are pleasant and grateful to the patient, so long we sedulously continue to apply and renew them: as soon as the patient dislikes them, they had better be intermitted.

Cold applications to the *chest*, and to the *belly*, in active inflammation of parts situated within those cavities, have been praised by some practitioners; but I believe are very seldom employed. I have no personal experience either of their utility, or of their hurtfulness. I confess that I should not like to use them. I should think that the effect of the cold, in driving the blood from the cutaneous vessels, and accumulating it in internal parts, would be likely to be injurious. We shall see, by and by, that the contents of the skull are differently situated, in this respect, from those of the thorax and abdomen.

Warmth.—The totally opposite measure, applying *warmth* to the surface, is of very great service in many cases of internal inflammation: especially in inflammations of the abdominal organs. We speak of cold *lotions*, and of hot *fomentations*. These last are managed in various ways, which I do not at present enter into. They seem to do good by determining to the surface; they promote perspiration; they mitigate pain, and persuade to sleep.

In cases of *external* inflammation, sometimes cold and sometimes warm applications are found to be of use. In this matter also the sensations of the patient afford the best criterion. Both of them tend, in different circumstances, to promote resolution. We have an illustration of the beneficial agency of cold applications for this purpose in the treatment of recent burns and scalds, particularly when the injury is superficial, and the skin has not been destroyed. Probably there is scarcely any one present who has not experienced the relief given to the *pain* of a burned finger, by dipping it in cold water; and the return of the pain upon taking the finger out again. The cold may be so constantly applied that the pain will cease to recur when the application is at length suspended. Dr. John Thomson relates a case in which a burned arm was kept immersed in cold water for

two days and two nights incessantly; and the inflammation was thereby wholly prevented. I have known this expedient fail, however. A nurse in the Middlesex Hospital fell as she was carrying a pail of hot water upstairs, and in her fall thrust one of her arms into the scalding liquid. Without loss of time she plunged the same arm into cold water; but after a while was obliged to desist; the cold immersion bringing on severe rigors.

In erysipelas, I am persuaded that warm fomentations not only afford more comfort, but are more effectual and safer than cold lotions.

Independently of their occasional influence in promoting *resolution*, warm applications, warm soft poultices for instance, are often used with the view of forwarding *suppuration*. Hence this rule. Whenever resolution of the inflammation is *possible*, but suppuration is *likely* to ensue, warm applications are the most proper: because under their use we have an equal chance of obtaining resolution, with less hazard of retarding, or rendering untoward, the process of suppuration, in case resolution does not take place.

Counter-irritation.—*Counter-irritation*, by means of blisters, sinapisms, irritating ointments, setons, issues, or moxas, is often very beneficial. It probably operates by attracting blood into the neighbouring parts, and in the same degree diverting it from the inflamed part. It is most serviceable in chronic inflammations, and towards the decline of those which are acute. It is particularly adapted to scrofulous affections. There is an objection to the use of counter-irritation during the height of the inflammatory fever, on account of the increase of general irritation which it would then occasion. Neither in local inflammation should counter-irritation be applied *very* near to the inflamed part. Blisters are not proper, therefore, at least in the early stages of the disease, in acute inflammation within the cranium; but they are sometimes applied in such cases, with advantage, to the lower extremities. Blisters to the chest, however, in thoracic inflammation, and to the belly in abdominal, are often not only perfectly safe, but of the greatest use, as will, I trust, be apparent, as we go on.

LECTURE XV.

HÆMORRHAGE:—MOST COMMONLY BY EXHALATION. HABITUAL HÆMORRHAGES. VICARIOUS HÆMORRHAGES. IDIOPATHIC HÆMORRHAGES, ACTIVE AND PASSIVE. SYMPTOMATIC HÆMORRHAGES. USUAL SITUATIONS OF HÆMORRHAGE. SYMPTOMS AND DIAGNOSIS. PRINCIPLES OF TREATMENT.

IN the course of that somewhat cursory account which I have been endeavouring to give you of the general facts and doctrines of pathology, as a preparation for the better understanding of special forms of disease, we reached, some lectures back, the subject of local plethora, or congestion. From that point our road branched off in three several directions. We have pursued the first and main branch to its termination; that which led to the discussion of inflammation. We must now go back to the same point again; and follow first the one and then the other of the two remaining branches, which conduct respectively to the consideration of *hæmorrhage* and of *dropsy*. These branches are shorter than that along which we were last travelling; but they are not uninviting: they will open to us, if I mistake not, some interesting views of the country of which we propose, in the end, to make a more particular survey.

You are to observe that I treat of *hæmorrhage*, only so far as it falls to the care of the *physician*. The subject is exceedingly full of interest in its relations to surgery: and it will receive at the hands of my colleague all the attention which its great importance, as a surgical accident, demands.

But *we* also, as physicians, have much to do with hæmorrhage; with what, for distinc-

tion's sake, I may call *medical* hæmorrhage; which differs in kind, in cause, in its consequences, and in the treatment it requires, from that which surgery contemplates.

Hæmorrhage by exhalation.—In surgical or traumatic hæmorrhage the blood flows from some considerable vessel, which has been cut, or torn, or somehow ruptured. You would greatly mistake if you inferred from that circumstance (as you naturally might), that it is *usually* so—the only difference being in the situation of the vessel—in medical hæmorrhage also.

Yet that is the popular notion. When blood rushes out from internal parts, through any of the natural apertures of the body, the person is said and supposed to have *broken a blood-vessel.* Yet this is rarely, though it is sometimes, the case. In nine instances out of ten, if there be any rupture at all, it is rupture of numerous capillaries only: but even of this there is often no evidence.

Whence then, and how, does the blood escape from its natural channels? Why, it exudes from the unbroken surfaces of organs, without any appreciable lesion of arteries, veins, or capillaries; just in the same manner as sweat oozes from the skin, mucus from the inner surface of the bowels, and serum or synovia from the membranes that respectively furnish those fluids; and probably by the very same outlets.

This certainly is a very remarkable circumstance, if it be true: and you will naturally ask what proof we have of its truth.

The proof is simple and conclusive. We examine the surface from which the blood must have proceeded, and we find it entire: we wash and even macerate it; we employ the microscope to assist our powers of vision; yet we fail, after this careful inspection, to discover the slightest breach of substance, or any appearance of erosion.

When, for example, hæmorrhage has occurred so profusely from the stomach or bowels that the death which ensued could be sufficiently accounted for by the mere loss of blood, the whole tract of the alimentary canal has been diligently scrutinized, and has exhibited no ruptured blood-vessel, no abrasion even of its surface, nor any perceptible alteration of texture. Sometimes its mucous membrane appears, here and there, of a red colour, and, as it were, charged with blood. Sometimes it is pale and transparent, while the vascular net-work visible immediately beneath it is gorged and turgid. Sometimes the whole is colourless, the same net-work of vessels having been completely emptied by the previous hæmorrhage: and sometimes, again, (and this is very illustrative of the mode by which the blood has issued,) vast numbers of small dark-coloured masses, like grains of fine sand, can be made to start from the surface of the membrane by slight pressure. There can be no doubt that these are minute portions of blood, which had remained and coagulated in the vessels or appertures forming the ultimate channels of the hæmorrhage.

We have absolute proof, therefore, that hæmorrhage may transude through an uninjured surface: nay, in some rare cases, the process has been actually witnessed. There are well-authenticated instances on record of *cutaneous* hæmorrhage; where a dew of blood has appeared upon some portion of the skin, has been wiped away, and has reappeared; and that again and again, without any perceptible alteration of the affected surface, beyond some occasional variation in its colour. So again the menstrual discharge has been seen to issue *guttatim* from the healthy surface of a living and inverted uterus. But I confess, that although this analogical fact helps our conception of the manner in which blood may be exhaled from an unbroken membrane, I should not lay much stress upon it for any other purpose. It is not exactly a case in point. The process of menstruation cannot be looked upon as a morbid process. During a certain portion of the life of an unpregnant female, it is not only consistent with perfect health, but even essential to it, and the fluid poured out is not strictly blood.

That the blood proceeds from the same vessels or apertures which, in health, pour out the fluids natural to the part, is rendered the more probable by this fact:—that certain hæmorrhages are ushered in and succeeded by an increased efflux of the fluids which belong to the surface concerned. In hæmorrhages from the mucous membranes the following succession of events is, in some persons, habitual. First, there is an augmented flow of mucus alone; then of mucus tinged with blood; then of pure blood: and the hæmorrhage

recedes by a similar, but inverse gradation, towards a mucous drain, which itself at length decreases or disappears.

When blood thus exudes, we say that the hæmorrhage takes place by *exhalation*. It is a convenient word, and will spare circumlocution. What the vessels or outlets to which we give the name of exhalants really are; whether they be branches from the capillaries not large enough in the natural state to admit the red particles, or whether they be mere pores in the sides of the capillaries; these are points concerning which we have no positive knowledge. We know, indeed, that such channels must exist, though we cannot demonstrate or see them; and we know that while every part of the body is in a state of health and integrity, they do not allow the blood, as such, to pass through them.

Now, although internal hæmorrhage may happen in other ways; as from the bursting of an aneurism, or from an opening made in a large vessel by progressive ulceration; yet in by far the greater number of cases it takes place by exhalation. Exhalation is the rule—other modes of hæmorrhage furnish the occasional exception.

I must exclude, however, from this general statement one very important hæmorrhage. In the brain, the former exception becomes the rule. In almost all cases cerebral hæmorrhage results from the rupture of a blood-vessel.

There are various kinds of hæmorrhage by exhalation. I will bring them before you, in succession, as clearly and concisely as I can.

Habitual hæmorrhages.—In the first place there are hæmorrhages which, although they do not belong to the state of health, if we take mankind in general, yet when they do occur, cannot properly be called diseases. There are some persons—I believe I may say there are many persons—who are subject, during the greater part of their lives, to discharges of blood; which happen again and again, commonly at regular intervals, without any perceptible detriment to the general health, independently of any obvious exciting cause, and (as it would seem) from some inherent property or necessity of the system.

Hæmorrhages thus occurring, I will call *habitual hæmorrhages*. They proceed more commonly from the rectum, or from the nares, than from any other part; although instances are recorded of their taking place from the bladder, and from the bronchi. Appertaining to the original constitution of the body, this disposition to periodic hæmorrhage has been sometimes observed to be hereditary.

You will at once be struck with the analogy which obtains between these habitual hæmorrhages occurring in either sex, and the monthly discharge which is peculiar to the female. The analogy is even closer than it may at first sight appear: but it is more distinctly marked in some individuals, liable to habitual hæmorrhage, than in others. It was one of the singular notions of the celebrated phrenologist M. Gall, founded upon this analogy, that there is such a thing as male menstruation. The points of resemblance between the two phenomena will be manifest in the following summary of the characters belonging to habitual hæmorrhage.

Like the catamenia, these hæmorrhages do not ordinarily prevail throughout the whole course of life. In most cases they do not commence before the period of adolescence; and they cease altogether, or recur at distant intervals only, in declining age. Their first eruption is sometimes preceded by a state of general indisposition, more rarely by slight febrile disturbance, and even (according to some observers) by a sort of chlorosis similar to that which affects young girls in whom the menstrual evacuation is delayed or suspended. The hæmorrhage sometimes recurs at precisely regular intervals, and by *monthly* periods more commonly than any other: being announced, on each occasion, by the same preludes, proceeding from the same part, continuing for the same space of time, and furnishing always about the same quantity of blood. Its accidental interruption is almost uniformly the cause or the consequence of some derangement of the health: and when it becomes excessive, it becomes, like too profuse menstruation, a disease.

Vicarious hæmorrhages.—It forms a very curious part of the general history of hæmorrhages that they are not unfrequently *vicarious*, or supplemental, sometimes of each other, but more often of the monthly discharge from the uterus. Females are liable to perverted menstruation (so to call it), through other channels than the natural one: and here again

the analogy between the catamenia and habitual hæmorrhage comes into view. The hæmorrhages which belong to the constitution are apt to wander in their seat. As bleedings from the lungs, stomach, rectum, or skin, sometimes follow upon the suspension of the menses, so bleedings from the bladder, from the mouth, and from other parts, has been occasionally observed to succeed the suppression of habitual hæmorrhoïs.

These hæmorrhagic deviations take place commonly by the same organ on each occasion; more seldom by different organs in succession. It is almost always in this supplementary manner that the rarer forms of hæmorrhage occur, and those of the skin in particular.

This singular migration, this interchange of place between certain hæmorrhages seems calculated to throw some light upon the obscure doctrine of *revulsion;* a doctrine to which I have already more than once referred, and which, though it is very imperfectly understood, is of frequent avail in the practice of physic.

Vicarious hæmorrhage always denotes a disordered state of the general health: and must be considered, in itself, as a malady.

Idiopathic hæmorrhages.—Again, there are certain forms of hæmorrhage, not habitual, which may be denominated *idiopathic;* inasmuch as they are apt to arise without any perceptible connection with antecedent local disease.

In other respects, however, they differ considerably, and require to be farther distinguished: and the terms *active* and *passive*, which are in common use, will sufficiently express the two forms of idiopathic hæmorrhage that I wish to bring under your notice.

Active hæmorrhage is preceded by active congestion, and therefore is akin to inflammation; and it often requires the treatment of inflammation.

Passive hæmorrhage often occurs without any apparent previous congestion of any kind. Hæmorrhage of this passive character has been ascribed to some change—different from that which we conceive to be produced by the distension of plethora—in the vessels or apertures through which the healthy exhalations are transmitted. The change is considered as being of the nature of morbid debility or relaxation. That such a state may sometimes exist is not impossible, nor even unlikely: but as we are altogether ignorant of the natural condition of these outlets, it is difficult to reason about the alterations to which they may be subject in disease. This hypothesis derives its chief support from the occasional efficacy of *astringent* substances (either applied locally, or taken into the system) in checking the effusion of blood; when other remedies have failed.

A more probable hypothesis perhaps is that which supposes some alteration in the consistence or composition of the blood itself; which thus becomes attenuated, and capable of passing through channels or orifices that healthy blood, under ordinary circumstances, cannot penetrate. In support of this supposition are adduced the facts that hæmorrhages are known to occur where the blood is more thin, pale, and serous than common: and still more remarkably where that fluid has undergone a demonstrable change in its chemical nature, or is even visibly altered in its sensible qualities; as, for example, in certain cases of purpura and sea-scurvy. And hæmorrhages of this kind are often cured by measures calculated to repair the blood; to restore it to its natural condition by improvements in diet, or by food of a peculiar kind, as the juice of lemons.

Whatever may be the true explanation of the differences in question, there can be no doubt that they exist, and are often strongly pronounced, in cases of hæmorrhage, which, inasmuch as they cannot be traced to any pre-existent local disease, we class together as *idiopathic*. And it will be worth while to run over the distinctive characters of active and passive hæmorrhage, as they are broadly and decidedly visible, in well-marked cases.

Active Hæmorrhages.—*Active* hæmorrhage (which is preceded, I repeat, by active congestion) occurs principally in persons who are young and robust, who live fully, and lead indolent lives, and are subject to the influence of those causes which tend to general plethora. Occasionally the hæmorrhage can be traced to some exciting cause, such as exposure to heat, strong mental emotion, violent exercise, or bodily efforts. More frequently, perhaps, no exciting cause is apparent. It is sometimes ushered in by a set of symptoms expressive of what has been called the *molimen hæmorrhagicum*. The patient expe-

riences a general feeling of indisposition, with wandering and obscure pains that gradually settle in the part from which the blood is about to be discharged. A series of local symptoms, such as a sensation of weight, or of tension, or of heat and tingling, sometimes a slight degree of turgescence and redness, and a visible fulness of the larger veins, indicate the afflux of blood towards the labouring organ, and the parts in its vicinity: while chilliness, paleness, and shrinking of distant parts, and especially of the feet and hands, denote an opposite state of the circulation in *them*. And to this state of things there often succeeds a general increase of heat, with a frequent, full, and bounding *pulse*,—a pulse which is so characteristic sometimes, as to have acquired a name: you may often hear or read of a *hæmorrhagic* pulse. The blood, when at length it breaks forth, commonly escapes with rapidity; is of a florid colour; proceeds from a single organ; and readily coagulates, though it seldom separates distinctly into serum and crassamentum. While it is flowing, the signs of local congestion diminish and disappear; warmth returns to the extremities, and the pulse regains its natural strength and frequency. The patient becomes conscious of a sensible relief; and feels stronger and more lively than before. This kind of hæmorrhage is, in some sort, its own remedy; it ceases in virtue of the discharge of a certain quantity of blood, and it is followed by morbid consequences only when that quantity has been excessive; or when it inflicts some mechanical injury upon the parts along which the blood passes.

I said that active hæmorrhage is preceded by active congestion, and is consequently akin to inflammation. Perhaps it may be more true that in some of these cases we actually have the initial stage of inflammation, of which the hæmorrhage proves the natural cure: strangling it in its birth; applying, in the very moment when it is most effective, that remedy which, in the last lecture, I told you was the most potent of all the remedies of inflammation; namely, *loss of blood*.

Passive hæmorrhages.—*Passive* hæmorrhage on the other hand is characterized by circumstances of an exactly contrary nature. It occurs in those who are naturally feeble, or who have been debilitated by disease, fatigue, insufficient nourishment, great evacuations, or the depressing passions. It is not, in general, announced by any precursory symptoms, nor attended by any reaction. The effused blood is of a dark colour, serous, and but little disposed to coagulate: and it often is poured forth from several parts of the body at the same time. If the quantity lost be at all considerable, the natural debility of the patient is rapidly augmented: his face becomes pale, and his body loses its heat. The hæmorrhage leaves him in a worse condition than that in which it found him. The flow of a certain quantity of blood is not, as in the cases of *active* hæmorrhage, suspensive of its further effusion; frequently, indeed, passive hæmorrhage resists the means opposed to it the more, in proportion as it has continued longer, or been more profuse.

Symptomatic hæmorrhages.—Hæmorrhages of the kind I have now been describing—that is to say, depending upon no palpable disease of any organ, and, therefore, idiopathic—are of no uncommon occurrence, whether we regard the active or the passive form in which they appear: but by far the greater number of hæmorrhages by exhalation are *symptomatic;* that is, they result from some previous disease, either in the organ from which the blood proceeds, or some other organ connected therewith by some community or dependence of function.

These secondary or symptomatic hæmorrhages are preceded by congestion, but for the most part the congestion is not of the active, but of the mechanical kind; and has more to do with the veins of the part than with the arteries.

Thus we have hæmorrhage from the bronchial membrane, in consequence of crude tubercular matter in the lungs, filling up a portion of the pulmonary tissue, and obstructing the circulation of the blood through it. This is an example of symptomatic hæmorrhage by exhalation, depending upon previous disease in the organ itself from which the blood proceeds.

In some of these cases the presence of pyrexia renders it probable that the hæmorrhage is the consequence and the relief of active congestion, provoked by the irritation of tubercles; rather than the result of a mechanical obstruction of the circulation.

Again, we have hæmorrhage into and from the lungs, as a consequence of such disease

of the heart as mechanically impedes the return of the blood from the lungs to that organ: a narrowing of the mitral orifice, for instance. Here the blood is barred up, as it were, in the lungs, till at length the capillaries, incapable of farther distension, either give way; or become so dilated as to allow of the exit of the blood through their exhalant openings, or through inorganic pores in their sides. In precisely the same way blood is poured out by the mucous membrane of the stomach and bowels, in consequence of disease in the liver, obstructing the portal circulation. These are examples of symptomatic hæmorrhage by exhalation, depending upon previous disease, not of the organ itself from which the blood proceeds, but of another organ intimately connected with the former.

When I say that hæmorrhage into and from the lungs may result from such a disease of the heart as implies an impediment to the circulation, you must not suppose that the lungs are the only channel through which the mechanical congestion can be relieved. Disease of the central moving organ of the circulation leads often, at length, to *universal* venous congestion: and the hæmorrhage, which is apt to be the consequence of such congestion, may burst forth from any part where the veins are so overloaded. Hæmorrhage from various portions of the mucous membranes are in truth very common effects of cardiac disease.

The influence of mechanical congestion as a direct cause of hæmorrhage is sometimes very distinctly seen in the bodies of persons who have been hanged. You know that when suffocation has been produced by suddenly cutting off the access of air to the lungs; the right side of the heart, the great veins, and indeed the venous system generally, become loaded and distended with dark blood. Dr. Yelloly examined the stomachs of five men who had been executed by hanging: he found them all exceedingly vascular; and in two of the five cases, blood was actually extravasated, and adhering to the surface of the membrane: there had been, in short, unequivocal hæmorrhage.

There are several things, worthy of notice, in respect to hæmorrhage by exhalation, of whatever kind.

In the first place it occurs much more frequently and readily from some tissues of the body than from others; and most especially of all, from *mucous* surfaces. Thus we have hæmorrhage from the mucous membrane lining the nasal cavities; from the pulmonary mucous membrane; from the stomach and bowels; from the urinary organs; and from the uterus; constituting distinct forms of disease, which we are by and by to investigate more particularly. *Epistaxis*, *hæmoptysis*, *hæmatemesis*, *melæna*, *hæmorrhois*, *hæmaturia*, *menorrhagia*, are names descriptive of hæmorrhage as it is apt to occur from different parts of one or other of the three tracts of mucous membrane met with in the body: and you will find that these comprise very nearly all the complaints enumerated by nosological writers under the head of hæmorrhagy.

Now, this is a very remarkable fact: and very interesting questions arise out of it. Has it any relation to the manner in which these membranes, and the tissues subjacent to them, are supplied with a capillary circulation? Or may the fact be explained by the laxity of their attachment, which facilitates and favours the accumulation of blood in the vessels of the submucous tissue? Or has the density or consistence of their natural exhalations any thing to do with this disposition to hæmorrhage in the mucous membranes? May we suppose that the vessels or orifices appointed to exhale *mucus*, afford a more easy passage to the blood than those which give egress to thinner fluids; serum, for example, or the cutaneous perspiration? Whatever answers may be given to these questions, you will do well to recollect the fact which has suggested them.

Hæmorrhages by exhalation are not, however, exclusively confined to mucous surfaces. They are liable to occur, but much more rarely, from serous membranes. In the majority of cases, however, in which the blood is found effused into any of the serous sacs, it has either been an event of inflammation, or the blood has been poured out from an accidental opening in some considerable vessel. Cutaneous hæmorrhage is also very rare; probably because the cuticle opposes a barrier to the exit of the blood: for the little red spots which characterize purpura are in fact hæmorrhages, although the blood has not penetrated the epidermis. There are some cases, however, as I mentioned

before, in which blood has transpired, in a sort of dew, from the external surface of the body.

Another important general fact in respect to hæmorrhages by exhalation is that they proceed more frequently from certain parts of the mucous membranes than others, according to differences of *age*. Thus in children they are most common from the membrane that lines the nasal cavities: in youth from the mucous membrane of the lungs and bronchi; in the middle years of life, and towards its decline, from the rectum, uterus, and urinary organs. I should add here, from the blood-vessels of the brain, in old age; except that this, as I have already intimated, is not (speaking generally) hæmorrhage by *exhalation*.

Of course when I say that, in the instances specified, the blood is commonly poured out by exhalation, you will understand that the hæmorrhage sometimes occurs from the laying open of a single vessel of some magnitude. Thus hæmorrhage from the fauces may be the result of ulceration there, which has penetrated the coats of a vein or artery: hæmoptysis is occasionally produced by the laceration of a blood-vessel during the softentng and expulsion of tubercles. Hæmatemesis sometimes is the consequence of the lesion of a considerable blood-vessel in the progress of cancer of the stomach, or by the extension of small corroding ulcers; hæmorrhage from the bowels is no uncommon effect of ulceration, such as happens in fever, of the mucous follicles of the small intestine; calculous matter in the kidneys will often lead to the rupture of some of the blood-vessels there, and to the discharge of the blood by the urethra. Aneurisms also may burst into almost any part of the body. But events of this kind are infrequent when compared with hæmorrhage from the same parts in the way of exhalation.

In the head, however, the ratio is reversed. Blood does sometimes, I believe, exude from the *membranes* of the *brain*, but much more commonly cerebral hæmorrhage is caused by the giving way of a diseased *artery* in the brain.

How, in all these cases, to distinguish whether the blood has oozed out by many orifices from a surface, or has escaped from a hole in the sides of a vein or artery, will form matter for future inquiry. Sometimes we *can* make the distinction; and sometimes, it must be confessed, we cannot.

You will readily understand that hæmorrhage must vary greatly, in respect to its importance, and to the danger which it implies, according to the part from which it proceeds, and the circumstances under which the blood is poured out. It sometimes happens that death ensues from the mere loss of blood; either at once, by one profuse bleeding, or more slowly, by repeated bleedings which we are unable to restrain: but this is comparatively rare, and when it does happen, the blood is generally found to have proceeded from one considerable vessel, which has been ruptured or eroded. The case approximates to traumatic hæmorrhage, except that we cannot cut down upon and tie the injured vessel. Much more commonly danger arises from the presence and pressure of the extravasated blood in and upon internal parts: upon the brain, for example, in cerebral hæmorrhage; in the lungs in pulmonary.

The symptoms also are liable to much variation in different cases. Even the diagnosis of hæmorrhage is not always equally easy, or certain. When the part into which the blood is directly poured communicates with the exterior of the body, the expulsion of some of that fluid will, generally, sooner or later, demonstrate the case to be one of hæmorrhage. I say *generally*, because cases have been known to occur, in which patients, previously in a state of great weakness, have died outright, by syncope, from the mere extravasation of the blood, and before any of it made its way out of the body. The stomach and bowels have been found full of blood, when none had passed either by vomiting or by stool. And when the blood does make its appearance outwardly, it is sometimes not easy to determine whether it has come from a certain organ, or from the parts that lie between the same organ and the natural outlet by which it ultimately escapes. For instance, it is sometimes a matter of uncertainty whether the blood in hæmaturia proceeds from the kidneys, or the bladder, or the urethra.

The blood itself, when it reaches the exterior, will generally be more fluid, and brighter, in proportion as it is effused in greater quantity, and nearer the surface: more in clots, and darker in colour, in proportion to the length of time that it has remained within the body

after its escape from its proper vessels; and this length of time may depend upon the smallness of the quantity of blood effused, and the consequent tolerance of the organs through which it may have passed; or, upon the actual space traversed. Respecting the *colour*, however, of the effused blood, I shall have some curious explanations to offer you when I come to speak of hæmatemesis as a disease. It would be superfluous to enter upon them now.

If the site of the hæmorrhage does not communicate with the external air, we are without that certainty which results from the actual spectacle of the blood. But in such cases we are much assisted by local disturbances of function, springing from the pressure upon, or the laceration or distension of the suffering organ, or of the parts contiguous to it. And we may derive good information from observing the indirect symptoms which declare themselves through the system at large; many of which indirect symptoms are the same whether the blood reach the exterior or not. They principally vary according to the quantity of blood poured out, and to the *rapidity* of its effusion: and some difference will occur according to the age and strength of the patient.

Some of these indirect symptoms have not always been imputed to their true cause. Paleness of the face, feebleness of the pulse, coldness of the extremities, and a tendency to syncope—symptoms which are apt to be connected with hæmorrhage—have sometimes been ascribed to the alarm and sense of danger which the sight of the blood is calculated to produce in the mind of the patient. This may, to a certain extent, be sometimes true; but the explanation cannot apply to those cases in which the hæmorrhage is strictly confined to the interior of the body, yet in which the symptoms just alluded to are often strongly marked. They *then* depend—and probably in all cases they *chiefly* depend—upon the actual abstraction of the blood from the circulation.

The management of individual cases of hæmorrhage must be mainly regulated by the particular circumstances under which they occur. The few observations that I have at present to make respecting their treatment cannot be otherwise than very general.

But a preliminary question, of some importance, presents itself. Is it in all cases of hæmorrhage proper, or safe, to attempt to stop the bleeding?

Without going into detail, it may, I think, be laid down as a rule, that what I have called habitual hæmorrhages ought not to be interfered with, so long as they have no perceptible injurious influence upon the health, and so long as they proceed (as they mostly do) from parts, of which the *structure* is not likely to be spoiled, nor the *function* impaired, by the repeated passage of the blood. The most common seat of these habitual hæmorrhages I have stated to be the rectum;—to which the two conditions just mentioned are, fortunately, both of them applicable. Epistaxis supplies a less frequent example of the same kind. When they deviate from their usual channel, and are transferred (as it were) to some more important organ, it will generally be right, among other remedial measures, to endeavour to *recall* the original hæmorrhage. It is very seldom that the metastasis takes place *for the better*,—i. e. from a part where the bleeding is attended with danger, to one where it is comparatively harmless.

However, when these habitual hæmorrhages happen, as they often do, in plethoric persons; and when they are urged and kept up, as they frequently are, by intemperate and luxurious habits; we ought not to content ourselves with merely looking on. Hæmorrhoïs often performs the office of a safety-valve in such persons; and there are many who have what are called bleeding piles, and who would rather continue to have them, than submit to any change in their mode of life, or to the employment of other means of evacuation. Certainly these are cases in which nothing should be done to stop the bleeding; yet such patients ought to be told that the hæmorrhoidal discharge is but a precarious, and often an inadequate relief of the plethora: that while the plethora is suffered to exist there is danger of a cessation of the piles, and of the supervention of serious or fatal affections of other parts, and especially of the head. Apoplexy, or cerebral hæmorrhage, has frequently been known to follow hard upon the suspension of constitutional hæmorrhoïs. The patients should be admonished also that the discharge of blood from the vessels of the rectum may become excessive; that if it be aggravated by exercise, or in any other way, it may lead to inflammation about the anus, and to great inconvenience; and that there are safe and

tolerably sure methods of getting rid of the plethora (which is what chiefly constitutes the danger of such cases), if they will submit to the observance of them. It is in the intervals between the hæmorrhages that the danger of which they are in some sort the token, may best be met.

Again, it will seldom be proper to employ *direct* expedients for stanching the flow of blood, in the small class of active idiopathic hæmorrhages; unless the quantity lost is so great as to endanger the safety of the patient. Such hæmorrhages have commonly a tendency to cure themselves, by relieving the general plethora, or the local congestion, on which they depend. For these hæmorrhages, which bear so strong an analogy to inflammation, the *treatment* of inflammation may often be requisite, as an indirect mode in which their amount may be moderated, and their recurrence obviated.

With these exceptions, both direct and indirect measures are to be used, for arresting the effusion of blood as speedily as may be.

To this end the patient is to be surrounded as much as possible with cool fresh air, and kept in a state of absolute quiet. All motion of the body, and emotion of the mind, all kinds of stimulating food and drink, every thing, in short, which has a tendency to hurry the circulation, should be diligently avoided: and that position of the body should be chosen which is the least favourable to the afflux of blood towards the part affected. The horizontal posture will be proper in hæmorrhage from the bowels, the uterus, or the urinary organs. In epistaxis, and in cerebral hæmorrhage, the head should be raised.

In two words, the *antiphlogistic regimen* should be strictly enjoined in all cases of hæmorrhage sufficiently severe to require medical assistance.

Of the actual remedies used for checking the farther escape of the blood, one of the most important has already been alluded to—I mean venesection. We are guilty of homœopathy in this matter: to prevent bleeding, we draw blood. After what was stated respecting the use of blood-letting in inflammation, I need not dwell upon the objects aimed at by this measure: they are briefly, to abate the vigor and force of the heart's contraction, to lessen general plethora when it exists, to remove local congestion, and to divert the current of the blood from the suffering organ. The method, and the amount, and the repetition of the blood-letting, must of course be regulated by the circumstances of each particular case. And the same objects may sometimes be effected by other modes of general depletion, especially by the use of purgative medicines.

Next to blood-letting, *astringents* constitute the great resource against actually existing hæmorrhage: and among these, *cold* is one of the chief. It may be placed in direct contact with the bleeding surface:—as when ice is *swallowed* to restrain hæmatemesis: or cold water injected into the rectum in excessive and exhausting hæmorrhoïs; or into the vagina, in flooding from the uterus. Or it may be applied to the surface of the body, as near as possible to the seat of the hæmorrhage; as to the nose and forehead in epistaxis; to the chest in hæmoptysis; to the epigastrium in hæmorrhage from the stomach; to the lower part of the abdomen or to the perinæum in hæmorrhage from the intestines, uterus, or urinary organs. But the influence of cold in constringing the smaller vessels is not confined to the part with which it is in contact; it will stop hæmorrhage by the sympathetic shrinking which it produces in distant parts. Epistaxis, for example, has often been arrested by the sudden apposition of cold water to the neck, back, or genital organs. The nursery remedy consits in slipping a cold key down the back between the clothes and the skin.

Of even the mischievous power of cold in this way we have continual illustration in the suppression of the catamenia by cold and wet accidentally applied to the feet.

There is a long catalogue of medicinal substances which are esteemed to possess more or less of a specific virtue, when taken internally, in checking the flow of blood. Most of these are of an astringent nature, and some of them are eminently useful. The acetate of lead enjoys, in this country, a higher character, perhaps, than any other of these substances.

Many vegetable matters, and some artificial compounds, frequently employed in internal hæmorrhages, seem to owe their astringent and styptic properties to the gallic acid which enters into their composition. Such are the rhatany root, uva ursi, bistort, tormentil, the

pomegranate, kino, catechu, the several preparations of gall-nuts, and the nostrum called *Ruspini's styptic.*

The power of arresting internal hæmorrhage has also been confidently ascribed, by different persons, to nitre given in large doses, to the mineral acids, to the muriated tincture of iron, to alum, to the oil of turpentine, to the secale cornutum or spurred rye, and to various other substances; a more particular account of the rules and indications for administering which, I may return to, when I have to speak of individual hæmorrhages.

LECTURE XVI.

DROPSY: ITS GENERAL PATHOLOGY. PASSIVE DROPSY; CARDIAC, AND RENAL. ACTIVE, ACUTE, OR FEBRILE DROPSY. GENERAL PRINCIPLES OF TREATMENT IN DROPSIES.

THERE remains now only one subject, of the pathology of which it will be convenient and, I hope, instructive, to take a short general view, before we enter upon the consideration of special diseases. I proceed to speak of *Dropsies: i. e.* of serous effusions into one or more of the shut cavities of the body, or into the cellular tissue, or into both; independent of inflammation.

We have already considered serous effusion occurring as an effect or event of inflammation. We are commonly able to say of this, that it *has* originated in inflammation; either from its being mixed with some of the less equivocal *products* of that disease, such as coagulable lymph; or from its having taken place while *symptoms* of inflammation existed. But there are numerous examples of serous accumulation, which cannot with any show of reason be regarded as events of inflammation. It is to these that I would apply the simple term dropsy.

It has been said—and said with much truth—that dropsy is rather a symptom of disease, than a disease in itself. And it has been affirmed that it would be more philosophical and scientific to treat of the original malady upon which the effusion or accumulation depends; to erase dropsy from the list of substantive diseases, and to place it in the catalogue of mere symptoms.

But this, in my mind, is a very mistaken view of the matter. For, first, it is oftentimes uncertain, while the patient is yet alive, what or where the primary disease may be; and even after death we sometimes can discover no organic change that would satisfactorily account for the effusion. Practically speaking, in such cases the dropsy *is* the disease, and the sole object of our treatment.

And, secondly, dropsy is, in fact, to a medical eye, in *all* cases, something more than an effect or symptom of disease. The imprisoned liquid is often a *cause* of various other symptoms; embarrassing, by its pressure, important functions, and even extinguishing life. The removal of the dropsy (although its original cause, of which it was a symptom, may remain behind, untouched, to be again productive of effusion under circumstances favourable to its operation)—the removal of the dropsy will often restore a person to comparative comfort; or even to what, so far as his sensations, and powers, and belief are concerned, *is*, to him, for the time, a state of health.

You see then, already, that in a dropsical person, whose dropsy depends upon organic disease, there are two sets of symptoms to be distinguished: viz. those which depend on the primary disease; and those which depend on the collected fluid. The latter, often the most grievous, are often to be got rid of. the former, frequently permanent, are frequently also but little complained of or felt by the patient, except when effusion is the result.

Some persons, I fancy, have regarded dropsy as a less attractive subject of investigation

than it might be if it were less frequently, in its nature, incurable. But as far as the dropsy itself is concerned, the complaint often *is* curable; and there are some forms of dropsy that are curable in a more absolute sense: that is, both the effusion, and that condition which was the physical cause of the effusion, are sometimes remediable.

Besides, it is our business to cure when we can; but whether we can cure or not, to relieve and palliate human suffering; and this, under Providence, we are able to do, in many or most cases of dropsy, to a very considerable extent.

Wherever there is a shut sac, or wherever there is loose and permeable cellular tissue, there we may have dropsy.

Thus there may be dropsy of the ventricles of the brain, or of the meshes of the pia mater, leading to death by *coma:* of the pleuræ, of the cellular texture of the lungs, or of the submucous cellular tissue of the glottis, any of which may cause death by *apnœa;* of the pericardium, producing death by *syncope*. I mention these instances in particular, to show that almost every mode of dying may result from dropsical effusion: and to win your attention to a disorder so full of peril.

When the cerebral ventricles are distended with water, we express the diseased condition by the term *hydrocephalus*. When serous liquid collects in the pleuræ, or in the pericardium, we say that the patient has *hydrothorax*, or *hydropericardium*. If the cavity of the peritoneum be the seat of the effusion, we call the complaint *ascites*. When the cellular tissue of a part becomes infiltered with serous fluid, the part is said to be *œdematous;* and *anasarca* is the name given to the more or less general accumulation of serum into the cellular tissue throughout the body, and especially to visible subcutaneous œdema of considerable extent. Finally, the term *general dropsy* signifies the combination of anasarca with dropsy of one or more of the large serous cavities.

Other local dropsies indeed there are, but, as they belong entirely to surgery, I need not enumerate them.

General pathology of dropsies.—Now what reasonable account can be given of these remarkable conditions? How is it that the hollows and interstices of the living body, or of parts of the body, became thus water-logged?

To solve this question, we must carry in our minds some physiological recollections.

The closed cavities or cells within which the fluid of dropsy is confined, are kept moist, during life and health, by a continual serous secretion from their surfaces; and they are kept *merely* moist, for the fluid thus constantly secreted is as constantly reabsorbed into the circulation.

When these cells or cavities, without having undergone inflammation, become filled and distended with the serous fluid which they habitually secrete, one of three things must have happened. Either the quantity of fluid exhaled has been augmented, the absorption remaining the same; or, the absorption has been diminished, the exhalation continuing the same; or else the exhalation has been increased while the absorption was either lessened or not proportionally increased at the same time.

The last is a mixed case; and we need only consider the two first.

Now the balance between exhalation and absorption is often deranged, and dropsies do actually arise, in each and all of these ways.

It will best suit my purpose to speak first of those dropsies which are occasioned by defective absorption, and which are usually called chronic or passive dropsies.

The direct agency of the blood-vessels in the production, as well as in the removal, of dropsy, although indicated by many common and obvious facts, has not been generally recognized till a comparatively recent period. Perhaps I should rather say that more importance used to be assigned, in these respects, to the agency of the lymphatic absorbents, than they are really entitled to. You will find that pathologists, even in modern times, speak of a want of tone, of deficient energy, in the absorbents, as a cause of dropsical accumulations; the superfluous fluid of the part is not adequately taken up (they say) by the enfeebled absorbents, meaning the absorbents strictly and anatomically so called. And this view of the matter, connecting dropsy always with debility as its cause, has led to a corresponding plan of treatment: the object aimed at being the stimulation of the absorbents to more energetic action.

But to the doctrine that dropsy is a consequence of the deficient action of the absorb-

ents, this obvious difficulty presents itself,—that absorption really goes on, and goes on very actively, in dropsical patients: their adipous matter disappears, they become wretchedly thin. There is no complaint in which wasting and emaciation go to a greater extent than in dropsy. You will find also that persons labouring under anasarca are readily enough affected by mercury; which must of course be absorbed before it can produce any of its specific effects.

It must be confessed that our knowledge respecting the mechanism of absorption is neither complete nor certain; but there is good reason for supposing that the process is shared among the lacteals, the lymphatics, and the *veins;* and it is probably distributed between these sets of vessels somewhat in this manner;—that the lacteals absorb the chyle from the surface of the alimentary canal, and convey into the blood the materials of its renovation; that the office of the lymphatics is to take up and carry into the blood those old and effete portions of the solid constituents of the body, which require to be removed to make way for a fresh deposit; while the veins imbibe the serous fluid exhaled from the surfaces of serous membranes, and into the meshes of the cellular tissue, as well as poisons and other substances that are soluble and dissolved in that fluid.

If this be so, the difficulty just now mentioned vanishes. Of the two sets of absorbing vessels, the lymphatics and the veins, one set may continue to perform its functions, while the other fails to do so. This theory is quite consistent with the actual phenomena of dropsical disease; and whether it be altogether true or not, a part of it is certainly true; that, namely, which assigns to the veins a *large* share in the whole process of absorption. The experiments of Magendie and of others are quite conclusive upon that point.

It has also been fully established, that fluids may and do pass into or out of the veins, in the living body, not by any vital process, but by mere physical imbibition and transudation, through the coats of those vessels; that when the veins are distended to a certain degree with watery fluid, the entrance of more of the same fluid, through their sides, is impeded or prevented; that, when the distension is still greater, the aqueous part of the blood may even pass in the other direction out of the vessel; and that, on the other hand, when the veins are comparatively empty, the surrounding serous fluid passes readily into them, or, in common language, is absorbed. The venous absorption is explicable therefore upon the principles of *endosmose* and *exosmose*, as laid down by Dutrochet; or I would rather say, according to the more general and more simple laws of *heterogeneous attraction*, as explained by Professor Daniell.

Imbibition, being a form of that attraction, belongs in various degrees to all the tissues of the body. Its rapidity—and even its direction in respect to the elastic coats of a vessel surrounded by fluid, and also carrying fluid of a certain consistence—will vary with the varying distension of the vessel. When the vessel is moderately full, the exterior fluid passes uninterruptedly inwards, and is conveyed away by the internal current. When, on the other hand, the vessel is kept much distended by its contents, the contained fluid, or its thinner part, passes continually outwards; and there is an intermediate degree of distension, at which the pressure is just sufficient to prevent the transit of fluid in either direction. Magendie found, accordingly, in a well-conducted and conclusive series of experiments, that by regulating the conditions of comparative emptiness or fulness of the circulating system, he could accelerate, retard, or suspend altogether, the operation of a poison dissolved in the humours of the body. In other words, he could thus accelerate, retard, or prevent, the process of absorption or imbibition through the blood-vessels.

Bearing these physiological truths in remembrance, we shall have no difficulty in showing that the chronic forms of dropsy are attributable partly, and chiefly, and in many instances entirely, to undue plenitude of the veins; and that this venous repletion is produced, almost always, by some impediment to the free return of the blood towards the heart.

Passive dropsy from venous obstruction.—When the cellular tissue of a limited part of the body becomes filled and distended by serous liquid, we call the swelling *œdema:* but this is exactly the same, in its nature, as anasarca. Now œdema is often the conse-

quence of some mechanical obstruction to the venous circulation. We can produce it whenever we will. Our countryman Dr. Lower, 170 years ago, tied the jugular vein of a living dog. When a few hours had elapsed, he observed that all the parts beyond the ligature, reckoning from the heart, were much swollen: and upon dissecting the animal after death he found that the cellular tissue of the head and face was filled, not with red blood, as he had expected it might be, but with clear and limpid serum. On another occasion he placed a ligature upon the vena cava, just above the diaphragm: death soon ensued, and a large quantity of water was discovered in the cavity of the peritoneum.

Precisely similar phenomena succeed the compression or obliteration of a large vein in various parts of the body. In operating for popliteal aneurism, Mr. Travers was obliged to tie the femoral vein: the cellular tissue of the limb was speedily infiltrated with serous fluid. Long-abiding œdema of one foot and ankle has been cured at once by the reduction of a crural hernia, which had been pressing for the same length of time upon the femoral vessels. You have heard, most probably, of the disease called *phlegmasia dolens;* a disease that is very common in women soon after childbirth, although it is not peculiar to them, nor to the female sex. The foot, leg, and thigh, become enormously œdematous. The essence of this disorder is inflammation of the femoral vein, which blocks up that vessel near the groin, and retards or precludes the return of the venous blood from the limb. One arm often swells in the same way, and from a similar cause, in women who are afflicted with cancer of the breast. In pregnancy, the gravid uterus sometimes presses upon the iliac veins, and obstructs the current of the blood within them: the consequence is, anasarca of the lower extremities, which disappears as soon as the pressure is removed by the delivery of the woman. The flow of blood through the vena portæ is frequently hindered, by disease in the liver, or by other causes; and serous liquid accumulates in the peritoneum, constituting ascites. A French physician, M. Tonnellé, narrates several cases in which serosity was found in the cavity of the arachnoid, in conjunction with obliteration of the venous sinuses of the dura mater. In all these instances we have retardation of the venous current, undue plenitude of the veins, and dropsy of the part from which they proceed. The natural exhalation goes on, and the exhaled fluid collects and stagnates, because the channel through which it ought to be drained away is choaked up. The larger the vein, and the nearer we approach the heart, the more extensive is the dropsical accumulation: and if we could plant an obstacle at the very termination of the venous stream, we should dam up the blood in the whole system of veins, and produce a general dropsy.

Such an obstacle is frequently placed there by disease. The returning blood is checked at its entrance into the heart, at the confluence of all the veins of the body, where they unite to empty themselves into the right chambers of that organ: and then anasarca of the universal cellular tissue comes on, and water collects in all or most of the great serous cavities.

It is no part of my present purpose to inquire how such disease of the heart as is productive of dropsy, arises. Commonly we find the right auricle and ventricle enlarged in capacity, the opening between them unnaturally wide, and the tricuspid valve unequal to its office of closing that aperture. Such a morbid state of the right heart may be occasioned by any cause which impedes the flow of blood *out of* its cavities. The diseased condition of those cavities may be primary; but it is oftener perhaps consecutive to other disease. It may be produced by disease of the lungs, preventing the right ventricle from freely delivering its contents into the pulmonary blood-vessels. Or the retarding cause may be still more distant, in the left side of the heart, keeping the pulmonary blood-vessels unduly full, and thereby hindering indirectly the escape of the blood from the right ventricle. The dropsy may ultimately depend, therefore, upon some bar to the circulation placed even at the mouth of the aorta. Obstacles situated anywhere in the circuit formed by the right heart, the lungs, and the left heart, have the effect of producing secondary changes in the parts behind them. But disease, thus propagated in a direction retrograde to the course of the blood, is propagated gradually, and sometimes very slowly. These are points of much interest, which we shall investigate together by and by. I allude to them now, that you may not be perplexed by a knowledge of the fact, that diseases

of the heart often exist for a long while without inducing dropsy. It is with disease of the right side of the heart, whether primary or secondary, that passive dropsy is especially associated.

As if to furnish the *experimentum crucis* in respect to this doctrine, disease does sometimes, with a curious precision, dam up one only of the two great venous trunks, at the junction of which the left auricle is placed: and then the dropsy is as curiously limited to that half of the body in which the tributary veins of the obstructed trunk originate. The first example of this which I ever saw was a most remarkable one. The patient was dropsical in his upper half only. His arms were so hugely anasarcous that he could not bring his elbows near his sides; his neck and face were hideously bloated and exaggerated, and his eyes prominent and staring; while his lower limbs were of their natural size, and appeared preposterously small and out of proportion. The poor man looked as if the upper part of his body had been stuffed, for acting some ridiculous part upon the stage. The cause of this strange and distressful state was found to be the obliteration of the vena cava superior, close to the auricle. Its sides had been pressed together by a large aneurism of the aorta; and a portion of the vein was fairly sealed up. I have seen two similar cases since.

Objections have, however, been taken to the accuracy of the conclusions drawn from such cases as I have related; and it is fit that you should be aware of them. Thus it is stated that veins have been found obliterated, and yet there was no dropsy. Now to this objection it may be answered, in the first place, that it is not every vein, the obliteration of which would cause manifest œdema. It must be the principal venous trunk of the part concerned. When some of the secondary and smaller veins alone become impervious, the blood may reach, and return by, the primary branches with sufficient readiness to relieve the turgid capillaries, and prevent any serous accumulation.

But (it may be said) the principal vein itself has been found converted into a solid cord, and still there was no dropsy. Granted: but it does not follow that there never *had been* dropsy. You know that when a large artery has been tied, the circulation is carried on in the corresponding limb, by means of collateral arterial branches: imperfectly indeed at first; but at length, as the supplemental channels become more numerous and free, the supply of blood to the limb is as copious as ever. It is precisely the same, *mutatis mutandis*, with the veins, only that the anastomosing venous tubes are not (perhaps) so readily developed as the arterial. Now I am not aware of any instance in which it has been shown that the principal vein was obliterated, and yet there neither was nor had been any œdema of the limb. The recorded cases have been met with in dissecting rooms, and the previous history of the subject has been unknown or unregistered. Mr. Kiernan has told me that he once examined the body of a woman who had excited much curiosity among the medical men by whom she had been seen during life, on account of a remarkable and enormous dilatation of the superficial veins of the abdomen. She was not dropsical, and the cause of the huge varix was sought for with great interest after her death. The inferior cava was obliterated. Here the compensating result was obvious to the sight; the new channels had answered their purpose, and performed the functions of the original channel. The history of this case was incomplete: it was not ascertained whether the woman had always been free from dropsy.

I hold this objection therefore to be invalid, until some authentic instance shall be brought forward of the obliteration of a large venous trunk, without a corresponding accumulation of serous fluid, either at the time when the observation is made, or at some previous time in the life of that individual. It is, besides, possible enough, that the obstruction of a large vein may be effected gradually, by the slow encroachment, for instance, of a growing tumour; and the collateral circulation may begin to be enlarged with the first impediment in the vein, and may keep pace with and counterbalance that increasing impediment, till the closure of the vessel is complete; so that, from first to last, there may be no noticeable dropsy.

Again, it is affirmed, and truly affirmed, that anasarca often occurs, without any obliteration of veins, and independent of any discoverable organic disease in the heart, or anywhere else. We see this every day, in weak chlorotic girls, with bloodless cheeks and pale lips. Some of you saw a case of this kind which was lately under my care in the

hospital; besides the anasarca, the systolic sound of the heart was accompanied by a loud, unmistakeable bellows sound. This girl got quite well, and left the hospital without bellows sound, or any other trace of disease. There could not then have been an organic change: in fact there was not. Yet was there, virtually, a retardation of the venous circulation: not by any mechanical obstacle opposed to its course, but in consequence of the debility of that hollow muscle, the office of which is to propel onwards, with a certain degree of force, the blood that reaches it. Girls of this description have weak and flabby voluntary muscles; and it is reasonable to presume that the involuntary muscle, the heart, partakes of the general debility of the muscular system, and becomes incapable of sending the blood forwards with the requisite energy. Nay, I believe that a heart thus feeble may yield a little and dilate under the resisting pressure of the blood that enters its chambers; and that so an occasonal but temporary bellows sound may arise, from the altered relation between the cavities of the heart and their outlets. Certainly this view of the matter is strengthened by the *juvantia* and *lædentia*. If you are tempted, by the pain complained of by your patient, or by the violence with which her heart is throbbing, to take away blood, you find that she is ultimately made worse by the depletion; on the other hand, if you give her steel, feed her well, keep her bowels free, and place her every morning under a cold shower bath, you find that she recovers her lost strength, that colour returns to her lips and cheeks, that her palpitations cease, and her dropsy vanishes. In proportion as the muscular system in general receives fresh tone and vigour, does that particular muscle the heart also regain the degree of power necessary for the effectual discharge of its proper function, which is very much that of a forcing pump. Such is the way in which I should explain both the cause of the dropsy, and the cause of its cessation. In such cases our patients do not simply *recover;* they are *cured*. I should apply a similar explanation to some other forms of dropsy. Andral describes a certain *cachectic* disposition of the body to be a cause of dropsy; persons may be bled into a dropsy, and starved or weakened into a dropsy. These are genuine instances of dropsy from debility, such as the ancients conceived all dropsies to proceed from. It may be that the thin and watery quality of the blood induced by frequent bleedings, by insufficient nourishment, by certain poisons, or by other causes, may facilitate its passage through the coats of the veins. But admitting this as a concurrent cause, I am disposed to the belief that all passive dropsies occurring under the circumstances just adverted to, and without any apparent organic disease or change, are mainly to be ascribed to debility of the heart: and viewed in this way, they are all brought under the same general principle; viz. the retardation of the blood in the veins.

General Dropsy, cardiac or renal.—A large class, then, of passive dropsies, depending upon mechanical congestion, and defective absorption by the veins, are traceable, in their origin, to the heart; and we call them, accordingly, *cardiac* dropsies. But another class, perhaps as numerous, are connected in a remarkable manner with certain diseased conditions of the kidneys; and these, for the sake of distinction, we style *renal* dropsies. I shall say a few words respecting them, after I have briefly considered the other source of dropsical swellings, adverted to in the commencement of this lecture; namely, excessive *exhalation* of serous liquid. Dropsy so caused comes on suddenly and tumultuously, and is spoken of as being *acute* or *active*. It borders closely upon inflammation, and sometimes can scarcely be discriminated from inflammation with serous effusion. The condition of the capillary circulation is supposed to be intermediate between that in which the ordinary amount of secretion is maintained, and that in which inflammatory effusion takes place. The excessive increase of secretion is analogous to what we observe in other parts and predicaments of the body; to the abundant perspirations, for example, that are occasioned by violent exercise: to the plentiful flow of tears caused by any irritation of the eye, or by the passion of grief; to the augmented watery discharges from the mucous membrane of the bowels, produced by purgative medicines; all of which may be independent of inflammation, but all of which are attended with congestion that might readily be pushed into inflammation. In point of fact, if the secretions to which I have now referred were poured into closed cavities, instead of proceeding from surfaces that are situated on the exterior of the body, or communicate readily with the exterior, they would *constitute* dropsies.

Active dropsy.—The phenomena of active dropsy are of this kind: a labourer is engaged in some employment, which, while it requires considerable bodily exertion, and causes copious perspiration, necessarily exposes him also to the influence of external cold and moisture: he has been digging (perhaps) in a wet ditch, in winter time, and he pauses to take his meal; or he has been unloading a wagon, and rides home, some miles, in a heavy rain that wets him to the skin; or he has been mowing, in the heat of summer, and lies down to sleep upon the damp grass. All these suppositions are derived from actual occurrences. The perspiration is suddenly checked, and in the course of a few hours he becomes universally anasarcous. Again, a patient recovering from scarlet fever ventures out into a cold atmosphere, while the process of desquamation is yet going on; and he is attacked with dropsy of the cellular tissue, and, it may be, of some of the larger cavities also. The urine at the same time is observed to be scanty, turbid, mixed with blood.

To comprehend this rapid change from a state of health to a state of dangerous disease, we must again have recourse to the findings of physiology.

Besides the constant exhalation that takes place from the inner faces of the shut serous cavities, a large amount of watery fluid is continually thrown out of the system, by all those surfaces that communicate with the air; by the skin, the lungs, the bowels, the kidneys. Now it is well ascertained, that when the excretion of aqueous fluid from one such surface is checked, the exhalation from some other surface becomes more copious. It is probable that the aggregate quantity of water thus expelled from the system in a given time, cannot vary *much* in either direction, without deranging the whole economy. But we are sure that the amount furnished by any excreting surface may vary and oscillate within certain limits consistent with health, provided that the defect or excess be compensated by an increase or diminution of the ordinary expenditure of watery liquid through some other channel. Sound health admits and requires this shifting and counterpoise of work between the organs destined to remove aqueous fluid from the body. This supplemental or compensating relation is more conspicuous in regard to some parts than others. The reciprocal but inverse accommodation of function that subsists between the skin and the kidneys affords the strongest and the most familiar example. In the warm weather of summer, when the perspiration is abundant, the urine is proportionally concentrated and scanty. On the other hand, during winter, when the cutaneous transpiration is checked by the operation of external cold, the flow of dilute water from the kidneys is strikingly augmented. All this is well known to be compatible with the maintenance of the most perfect health. But supposing the exhalation from one of these surfaces to be much diminished, or to cease, without a corresponding increase of function in the related organ, or in any excreting organ communicating with the exterior, then dropsy, in some form or degree, is very apt to arise. The aqueous liquid thus detained in the blood-vessels, seeks and at length finds some unnatural and inward vent, and is poured forth into the cellular tissue, or into the cavities bounded by the serous membranes.

Dropsy of one part sometimes supervenes suddenly upon the rapid disappearance of a watery collection from another part. It is no uncommon thing to see the swollen unwieldly legs and thighs of an anasarcous patient quickly unload themselves, and resume their natural bulk and symmetry. His friends congratulate him, and each other, that his disease is leaving him; but as his legs are emptying, he becomes drowsy, forgetful, comatose, apoplectic; and after his death we find the ventricles of the brain distended with serous fluid.

Or the dropsical accumulation may be transferred from its place through a safer channel. The best instance of this that occurs to my recollection I heard related by Dr. Farre. A gouty individual had hydrocele; dropsy of the tunica vaginalis. After the disease had lasted for some time he got very drunk one evening, with rack punch, which greatly disordered his alimentary canal, and brought on a kind of cholera. He had profuse vomiting and purging, which quite exhausted him: and at length he fell asleep. When he awoke in the morning he found that his hydrocele, which had been a large one, was gone: and it never returned. Such an accidental cure is most instructive.

If water be injected, in some quantity, into the blood-vessels of a living animal, the

animal soon perishes; dying generally by coma, or by suffocation: and when the carcass is examined, the lungs are found to be charged with serous liquid, or water is discovered in the cellular tissue of some other part, or in the shut serous membranes. If, however, the animal be first bled, and then a quantity of water be injected equal to the quantity of blood abstracted, the injection is followed by no serious consequences.

Facts like these throw, as it seems to me, a strong light upon a confessedly obscure part of pathology. It appears that under various circumstances the blood-vessels may receive a considerable and unwonted accession of watery fluid; and that they are very prone to get rid of the redundance. When they empty themselves through some free surface, their preternatural distension is relieved by a flux: if, on the other hand, the surface be that of a shut sack, in discharging their superfluity they cause dropsy. Why sometimes this organ, and sometimes that, is selected as the channel by which the superabundant water shall be thrown out of the vessels, we can seldom tell. We often find it difficult to determine which of the two facts in question is to be considered the antecedent, and which the consequent. For not only is it true that when the blood-vessels become overloaded with serous fluid, they readily deposit a part of it; but also that when they are in the opposite condition of comparative emptiness, when they contain less blood than is natural, they are equally ready to replenish themselves by absorbing fluids from any source to which they can find access. In the case of the man who was cured of his hydrocele upon the occurrence of profuse watery discharges from the stomach and bowels, it seems clear that the expenditure of serous liquid from one part led to its absorption into the blood from another. When anasarca suddenly leaves the extremities, and fatal coma follows, it appears probable that the absorption is the first of the changes, and the effusion the second: and had this effusion been determined to the mucous membrane of the intestines, to the skin, or to the kidneys, it would have brought relief and safety to the patient, instead of causing his death.

We have obtained, then, a glimpse of one or two most important principles in respect to the pathology of dropsy. The blood-vessels, when preternaturally full of aqueous fluid, have a strong tendency to empty themselves; when preternaturally empty, they readily drink up watery fluid wherever they come in contact with it. From the discharge of their superfluity of water arises a dropsy or a flux. The cause, and the cure, of many dropsies, lie in these propositions.

The application of these principles to the supposed case of active dropsy must be obvious. No doubt, in some such cases, actual inflammation takes place; but in many of them there is merely the dropsical effusion, without any other trace or evidence of inflammatory action. The two facts which it chiefly concerns us to remark are these—first, that the aqueous portion of the blood, which in health is habitually carried off to a very considerable amount by the skin, is suddenly diverted from that tissue; the perspiration, sensible and insensible, is suppressed: and secondly, that the cellular tissue, or the large serous bags, or both, become filled with serosity.

It is not by any necessity, however, that the vicarious excretion is turned upon these serous surfaces. In truth, the intercepted perspiration more often escapes, or labours to escape, from some free surface; and then we have, not a dropsy, but a flux. Diarrhœa, for example, is more common, under the supposed circumstances, than anasarca or ascites: apparently because there is a closer analogy of structure, and a more direct consent or agreement in function, and a stronger reciprocal influence between the skin and the mucous membrane of the alimentary canal, than between the skin and the serous tissues.

Brief allusion has been made to a large class of chronic dropsies connected with and dependent upon a peculiar renal disease. This important species of dropsy will require a detailed examination hereafter. It is more complex, and of more obscure pathology, perhaps, than *cardiac* dropsy. It certainly has a more direct relation also to what I have just been describing as active dropsy: of which it may almost be regarded as the chronic form. Sometimes the kidney disease, of which the dropsy is an incidental and not an essential symptom, springs up silently, and without obvious cause. Sometimes it may distinctly be traced back to its origin in an attack of acute dropsy; in which complaint the kidney always and manifestly labours, its functions being violently deranged, and the urine being small in quantity, and mixed with blood.

In this chronic and renal dropsy, the watery accumulation is accounted for by the deficient excretion through the customary channels. The blood-vessels, deposit that excretion in a wrong place. The urine, in the outset of the dropsy at least, is scanty. The skin is almost always dry, harsh, and unperspiring. The anasarca usually increases or decreases, as the quantity of urine diminishes or augments. Remarkable alterations take place also in the qualities and composition of the urine itself: it has a very low specific gravity, contains albumen, and is deficient in urea. The blood degenerates too; and other organs of the body, and especially the heart, are apt to fall into disease. The suppression of perspiration, and the appearance in the urine of blood or serum, unchanged by the secerning power of the kidney, form striking links of connection between acute and renal dropsy.

In the sketch that I have been endeavouring to give you of the pathology of dropsy, I have taken extreme cases to elucidate the two varieties of that disease which have been respectively denominated active and passive. Let me once more present to you, in a summary view, the points of resemblance, and the points of distinction between them.

They resemble each other in the result; namely, in the collection of serous liquid in the circumscribed cavities and vacuities of the body. They differ in the rate at which the collection augments.

In the well-marked acute dropsies the liquid is rapidly effused, in quantity much beyond the natural amount of exhalation. In the well-marked passive dropsies the exhalation goes on as usual, but the fluid exhaled is not taken back again into the circulating vessels with sufficient facility. In the one case the circulation is disturbed and tumultuous; in the other, it remains tranquil. It is probable that in the more acute forms, the serum transudes through the coats of the arteries, or of the capillary vessels next adjacent to the arteries. In the completely chronic and cardiac forms there is a defect of absorption by the veins. Active dropsies are sometimes spoken of as belonging to the left side of the heart; passive dropsies to the right.

But there are intermediate degrees, in which the full veins are not only unable to admit any addition of aqueous liquid, but also to retain that which they already hold; and serosity gradually exudes through their parietes.

What connects all these forms of dropsy is a preternatural fulness in some part, or the whole, of the hydraulic machine. And this seems to be the grand key to the entire pathology, as well as to the remedial management of the disease.

I scarcely need point out to you the fact, that the water of dropsy is liable to change its place, in obedience to the force of gravity. In general anasarca, when the serous accumulation slowly augments, it first becomes visible about the feet and ankles. There are two causes for this; the one occasional in its operation, the other general. The veins of the lower extremities are apt, when the patient is erect, to be more turgid than other veins; for unless the action of their valves be quite perfect, those vessels sustain the weight of a large superincumbent column of blood, which concurs with other causes to retard the upper current, and to keep the depending capillaries unduly full. Under such circumstances the effusion, or the arrest of absorption, may *take place* around the insteps earlier than in any other part. But in general it is not so. In most cases, the truer and simpler reason of the earlier manifestation of dropsical swelling about the ankles, is merely that the serous liquid which fails to be removed from the cellular tissue in all parts of the body, gravitates towards the *lowest* part; and being thus collected into a comparatively small space, is rendered more perceptible. During the night, when the horizontal posture is maintained for several hours, the œdema of the ankles disappears, but the neck and face, perhaps, become bloated and puffy. And it is obvious why, in these cases, the feet, towards evening, swell more than the hands. The hands receive the serous fluid from the cellular tissue of the arms alone; the feet, that which sinks down, not only from the legs and thighs, but from the head and trunk also. The limbs may be looked upon as bags, which fill up in proportion to the quantity of liquid detained. And the lungs are similarly bags: and in these cases we commonly may hear the crepitation of pulmonary œdema in their lowermost portions.

I mentioned an instance in which one-half only of the body was anasarcous, and that

the upper half. The descent of the dropsical fluid was prevented by the dress of the patient; the waistband of his trowsers having compressed the cellular tissue through which alone the gravitating liquid could seek a passage. So, sometimes, it is stopped at a lower point of its descent by tight garters, and the thighs swell earlier than the insteps. It is not at all uncommon to see persons who, in the daytime at least, and in the erect posture, are anasarcous in the lower half only of the body. We do not so often meet with anasarca of one moiety of the body, the division being made by an imaginary plane drawn through its axis. Yet this does occasionally happen. This curious phenomenon is usually the result of a mere accident, the anasarcous patient being unable to leave his bed, or to lie at all except on one side; and then the accumulating liquid gravitates to that side. I have, however, seen one case to which this explanation would not apply. I believe that some local obstruction to a large vein in the neighbourhood of the shoulder caused œdema there, and the fluid sunk down and filled the cellular tissue of that side alone. As the man recovered, I had no means of verifying the truth of this conjecture.

Cæteris paribus those parts of the body become the most loaded with serous fluid, and show the anasarca the plainest, of which the cellular tissue is plentiful and loose; as the eyelids, and the scrotum. But in extreme cases the liquid pervades the cellular tissue, where it is much more dense and compact: as where, for example, it is subjacent to mucous membranes. In the examination of a dropsical corpse, the mucous coat of the intestines may sometimes be seen to be elevated by the water collected beneath it. It then looks like jelly, and the *valvulæ conniventes*, which are flat and thin in their ordinary state, become round and convex. Dropsy of the submucous tissue of the air-passages is frequently a cause of death.

Many persons seem disposed to ascribe these anasarcous swellings, especially when they make their appearance suddenly, to inflammation; and much is said about the frequency of *inflammatory dropsy*. But the facts we have just been considering sufficiently refute this theory. If the serous liquid be the product of inflammation, what is the part inflamed? It cannot be, as some appear to think, the distended cellular tissue itself; for if so, the inflammation must shift its quarters under the influence of gravity. The term inflammatory dropsy may not perhaps be indefensible when applied to that class of dropsical affections that have been spoken of under the head of active dropsy. I am far from denying the frequent agency of inflammation in producing changes which, in their turn, lead to dropsy, but we shall do well not to confound those collections of serum mixed with blood or with coagulable lymph, which are distinctly events or products of inflammation, with other collections of serum which resemble the former in that respect only, but differ entirely from them in every other particular. To the class denominated active, which occur suddenly, from defect of some one or more of the usual channels of aqueous excretion, and which are usually attended with much disturbance of the whole system, the epithet *febrile* would not be inappropriate. There may be some few cases in which it is impossible to determine whether the effusion be inflammatory in its origin or not. If the serum be turbid, if we can discover in it the smallest admixture of pus, or of flakes of lymph, or if the disease has been marked by the ordinary signs of internal inflammation, we need not hesitate in our opinion. One of the latest systematic writers on dropsy in this country holds that all dropsies are more or less inflammatory. We can see one reason for this mistake (for a mistake it surely is) in the relief and amendment which often ensue upon the employment of blood-letting in dropsy. And this brings me to a very few final remarks concerning the principles upon which dropsies are to be treated.

Treatment of Dropsies.—Of course the first object is to get rid of the preternatural accumulation of watery fluid: and the second object is to prevent its collecting again; in other words, to remedy the diseased conditions which gave rise to the dropsy. Indeed, if we can accomplish this second object without delay, the dropsy will generally disappear of its own accord. In what has been called active or febrile anasarca, general blood-letting is advantageous in several ways. It helps to relieve the congestion, akin to inflammation, upon which the effusion depends: it tends to abate the undue action of the heart: and by emptying the blood-vessels, it facilitates the reabsorption of the effused liquid, and its ultimate ejection from the system.

But although blood-letting is the most direct and certain way of unburdening the loaded

veins, and therefore, in many instances, the most effectual remedy for the dropsy, it is by no means adapted to all, nor even to many forms of the malady. It will always indeed remove a portion of the aqueous ingredient of the blood, but it expends at the same time its fibrin and its red particles. It impoverishes the circulating fluid, and so enfeebles the patient more than the indirect measures, to be mentioned presently, for evacuating the collected liquid. Perhaps, by rendering the blood more watery, venesection may indirectly favour the transudation of its serum outwards whenever the venous current happens to be retarded. It certainly weakens the central organ of the circulation; and to muscular debility of the heart we have already seen that certain forms of general dropsy may owe their origin: and thus it is that ill-timed or excessive bleeding may be the cause of dropsy. In these forms of anasarca, instead of robbing the veins of their blood, we seek to repair the quality and richness of that fluid, and so to restore the deficient tone and vigour of all the muscles, and of the heart among the rest.

In many cases then it is inexpedient to let blood; and we endeavour to empty the vessels indirectly, and in such a manner as to withdraw from them the more watery parts only of their contents. In other words, it becomes our object to augment the discharge of watery fluid from one or more of the secreting surfaces of the body: but it must not be the inner surface of a shut sac.

I noticed before the close analogy that obtains between dropsies and fluxes. Dropsy is a flux into a closed cavity. Fluxes would be dropsies if the fluid poured forth did not escape. And you are to observe that we frequently try to cure a dropsy by producing a flux.

By what surface or channel this artificial drain shall be attempted, is often a matter of great nicety and importance. In some cases we strive to promote the discharge of the superabundant water by the way of the kidneys: in others by the mucous lining of the alimentary canal: in others by the external skin. The circumstances by which our choice must be determined will come under review hereafter.

Passive dropsies are much more difficult of cure than active, and will often baffle our best-directed efforts. You are not, however, to regard those passive dropsies that depend upon the obliteration of a large vein as necessarily incurable; because, if a collateral venous circulation be accomplished, the dropsy will permanently disappear. But we must give nature the credit of the cure in such cases. Time is the best remedy; and all that we can sometimes do is to alleviate in the meanwhile the most distressing or threatening of the symptoms.

I mentioned in the outset of the lecture, that the presence of the dropsical fluid may constitute nearly all the suffering of the patient, as well as much of his danger. Now, when we cannot get rid of the water by bleeding, or by internal remedies which excite serous discharges, we may often afford great present comfort to our patient, and prolong his days, by letting the water out by a slight mechanical operation. *Paracentesis* is the scientific, and *tapping* the vulgar name for this proceeding. It has been performed successfully, by means of a small trocar, to evacuate the water from the brain in chronic hydrocephalus: it is often resorted to for the purpose of emptying the peritoneal cavity, and the tunica vaginalis testis; and it is not seldom practised to let out the fluid of anasarca; for *acupuncture* of the legs and thighs and scrotum is only another form of tapping.

In the local variety of dropsy that is called *hydrocele*, the reaccumulation of the liquid is sometimes prevented by exciting just so much inflammation of the membrane as may cause its opposite surfaces to cohere; whereby the cavity itself being abolished, any return of the disease is rendered impossible.

This is an expedient which we do not dare to employ in other species of dropsy; in ascites, for example: First, because the inflammation itself would place the patient's existence in imminent peril; and secondly, because, if it could be safely conducted, the adhesion and obliteration would seriously embarrass and impede the functions of important organs.

The circumstances which require and justify this mechanical remedy; the rules and precautions to be observed in its performance; and the measures to be adopted for preventing the recurrence of the accumulation, by the removal of its efficient cause, will all be considered in detail when we come to treat of the special forms of dropsy.

LECTURE XVII.

Diseases of the eye. Catarrhal ophthalmia. Purulent ophthalmia of adults.

Having brought my observations on general pathology to a close, I next proceed to the consideration of individual diseases; and I shall take them up, one by one, in that anatomical order to which I adverted in the introductory lecture of this course. That is to say, I shall go *a capite ad calcem:* interpolating those disorders which, although they have a name, have as yet no ascertained local habitation, wherever it may seem most convenient to introduce them. I mentioned before one advantage, as it seems to me, of bringing together, in juxta-position, all the diseased conditions to which the same part, or the same neighbourhood, of the body is liable, namely, the facility thus afforded of comparing the phenomena by which they are characterized, and of discriminating one disorder from another. In taking the parts in succession from the head downwards, we adopt a sort of order, definite enough for the purpose of aiding the memory, and yet free from the trammels that belong to all attempts at arranging diseases according to their essential nature and affinities.

I propose then, in the first place, to speak of certain diseases of the organ of vision. Diseases of the eye occupy a sort of neutral ground, upon which the surgeon and the physician may both lawfully enter. For some of them there are no means of relief, but in manual operations of the most delicate kind. On the other hand, many of the internal parts of the eye require, when diseased, exactly the same species of general treatment which the physician adopts in diseases of *other* internal parts. We seek to change the condition of a small portion of the body, by remedies that act upon and through the system at large. My real and only motive, however, for beginning with a few of the numerous morbid states to which this little part is liable, is this:—that we find, in the eye, more satisfactory and plain illustrations of the *general* facts and doctrines of pathology, as I have been endeavouring to set them before you, than in any other single organ of the body. "Here (to use the words of Dr. Latham, in his little volume on *Clinical Medicine*—a book which I strongly recommend you to study)—here you see almost all diseases in miniature: and from the peculiar structure of the eye, you see them as through a glass; and you learn many of the little wonderful details in the nature of morbid processes, which, but for the observation of them in the eye, would not have been known at all."

"Within the small compass of the visual apparatus," says Mr. Lawrence, "we meet with a greater variety of *structures* than in any other part of the body. Indeed the eye, with its appendages, exhibits specimens of every one of the animal tissues. We find in it bone, cellular and adipous substance, and blood-vessels: mucous, fibrous, and serous membranes; the conjunctiva exemplifying the first; the sclerotica, the sheath of the optic nerve, and the lining of the orbit, the second; the surfaces containing the aqueous humour, the third: muscular, nervous, and glandular parts: common integument, and hairs. Besides these, it contains several tissues of peculiar nature, to which there is nothing strictly analogous in other parts."

The eye itself, taking it apart from its appendages, the spheroidal eye*ball* itself, is scarcely an inch in its longest diameter. Yet it seldom happens that disease, of any kind, occupies the whole, even of this small space, at once. Inflammation, for example, is often confined to one of the tunics of the eye, external or internal; and when it affects

more, it is usually in consequence of the extension of the inflammatory process, from some one texture in which it took its rise. You will not expect me to treat of the vast number of disorders to which the several parts of the eye are liable. I shall bring, I repeat, a few of them only under your notice; and I shall select those concerning which the physician is most frequently consulted; which every one, whatever branch of the profession he may follow, ought to be competent to treat; and, more particularly, which are calculated to elucidate other diseases, and above all, other internal diseases, that are usually assigned to the care of the physician. With the anatomy and physiology of the organ, I may take for granted that you are already acquainted.

I will first briefly inquire into the inflammatory affections of what may be considered the mucous membrane of the eye, which, like other mucous membranes, forms a surface communicating with the external air. Some of these affections are very trifling; some are very severe.

There is a mild form of inflammation of the conjunctiva, which constitutes the most common disease of the eye to which adults are subject. It results, in most cases, from vicissitudes of temperature, or from certain conditions, or sudden variations, of the atmosphere. It is very apt to be excited by exposure to a stream or draught of air, especially in the night, and during sleep. It has a strong analogy—indeed it is the *same disease*, except in *situation*—with that moderate degree of inflammation, produced by the action of the same causes, in the mucous membrane of the nasal cavities, the throat, and the bronchi, which in common parlance we style a *cold* in the *head*, or in the *chest*, as the case may be: and accordingly the inflammation of the conjunctiva of which I speak is often called, by the unlearned, a *cold in the eye:* and the same analogy is expressed in its technical appellation: the cold in the head or chest is termed by nosologists a *catarrh;* and the *cold in the eye* of the vulgar, is with them *catarrhal ophthalmia.* The suddenness (sometimes) of its accession, has procured for it also the denomination of a *blight* in the eye.

The term ophthalmia is at present used to denote inflammation of the eye *generally:* it conveniently expresses in one word what would otherwise require more. Formerly, when the diseases of the eye were not so well understood in this country as they are at present, almost all the inflammatory conditions to which that organ is subject were lumped together under the common appellation of ophthalmia, or *the* ophthalmia. That word now requires some epithet to distinguish the seat or the kind of inflammation that is meant.

It can scarcely be otherwise than interesting to mark the phenomena which occur in catarrhal ophthalmia, when we know that in its cause and nature, it is the same with inflammation of a similar surface, in parts which we cannot so well inspect as we can the conjunctiva. This membrane, as you know, lines the eyelids, and covers about the third part of the globe of the eye anteriorly. The inflammation, in catarrhal ophthalmia, is confined to the conjunctiva and the meibomian follicles. Its leading symptoms are *redness* of the surface of the eye; *some pain* and uneasiness there; an increased *discharge* from the affected membrane and the follicles; and a *sticking together* of the *eyelashes and lids.*

The *redness* is worth notice, both in respect to its tint, and to the arrangement of the vessels in which it appears. It is *superficial;* and of a *bright scarlet* colour; and usually *irregular* or diffused in patches, some fasciculi of vessels being more distended than others. When, however, the inflammation is more intense, the whole surface, except that of the cornea, becomes of a scarlet red. The vessels of the conjunctiva, which are thus rendered visible by inflammation, anastomose continually with each other, and form a network; and they can be slipped and dragged about over the subjacent surface by moving the eyelids with the finger. Frequently some of the meshes of this network are filled up with little patches of extravasated blood; the eye is what is called blood-shot, or to speak learnedly, there is ecchymosis: and sometimes all distinction of separate vessels is nearly lost. In the commencement of the complaint the redness is confined to that part of the conjunctiva which lines the lids; and it afterwards advances gradually, from the angle where it is reflected over the eyeball, towards the cornea.

Now all these particulars are of consequence, since they are diagnostic of the seat of

the disease; and to show this I must mention, by anticipation, the appearance and the arrangement of vessels that are observed when inflammation affects some of the textures that lie deeper than the conjunctiva, and especially the sclerotica. The sclerotic redness is seen *through* the conjunctiva: it is of quite a different *tint* from that of the conjunctiva; instead of showing a *bright scarlet* colour, it is *pink*, or sometimes of a slight *violet* hue; the vessels are much *smaller* and *finer* than those belonging to the conjunctiva, like hairs; and they are *straight* also, and arranged regularly like the radii of a circle; they lie in the sclerotic round the cornea like what is called a glory by painters, or like a halo, or zone surrounding the central cornea; and they *cannot be made to shift their place* by any dragging of the lids. These are very important distinctions. They are such as are easily recognised when two eyes are examined in which the two membranes in question are separately inflamed and vascular; and they are still more palpable perhaps when both membranes are simultaneously inflamed, as they often are, in the same eye. Then, unless the conjunctiva is so universally red as to prevent our seeing the sclerotica through it, the contrast between the larger, more tortuous, scarlet, and reticular vessels of the conjunctiva, and the fine, straight, rose-coloured, radiating vessels of the sclerotic, is exceedingly striking: and those of the conjunctiva, which lie naked on the loose mucous membrane, admit of being slipped about over the fixed zone of vascularity which is presented by those of the fibrous tunic.

The pain that attends catarrhal ophthalmia is slight and trifling. At the outset there is generally some uneasiness when the eye is exposed to the light; but there is no intolerance of light when the disease is fairly developed. The patient complains rather of a sensation of stiffness and dryness, and feels as though there were some foreign body in the eye, between the globe and the lids, especially when the eye is moved; a grain of sand, or gravel, or a little fly. So exact is the resemblance of this feeling, that you can with difficulty persuade the patient that there is nothing of that sort in his eye. No doubt this sensation is produced by the inequality and roughness of the surface consequent upon the irregular distension of the vessels of the inflamed membrane; irritating the organ mechanically, just as a piece of dust would irritate it.

Now in this respect, again, there is a marked difference between conjunctivitis and sclerotitis. In the latter disease, the pain is much more severe, of a dull aching character, with a sense of tightness: the part inflamed is denser, and less yielding than the conjunctiva. The pain is attended also, frequently, by throbbing, and it is felt, even more severely perhaps, in the surrounding parts, than in the eye itself; in the brow, temples, and head. It is a very remarkable circumstance, too, that the pain is distinctly aggravated towards night; increasing in violence from the evening till after midnight, abating towards morning, and ceasing in a great measure during the day, to be again renewed in the evening. I am speaking now particularly of inflammation of the *sclerotica* produced by the same *causes* as give rise to catarrhal ophthalmia: of what is generally called *rheumatic* ophthalmia.

The increased discharge that takes place from the eye in catarrhal ophthalmia is *not* a discharge of *tears*. In the beginning of the complaint there is sometimes a slight degree of lachrymation. But this soon ceases, and the mucous secretion from the surface of the membrane is augmented in quantity, and changed in quality. At first it is somewhat thin, but it soon becomes thicker, and it is often puriform; *i. e.* opaque and yellow: sometimes it retains more exactly the character of mucus, is transparent and viscid; so that the eye looks moist to a bystander, while to the patient it feels gummy. The puriform secretion is not, in general, in any great abundance. You may see it lying in the angle between the eye and the lower lid, by pulling them apart; or it makes itself visible at the corner of the eye, or between the eyelashes along the edges of the lids, which it glues together at night. Sometimes however the discharge is more copious, so as to approximate to what is observed in the less severe forms of another disease I shall presently mention; viz. *purulent* ophthalmia.

There is seldom much swelling of the conjunctiva. If there be any, it results from an effusion of serous fluid into the meshes of the cellular tissue that connects the membrane with the subjacent sclerotica: by which effusion the conjunctiva is partially raised and separated. This kind of effusion often goes to a very great extent in purulent oph-

thalmia, or in violent inflammation of the external membranes, as I shall show you by and by.

So much, then, for the symptoms, and causes, of catarrhal ophthalmia. It is necessary that you should be familiarly acquainted with them; not so much because the complaint is very *serious* in its nature, but because it is *common;* because you are sure to be again and again consulted about it, and because it is of great importance to distinguish it from other forms of ophthalmia, in order to adopt the proper treatment. A mistake of diagnosis might lead to mischievous activity on the one hand; or to still more pernicious inertness on the other.

When the inflammation does not extend beyond the mucous membrane, it will run a certain course, and then, under favourable circumstances, subside. But if it be improperly treated, or if the patient cannot guard himself against a repetition of its exciting causes, it may continue for weeks, and harass him a good deal, and even produce such a change in the inflamed lids as may prove a source of permanent irritation, and of chronic disease of the cornea over which they sweep.

Treatment.—Active general remedies are scarcely ever necessary. The patient should observe the main particulars of the antiphlogistic regimen, and avoid exposure to drafts or currents of air, and to cold and moisture generally. When the external weather is inclement, he should remain in rooms of a uniform temperature. It will be right to purge him in the outset with calomel and jalap, or with calomel followed by a black dose.

If the system at large sympathizes with the local disease, it may become necessary to take blood from the arm, or to apply leeches; but neither of these measures are requisite, except the inflammation is unusually severe, or the disease has been neglected or mismanaged.

After the bowels have been thoroughly cleared by an active purgative or two, remedies which encourage moderate perspiration will be likely to forward the cure: such as warm diluent drinks: five grains of Dover's powder, and immersion of the feet in warm water, at bed-time; and saline draughts containing 2 or 3 drachms of the liquor ammoniæ acetatis, taken at intervals during the day.

But in this complaint *local* measures are of greater importance than those which are addressed to the general system: stimulating or astringent applications to the affected membrane itself. Almost all modern writers on diseases of the eye agree in this. Dr. Mackenzie, of Glasgow, states it as the result of his observation on Beer's practice in Vienna, and of his own subsequent experience, on an extensive scale, at the Glasgow Eye Infirmary, "that general remedies in this disease are inferior to local ones; that *violent* general remedies are worse than useless; and that a local stimulant treatment may almost entirely be relied on." Mr. Melin, in a report of ocular diseases at the General Hospital, Fort Pitt, states that he had treated nearly 300 cases, some of them severe, upon the same principle, without either local or general bleeding; and that he had satisfied himself of the efficacy of this plan of management. And Mr. Lawrence, who for ten years was one of the surgeons to the Ophthalmic Infirmary, in Moorfields, and who during that period had ample opportunities of studying *this* disease of the eye, as well as others, says that it is one to which the use of strong astringents is more particularly applicable. In disorders that have a strong natural tendency to terminate in recovery, it is only by taking advantage of the conclusions derived from extensive observation that we can be quite sure of our ground; and when the same result is reached by different and independent observers, we may safely place confidence in their concurrent testimony.

Dr. Mackenzie and Mr. Melin both employ and recommend the same application; viz. a solution of the nitrate of silver in distilled water, in the proportion of four grains to the ounce. A large drop of this solution is to be applied to the membrane once or twice, or three times, in the course of the day. If the patient recline his head backwards, and the drop be placed in the hollow formed by the internal angle of the eye, it will be diffused over the globe upon the separation and subsequent winking of the lids. After a minute or two this causes a pricking or smarting sensation, which subsides in from ten to twenty minutes, and the eye then feels much easier than it did before the drop was applied. Dr. Mackenzie says that the feeling as if of sand in the eye, is uniformly relieved, and the inflammation abated by the use of this solution, which he speaks of as a remedy of sovereign

utility in the puro-mucous inflammations of the conjunctiva. The eye continues easy, after its application, for five or six hours perhaps; and when the symptoms return, they are again to be met by the introduction of another drop. As the disease subsides, the remedy gives less and less pain, till at last it is scarcely felt. He tells us that "he has sometimes alarmed other practitioners by proposing to drop upon the surface of an eye highly vascular, affected with a feeling as if broken pieces of glass were rolling under the eyelids, and evidently secreting puriform matter, a solution of lunar caustic; and that he has been not a little pleased and amazed at their surprise when, next day, they have found all the symptoms much abated by the use of this application." He declares also that the acetate of lead, and the sulphate of zinc, substances which are much used in what are called *collyria*, or *eye-washes*, are greatly inferior, as local applications, to the nitrate of silver, in this disease.

There is another expedient that requires to be attended to in these cases. When the eyelids are gummed together by the viscid discharge, much hurtful irritation is often produced by the attempts made by the patient to separate them. Now all this may be obviated by greasing their tarsal edges at bed-time with any mild ointment; the spermaceti ointment, or a bit of lard. There is no necessity, as I believe, in this form of disease, to use medicated or stimulating salves: the object is to prevent the mutual adhesion of the lids; and this is accomplished by simple grease.

Purulent Ophthalmia—is another disease of the conjunctiva; differing from catarrhal ophthalmia in degree, in the severity of its symptoms, in the danger which it implies to the sense of vision, and in its exciting causes. It takes its names from the profuse discharge of pus, or of altered mucus which cannot be distinguished from pus, that pours from the inflamed surface. There are three remarkable varieties of purulent ophthalmia; called respectively—1, purulent ophthalmia of newly born children; 2, purulent ophthalmia of adults, or Egyptian ophthalmia, or contagious ophthalmia; and 3, gonorrhœal ophthalmia.

The symptoms of the two last-mentioned varieties, especially in their severer forms, are so much the same, that it would involve us in mere repetition if I did not take them together. In truth it appears to me much the simpler and better mode to look upon purulent ophthalmia as one disease; and to specify, as we go on, the differences by which its several forms are characterized: and not to split it into three different diseases, and to give a separate description of each.

Although purulent ophthalmia is inflammation of the very same part that is inflamed in catarrhal ophthalmia, and differs from it chiefly in degree, it is a hideous complaint, either to suffer or to treat; on account of the rapid progress it frequently makes, and its destructive tendency. The inflammation is *greatly* more intense, the surface becomes, in the worst cases, highly vascular throughout. A copious discharge of thick, yellow, puriform matter speedily establishes itself: this flows out from between the swollen lids, and runs over the cheek, which it often excoriates. At the same time considerable effusion takes place into the cellular tissue that connects the sclerotica and the conjunctiva. You are aware that the conjunctiva extends over the whole anterior face of the globe; but that it adheres so much more closely to the cornea than to the sclerotica, that we might doubt at first whether it did not stop at its margin. This close and firm adhesion over the cornea, and the looser attachment to the sclerotica, give rise to a very singular phenomenon. The conjunctiva is raised to some distance from the subjacent sclerotica by the effusion that takes place between them; and it projects around the cornea in the shape of a large thick ring, leaving the cornea buried, as it were, in a pit: nay, sometimes the swollen and prominent membrane will lap over, so as nearly to exclude the cornea from our sight. The same kind of effusion takes place also, sometimes with great rapidity, into the cellular tissue connecting the conjunctiva with the palpebræ, producing great external tumefaction, and a livid red appearance of the eyelids, which project forwards in large convex masses, and often prevent our seeing the globe of the eye at all: the upper lid especially becoming hard and stiff, and completely overhanging the lower. This swelling from effusion into the subconjunctival tissue is of a pale red, fleshy colour, sometimes marked here and there with patches of extravasated blood. The appearance is called *chemosis:* not ecchymosis, as the similarity of the sound has led some erroneously to suppose, but chemosis. Ecchy-

mosis is a barbarous term of modern fabrication, from ἐκχέω, effundo; and signifies an effusion, and by common consent among medical writers, an effusion of blood. χήμωσις is a genuine Greek word, the root of which is χῆμα, hiatus; and it means a gap or hollow.

Now this puriform or purulent inflammation, so long as it is confined to that part of the membrane which lines the eyelid, is not of any serious importance; but it is prone to extend itself to the cornea, and the whole anterior surface of the eye, causing ulceration or sloughing of the cornea, either in consequence of the actual inflammation of that part, or in consequence of the pressure made upon and around it by the swelling of chemosis. Frequently, when the cornea remains visible, a furrow or trench of ulceration may be seen at its margin; sometimes forming a complete circle, sometimes portions of a circle, sometimes going quite through; and when this happens, or when the cornea bursts, from the effects of deeper-seated inflammation, the aqueous humour is evacuated, and the iris protrudes through the aperture. And even when these horrible consequences do not take place, the eye is often as effectually spoiled for the purposes of vision by an interstitial deposit between the laminæ of the cornea, rendering it opaque, and permanently excluding the passage of light to the retina.

And when neither of these lamentable effects of the inflammation are produced, it is apt to leave behind it a chronic and very troublesome condition of the membrane. The conjunctiva that lines the lids remains thickened and granular, and hard and rough, instead of regaining its natural smoothness and softness, and polish. And the consequence of this is a perpetual irritation of the surface of the cornea, by the mechanical friction of the rough and hard lid in opening and closing the eye, and in the various motions of the eyeball. The continuance of this irritation leads at length to haziness or opacity of the cornea, which becomes traversed also by visible red vessels. Chronic inflammation of its investing membrane is produced, and kept up.

The most severe forms of this disease are attended, at length, with a good deal of pain; doubtless because the inflammation penetrates to the deeper-seated textures of the organ. The pain then presents those characters which I mentioned before as belonging to certain inflammations of the sclerotica: *i. e.* it is pulsative; and sometimes sharp and lancinating, sometimes dull and aching; and it is intermittent, or, if constant, it is aggravated by paroxysms; the paroxysms coming on at night, and abating towards morning; and it is not confined to the eye itself, but extends to the parts around it. This circumorbital pain is characteristic of inflammation of the sclerotica and cornea, and of the internal tunics, the choroid and iris. When the eye is not visible, from the swelling, we may conclude that the inflammation is as yet confined to the *conjunctiva*, when the pain is only scalding or "sandy;" and that it has extended to the sclerotica and cornea when the pain is severe, throbbing, and paroxysmal. In the cases in which the latter kind of pain is felt, the cornea generally gives way; and sometimes this circumstance brings relief to the pain, and sometimes the pain contrives to return after the bursting of the cornea. It is curious that with all this, there is seldom much intolerance of light manifested.

In the earlier stages of this malady, it is entirely local: the system at large is scarcely disturbed at all. But the constitution begins to sympathize and suffer when the local symptoms increase in severity: the pulse becomes frequent, and the tongue white, but there is seldom much thirst or fever; and when blood is drawn from a vein, it does not, in general, exhibit the buffy coat. A good deal of variety in these respects has been noticed however in different cases. Children appear to suffer more constitutional disturbance when labouring under purulent ophthalmia than adults. If there be not much fever, there is always much uneasiness and irritation, and the sleep is broken by the nocturnal accessions of pain.

Such being the general features and course of the disease, at least as it occurs in adults, or in patients beyond the period of infancy, we may next inquire into the circumstances under which it has been observed to occur.

Purulent ophthalmia has been ascertained to be a common disease in hot climates: in India, Persia, and Egypt. It was brought into England, from the latter country, by our troops in the beginning of the present century, after the celebrated struggle which there took place between our army and the French under Napoleon. In this way it got the

name of the *Egyptian* ophthalmia. It naturally excited very great attention at that time, and it does not appear to have been accurately described before.

To give you some notion of its prevalence in certain places and at certain periods, and of its serious nature, I may state that it was ascertained, by returns made from the Military Hospitals at Chelsea and Kilmainham, that on the 1st of December, 1810, there were no less than than 2317 soldiers a burden upon the public from blindness in consequence of ophthalmia; and that in this number those soldiers who had lost the sight of one eye only were not included.

Again, in the year 1804, within nine months, *i. e.* from April to December, nearly 400 cases of purulent ophthalmia occurred at the Royal Military Asylum; and within six years from that time, without including relapses, upwards of 900 cases had taken place in the same establishment.

You will find these statements in a paper in the third volume of the "*Transactions of a Society for the improvement of Medical and Chirurgical Knowledge*," by the late Sir Patrick Macgregor. Many of our best regiments were for a time crippled and rendered unfit for service by this disease; which they carried from Egypt to other foreign stations as well as to this country, especially to Sicily, Malta, and Gibraltar. Nor were the French troops less extensively affected by it. Assalini, who wrote an account of the ophthalmia of Egypt, states that two-thirds of the French army were labouring under it at one time. It occurs also, but fortunately not to such an extent, in civil life. It broke out some years ago, in a large boys' school in Yorkshire; and blindness of one or both eyes, or serious injury to sight, from opacity of the cornea, and other consequences, took place in nearly twenty cases.

You perceive, therefore, that this formidable complaint has been ascertained, within the last forty years, to have prevailed as an epidemic; attacking great numbers of persons living under the same circumstances, and having constant communication with each other. And one of the first questions that naturally arises in one's mind, is whether it is capable of being propagated or not from one person to another by *contagion*. Much difference of opinion has existed on this subject. For my own part I cannot imagine how any one can doubt its contagious properties.

I will give you a case or two, as related by Sir Patrick Macgregor, proving two very important facts; first, that the disease is capable of being excited in the eye of a person, previously healthy, by the direct application of the puriform discharge from an eye affected with this ophthalmia; and secondly, the very rapid operation of the poison so applied.

One of the nurses employed at the Military Asylum, while syringing the eye of a boy who had much purulent discharge, found that a considerable quantity of the matter had spurted into her right eye. This was at 4 o'clock in the afternoon: she felt little or no smarting at the time; but towards 9 o'clock the same evening, her right eye became red and somewhat painful, and when she awoke the next morning, the eyelids were swelled, *there was purulent discharge*, and she complained of pain in the eyeball. The usual remedies were begun in the morning, and she recovered in the space of three weeks or a month. The left eye (into which none of the matter had gone) remained free from disease.

A precisely similar accident happened on another occasion to another of the nurses, except that the matter spurted into her *left* eye, about nine in the morning. Sir P. Macgregor happened to be in the hospital at the time when the accident occurred. He desired the nurse to bathe her eye immediately with luke-warm water, and she did so for several minutes; but notwithstanding this early precaution, about seven o'clock in the evening the left eye began to itch to such a degree, that she could not refrain from rubbing it. When she awoke next morning the eye was considerably inflamed, the lids were swelled, and when she moved the eyeball she had a sensation as if some sand was lodged beneath the eyelid. In the course of the same day purulent fluid issued from the eye, and other symptoms followed which were similar to those of the children under her care. The disease subsided under the usual treatment in fourteen days. In this case also the other eye remained sound.

Another of the nurses in the same institution did not come off so well. She was

sponging, with warm water, the eyes of a boy suffering severely from purulent ophthalmia; and she inadvertently applied the sponge she was using to her right eye. This happened at 8 o'clock in the morning. She mentioned the circumstance to the other nurses, but she took no means to prevent infection. Between 3 and 4 o'clock of the afternoon of the same day, great itching of the right eye came on; and before she went to bed it was considerably inflamed. Next morning her eyelids were swollen, she complained of pain in moving them, and the whole anterior surface of the eyeball was much inflamed; and a purulent discharge began to trickle down the cheeks from the inner canthus. The symptoms increased in severity in spite of all the means employed to check them, and on the fourth day the eyeball burst. The sight of the eye was irrecoverably lost, and the inflammation continued upwards of three months. The left eye did not suffer.

These were cases in which the poisonous matter was *accidentally* applied. But a similar application has been made *intentionally*, and by way of *experiment*, and *with the same results*. Dr. Guillié, of Paris, introduced the puriform secretion, which was furnished by some children affected with purulent ophthalmia, under the eyelids of four other children belonging to another institution; an institution for the blind. These four children were amaurotic, but the external surface of their eyes was healthy and entire. In each instance a regular attack of purulent ophthalmia followed the introduction of the matter.

Facts of this kind, I say, prove, beyond the possibility of question, that the disease may be propagated from a diseased to a healthy eye by actual contact of the puriform matter. Here we have not one case (which might be considered as an accidental coincidence), but several: the morbid secretion is applied to one eye only; the symptoms of inflammation commence, and the regular form of the disease is fully developed within a few hours after the first application of the pus; and that eye only is affected. It is impossible to get over evidence of this kind.

The only questions therefore that can be raised are, whether the disease is ever produced in any *other* way than by contagion; whether it can be occasioned through the medium of the atmosphere impregnated with the specific effluvia proceeding from the diseased part, without any actual contact of the pus in substance; and supposing that the complaint does not always result from contagion, how it is originally produced.

I ought to observe, that independently of such isolated examples of the direct communication of the complaint, by contact with the diseased matter, as I have just laid before you, the history and progress of ophthalmia, since it has been noticed in Europe, are very strongly indicative of its contagious nature. I have already stated that it was not known in Europe till the commencement of the present century—till after the Egyptian campaign, in fact. It is not alluded to by any of the authors on diseases of the eye who wrote previously to that period; although some of the Italian physicians and surgeons, and many of the Germans, had paid great attention to that class of diseases. It spread from Egypt both to France and to this country, and to other places in which detachments of the Egyptian force were subsequently stationed: in Sicily, to wit, and in Gibraltar and Malta. Whenever it has prevailed among our troops at home, *this* circumstance has been *uniformly observed*,—that it first broke out in soldiers who had come from Egypt, or had been in communication with regiments that had been in Egypt. In all cases its origin could be traced to the introduction of fresh troops into the regiment or the barracks.

Again, the manner in which it spreads is exceedingly instructive on this point. It extends itself rapidly, when once introduced, in places where a considerable number of persons are collected together; especially under circumstances favourable to the propagation of contagious maladies; as among soldiers assembled in barracks, where many of the men live in the same apartments, and use the same towels; while the officers, who live in larger and better ventilated rooms, and apart from each other, generally escape. And the good effect, in checking the farther extension of the disease, of separating the healthy from the sick, and of confining every one to his washing utensils, and clothes, and towels and sponges, leads to the same conclusion. Rust, a German author, mentions this striking fact in corroboration of what I have just been saying. The disease broke out in the town of Mayence. This town was garrisoned by Prussian and Austrian troops.

The ophthalmia began to spread extensively among the Prussian soldiery; while the Austrians, who were stationed in separate barracks from the Prussians, in another quarter of the town, remained quite free from it.

Those persons who deny, or who doubt, the contagious nature of purulent ophthalmia, rest their opinions upon some such considerations as these. They hold, in the first place, that the peculiarities of the atmosphere, in Egypt, where the disease has been found so common, are *sufficient* in themselves to account for it;—that the inhabitants of that country never dream of its being caused by contagion. Assalini, who saw the complaint raging in the French army, professes his belief that it did not arise or spread by contagion. He remarks, that the atmospheric conditions which are known to occasion *catarrhal* affections, are very frequent and powerful in Egypt: the days are very hot, the nights chilly, and attended with heavy dews; and the eyes are perpetually exposed, in the daytime, to a dazzling glare of light from the white sandy surface, while the air is full of floating particles of hot sand, which are raised from the ground by the slightest breeze. His opinion therefore, and that of others who saw the disease as it prevailed in that country, was, that it consisted simply of acute catarrhal inflammation of the conjunctiva; and that it affected those persons most who were most exposed to the exciting causes of such inflammation: the common soldiers, therefore, more frequently than the officers.

Other circumstances adduced by the disbelievers, or sceptics in respect to contagion, are, that many who have intercourse with the sick escape the disease; and that when bodies of men, among whom purulent ophthalmia has been prevailing to a great extent, are broken up and dispersed, the complaint is not thereby disseminated, as they say it ought to be, supposing it to be communicable from one person to another; that, in fact, this dispersion, the disbanding of troops for instance, and sending them to their friends and families all over the country, is the surest way of stopping the disorder.

Again, many ineffectual attempts have been made to inoculate the eyes of animals with the matter of purulent ophthalmia. Müller, a German, with that care and industry for which the Germans are so remarkable, took matter from the eyes of patients labouring under purulent ophthalmia early in the morning, before they had washed them, collected it on a camel's hair pencil, and inserted it under both the lids of each eye, in a great number of animals, leaving the pencil there for a few seconds, and then pressing it so as to squeeze the matter out. He also smeared the pus copiously and repeatedly along the edges of the lids. He served in this way five cats, ten dogs, two rabbits, two squirrels, two blackbirds, a starling, a yellow-hammer, and a cock. And in none of them did the inoculation produce the slightest effect.

It is a sufficient answer to these *negative* experiments, however, that other persons were more successful in producing the disease in this manner. Vasani and Gräfe have both excited it repeatedly in dogs and cats, by the application to their eyes of matter taken from human patients. And I have already informed you of many instances in which the disease was generated in men by accidental, and even by intentional, inoculation. No amount of negative evidence can do away with positive testimony so often repeated.

And with respect to the other objections, and especially the *main* objection, that persons may associate and hold close intercourse with individuals labouring under purulent ophthalmia without contracting the disease; I would have you remark that this is no more that what continually happens in regard to diseases that are acknowledged on all hands to be contagious, and to have no other source at present, however they might have originated at first, *but* contagion: the small-pox for example. I think there is good reason for believing, from the facts which I have been relating, that purulent ophthalmia, like the small-pox, is capable of being communicated from one person to another, not only by positive contact, but by transmission of the specific poison somehow for a short distance through the air. But many persons exposed to the contagion of small-pox escape it altogether; and more persons still, perhaps, fail to be affected, though fully exposed, at one time, and yet readily accept the disease at another time, even when the exposure may seem much more slight than on previous occasions. Now what is true of the small-pox may be presumed to be likely, although perhaps in a different degree, of purulent ophthalmia.

As to the circumstance that the disbanding of a regiment infected with the disease pre-

vents instead of favouring its dissemination, that circumstance is really no argument at all against our belief in its contagious nature. We shall see hereafter, that when fever patients are collected in numbers in distinct wards, or in fever hospitals, *that* disease is very apt to be communicated to the nurses and medical attendants of the sick; whereas when such patients are distributed here and there among others, in a *general* hospital, the disease is scarcely ever known to spread. In the one case the poison is *concentrated* and *effective*, in the other it is *diluted* and *harmless*.

Dr. Mackenzie indeed has come to the conclusion, from what he has himself observed, that the discharge in *catarrhal* ophthalmia, especially when it is distinctly puriform, if conveyed from the eyes of the patient to those of others by the fingers, or by towels, and so forth, is capable of exciting inflammation of the conjunctiva, still more severe, more distinctly puriform, and more dangerous, than was the original ophthalmia. And with respect to the disease which I have been speaking of as purulent ophthalmia, or Egyptian ophthalmia, this author calls it, *contagious* ophthalmia; he holds that the inflammation of the conjunctiva, whether in the mild or more severe form, may and often does originate from common atmospheric influences, but that when so caused it may be communicated from person to person, especially when it is attended with a puriform discharge.

And this is an opinion which, I think, is fully warranted by the facts which we are in possession of upon this subject. There is a strange reluctance, which I have never been able to account for, in some medical men, to admit of the operation of contagion, as a cause of disease. There are undoubtedly some difficulties belonging to the doctrine of contagion, and I hope in the progress of the course, and especially when I come to speak of fever, to give that subject the careful attention which its great importance demands; and to enable you to make up your minds respecting it. At present I will only remark, that there is nothing absurd, or unlikely, in the supposition, that diseases may first arise from some other source, and then become capable of spreading by contagion; and that in all cases, even when the contagious principle is most manifest, there seems to be something else required besides the presence of contagious matter; there must be a power of receiving it, a susceptibility of its influence, on the part of the person exposed to it: a predisposition which is less common in regard to some diseases than to others; but without the presence of which there is scarcely any complaint that can be so propagated.

At any rate I would desire to impress upon you the expedience and propriety of *acting*, whatever your doubts or your belief may be, *upon the safe side*. We are bound to proceed, in all questionable cases of this kind, upon the most unfavourable supposition. Very great discredit and loss of reputation have befallen practitioners who, having themselves no belief that a given complaint was contagious, have neglected those precautions which, under a contrary impression, they would have thought necessary. Perhaps they may have sometimes suffered unjustly: but you had better not commit yourselves, especially while you are young in years and in experience, by strong assertions of the noncontagiousness of any disease, the mode of propagation of which is at all questionable. And as for the disease that we are now concerned with, you will do well to act as though it were certainly contagious; whether you meet with it as a sporadic or as an epidemic complaint, whether it be severe in its symptoms, or mild. You should forbid the use of your patient's towels and washing vessels by other members of the family; you should avoid employing the same instruments or sponges to any sound eye which you have been using for one that is affected with this complaint; and you should take care to wash your own hands, after handling a diseased eye, before you apply your fingers to another that is yet, in this respect, healthy.

LECTURE XVIII.

Purulent ophthalmia continued. Gonorrhœal ophthalmia. Purulent ophthalmia of infants. Strumous ophthalmia.

When we last met, I spoke of catarrhal ophthalmia, *i. e.* a mild and common form of inflammation of the conjunctiva, resulting from atmospheric influences. I described its characteristic symptoms, and explained the treatment that has been found most successful for its cure; consisting chiefly in local stimulating or astringent applications.

I began also to speak of the severer forms of inflammation affecting the same part, and included under the head of *purulent ophthalmia*. The symptoms and course and consequences of the two varieties of this complaint, as it occurs in adults, are so essentially the same, that one description of its phenomena is enough. There are certain differences, however, that require to be noticed, in respect to its exciting causes. I laid before you the reasons which satisfy me, that what is called the Egyptian ophthalmia is a contagious disease; and which make it probable that the complaint is capable of being propagated from person to person, through the medium of the air, without the necessity for any substantial application of the morbid secretion from a diseased to a sound eye. These reasons, briefly stated, are as follows: that the disease was unknown in Europe till after the war in Egypt; that, arising among our own and the French troops in that country, it was conveyed by them to various places, and extended itself to soldiers who had intercourse with those troops; that when once introduced it spreads rapidly wherever men are crowded together within a small compass, pay insufficient attention to cleanliness, and use the same towels and utensils; that it has been propagated again and again, by the direct application of the morbid secretion; and that its progress is checked by measures that provide against such accidental application, and by separating the diseased from the healthy.

On the other hand, it has been contended that the disease is nothing more than an extreme degree of catarrhal ophthalmia; that the peculiar conditions of the atmosphere in Egypt and other hot countries, where it is prevalent, are enough to account for it; and that when troops are disbanded, they do not give the disease to their friends and families all over the country, but that, on the contrary, the dispersion of the sick in this way is the most effectual mode of stopping the disease.

To these arguments the proper answer is, that the same difficulties meet us in respect to some other diseases which are acknowledged by all persons to be strictly contagious.

My own creed upon this matter is, that the disease may, and often does arise, independently of contagion, from the agency of ordinary causes of inflammation; and that having so originated, it acquires contagious properties, which develope themselves only under circumstances that favour the propagation of most of the contagious complaints.

Gonorrhœal ophthalmia.—I shall next advert to purulent ophthalmia as it is observed to occur, in the adult, in connection with *gonorrhœa*. If we look to the mere phenomena presented by the inflamed eye, we find nothing to distinguish the gonorrhœal from the Egyptian ophthalmia. Taking the average of a large number of cases, the gonorrhœal is the severer form of the two, and runs the more rapid course. It is said, also, that the inflammation usually commences on the lids in the Egyptian variety, while it attacks the whole conjunctiva at once in the gonorrhœal. But comparing individual instances, these mere differences, and slight differences too, in degree and situation, will not help our diagnosis.

observe the state of the eye. The pupil was completely dilated, and perfectly motionless, though the light fell full upon it. Who can doubt that here the insensibility of the retina depended on the deficiency of its circulation?"

One might ask, also, who can douht that the retina *may* become insensible from a similar state of the circulation in it, brought on by some *long-continued* drain upon the system? Amaurosis of this kind, proceeding from too profuse and protracted a secretion (which may be considered a kind of hæmorrhage), is sometimes noticed in nurses. Mr. Lawrence describes the case of a young mother of slender make, who suckled her first child, which was strong, and took the breast very often: her milk was abundant. After two or three months she began to feel very weak, could not lift a weight, and cried frequently, without having any reason for uneasiness or complaint. She became totally blind, and was led to his house by a friend. He found her pallid, with a small feeble pulse. The pupils were of middle size, and the irides moved slightly. The retina was completely insensible. She could not discern the situation of the window, nor see a lighted candle held close to her. After weaning the child, and using generous diet, she got perfectly well. Some counter-irritation was employed in this instance, but I question whether it had any thing to do with the recovery. Such cases are not uncommon, and their well-known occurrence has probably tended to encourage the notion—too prevalent among both patients and practitioners—that amaurosis is always essentially a disease of debility, and requires tonic and stimulant remedies; bark, and high feeding, and strychnia, and electricity. "Our eyes are *weak*," say they, "and we require strengthening medicines." You must perceive, from what has already been said, how necessary it is to *discriminate* in such cases: to look closely into all the circumstances under which the disease has occurred.

When amaurosis is the result of pressure, or disease, in the course of the optic nerve, or in the sensorium, the complaint is generally less within the power of remedial measures. We cannot say, indeed, in many instances, where the cause of defect lies; and in obscure cases, I should always advise that a trial be made of the mercurial treatment. I have again and again seen slight palsy of some of the voluntary muscles, evidently depending upon some morbid condition of the brain, clear away rapidly upon the affection of the gums by mercury; and the lost power of the retina will sometimes return under the same plan of treatment.

There is something very peculiar in the expression of countenance, and in the gait, of an amaurotic person, by attending to which alone, you may almost recognize his disease. He comes into a room with an air of uncertainty in his movements; the eyes are not directed towards surrounding objects; the eyelids are wide open; to use a strange but common and intelligible phrase, the patient seems gazing upon vacancy; has an unmeaning stare; and there is a want of that harmony of movement and expression which results in a great measure from the information obtained by the exercise of vision. This seeming stare at nothing at all, is not observed in patients who are blind in consequence of opacity of the crystalline lens or its capsule, *i. e.* in consequence of cataract. They, on the contrary, although they cannot see, still seem to look about them, as if they were conscious that the power of sight remained to the retina, although light was shut out from it.

When the amaurosis is incomplete, the motions of the iris are sluggish, and the pupil larger than ordinary. When the blindness is total, the commonest condition of the eye is that of great dilatation of the pupil, so that a mere ring of the iris is visible; and complete immobility of the iris. No change takes place in the diameter of the pupil, under the greatest variation of the light that falls upon it.

Sometimes, on the other hand, though the amaurosis is total, the iris is as active as ever; and this is a very interesting circumstance, and may enable us, in some degree, to conjecture the actual seat of the malady. When the amaurosis is confined to one eye, *this* may happen. You examine the diseased eye, and you find that the pupil enlarges, or contracts, as you diminish or increase the light. But the other eye is open. Shut the sound eye, and try the amaurotic eye again, and you find the pupil fixed, although you vary the light. The motion formerly noticed was sympathetic of the motion of the iris in the healthy eye. We express this otherwise by saying that the *associated* movements of the iris were natural and lively, but its *independent* movements were lost. But some-

times the independent movement is unaffected; nay, the motions of both irides may be perfect, although both eyes are completely amaurotic. I may state, by the way, that *cæteris paribus*, when both eyes are affected, that is a ground for supposing the cause of the disease to be situated within the cranium. And I should come to the same conclusion if, in the case where one eye alone was amaurotic, I found the independent motion of the iris of that eye unimpaired. We know that in the healthy condition of the parts, the brightness of the light admitted to the retina determines the size of the pupil; but the motions of the iris do not depend solely or directly on the retina. It has been ascertained, by experiments made on animals, that the pupil can be made to contract either by mechanical irritation of the *optic* nerve within the cranium, or by irritation of the *third* nerve; a motor nerve which sends filaments to the ophthalmic ganglion, whence the ciliary nerves, passing to the iris, are derived. Now the optic and the third nerves have some link of connection within the brain; and if the morbid condition upon which the amaurosis depends is situate deeper than that point of connection, we may understand, I think, how disease so placed may destroy the power of vision, and yet leave the connection between the retina and the third pair unaffected: and then the influence of light falling on the retina, though it fails to create a perception in the mind, will be reflected back upon the third pair of nerves, and so continue to govern the motions of the pupil. In conformity with these views, M. Andral relates cases in which amaurosis resulting from disease in the cerebellum was attended with brisk movements of the iris.

There are other causes of amaurosis besides those that I have already adverted to. It is sometimes produced by the presence of worms in the alimentary canal. It has some obscure connection with teething, probably through irritation of the facial branches of the fifth pair. A physician of my acquaintance, residing in London, has a young son, who on two or three occasions has caused him great uneasiness, by becoming blind in one eye without any obvious cause, and with no visible change in the organ; but the blindness on each occasion has gone off again, apparently in consequence of the extraction of some teeth which had grown irregularly. I am assured by Dr. Ashburner that such cases are common. Mr. Lawrence relates a very singular instance of *dental irritation* giving rise to amaurosis. A man, thirty years old, was suddenly attacked with violent pain in the left temple near the eye, and in that side of the face generally. The pain continued to recur from time to time, and at length he discovered that he was blind in the left eye. By and by the cheek swelled, and some spoonfuls of bloody matter were discharged by a spontaneous opening in the lower eyelid, and then the pain subsided; but after some months it returned with great severity. The patient then went to Wilna, with the view of having his eye extirpated, and consulted Professor Galenzowski, who found the left eye totally insensible to light, with the pupil dilated, and no other visible alteration. He ascertained, however, that the first molar tooth on that side was carious: it had never caused the patient much uneasiness; and the toothache which he *had* occasionally suffered had not been coincident, in point of time, with the pains in the head and eye. Dr. Galenzowski thought fit to extract this tooth, and was greatly surprised at seeing a small substance protruding from the extremity of the fang. This proved to be a little splinter of wood about three lines in length, which had perforated the centre of the tooth, and had probably been introduced in using a wooden toothpick. A probe passed from the socket into the antrum, from which a few drops of a thin purulent fluid escaped. The pain ceased almost entirely, and on the same evening the eye began to be sensible to light. The vision gradually improved, and on the ninth day from that time, after thirteen months' blindness in that eye, he was able to see with it as perfectly as with the other. M. Galenzowski has since been in England, and he showed Mr. Lawrence the tooth, and the splinter of wood. Doubtless he felt some pride in exhibiting these trophies of his exploit.

Amaurosis is said also to occur as an *hysterical* affection: and I am certain that I have seen this myself. An unmarried lady, of a very nervous and susceptible habit, came to town in great apprehension about her eyes, the sight of one of them being quite gone. I could perceive no defect in the eye itself. I saw her in consultation with Mr. Travers, who took an unfavourable view of the case, and thought the chance of recovery was very slender. I had one reason for hoping a better result, in the knowledge of some facts

which Mr. Travers was not aware of till I mentioned them to him. I had been acquainted with this lady for some years, and during that period she had several times almost entirely lost, and again recovered, the use of her lower extremities. On two occasions she had been affected with aphonia, and unable to speak, except in a whisper, for months together; and then, on a sudden, without any apparent cause, her voice returned. I trusted, therefore, that this suspension of the power of vision in one eye might be a similar freak; and so it turned out. After a few weeks the sight returned, she knew not how; and she has since lost it a second time, and a second time regained it.

Certain poisons will produce temporary amaurosis; and the suppression of certain natural evacuations, as of the perspiration, of the menstrual fluid, and of the bleeding from piles, and the repulsion of certain eruptions, have been charged, by authors, with producing the same complaint.

In those cases in which amaurosis creeps on slowly and insidiously, as it is apt to do from various causes, and more particularly when it depends upon a low and chronic inflammation, engrafted upon habitual congestion of the vessels of the internal tunics of the eye, its approach is marked by sundry curious affections of the vision. The eye feels full or stiff, and sometimes there is pain of the head in its neighbourhood; the patient complains that he sees things through a fog or mist, or as if a thick piece of gauze were interposed between his eye and the object he is looking at. In the daylight, the gauze or fog seems dull and murky, but in the dark it often appears shining, reddish, and fiery: the flame of a candle is seen surrounded with a halo of prismatic colours. That amaurosis of this kind is often really dependent upon local congestion we are taught by the *lædentia;* by the circumstances that aggravate it: thus *straining* of any kind, which augments for the time the fulness of the vessels about the head, will make the mist appear more dense; the same effect may be produced by tying the neckcloth tight; or even by stooping. Boerhaave relates the case of a man who, whenever he was intoxicated, laboured under complete amaurosis: it came on by degrees, increasing according to the quantity of wine he drank; and after the drunkenness went of, his vision returned. Surely these phenomena are very illustrative of the way in which nervous disorders may arise, or be made worse, from mere local plethora, in almost any part of the body.

Sometimes the perfect amaurosis is preceded by a remarkable diminution of the apparent *size* of the objects looked at. A patient told Dr. Farre that a carriage, which happened to pass the window, seemed to him as small as a wheelbarrow, and the horses no bigger than dogs. More commonly ocular spectra become visible: that is, parts of the retina lose their power, or perhaps are eclipsed by turgid vessels: the patient sees flies in the air, *muscæ volitantes*, particles of soot, *blacks*, as we, who live in London, call them, which always float before his eyes, and seem to follow their motions; and which are especially plain and troublesome when he is looking upon a white surface. They multiply in number till the whole becomes dark.

Do not, however, suppose that the appearance of these muscæ volitantes, even when they are permanent, necessarily implies the approach of amaurosis. I should be sorry if it were so, for I see two of them every morning, when my eyes are directed towards a white basin, while I am washing my face. I can find them at other times if I look for them: else, I am not sensible of their presence. They bode no farther evil, if they are associated with no other defect, in function or in appearance, of the instrument of vision.

It is obvious that no particular rules, no rules, that is, which will fit all cases, can be laid down for the treatment of so multiform a complaint as amaurosis. When it manifestly results from disease of the brain, as when it accompanies hydrocephalus, or remains after a stroke of apoplexy, our attention must be directed to the disease from which it has sprung. When there is any reason to suppose that congestion or chronic inflammation of the internal tunics of the eye itself is concerned in the production of the amaurosis, we must adopt the measures that I have already described, as the most likely to remove the congestion; and especially the mercurial plan. When there is ground for suspecting that the blindness takes its rise in vascular exhaustion, and nervous debility, we must have recourse to tonics; bark, preparations of iron, nourishing diet, the cold bath.

After all, you will find too many cases, which will baffle your best directed attempts, and in which you will be required and warranted to try other expedients. When what I

may call rational measures have been expended in vain, you may have recourse to such as are empirical and tentative. There are various *stimulants* which have occasionally been found serviceable: but most of them, I believe, fail much oftener than they succeed. Electricity is one of these: it is applied by taking small sparks from the eyelids, and the integuments round the orbit. The object of this is to rouse the dormant energies of the impassive nerve: and it appears sometimes to do this for the retina, as well as for the nerves supplying voluntary muscles. Mr. Ware tells us that electricity is most beneficial in those cases in which amaurosis has succeeded a stroke of lightning. You must take great care not to employ this remedy when there is any inflammatory action at the bottom of the complaint: it should seldom be tried therefore when the affection is recent.

Strychnia has, of late years, been used for the cure of amaurosis. I shall hereafter take an opportunity of telling you the ordinary effects of that substance upon the body, when given in a certain dose—what is its poisonous operation, and what may sometimes be hoped from it as a remedy. In amaurosis it does good, when it is useful at all, by stimulating the exhausted or atonic nerve into action. With respect to this remedy also I may say—first endeavour to ascertain that it is not likely to do harm: that is, that the blindness is not dependent upon any condition akin to inflammation. Mr. Middlemore, of Birmingham, has probably given this remedy an ampler trial than any other person, and he speaks very favourably of its effects in certain cases: in others he found it to produce so much pain, and spasm, and distress, that he was obliged to discontinue its use. It is not given, in these cases, by the mouth, but applied locally, and Mr. Middlemore considers that it is most efficient when placed over the supraorbitary nerve. He puts a narrow blister over the eyebrow; when it has risen he cuts off the cuticle, and applies a piece of linen, for half an hour, to absorb the serum that continues to ooze forth; then he sprinkles the strychnia, finely powdered, upon the raw part, and covers it with linen smeared with the *unguentum cetacei*. He repeats this every twenty-four hours, cautiously increasing the dose till the vision improves, or some sensible evidence of the agency of the strychnia becomes apparent. He commences with the sixth part of a grain.

I must here leave this subject of diseases of the eye.

In addition to the lessons which I pointed out before as capable of being learned by attending to the disorders of this small organ, I may now mention a few others, of no little moment, since we shall meet with their application again and again, as we proceed to investigate the morbid conditions of other parts. We have seen enough to convince us that mercury, properly administered, has the invaluable power of stopping adhesive inflammation; of arresting the effusion of coagulable lymph from the blood-vessels: that inflammation of the same part may be sensibly modified by the simultaneous agency of some specific poison upon the system, as that of syphilis; or by the presence of constitutional tendencies to disease, such as are observable in gouty and rheumatic people. And we have seen that the functions of a nerve may be perverted, suspended, or abolished, in various ways: by *pressure* made upon it; by a *plethoric* state of its blood-vessels, or by an *empty* state of them; by *inflammation* of its texture, chronic or acute; or even, in some mysterious, or hitherto unexplained manner, by mere irritation of a distant part; by worms, for example, in the alimentary canal; by poisonous substances introduced into the stomach; and by what, in our ignorance, we denominate the freaks and caprices of hysterical disorder. All these lessons we shall find repeated, as the course advances.

LECTURE XXI.

DISEASES OF THE BRAIN AND NERVOUS SYSTEM. DIFFICULTIES OF THE SUBJECT. SHORT REVIEW OF SOME POINTS IN THE PHYSIOLOGY OF THE BRAIN AND NERVES. PECULIARITY OF THE CEREBRAL CIRCULATION. PRESSURE.

Diseases of the brain and nerves.—Having considered some of the most important disorders of the eye, because they afforded me the means of illustrating many of the doctrines and principles which I had previously endeavoured to lay before you, of *general pathology*, I go next to the diseases of that portion of the body, which, though it includes many distinct parts, is called, collectively, the head. I pass over the maladies to which the integuments of the head are liable, because they will be treated of more naturally and conveniently among the cutaneous disorders; and I come at once upon one of the most interesting, and at the same time most difficult and obscure subjects of special pathology —that which embraces the diseases of the brain and nerves. Though it will be a slight departure from the plan I have proposed of taking diseases as they affect different parts of the body from the head downwards in succession, I shall speak of the diseases of the spinal cord, and of the nervous system generally, in connection with those of the brain. To disunite them would neither be easy nor useful.

Difficulties of the subject.—The study of the maladies and disordered conditions of the brain and nervous system, is surrounded with peculiar difficulties: and, accordingly, our knowledge of these diseases is less precise than of the diseases of most other parts of the body.

1. One source of difficulty lies in the circumstance that the structure of the nervous system has no perceptible or understood connection with its functions. We do not discover in the mechanism of this system that adaptation of means to an end which is so conspicuous in many other parts of the body: and consequently, though such adaptation doubtless exists, we are not able to trace the reason or the manner of its interruption. We find in the lungs an apparatus of tubes and cells fitted for the reception of air, upon the expansion of the chest by the contraction of certain muscles; of which muscles also we can see and understand the action. If we meet with any obstruction of those tubes, or any obvious impediment to the play of those muscles, we perceive at once how and why the function of respiration is deranged. But no alterations that become visible, after death, in the brain or spinal marrow, afford us any explanation of the interruption of their proper functions; which are, in three words, *sensation*, *thought*, and *voluntary motion*. An apoplectic cell has no relation, direct or inverse, that we are capable of appreciating, with a sentiment; nor a distended lateral ventricle with the exercise of the will. The morbid anatomy does not in any degree elucidate the disorder, simply because the natural structure throws no light upon the healthy office of the parts concerned.

2. It is a farther source of difficulty, that physiologists have not yet been able to determine, with any thing like precision or certainty, what share the several parts of the brain and spinal cord have in regulating, respectively, the functions which all physiologists acknowledge to belong to the nervous system in the aggregate. There are many and convincing reasons, for believing that the brain is a complex organ; but we can seldom put our finger upon this or that portion of the nervous matter which composes it, and say, *here* resides the influence that governs this or that particular function.

3. Again, the brain and cranio-spinal axis are so encased by their bony coverings, that, in the living body, we are unable to ascertain their physical conditions by means of any

of our senses. Of many parts of the frame we ascertain the state by the sense of sight; and of many parts that we cannot see, we still may recognize the changes by the faculty of touch, or by the ear. The brain and spinal cord we can neither see, nor hear, nor handle.

4. Besides these obstacles to the acquisition of information, by the exercise of our senses, concerning the organs affected; the very disturbance of the functions of the brain cuts us off, in many cases, from that kind of information which we might otherwise derive from the statements of the patient himself.

5. There is a still greater cause of perplexity, with which we have to contend. The very same symptoms accompany alterations of the brain apparently of a very different, nay of the most opposite kind: and on the other hand, changes of structure, which, as far as we can perceive, are absolutely identical in their nature, are associated, in different cases, with totally different symptoms; and more frequently than not, nervous diseases are attended with *no* alterations of structure, appreciable by our senses.

6. And lastly, we are perpetually asking ourselves, when we find the proper functions of the nervous system disordered,—is this disorder the result of disease in the nervous matter itself? or is it merely sympathetic of disease in other parts? for there are few diseases of any kind which do not, in some degree, modify or disturb the due exercise of the offices of the brain and nerves: and it is very difficult often, and sometimes it is impossible, to determine whether and how far the disturbance is primary or secondary.

With all its difficulties, however, the pathology of the brain and nerves is always full of interest. How can it be otherwise, when we reflect that the nervous system is the medium through which we hold communion with the world around us; the stage upon which all the phenomena of animal life are transacted; the instrument of the mind?

And with all its difficulties, there is also a good deal, in the pathology of the brain and nerves, that is fairly made out, and well understood; and we are at present in the right way for advancing our knowledge of this intricate and mysterious subject, by that careful collection of facts, and rigid induction of particulars, that will lead, at length, to a safe and useful generalization.

I shall endeavour to point out to you what is *known* of the morbid conditions of the nervous system; I shall also state the conjectures and probabilities by which our judgment and practice must be guided, when absolute certainty is unattainable. With mere speculative questions, that have no practical bearing, I shall meddle as little as I can.

Physiology of the subject.—Our knowledge, I say, of the exact functions of the different parts of the nervous apparatus, is scanty. Some certainties, however, we possess; and some strong probabilities which almost amount to certainties. Without first expounding my creed upon these matters, it would be impossible for me to explain, as it would be for you to understand, the notions I entertain respecting many of the diseases of the brain and nerves.

Omitting the sympathetic nerve and its ramifications, (for we know but little of its office, and still less of its disorders), the nervous system is made up of certain masses of nervous matter, called the *nervous centres;* and of *nerves* therewith connected.

The nervous centres consist of the cerebrum and cerebellum, the medulla oblongata and the medulla spinalis. I shall include the cerebral hemispheres, and the lobes of the cerebellum, under the common term, the *brain*. So I shall speak of the oblong and of the spinal marrow, in the single phrase, the *spinal cord*, or the *cranio-spinal axis;* their endowments appearing to differ more in degree than in kind.

I adopt the belief that the gray (which are much the more vascular) portions of these nervous centres, form the part in which their peculiar powers reside, or are generated; and that their white or fibrous portions are, like the white and fibrous nerves, mere conductors of the nervous influence.

I incline also to the opinion (recollect, if you please, that I do not press these opinions of mine upon you as being necessarily correct), that the influence which originates in the gray matter, and is transmitted by the white, will at last be found to consist in, or to be closely connected with, some modification of electricity. We know that some of the

effects of this influence may be very exactly imitated, in animals recently dead, by galvanism.

The functions of the brain and nerves are sensation, thought, volition, and the power of originating motion. There may be others; but these four are all that we need, at present, concern ourselves with.

Now it is a part of my creed that the faculties of sensation, of thought, and of the will, belong to the brain: perhaps to the cerebrum alone. The precise office of the cerebellum is involved in much obscurity and dispute. Some of the opinions that have been formed respecting it, I shall notice hereafter.

The chief grounds for believing that the brain proper is, exclusively, the instrument of the mind, are these:—

1. Because this portion of the nervous centres is superadded to the cranio-spinal axis, in the greatest bulk and most complicated form, in man: and after him, in those of the inferior animals which show the largest share of reason.

2. Because, in inferior animals which evince a certain amount of mental endowment, all manifestation of intellect ceases upon the gentle and gradual removal of the cerebrum and cerebellum: the animals continuing to live, for a long time, notwithstanding this mutilation.

Again, it forms a part of my creed on these subjects that the motive power resides in the spinal cord.

The muscles furnish the instruments of motion.

Now there is a certain class of muscles which contract without our willing their contraction; and generally without our being conscious that they are contracting. Such are the heart, the muscular fibres of the alimentary canal, and of the bladder. These are, therefore, called involuntary muscles.

There is another large class of muscles, which obey the bidding of the will, and serve the purposes of prehension, locomotion, and bodily effort. These are considered and called voluntary muscles.

There is still another distinct set of muscles, of which the habitual action is involuntary, yet which submit also to the interposing control of the will. You will call to mind at once the muscles of respiration, which act while we are asleep, or otherwise unconscious; and the sphincters, which regulate the entrances and outlets of the body. Here, I say, the habit is involuntary, but the occasional action is prompted and governed by volition. But sometimes the involuntary action rebels against the willed action and overcomes it. The muscle contracts in spite of the will.

Nay, those muscles which, ordinarily, move only in obedience to volition, do sometimes, under the influence of strong emotion, or of disease, contract independently of any effort of the will, and even in opposition to, and defiance of, the voluntary power.

Under certain circumstances the limbs move with much briskness and force in decapitated animals, in which sensation and volition are extinct. Some physiologists hold, indeed, that sensation and volition are properties of the spinal cord; and they would object to these cases, that no one is warranted in affirming the movements in question to be independent of the will. The animal has no means of informing us whether it feels or not, any more than the human head that has been severed by the axe or the guillotine.

This point, however, has been settled by certain phenomena which are observed to occur, in the human body, under disease. Limbs completely palsied as to voluntary motion, and quite dead as to sensation, do yet, under certain conditions, contract and move when the integuments are pinched; the rational patient not feeling the pinch, and not being conscious of the movements.

Whence does the impulse that leads to motion in these cases proceed—how is the motive power awakened?

The answer to this physiological question has a most important bearing upon the pathology of the nervous system.

It is no part of my purpose to enter into any history of the steps by which this curious problem has been worked out. Its solution is an achievement of our own time; and I may add, of our own country. I profess no more than to sketch, in mere outline, the leading facts that have been ascertained; yet I must, in passing, pay the tribute due to

one indefatigable labourer in this department of science, whose sagacity has enabled him to seize the clue, and in a great measure to unfold the mazes, of the labyrinth in which this part of the physiology of the nervous system was so long entangled. Dim and uncertain glimmerings of the truth appear in the writings of bygone authors, but it was never clearly discerned, and plainly stated, and successfully applied to the elucidation of a large class of disorders, until the publication, in 1832 or 1833, of Dr. Marshall Hall's ingenious and most interesting researches into "the functions of the medulla oblongata and spinal cord." Similar views appear to have suggested themselves, about the same time, to Professer Müller of Berlin. I must recommend you to study the works of these authors; and I may also point out, as fit writings for your perusal (since the doctrines I am now speaking of are comparatively new), Dr. Grainger's *Observations on the Structure and Functions of the Spinal Cord;* Dr. Carpenter's *Inaugural Dissertation on the Physiological Inferences to be deduced from the Structure of the Nervous System in the invertebrated classes of animals;* and a very able paper on the Pathology of the Spinal Cord, by Dr. William Budd, in the 22d volume of the *Medico-Chirurgical Transactions.*

If, on the other hand, you wish to see how nearly the idea, which has been so happily simplified into an intelligible principle by Dr. Hall, was reached by earlier observers, you may consult the writings of Dr. Whytt, upon nervous diseases.

What, then, are the main facts and doctrines, respecting this intricate subject, which modern research has made clearer?

It seems ascertained, that movements of those muscles which acknowledge the empire of the will, depend essentially upon some momentary change in the condition of the spinal cord. This change (whatever may be its nature) is capable of being affected in three several ways.

First, volition, originating in the brain, may send down an influence, which travels with electrical rapidity to the spinal cord; whence, the requisite change having been instantly produced, the motive influence passes, with proportioned speed, along the nerves which connect the cord with the muscles so moved.

Secondly, the change productive of motion may be wrought in the cord, whether the brain be attached to it or not, by mechanical, chemical, or electrical agencies, operating directly upon the cord itself.

Thirdly, the change productive of motion may be wrought in the cord, by an influence carried to the cord, not from the brain, but from the extremities of nerves distributed upon the internal and external surfaces of the body.

The action of this nervous circle, whereby, I say, an influence is first carried from the surfaces of the body, along nerves, to the spinal cord—whence again an influence is transmitted, or *reflected*, as it were, to certain muscles along certain other nerves—has been called by Dr. Hall the *reflex function* of the spinal cord. The apparatus subservient to this function is named by him the *excito-motory system;* the nerves which carry the impression *to* the cord are *incident* or *excitor* nerves; those which convey the motive impulse *from* the cord, *reflex* or *motor* nerves. Mr. Carpenter's terms (which I like better, except for their similarity in sound) are *afferent* and *efferent* nerves.

Dr. Grainger believes that physiology indicates, and anatomy can exhibit, *four* sets of fibres belonging to the nerves and the nervous centres. *Sensiferous*, and *volition* nerves, connected with the gray substance of the cerebrum, and subordinate to the exercise of feeling and the will; and *incident* and *reflex* nerves, connected with the gray matter of the cord, and belonging to the excito-motory system.

Whether this be the true state of the case, or whether the efferent fibres be the same, while the afferent fibres are different; the latter coming to the spinal marrow both from the brain and from the various surfaces, just as two trains may arrive at Euston Square ultimately by the same rail, although the one starts at Derby and the other at Birmingham; or (which is perhaps the better illustration) just as, in some houses, the same bell is made to ring in the servants' hall by pulling, indifferently, the dining room or the drawing room rope:—which of these two hypotheses is the more correct, I am not competent to determine.

This reflex action, independent of the will, and although attended often by conscious-

ness and sensation, yet often also exercised when there is neither, governs the orifices by which air and food are introduced, and excrements are voided. The infant breathes and sucks by it; the adult uses his will for bringing nutriment into his mouth; in both, the act of deglutition, after the food has reached a certain point, is involuntary. The expulsion of the fæces, the urine, the semen, and the fœtus, is regulated by the same function. Nevertheless, most of these muscular acts are capable of being moderated and directed by volition. The reflex power, on the other hand, extends, both in health and in disease, to the entire system of the strictly voluntary muscles; during health it is manifested only in the maintenance of what is called their *tone*, their natural tension and firmness: in disease, as we shall hereafter see, it sometimes acts upon them with terrific energy.

Some of the difficulties which I enumerated in the beginning of the lecture, as impeding our researches into the diseases of the nervous centres, are insurmountable. One or two of them, however, appear to call for a more attentive consideration.

I say we often fail to discover *any* deviation from the natural condition of these nervous centres, or of their appendages; even when the disorder of their functions has been strongly pronounced.

We are not to infer, from this, that no change has taken place in these parts. The only legitimate conclusion is, that the nervous functions are liable to be deranged, impaired, or suspended, by altered conditions, not traceable by our senses, or at least not yet discovered by us, of the organs which minister to those functions.

There may be only one such undiscovered disturbing cause, variable in degree in different cases; or (what is more probable) there may be several such conditions differing in kind. A blow or fall, which *jars* the brain; a sudden mental emotion; an electric shock; a tea-spoonful of prussic acid; any one of these causes may destroy life, yet leave no vestige of its action in the nervous substance upon which it operates. It is probable that the fatal condition is not, in each case, the same.

We may even form a reasonable conjecture of the manner in which the invisible changes are sometimes brought about. We can conceive, for example, that *undue pressure* upon the nervous pulp on the one hand, or *insufficient pressure* on the other, may constitute conditions of the kind we are in search of; and I shall be able, I think, to convince you that such is sometimes the case. Again, we can conceive that such conditions may be furnished by the varying state of the cerebral circulation. In point of fact, we *know* of some changes in the circulation through the brain which have the effect, invariably, first of modifying, and at length, if they are continued, of arresting, the cerebral functions. If *no* blood be sent through the arteries of the brain, death in the way of *syncope* ensues; if *venous* blood circulates in those vessels, it leads to death by *coma*.

But whatever may be the nature of the unknown, and perhaps fugitive, physical conditions of the nervous centres, thus capable of disturbing or abolishing their functions, it is useful to keep in our minds a distinct and clear conception of the fact that there must be some such physical conditions. By steadily retaining this idea of their real existence, we may hope, at length, to get some insight into their nature; which we are the less likely to obtain, if we dwell only on the obvious and visible injuries effected in the nervous substance; associated, as they are apt to be, with so perplexing a diversity of symptoms. Indeed, by the help of this distinct conception, we are at once enabled to reconcile some of the seeming anomalies and inconsistencies to which I before adverted. The same symptoms, I repeat, have been found to accompany physical lesions of the nervous centres, apparently different in kind, place and degree: and, on the contrary, physical lesions, apparently identical in their nature, extent and situation, are attended by different and contradictory symptoms. We must not attribute the symptoms, in such cases, to the visible physical lesions, but to some unperceived condition of the nervous centres, concomitant with those lesions. The *proximate cause* of the *symptoms* escapes our notice. The obvious physical changes may be remoter causes of the symptoms—causes of this proximate cause: but they may also be merely contemporaneous effects of some other remote agency.

Peculiarity of the cerebral circulation.—I have adverted to deviations from the natural

and healthy circulation of the blood through the brain, as being capable of modifying the nervous functions. But there is a very remarkable peculiarity in the circumstances under which the cerebral circulation is carried on *at all times*, which it is very necessary that you should know, or be reminded of. There is nothing like it in any other part of the body. The brain is the only organ which, under the ordinary state of the parts, contains at all times the same quantity, or very nearly the same quantity of blood. This depends upon the mechanical construction of the head, and is capable of explanation upon the known principles of hydraulics. The brain is closely shut up in an unyielding case of bone, and is therefore exempt from the influence of atmospheric pressure: so that, supposing the solid parts unaltered, you cannot empty the blood-vessels of the brain. The cavity being completely full, the blood which circulates in the vessels cannot be materially increased, unless something is displaced or compressed, to make room for the addition. Nor, as it would seem, can the quantity be materially diminished, without the entrance of something to supply the place of the blood subtracted. We should arrive at these conclusions by *à priori* reasoning; and we find that they are confirmed by certain very curious facts, which I will briefly mention, because they suggest considerations of much practical interest and importance.

Dr. Kellie, of Leith, performed, with reference to this subject, a series of experiments upon animals. He bled the animals to death, under various circumstances; and he found, as might naturally have been expected, that all the other organs of the body were blanched and emptied of their blood. But, (what would have been very surprising to him, if he had not been aware of the hydraulic principle just adverted to,) the *brain*, in these cases, presented its ordinary appearance; or even seemed to contain more blood, in its superficial vessels, than usual. In one instance, he describes the sinuses as being loaded with dark blood, and the vessels of the pia mater as being delicately filled with florid blood. In another, the sinuses were charged with blood, the veins of the pia mater were filled, and the choroid plexus remarkably turgid. In a very few cases only did he remark that the vessels of the brain contained sensibly less red blood than in the others; and in all of these few, some serious effusion was observed. Having satisfied himself, by repeated trials, upon these points, he varied the experiment. He first made a small opening in the skull by means of the trephining instrument, taking away a little circular piece of bone, and *then* he bled the animals until they died; and in *all these* cases he found that the brain was as completely drained of red blood as any other part of the body. He did *that* with respect to the blood contained in the brain, which housekeepers do when they tap a barrel of beer. You know that if the barrel is quite full, you may introduce a faucet at its lower part, but no beer will run out through it. The pressure of the atmosphere operates upon that portion only of the fluid which is now exposed to the air, and its effect is to keep the beer in. But if you bore a small hole with a gimlet through the top of the cask, and so admit air into the barrel, the beer will then flow readily through the lower outlet. Dr. Kellie imitated this process of making what I believe is called a *vent-hole*, when he trepanned the skulls of the sheep, upon which he made his experiments, and admitted air to the yielding membranes of the brain.

He availed himself also in these researches of what may be considered the converse experiment. He desired to ascertain whether, under circumstances calculated to gorge the vessels of the *head*, those of the *brain* were or were not made really more full than usual. He examined the brains of two men who had been hanged. When the scalp in these cases was divided, a great quantity of blood escaped, marking plainly enough the congestion of the vessels *exterior to the cranium:* but there was no such congestion observable within; "the sinuses contained blood, but in no extraordinary quantity; the larger vessels on the surface, and between the convolutions, were but moderately filled; and the pia mater was, upon the whole, *paler* and less vascular than we often find it in ordinary cases." I can corroborate the accuracy of Dr. Kellie's observations in these last instances by what I have noticed myself. I paid particular attention to the condition of the head, when the body of Bishop, the murderer of the Italian boy, was examined below stairs. When he was brought here after the execution, the eyes were blood-shotten, and the lips and countenance turgid and livid. The inner surface of the scalp, when it was turned back, and the exposed surface of the skull, were very red and bloody, and in one

part, on the right side of the head, there was some blood *extravasated:* but when the bone had been sawn through, and the skull-cap removed, the large veins of the brain did *not* appear unnaturally full.

In the year 1826, I was present in St. Bartholomew's Hospital, at the opening of the head of a woman who had been hanged, the day before, for murder. I find the following statement in a note which I made at the time:—"The scalp was bloody; but the brain was of a very natural texture and appearance, and not more than usually full of blood."

In France, you know, they execute criminals by means of the guillotine; and some curious speculations have been raised as to whether the head, after decapitation, remains for a short space of time sensible of what is going on or not. These speculations have led to a closer observation of the phenomena that immediately succeed this mode of punishment; and it has been noticed that, although much blood is effused as soon as the head is severed from the body, it comes from the vessels of the *trunk;* and that the arteries and veins of the *head* do not discharge themselves of their contents for some little time. All these facts go, you see, to the same conclusion; viz. that so long as the solid contents of the cranium remain entire, the quantity of blood contained in the vessels does not, and cannot, vary much.

But although the actual quantity of blood in the cerebral vessels may continue the same, it does not follow that the relative quantities contained in the arteries and veins should remain unaltered. The healthy state of the cerebral circulation consists, in all probability, in the nice balance and adjustment of the blood in these two sets of vessels. I have already sufficiently explained to you the consequentes of the circulation of venous blood through the arteries of the brain, showing that without any change of texture in the cerebral matter itself, a mere alteration in the nature of the fluid circulating in its vessels may have fatal results. And the altered ratio of the arterial and venous blood in the organ may, and most probably does, disturb its functions very seriously.

In a very plethoric condition of the body, the arteries which go towards the head partaking of the general fulness, it is not difficult to conceive that there will be an impulse, or effort, *tending* to the propulsion of an undue quantity of blood into the arteries *within the cranium;* and, under certain circumstances, actually producing a fuller state of those arteries, at the expense of the cerebral veins. On the other hand, any sensible interruption of the return of the blood through the veins will virtually augment that impulse upon the arterial current, which arises from the force of the general circulation. It is true that we cannot measure or weigh, so as to compare them together, the actual quantities of arterial and venous blood circulating at any period in the cerebral vessels. We never, therefore, can have any demonstrative proof that the kind of derangement, the alteration of balance, that has just been supposed, does really occur: but, as it evidently, in the nature of things, may occur, so many physiologists believe that it actually does take place, under various circumstances of disease. And taking for granted not only the possibility, but the positive existence of such a derangement, we are enabled to explain many remarkable circumstances connected with the pathology of the brain, which might otherwise be altogether mysterious and inexplicable; we can understand how it may happen that a person shall fall down insensible, become completely comatose, and perish; and yet, on the examination of his brain, there shall be found no trace of inflammation, or of softening; neither extravasated blood, nor effused serum, nor any change that our senses are capable of estimating.

Again, supposing such a derangement as has been alluded to really to exist, and to be attended with disordered functions of the brain, (giddiness, or headache, or drowsiness, or delirium,) there would also exist (by the supposition) a tendency in the force of the general circulation to introduce more blood into the arteries of the brain than they could receive. And one consequence of this would be, an increased flow of blood into the *external* vessels of the *head:* and we know that in apopletic attacks, with such symptoms as were just mentioned, there often are marks of what is called a determination of blood towards the head, of external plethora of the head; such as redness and turgidity of the face and neck, throbbing of the temporal arteries, a loaded condition of the conjunctiva, and so on. Now we see, in this state of things, how it is that we are able to relieve that condition upon which the symptoms and danger depend. We cannot, by blood-letting

or other evacuations, diminish the quantity of blood in the cerebral vessels, but we can take off from those vessels the excessive impulse and stress arising from the general circulation, and even reducing it below what would naturally constitute a healthy impulse; and so leave the vessels of the brain in a state favourable for recovering their proper balance. And that we *have* taken off this unnatural stress, we learn from the disappearance of the *external* redness and turgescence.

We may explain, by the help of this same theory, a very singular phenomenon observed in certain forms of cerebral disease, I mean, the *occasional* recurrence only of the symptoms, although the organic disease itself be *permanent*. For example, we see continually persons who are epileptic: that is, they have fits of convulsion and stupor *now and then*, and appear perfectly well in the intervals. After the death of such patients we sometimes find organic disease of the brain: a piece of bone perhaps projecting from the cranium, or a tumour, or a cyst: and this we are apt to consider as a sufficient explanation of the preceding disease; but we are almost pressed with this difficulty: if the tumour or piece of bone was the cause of the paroxysms, why had the paroxysms any cessation?

It seems probable, or not improbable, that in such cases as these, and in many others, the permanent morbid condition does not interrupt the circulation in the brain, except when there is some increased impulse given to the circulation. The paroxysms are frequently accompanied by outward signs of plethora, or brought on by circumstances that are known to give a temporary increase to the force of the general circulation; such as intemperance, bodily exertion, mental emotion, a costive state of bowels, straining, and the like. A healthy brain might bear the variation thus induced in the pressure with which the blood from the heart seeks to enter the cerebral arteries; in the diseased brain the balance is more easily destroyed.

There is a totally opposite condition of the system to those which I have hitherto spoken of; marked, however, by symptoms of a like kind. And this also may be explained, upon the principle that the functions of the brain require, for their due manifestation, a certain equilibrium of the arterial and venous circulation.

Suppose we have anemia instead of plethora; suppose that the whole mass of blood in the body is diminished: the effect of this upon each artery, (and therefore upon the arteries of the brain,) is that the calibre of the vessel will lessen, and consequently a less quantity of blood will be transmitted towards and into the cerebral arteries; but the whole volume of blood in the brain remains the same; therefore, blood will accumulate more in the veins; and upon this increased mass of blood in the veins, the volume in the arteries will now act with a proportionally diminished and inadequate impulse. The balance of the circulation will be deranged, but in the *opposite direction* to that in which the former derangement was effected. It is probably in this way that the appearance of congestion in the superficial veins of the brain is brought about in animals that are bled to death.

Now disturbances in the functions of the brain, very much resembling those produced by the contrary derangement, will follow from this predominance of the blood in the cerebral veins also. Take one short case, very much to the point, narrated by Dr. Abercrombie, of whose full and clear statement respecting the peculiarity of the cerebral circulation I have made a free use: "A lady, aged 25, had been bled on account of head symptoms, which had supervened upon an injury. Considerable relief had followed each bleeding; but the symptoms had soon returned, so as to lead to a repetition of the bleeding at short intervals; and this had been going on for several months. When Dr. Abercrombie saw her, she was stretched upon a couch: her face of the most death-like paleness, or rather of the paleness of a stucco figure; her pulse very rapid, and as small as a thread; her general weakness extreme. The mass of blood appeared to be reduced to the lowest point that was compatible with life; but she still complained of frequent headache, violent throbbing in the head, confusion, and giddiness. It was evident that evacuations could be carried no farther; and, as a last experiment, trial was made of an opposite system of treatment: nourishing diet and tonics were prescribed. *In a fortnight* she was restored to very tolerable health."

We see scores of cases resembling this every day, in chlorotic girls; in children with

head affections; in women who have lost much blood in parturition; and in various other sick persons. These considerations are pregnant with the deepest interest. Dr. Marshall Hall, and the late Dr. Gooch, have deserved well of the profession and of the public, in having solicited attention to these cases, in which symptoms like those produced by plethora or inflammation are presented, and in which the treatment proper for plethora or inflammation has, for that reason, been too often adopted, with well-meant, but fatal energy. I do not pretend to tell you that our knowledge on these subjects is yet complete: for the points I have been adverting to are comparatively new to our contemplation: but I wish you to bear them steadfastly in mind in your future observation of disease. I am confident that you will often find in them the key to correct and successful treatment. I showed you, in the last lecture, how amaurosis might result from a comparative emptiness of the arteries of the eye: I am tempted here (as it illustrates also what I am at present speaking of) to cite an analogous affection, from a similar cause, of the sense of hearing. A gentleman, thirty years old, was reduced to a state of extreme weakness and emaciation by some complaint of his stomach. As the debility advanced he became very deaf; and this symptom varied in the following instructive manner. He was very deaf while sitting erect, or standing; but when he lay horizontally, with his head very low, he heard perfectly. If, when standing, he stooped forwards, so as to produce flushing of the face, his hearing was perfect; and upon raising himself again into the erect posture, he continued to hear distinctly as long as the flushing continued: as this went off the deafness returned. (*Abercrombie.*) An old clergyman, who is sometimes my patient, is troubled by two grievances; deafness, and an intermitting pulse. They are both always benefited by quina.

Pressure.—I have thought it right to lay before you this view of the peculiar predicament in which the circulation of blood through the cerebral vessels is placed, and the probability thence arising that a disturbance of the equilibrium between the arterial and venous blood in that organ may be a frequent cause of certain derangements of its functions. But there is another principle by which many of the same derangements, that leave no vestige behind them in the corpse, may, with *at least equal probability*, be explained. I mean the principle of *varying pressure* upon the nervous substance. Physiologists say that the cerebral matter is incompressible: I know not on what grounds this opinion rests; but whether it be compressible or not, whether, that is it be or be not reducible by pressure into a smaller compass, it is clearly capable of having different degrees of pressure applied to it, and of being pressed out of its ordinary form. We shall see, hereafter, that by pressure exercised from within, by the distension of what are called the ventricles of the brain, the convolutions on its surface are sometimes flattened, and the natural furrows between them nearly effaced. Pressure there certainly is in what I shall have to describe to you as *hypertrophy* of the brain. There must be considerable pressure on the nervous pulp when blood is poured out within it from a ruptured artery in cerebral hæmorrhage. But the phenomena noticeable when a portion of the skull has been removed by the trephine, show very clearly that the encephalon sustains pressure from varying states of the circulation during perfect health. The surface of the brain, seen through the circular opening in the bone, is observed to pulsate; and to pulsate with a twofold motion. With every systole of the heart, the surface protrudes a little; and it again subsides with the succeeding diastole. This shows that the tension of the arteries, produced by every contraction of the ventricles of the heart, exerts a degree of pressure upon the contents of the cranium. But the brain has an alternate movement also, corresponding with the movements of the thorax in breathing; rising with every act of expiration, and sinking with every act of inspiration. Now, during expiration, the blood escapes less freely from the head through the veins; and thus again vascular fulness is found connected with evidence of pressure on the parts within the head. In further proof of this, if any were needed, I may again refer to Dr. Kellie's experiments. He removed a portion of the cranium of a dog by the trephine. The brain was observed to rise and fall alternately, but so as always to fill the cranium; so that the rise was a sort of protrusion through the opening that had been made. One of the carotid arteries was now opened, and in a minute or two afterwards there was an evident gradual sinking and receding of the brain from the margin of the opening.

It is certain then that, whether the cerebral pulp yields to it or not, there is a constant alternation of a greater and a less compressing force, exerted upon it during life. It is not improbable that this continual variation of the compressing force may be essential to the performance of the cerebral functions. May not the brain be thus incessantly *charged*, if indeed it be (as has been suggested by no less a philosopher than Sir John Herschel) "an electric pile, constantly in action," discharging itself by the nerves, at brief intervals, "when the tension of the electricity developed reaches a certain point?" However this may be, it is equally certain that the compressing force may transgress its natural limits, in either direction; may be too great or too little. The functions of the nervous centres may be perverted, or lost, when the pressure becomes excessive; or, on the other hand, when the pressure is insufficient. And this hypothesis of pressure being at the bottom of many nervous disorders, will explain equally well the obscure cases to which reference has been made. The pressure may cause fatal coma, and yet no evidence of its operation be left in the dead brain; in cases of *permanent* disease with *occasional* symptoms, accidental circumstances may from time to time determine an undue amount of compressing force, or a deficient amount: and I think Dr. Abercrombie has gone too far when he says, "we may safely assert, that the brain is not compressible by any such force as can be conveyed to it from the heart through the carotid and vertebral arteries." Dr. Kellie narrates the following curious circumstance:—"Mr. G., with a numerous train of distressing symptoms, which too well marked the existence of enlargement of the heart, and of the violent propulsive energy of that viscus, had only one, characteristic of any disturbance within the head. On looking upwards to the whited ceiling of a room, he saw a darkened spectrum, which vanished and reappeared with great regularity. It was soon discovered that the appearance of this umbra was synchronous with the systole of the heart, so that he used often, in my presence, to count his pulse with the utmost precision, by keeping his eye fixed on the ceiling, and numbering every appearance of the spectrum."

Objections, I should tell you, have been raised against this theory of pressure affecting the functions of the nervous centres; but I think the objections are susceptible of a satisfactory answer. I must content myself, however, for the present, with having pointed out the main grounds upon which the theory rests. The difficulties that attend it, and the considerations which diminish the force of those difficulties, will come necessarily before us on a future occasion.

The two principles which I have here touched upon, as relating to derangements of the nervous functions—namely, an altered proportion of venous and arterial blood in the cerebral vessels; and the agency of pressure;—may perhaps both run up into one and the same principle. Pressure may act by disturbing the balance of the circulation: or the disturbed balance may act by inducing a variation or shifting of pressure. The evidence in favour of the operation of pressure is, to my mind, the more certain and satisfactory of the two. To the one principle or the other I shall frequently be obliged to refer, in endeavouring to account for the phenomena of cerebral diseases: some of which I shall proceed to consider in the next lecture.

LECTURE XXII.

SYMPTOMS OF CEREBRAL DISEASES.—INFLAMMATION OF THE DURA MATER AND ARACHNOID, FROM EXTERNAL INJURY; FROM DISEASE OF THE BONES OF THE EAR, AND OF THE NOSE. INFLAMMATION OF THE PIA MATER.

THE functions of the brain, summarily expressed, being sensation, thought, and voluntary motion, we naturally look for disturbances of those functions whenever the organ suffers disorder or disease. And experience has made us familiar with various forms of

disturbance to which these same cerebral functions are liable. Let us pass them shortly in review.

1. The faculty of *sensation* may be morbidly keen, or morbidly obtuse; or it may be perverted: in other words, it may deviate in degree, or in kind, from the healthy standard.

The sensations referred to the several surfaces and structures of the body, and to the organs of sense, may (without any fault in those parts and organs) be preternaturally acute. Tenderness ascribed to different parts, their natural sensations being heightened into pain; a general state of irritability; intolerance of light, and of noise; are so many instances of this over-sensitiveness of the percipient organ.

Under the head of diminished or defective sensation may be ranked, numbness in all its degrees, up to total loss of sensibility or *anæsthesia;* dulness of hearing, deafness; dimness of sight, blindness; failure, or absolute extinction, of the senses of taste and of smell.

Perverted sensations, sensations unnatural in kind, are very numerous. To mention a few: giddiness; nausea; ringing in the ears; ocular spectra; ill smells in the nostrils; false tastes on the palate; itching; and sundry uneasy feelings, many of which are indescribable. Various kinds of pain belong to this class; spirits violently high; causeless depression, anxiety and dread.

2. Innumerable degrees and varieties of disturbance of the faculty of *thought* are met with. Delirium in all its shades; dulness and confusion of intellect; sundry defects of memory; incapacity of judgment; and every degree of stupor up to complete coma.

Of the function of *voluntary motion* there are also various kinds and gradations of derangement: twitchings of the muscles; tremors of the limbs; rigidity from spasm; irregular and involuntary jactitation; convulsions; muscular debility; palsy.

Now, as I stated before, there is no physical exploration of the living brain. We are limited therefore, in studying its diseases, to the rational symptoms. It becomes our task to interpret the import of the multiform disturbances of function just enumerated, in every case in which more or fewer of them appear; and when you are told that these symptoms are apt to present themselves in almost every conceivable order and combination, and, moreover, that many of them may be sympathetic of diseases of other parts than the brain, you will scarcely need to be farther informed, that the language they speak is often very hard to construe; that we frequently fail to reach and discover, by these outward signals, the inward things they denote.

I am about to consider, in the first place, some of the inflammatory affections of the brain, and some which may easily be mistaken for inflammatory affections; and I warn you beforehand, that, in respect to exactness of diagnosis, we are sadly barren of certainties in these matters. Hints, sketches, approximations, are nearly all that I can promise concerning not a few of the many diseased conditions to which the brain and nerves are obnoxious.

In the brain, as in other composite organs, inflammation may be general or partial. It may attack certain tissues only; it may be seated in the substance of the cerebral mass, or in the membranes that envelope it.

I need not tell any of you that the membranes which invest the brain are three in number; the fibrous dura mater, the serous arachnoid, and the pia mater, which is composed of blood-vessels held together by a web of cellular tissue.

Speaking generally, inflammation of the cerebral substance alone, is perhaps more common than inflammation of the investing membranes alone. The central parts of the nervous mass may and do suffer inflammation, while the membranes escape. But it seems to me scarcely possible that inflammation of the pia mater should take place without implicating also the surface of the convolutions, with which it has so close a relation, and so intimate a vascular connection.

Again, with respect to the membranes themselves, the dura mater may be inflamed while the pia mater remains unaffected. I believe also that the arachnoid may suffer inflammation, and leave the subjacent pia mater untouched. Whether the arachnoid ever escapes participating in the inflammation of the dura mater on the one side, or the pia mater on the other, is to be doubted.

Can we separate and distinguish these several inflammations by assigning to each its

proper external phenomena? Seldom; scarcely ever. Doubtless each has its peculiar symptoms; and if inflammation was often strictly limited to the one membrane or the other, and if the course and events of the inflammation did not modify the condition of the brain itself, by pressure, or by affecting the circulation of blood through it, then we might expect greater uniformity, and hope by careful and repeated observation to seize upon the desired distinctions. But this simplicity is not exhibited by the inflammatory affections of the parts within the cranium. Inflammation commencing in one membrane is apt to spread readily and rapidly to the rest, and to the cerebral substance; and the complication of diseased conditions coexisting within the skull at the same time, throws confusion over the whole subject. This uncertainty of exact diagnosis is however of the less consequence, inasmuch as when we have learned that inflammation is going on in any part of the encephalon, we have learned enough to direct us as to the general plan of treatment to be adopted.

After all, certain symptoms do present themselves more frequently when one part is inflamed, and certain other symptoms more frequently when another part is inflamed; and it will be proper and convenient to contemplate certain forms of meningeal inflammation separately.

Inflammation of the dura mater.—Let us first then consider inflammation as it is confined, occasionally, to the dura mater—or to the dura mater and arachnoid.

This very rarely happens as an idiopathic or spontaneous disease; but it is not at all uncommon as a result of external injury. And we may advantageously trace its ordinary phenomena and consequences, by attending to these instances of traumatic inflammation of the dura mater. They were excellently well described, many years ago, by Mr. Pott. A man receives a blow on the head; the blow stuns him perhaps at the time, but he presently recovers himself, and remains, for a certain period, apparently in perfect health. But after some days he begins to complain; he has pain of the head, is restless, cannot sleep, has a frequent and hard pulse, a hot and dry skin, his countenance becomes flushed, his eyes are red and ferrety; rigors, nausea, and vomiting supervene; and, towards the end, convulsions and delirium. Meanwhile the part which was struck becomes puffy, tumid, and sometimes tender; and if this tumid portion of the scalp be cut through, the pericranium beneath it is found to be separated from the bone; and the bone itself is observed to be altered in colour, whiter and drier than the healthy bone; and if a piece of this bone be removed, it is also seen that the dura mater on the other side of it is detached from the cranium, and sometimes smeared with lymph or puriform matter. This is a form of disease very often met with by the surgeon. I have watched, with much interest, several such cases under the care of my hospital colleagues. One or two of them I will briefly describe.

In the year 1833, during Christmas time, the coachman of a lady living in my neighbourhood fell, being intoxicated, into a cellar or area, struck in his fall one side of his head, and tore up the scalp over a considerable space. He was carried to the hospital, where the loose flap of integuments was cleansed and replaced. After some days erysipelas came on, and then a much larger portion of the scalp sloughed away, so that the bone was laid bare to a frightful extent, and looked, at a little distance, as he sat up in bed, like the tonsure of a monk. Nevertheless the man seemed wonderfully free from suffering or distress: his pulse, indeed, was frequent, but it was said to be so during health. His intellect was clear, and he had *no* head symptoms; or rather, no brain symptoms.

In the early part of February, 1834, he had a shivering fit, which was followed by convulsions of the right side of the body, and subsequently by paralysis of the right arm and leg, and by stupor, from which he could easily be roused. He would put out his tongue when desired to do so; but to every question he answered "yes." A portion of the left parietal bone was evidently dead: here the trephine was applied; and a piece of bone being removed, the dura mater was exposed. It looked as if it also had lost its vitality. Some pus lay upon it. No relief followed the operation.

On the 10th of February, fluctuation was detected beneath the dura mater, which was slit open. About three drachms of puriform fluid escaped. The patient died soon afterwards, having had no active delirium throughout.

The surface of the dura mater was found to be nearly of its natural appearance, except where the trepanning had been performed. At that spot it was dry and sloughy. Over the whole of the anterior and lateral surface of the left hemisphere lay a thick coating of coagulable lymph, smeared with pus: this extended down the posterior part of the hemisphere also, nearly to its base. There was no other morbid appearance; no fluid in the pia mater, or in the ventricles. The substance of the brain was everywhere perfectly sound and healthy: it was divided in all directions in search of an abscess, but nothing unnatural could be detected.

Another man came to the hospital to have a small incised wound of the scalp looked at. The injury appeared to be trivial; the cut was dressed, and the man made an outpatient. A few days afterwards he came again, imperfectly paralytic on one side of the body. I saw this man's skull trepanned: he was perfectly calm and collected: that part of the dura mater which corresponded to the wound was found to be inflamed; and there was pus diffused over the arachnoid covering the cerebral convolutions on the same side. He sank quietly into a state of coma, and so died. Not the slightest incoherence or delirium had been manifested, there had been no convulsions, nor was there any other morbid appearance within the cranium.

I mention these cases to show you the grounds of my own opinion that inflammation, beginning in the fibrous membrane, *may* affect the arachnoid, without *necessarily* extending to the pia mater; just as inflammation *may* overspread the pleura, or the pericardium, without touching the lung or heart which those serous membranes respectively invest. Here no sensible traces of inflammation were discovered, deeper than the free surface of the arachnoid; and there had been no disturbance, till towards the end, of the proper functions of the brain. I conclude that the disease did not pass beyond the serous membrane; for I can scarcely conceive inflammation of the pia mater to exist without involving, in some degree, the surface of the brain; nor inflammation of the surface of the brain to exist without some manifest derangement of the cerebral functions. In the instances that I have been relating the final stupor and palsy may reasonably be ascribed to *pressure* resulting from the *events* of the inflammation of the arachnoid; from the effused pus and lymph.

Inflammation of the dura mater is very rare as a simple and idiopathic affection. Dr. Abercrombie relates one instance of it, as the only one he had seen; and even that was not a pure case of inflammation of the dura mater. There was pus upon that membrane; and it adhered to the cranium over a space as big as a crown-piece; and at that spot it was ulcerated. But there was also found an adventitious membrane *beneath the arachnoid* where it covers the brain.

Speaking generally, this complaint is marked by pain of the head, by fever, and by rigors which intermit; and so regular sometimes are the intermissions, that the practitioner may be tempted to believe that he has got an aguish patient, and to administer bark. The intellectual functions, especially at the outset of the disease, are but little affected; which is just what we might expect.

Although inflammation of the dura mater is very uncommon as an idiopathic or primary disorder, we very frequently meet with it as a secondary affection; and then there are few diseases more surely fatal, or less within the reach of remedies. It is as a consequence of what is called *otitis*, that physicians are chiefly accustomed to encounter inflammation of the dura mater. It results from disease of the internal ear, and of the petrus portion of the temporal bone. Sometimes acute inflammation arises within the tympanum, when there has been no previous disease: the patient has severe earache; at length a gush of matter comes from the external meatus, but the pain does not, as it usually does in such cases, cease; it continues, or even increases in intensity: the patient begins to shiver; he becomes dull and drowsy; slight delirium perhaps occurs; and by degrees he sinks into stupor. In some instances no pus issues externally. More commonly symptoms of the same kind supervene upon a *chronic* discharge of purulent matter from the ear. It is scarcely possible to sketch an accurate general picture of this insidious, but most dangerous complaint. Next to seeing and watching actual cases of it, the best way of becoming acquainted with its phenomena is by attending to recorded instances. I will bring before you, therefore, some examples of inflammation of the dura mater, occurring in connection with disease of the interior of the organ of hearing.

A youth, sixteen years old, applied to the late Dr. Powell (who has related the case in the fifth volume of the *Transactions of the College of Physicians*) on account of an eruption, with an acrid discharge, behind the right ear. He had become deaf five years before, after scarlet fever, but no discharge took place at that time from the ear. In the following year, however, he had the measles, and then an abscess formed in the right ear, and after giving him much pain, it burst. He had again suffered, three days before Dr. Powell first saw him, a sudden attack of very severe pain in the same ear. The pain quite deprived him of rest; but he had no fever, nor delirium, nor coma. He slept, indeed, a great deal, but that was the effect of opiates, which he took to relieve the pain. This symptom was quieted by the opium; but it always returned with severity if the medicine was suspended. A fœtid discharge came from the ear. On the tenth day of this attack, after a most violent paroxysm of pain, his strength rapidly declined, and he died.

"When the head was examined, the structure of the dura mater was healthy and natural, but beneath this membrane the whole superior surface of the right hemisphere was covered with a layer of coagulable lymph and pus. The vessels of the substance of the brain were not more numerous or loaded than usual, and the brain itself was healthy in every part. In the base of the skull the dura mater adhered to the bone, except at one part, of about half an inch diameter, just over the petrous portion of the temporal bone, where it was black and sloughy. The subjacent portion of the bone itself was carious, black, and crumbling; and contained fœtid pus."

In this case, you will observe, there was no symptom to mark the extensive mischief within the head, except the *pain:* the pulse never exceeded 72; the skin was warm and moist; there was neither fever, nor delirium, nor convulsion, nor coma.

A girl, aged nine, (I take this case from Dr. Abercrombie, whose volume on the diseases of the brain is full of practical and instructive *examples*) had been liable to attacks of suppuration of the ear, which were usually preceded by severe pain, and some fever. She suffered one of these attacks in the left ear, in July, 1810, from which she was not relieved, as before, when the discharge of matter took place, but continued to be affected with pain, which extended over the forehead. When Dr. Abercrombie saw her, he found that, besides the pain, she had some vomiting, and impatience of light. Her look was oppressed; the pulse 84. Blood-letting, purging, blistering, and mercury, were employed without relief. Two days afterwards there was slight and transient delirium, a degree of stupor, and slight convulsions. She lay constantly with both her hands pressed upon her forehead, and moaning from pain, of which there had not been the least alleviation. On the fifth day from the commencement of the discharge, she continued sensible, and died suddenly in the afternoon, without either squinting, blindness, or coma, the pulse having been always under 90. A considerable quantity of colourless fluid was found in the ventricles of the brain, which, in other respects, was healthy. In the left lobe of the *cerebellum* there was an abscess of considerable extent, containing purulent matter of intolerable fœtor. The dura mater, where it covered this part of the cerebellum, was thickened and spongy, and the bone corresponding to this portion was soft, and slightly carious on its inner surface; but there was no communication with the cavity of the ear.

Here again the pain was the most prominent symptom, and probably resulted from the partial inflammation of the dura mater. It is interesting to mark these two points:—that the disease in the bone imparted disease to the dura mater, although no passage was opened from the tympanum; and that this inflammatory state of the external membrane of the brain led (apparently) to deep-seated suppuration in the cerebellum; the parts lying between the abscess and the dura mater escaping.

This last, and somewhat singular circumstance, might have been owing (so at least I conjecture) to the introduction of pus from the suppurating ear, into some of the cerebral veins, and the consequent formation in the cerebellum of one of those secondary abscesses so commonly noticed in uncircumscribed phlebitis. Two very remarkable instances of diffused inflammation of veins, and of its terrible effects, occurring in connection with purulent otorrhœa, have fallen under my own observation: one of them in private practice, the other in the hospital. As I am not aware that such consequences as supervened in these cases upon otitis, have received much attention, I will briefly describe them.

The first of these two patients was a boy, eleven years old, whom I attended with Dr. Mac Intyre, and Mr. Arnott. He had had a discharge of offensive purulent matter from his ear since the time when, four years before, he had gone through scarlet fever. In August, 1833, he went, for a walk, into Kensington Gardens, and there lay down, and slept upon the damp grass. The next day he was attacked with headache, shivering, and fever. Strong rigors, followed by heat and perspiration, occurred very regularly for two or three days in succession; suggesting a suspicion that the complaint might be ague; but then pain and swelling of some of the joints came on, and were, at first, considered rheumatic. However, the true and alarming nature of the case soon became apparent. Abscesses formed in and about the affected joints, and one of these fluctuating swellings was opened, and a considerable quantity of foul, grumous, dark-coloured matter let out. After about a fortnight the child sunk under the continued irritation of the disease. The hip-joint presented a frightful specimen of disorganization; it was full of unhealthy sanious pus, the ligamentum teres was destroyed, the articular cartilages were gone, and matter had burrowed extensively among the surrounding muscles. The knee and ankle-joints of the same limb were in a similar state. It is curious that the destructive disease of the joints was limited to those of the right lower extremity, while the primary suppuration was in the left ear. Unfortunately the head was not examined; but that the fatal disorder had penetrated from the ear to the dura mater, I entertain no doubt; in all probability the inflammation had involved the veins and sinuses of the head.

The second case had many points of similarity with this.

William Marriott, aged nineteen, was admitted under my care into the Middlesex Hospital on the 18th of October, 1834, having pain and tumefaction of the right shoulder, wrist, and foot, with redness of the latter. He complained also of headache, vertigo, drowsiness, and of an occasional feeling of stupor. His skin was hot and dry, his face flushed, his tongue furred, his pulse frequent (112), and his bowels were relaxed. A puriform discharge came from his right ear.

He had been suddenly seized, a week before, with sharp pain in that ear, which lasted twenty-four hours, when the discharge began, and the pain was relieved. He then began also to have headache, which had never left him, and to be sometimes dizzy. Three days previously to his admission the rheumatism (as he supposed it to be) commenced in the foot. When this part was examined, the redness was found to be circumscribed, somewhat livid, and limited to the great toe. It had much the appearance of gout.

He soon began to be troubled with shivering fits, which recurred regularly every morning about the same hour, and were followed by burning heat of the skin, but no sweating. An abscess formed near the toe, and was opened by Mr. Mayo, and some healthy looking pus evacuated. Next a large fluctuating tumour near the shoulder was punctured, and three ounces of pus, mixed with blood, came out. After this incision the rigors ceased; but the abscesses continued open, and the discharge had an offensive smell. On the 14th of November it was discovered that matter had collected in the left hip: this also was emptied by puncture. On the 1st of December, a very large quantity, not less than three pints, of unhealthy and grumous pus, was let out from a vast abscess which had formed in the loins: and pus was noticed in his stools. The discharge from the shoulder came at last to resemble the lees of port wine.

During all this while the patient remained feverish, with a dry parched tongue, and a rapid and feeble pulse. The diarrhœa continued, more or less, throughout. For some time before his death, which happened about the middle of the month of December, the left leg and thigh had been much enlarged by œdema.

I was not able to be present at the inspection of the body; and I have to regret that in the report which I received of it, the condition of the brain, of its membranes, and its veins, was not noted.

The right shoulder-joint was extensively diseased; the cartilages were destroyed by ulceration over a considerable space. Those of the left hip were entire, but the synovial cavity was full of foul matter. The joint of the great toe was implicated also in the abscess which had formed there. The femoral vein, on the left side, was plugged up, throughout its whole extent, by a coagulum, which was firm and of a reddish brown colour at the upper part of the vessel, loose and darker towards the ham. The saphena was pervious: the iliac was free from disease.

The lungs had undergone partial disorganization. Several distinct portions of the pulmonary tissue were nearly solid, while the tissue immediately around them was crepitant and healthy. From the vessels belonging to these solidified portions, purulent matter could be expressed.

The mastoid cells of the right temporal bone were filled with pus, and there was a slit-like opening in the membrana tympani. The small bones of the ear were sound.

I much lament that in these two instances, the direct link of connection between the disease of the ear and the disorganization of the joints was not demonstrated: for seeing (they say) is believing. Yet the pain of the ear, the discharge of pus from the external meatus, the subsequent pain of the head, coming on with fever and rigors, and followed after a short interval by destructive suppuration in several distant parts, and, in the last case, the actual femoral phlebitis: these circumstances form a chain of presumptive evidence, amounting, in my judgment, to moral certainty, that the fatal mischief, in each case, found entrance through "the porches of the ear;" and that the dura mater underwent inflammation. The same evidence is scarcely less affirmative of the complication of cerebral phlebitis. The immediate vicinity of some of the sinuses to the diseased bone, and their formation by a duplicature of the dura mater, would seem to render such a complication highly probable.*

Dr. Griffin has published, in the *Dublin Journal of Science*, two cases of otitis attended with symptoms exactly resembling those of intermittent fever. One of them is as follows:—A young man, previously healthy, was attacked with fits of shivering, accompanied by pain in the left side of the head. At first, the paroxysms were rather irregular, but they soon assumed the form of *tertian ague;* coming on every other day, at about the same hour; the cold fit commencing at noon, and lasting about half an hour, followed by a hot stage of somewhat longer duration, and terminating in profuse perspiration. In the intermissions the pain in the head was trifling: there was no thirst, or heat of skin, but he did not sleep. A tumour formed over the mastoid process of the left side, and was opened, and a quantity of extremely offensive brownish pus sprang out with great force. This gave much relief. The bone was carious over a space as big as a shilling. After about ten days, the pain in the head and in the mastoid process became very severe: the patient had violent shivering fits many times in the day, great thirst, heat of skin, vomiting, and delirium; his face was flushed, and his pulse hard; and he died within a few hours after the accession of these last symptoms.

The most remarkable features in this case were the similarity of the fits of shivering to the paroxysms of ague, their regular recurrence at periods of forty-eight hours, and the circumstance that they seemed, for some time, benefited by the treatment proper in ague; namely, the exhibition of bark. The occurrence of *quotidian* paroxysms of the same kind has been noticed in relating some of the previous cases.

I have related them to show you what different symptoms may result from inflammation of the dura mater; and to put you upon your guard against overlooking the cause from which such inflammation does frequently originate. The suppuration of the tympanum, and consequent disease of the bone, are more common in scrofulous persons than in others; and they are more apt to occur as sequelæ of scarlet fever than in any other way. I conceive that the inflammation of the throat, which belongs to that disorder, and often constitutes all its danger, creeps along the eustachian tube into the interior of the ear. In strumous subjects the fire thus lighted smoulders on, or if it ever goes out, is readily rekindled; that part of the temporal bone, in which the organ of hearing is principally lodged, becomes carious; the membrana tympani is perforated; the little bones of the ear come away; more or less deafness ensues; and from time to time, or habitually it may be, there is a discharge of pus from the external orifice. At length the inner surface of the bone participates in the disease; and then the inflammation is apt to be propa-

* Almost while this lecture was passing through the press, an interesting communication from Dr. Bruce, of Liverpool, upon the very point here discussed, has appeared in the MED. GAZETTE (pp. 608 and 636 of the present volume). Dr Bruce narrates two cases, witnessed by himself, of "Phlebitis of the Cerebral Sinuses as a Result of Purulent Otorrhœa:" and he refers to several others recorded by different authors. This combination of disease is doubtless more common than has been heretofore supposed; and the important pathological considerations connected with it will probably receive further illustration, now that the attention of the profession has been called to the subject by Dr. Bruce's paper.—T. W.

gated to the dura mater, in the manner of which I have given you some instances. It is in the first onset of the inflammation in the ear that remedies are most likely to be efficient in preventing this catastrophe. Leeches applied early and repeatedly to the mastoid process, especially when that part becomes tender, as it often does in such cases, and counter-irritation afterwards, are the best means in our possession. If symptoms of inflammation within the head supervene, the complaint requires more vigorous treatment, which I shall describe when I have spoken of inflammation of the other membranes of the brain. After what has been said, it is unnecessary to point out to you that the prognosis in these cases is very unfavourable. But we are not to abandon them in despair. That inflammation of the dura mater may be recovered from, we know, by what happens in certain injuries of the head: and the following would seem to be an instance of recovery when the source of the mischief was situated in the ear:—A young lady, after the usual symptoms in the head, lay for three or four days in a state of perfect coma, and her condition was thought utterly hopeless. Her medical attendants continued to visit her as a matter of form; and one day they were agreeably surprised to find her sitting up, and free from complaint: a copious discharge of matter had take place from the ear with immediate relief; and she *continued* in good health.—(*Abercrombie.*) We cannot be sure in such a case that the matter came from the brain; but the symptoms made that supposition exceedingly probable. The case shows clearly one of two things; either that pus may thus escape from the interior of the skull and the patient get well; or that pus shut up in the cavity of the tympanum may produce the urgent symptoms that are known to result from cerebral pressure.

Cases are recorded of analogous disease communicated from the carious *æthmoid* bone to the dura mater; the patients having had pain in the forehead and purulent discharge from the nose, and becoming at last forgetful and delirious, and dying in a state of coma. I have never met with an instance of this kind; nor of inflammation spreading inwards from the socket of the eye: but I make no doubt that both may occasionally happen.

These three then—*idiopathic* inflammation of the dura mater—very rare; inflammation of the dura mater by extension of disease from the æthmoid bone, or from the orbit —also infrequent; and inflammation of the dura mater by extension of disease from the petrous portion of the temporal bone—very common: constitute those forms of inflammation of the outermost tunic of the brain, which the physician may be called upon to treat. The inflammation is not always—nay, perhaps it is seldom, if ever—restricted to that tunic; but it begins there; and the essence of the disease is inflammation of the *dura mater.*

Acute *arachnitis*—by which I mean active and *uncombined* inflammation of the arachnoid membrane—is, I apprehend, a very uncommon disorder; although that term is of frequent occurrence in medical writings. I have shown you already that inflammation may pass from the fibrous dura mater to the serous membrane reflected over it; and thence (by what is sometimes called contiguous sympathy) to the opposite portion of the same membrane spread over the surface of the brain. So, likewise, inflammation may extend from the pia mater to the arachnoid. If simple arachnitis, of an acute kind, ever happens, it has not been my fortune to see, or recognize it; and I can tell you nothing about it. In truth, the authors who use the term arachnitis do not intend thereby to express unmixed inflammation of the arachnoid; but include under that appellation inflammation of the pia mater also. Some apply the name *meningitis* to that compound affection; and the only objection to this nomenclature is, that the dura mater is as much one of the meninges of the brain as the other two.

Inflammation of the pia mater.—In the few remarks which I have to make upon inflammation of the pia mater (or, if you will, of the pia mater and arachnoid at once) I shall chiefly follow Dr. Abercrombie: because his observations are comparatively recent, and carefully made; because his veracity, and sobriety of judgment, and philosophical turn of mind, are well known; and because his cases (as regards this particular affection) are quite to the point, and his descriptions clear and concise.

But I must premise a word or two respecting the anatomical characters of the disease.

When the upper part of the skull, and the dura mater, have been removed, you may frequently see, on the surface of the exposed brain, what seems to be a thin layer of a clear gelatinous substance; but this appearance is fallacious. Puncture here and there the transparent arachnoid, and a limpid fluid, like water, trickles out; and the jelly-like investment of the convolutions is gone. Now this thin serous liquid, thus collected in the meshes of the pia mater, may be the event of inflammation of that membrane; but it may also be produced, and it very often indeed is produced, by simple congestion and remora in the cerebral veins: we cannot, therefore, with any certainty, infer, merely from seeing this serous effusion, that there has been inflammation: we judge of its import, by noting the coexistence, or the absence, of other traces of inflammation; and by the character of the symptoms that preceded death.

On the other hand, we may be sure that there has been inflammation of one or both of these tunics of the brain when we find *false membranes* between them; layers, *i. e.* of coagulable lymph. In the effusion of this substance I conclude that the vessels of the pia mater play the main part; both because it is always, in such cases, excessively vascular, while the arachnoid is seldom found to be so in any remarkable degree, if at all: and also, because this false membrane commonly, though not always, sends down processes, between those duplicatures of the pia mater which descend into the sulci formed by the convolutions; where, as you know, the arachnoid does not go. In fact, considering the arachnoid as the serous membrane of the brain, we should expect that, when inflamed, it would present the events or products of inflammation on its free surface; and we sometimes find them there; but this is very rare; and for my own part, I look upon those effusions which lie beneath the arachnoid, between it and the pia mater, as being furnished exclusively by the vessels of which the latter membrane is mainly composed.

Now the inflammation of these membranes (taking them together) commences, and declares itself, by no fixed or uniform symptoms. The most common and striking phenomenon is a sudden and long-continued paroxysm of *general convulsions*. Sometimes this is the first thing noticed; sometimes it comes on after a few days of discomfort and headache; and among the symptoms thus preceding the convulsions, *vomiting* is of frequent occurrence: the convulsions recur, and at length end in coma. Sometimes, again, the first attack of convulsions comes on after violent pain in the head, setting in quite suddenly, and attended with screaming. I do not find in any of the various examples given by Dr. Abercrombie any mention made of violent or continued *delirium*; which is generally set down by authors as marking inflammation of the membranes. He does give cases, indeed, in which there *was much* delirium; but they were not cases of meningitis of any kind. He relates them as instances "of a very dangerous modification of the disease, which shows only increased vascularity." I venture, with great humility, to question or criticise any opinion of Dr. Abercrombie's: but I entertain no doubt about the nature of the cases which he so describes; and I hope to convince you by-and-by that they are not examples of inflammation at all: they neither show the anatomical characters of inflammation, nor yield to the remedies of inflammation. Excluding these cases, I do not find delirium marked as a symptom of uncombined meningitis. I shall abridge one or two of the well-marked examples of the disease.

A girl, aged nine, woke suddenly in the middle of the night, screaming from violent headache, and exclaiming that some person had given her a blow on the head: for the next two days she complained of some, but not much pain in her forehead, and did not even remain constantly in bed; no alarm was felt about her. On the third day she was seized with violent and long-continued convulsions, and immediately after the convulsions she fell into a state of deep coma: she remained in this state, with a natural pulse, till she died, on the sixth day of the disease.

When the dura mater had been removed, the other membranes appeared highly vascular, except where this appearance was concealed by a layer of yellow adventitious membrane, spread out betwixt the arachnoid and the pia mater. This was distributed in irregular patches over various parts of the surface of the brain, but was most abundant on the upper part of the right hemisphere. It was as thick as a wafer, and in some places dipped down between the convolutions. A considerable quantity of it extended over the surface of the cerebellum also.

A child two years old was suddenly attacked one morning with severe and long-continued convulsions. The convulsions recurred many times; in the intervals she was dull and torpid, in a state of partial coma, with occasional starting, and a frequent and feeble pulse. On the fourth day she sunk.

The surface of the brain, when the dura mater was removed, was covered in many places, betwixt the arachnoid and pia mater, by an adventitious membrane. It was chiefly found above the openings between the convolutions, and in some places appeared to descend a little way between them. The arachnoid when detached seemed to be healthy; but the pia mater was in the highest state of vascularity throughout; and when the brain was cut vertically, the spaces between the convolutions were most strikingly marked by a bright line of vivid redness, produced by the inflamed membrane. There was no effusion in the ventricles, and no other morbid appearance.

In another example, the whole surface of the brain was covered by a continued stratum of yellow false membrane, lying between the arachnoid and pia mater, and in some parts following the course of the pia mater through the whole depth of the convolutions; the pia mater and arachnoid adhered together everywhere, very firmly, by means of it: not a trace of it could be found either on the outer surface of the arachnoid, or the inner surface of the pia mater: the arachnoid itself, when separated, presented no unusual appearance, but the pia mater was everywhere excessively vascular. There was no serous effusion, and the brain and cerebellum were perfectly healthy.

Now in this dissection there was unequivocal evidence of acute and extensive inflammation of these membranes, or, I should say, of the pia mater; yet the symptoms had been very obscure. The child in whom the disease occurred was convalescent from a mild attack of scarlet fever. One evening he became very feverish, and complained of his belly. Three days afterwards he had frequent *vomiting*, followed by coma, and some convulsive movements of his face and arms, and death took place four days and a half after the feverishness began. We learn from this case, that general and severe inflammation of the innermost membrane may exist, and prove fatal, without giving rise to any violent symptoms at all.

I must trouble you with one more history, because it affords another example of what I have mentioned as being rare; viz. the effusion of the products of inflammatory action upon the outer surface of the arachnoid, and marking therefore very distinctly the inflammation of that membrane. It was evidently combined, however, with inflammation of the *pia mater* also. A child, eight months old, died after more than three weeks illness; which began with fever, restlessness, and quick breathing; afterwards there were frequent convulsive affections, with much oppression; and at last severe convulsions, squinting, and coma. At an early period of the complaint, a remarkable prominence of the anterior fontanelle was noticed; in the second week this *increased* considerably; and in the third week, it was elevated into a distinct circumscribed tumour, which was soft and fluctuating, and pressure upon it occasioned convulsions. It was opened by a small puncture, and discharged at first some purulent matter, afterwards bloody serum. No change took place in the symptoms, and the child died four days after.

A deposition of thick flocculent matter mixed with pus was found covering the surface of the brain to a considerable extent, and lying *upon* the free surface of the arachnoid. There was a similar deposition also between the arachnoid and the pia mater, and considerable effusion into the ventricles.

If the sketches I have been giving you afford a true outline of the phenomena which attend acute inflammation of the pia mater, or of the pia mater and arachnoid jointly, what, you may naturally ask, is the nature of those cases in which there is high excitement, and much fever, and great delirium, and which are sometimes spoken of as *phrenitis*, or as brain-fever? Why these *are* instances of acute inflammation of the whole contents of the cranium; of the brain *and* its membranes; of the encephalon in short; and, therefore, the disease has, not improperly, been called *encephalitis*. Of this formidable malady I shall give you some account to-morrow.

LECTURE XXIII.

ACUTE AND GENERAL INFLAMMATION OF THE ENCEPHALON. PERIOD OF EXCITEMENT. MODES IN WHICH THE DISEASE MAY COMMENCE. PERIOD OF COLLAPSE. TREATMENT. DELIRIUM TREMENS.

ACUTE inflammation does sometimes appear to invade at once the whole of the parts that are lodged within the skull; or, beginning in one part, it extends rapidly to all the rest. As the contents of the cranium are called, collectively, the encephalon, so the disorder which I am about to consider has been named *encephalitis*. It is an uncouth appellation, but it will serve its purpose. Cullen, and many others, apply the term *phrenitis* to the same disease. You may choose between these names, taking care to remember what they signify. The malady is sometimes described as inflammation of the *membranes* of the brain. I believe this to have arisen from the circumstance that the effects of the inflammation, which become visible after death, are often more striking and obvious on the surface of the brain, or in its ventricles, than in the cerebral substance itself. An *abscess* in the nervous mass can scarcely be overlooked: a *softening* of the cerebral pulp may escape the notice of a hasty or inexpert observer: and those changes of colour, which sometimes denote increased vascularity of the same part, may very easily be passed over without attracting much attention.

Phrenitis, or encephalitis, or acute and general inflammation of the brain and its membranes, as it occurs in adults, presents two periods, which are marked by different symptoms, and in most instances are very distinctly observable. In the first period what are called symptoms of excitement predominate; the functions of the organ are exaggerated as well as disordered: in the second period those symptoms appear which are comprised under the term collapse. Sometimes these two periods, instead of following each other, are more or less mixed and confounded together. But the distinction is real, and requires to be attended to.

Period of excitement.—The symptoms that characterize the period of excitement, are pain of the head, often intense and deeply seated, or extending over a large part of it; a sense of constriction across the forehead; throbbing of the temporal arteries; flushing of the face; injection of the eyes, which have a wild and brilliant look; contraction of the pupils; preternatural sensibility to external impressions, amounting frequently to impatience of light, and of sound; violent delirium; want of sleep; paroxysms of general convulsion; a parched and dry skin; a frequent and hard pulse; a white tongue; thirst; nausea and vomiting; constipation of the bowels.

You are not to look for all these symptoms in every case; nor to conclude that your patient has not inflammation of the brain because the symptoms I have been enumerating do not all present themselves, or do not take place in any regular order of succession.

Modes in which the disease commences.—In fact, we find, in actual practice, that encephalitis is apt to come on, to commence I mean, as far as symptoms are concerned, in three or four different ways.

Sometimes there is a sudden alteration of manner, and the patient, complaining probably of his head, becomes all at once and furiously delirious; and fever is lighted up. These are symptoms which cannot pass without notice, and which immediately direct one's attention to the head. They may, however, be fallacious, as we shall see by and by.

In other cases the first thing noticed is nausea and vomiting: and these symptoms may soon cease; or they may continue several days, and even sometimes throughout the whole

course of the disease. Great quantities of yellow bitter fluid are brought up, evidently containing a good deal of bile; and whatever is introduced into the stomach, even a small quantity of the most simple drink, is immediately rejected. With this state of matters there is generally much constipation, and the bowels refuse to act except under the influence of strong purgatives.

It is important to attend to these symptoms; for occurring, as they usually do, with headache, they may easily deceive a person who is not previously aware of what they may portend. If the patient has not been accustomed to sick headaches, and if the epigastrium and abdomen be natural, not tender, nor distended, as they are apt to be when the stomach itself is in fault, we have the more reason to look narrowly into the case, and to suspect that some serious mischief, of which the nausea is a token, is going on in the brain. I may observe by the way, that where there is much vomiting of bile, persons are apt, both patients and their doctors, to blame the liver, to set down the disorder as bilious; but you ought to be aware that whenever vomiting is often repeated, or long continued, bile is to be expected in the matters brought up: the action of the duodenum, as well as that of the stomach, is inverted; and the bile passes in the wrong direction. If you have ever suffered from sea-sickness, you must know that after the puking has gone on for a little while, bile is constantly voided.

Again, some cases of acute inflammation of the brain set out neither with sudden and great disturbance of the intellectual functions, nor with sickness and vomiting, but with a paroxysm of general convulsion, such as often ushers in an attack of meningitis. This symptom, according to Andral, is a much more certain sign of cerebral inflammation, than the occurrence of active delirium: and I quite agree with him in so thinking.

It is probable (but I speak conjecturally only) that this diversity of symptoms, marking the onset of encephalitis, may depend upon the part in which the inflammation begins; and that it is soon propagated from that part to the whole of the organ. I should suppose that when nausea and vomiting are the earliest symptoms, the inflammation has taken its point of departure in the cerebral pulp; in the substance of the brain: and that when the attack comes on with a sudden fit of convulsion, the inflammation has commenced in the pia mater or arachnoid. This is consonant with what we know of inflammation of those parts, when they are separately affected. Again, it seems to me presumable that the cases which are characterized by early and fierce delirium are cases in which the inflammatory action has invaded the whole of the encephalon, substance and membranes, simultaneously. I say I offer these as conjectures of my own: what it is of importance for you to remember is, that inflammation of the brain does commence in the three several ways that I have been describing.

There are some cases, however, that cannot be brought within even this general rule. They begin in some irregular or obscure manner, or with some unusual phenomenon. Andral states that he has seen a few striking instances of inflammation of the brain, of which the first sign was a sudden loss of the power of speech: and Dr. Abercrombie relates a very remarkable case in which the same thing happened. I call it very remarkable, as well on account of the singular manner in which the disease first showed itself, as that it furnishes an example of encephalitis produced by direct exposure to intense heat of the sun—insolation; an event very uncommon in our climate. It occurred in the practice of a surgeon at Selkirk, in Scotland:—

"A young man, aged 16, bathed twice, on the 5th of June, 1818, in the river Tweed. After coming out the second time he lay down on the bank, and fell asleep without his hat, and with his head exposed to the direct beams of a hot sun. On awaking, he was speechless; but walked home, and seemed to be otherwise in good health. He was bled and purged, and the next day recovered his speech, but lost it again at intervals several times during the three or four following days. He was forgetful, and his look was dull and heavy: he made little complaint, but when closely questioned said he had a dull uneasiness at the back of his head. After a few days more he had squinting and double vision, and a very obstinate state of bowels, and his pulse was 60. After farther bleeding the pulse rose to 86; but he sank gradually into coma, and died on the 30th."

The substance of the brain in general was found highly vascular, and a very considerable extent of it was in a state of softening mixed with suppuration. The ventricles were

distended with fluid, and the membranes in many places were much thickened. One very curious circumstance (affording perhaps some explanation of the readiness with which the inflammation was produced) was, that the cranium was of very unequal thickness at its upper part; one spot, as big as a sixpence, was as thin as writing paper, and transparent.

Period of collapse.—However, the phenomena which I mentioned at first constitute the common and ordinary symptoms of acute inflammation of the brain and its membranes. They continue for a variable period; from twelve hours, to two days, or more; and then they are succeeded by others, which characterize the second stage of the complaint, or the period of collapse, as it is called. They result, I apprehend, from the events and products of the inflammatory action; the violence of which is over or abated. The patient ceases to complain of headache; instead of being excited or wildly delirious, he mutters indistinctly, and falls into a state of stupor, from which it is difficult, and at length impossible, to rouse him. His vision and hearing are no longer painfully acute, but dull, or perverted; strabismus and double vision are not uncommon; and the pupil, from being contracted to the size of a pin's head, becomes first oscillating, then widely dilated, and ultimately motionless. The patient is not shaken, at this period, with violent convulsions; but twitchings of his muscles, and startings of their tendons come on, and some of his limbs are agitated with tremors, or become powerless and palsied; the countenance is ghastly and cadaverous; cold sweats break out; the sphincters relax: at length the coma becomes profound, and life ceases.

The disease, when it proves fatal, as it too often does, mostly runs a rapid course. It may kill in as short a time as 24 or even 12 hours; or the patient may struggle on for two or three weeks. The morbid appearances met with in the dead body are very various. Serous or puriform effusion into the ventricles, and into the meshes of the pia mater; layers of coagulable lymph between that membrane and the arachnoid; softening of the cerebral substance, with pus infiltrated into the softened parts; or great vascularity, shown by a pink or purplish mottling of its cut surface, giving it a stained appearance.

Treatment.—Let us next consider the treatment required for this frightful disorder.

It is quite plain that in an organ so essential to life, and of such delicate organization as the brain, and in which changes so irreparable in their nature as many of those I have just enumerated, so readily take place under acute inflammation, we cannot hope to be of much service unless we see and treat the case at an early period. On this account it becomes exceedingly important to recognize the nature of the disease at its very commencement; and, therefore, I have taken pains to point out to you the various forms which it may assume, while it is yet within the reach of remedial measures.

The principal of those measures are *blood-letting, purging, and the application of cold to the head.* All the particulars of the antiphlogistic *regimen* are to be rigidly observed; the patient should be kept as much as possible in silence, and darkness, with his head high, and on a firm pillow. And the antiphlogistic *remedies* are to be employed with decision and energy.

Blood-letting.—With respect to bleeding I can only repeat what I have said before: the blood should be taken in a full stream, and suffered to flow till some decided impression is made upon the pulse; or until syncope occurs, or is evidently at hand. After the patient has rallied a little, blood should be taken by cupping or leeches from the back of the neck, or the temples, or the mastoid processes; and these depletory measures must be repeated according to the violence or continuance of the symptoms which first demanded them.

Cold.—The application of cold to the head is a remedy of great importance in this disease. The head must be first shaved; and the mere removal of the hair is sometimes followed by a manifest abatement of some of the most urgent symptoms; of the pain, for example, and of the delirium. In cases such as I am now supposing, it will not be enough to apply wetted cloths to the head: the application must be colder than the ordinary temperature of cold water; and it may be made colder by ice; and the best way to ensure a permanent depression of the superficial heat is to put some pounded ice with a little water into a thin and flexible bladder, and to lay it on the patient's head: there should not be too much ice, or its weight may be injurious. This is generally very grate-

ful and pleasant to the feelings of the patient; and we often have the satisfaction of perceiving that, with the reduction of the external heat of the head, there is also an evident mitigation of the violent symptoms; the agitation and delirium are calmed, and the patient sleeps, or recovers his senses.

Another excellent and most powerful method of applying cold, is by pouring cold water in a slender stream upon the vertex of the head, until it produces some marked effect. Of course this, as well as all other strong measures, must be adopted with great caution, and its influence closely watched: I mean it is not to be left to the discretion, or indiscretion, of domestics and nurses. Dr. Abercrombie tells us that he has seen a strong man, submitted to the operation of this cold douche, "thrown in a very few minutes into a state approaching to asphyxia, who immediately before had been in the highest state of maniacal excitement, with morbid increase of strength, defeating every attempt of four or five men to restrain him." He gives an instance of the use of this measure in a somewhat different morbid condition, which I will quote, because it shows in the first place the striking power of the remedy: and, secondly, the simple mode of applying it. A strong plethoric child, five years old, after being for one day feverish, oppressed, and restless, fell rather suddenly into a state of perfect coma. She had been in that state about an hour when Dr. Abercrombie saw her. She lay stretched on her back motionless, and completely insensible; *her face flushed and turgid.* She was raised into a sitting posture, and, a basin being held under her chin, a stream of cold water was directed against the crown of the head. In a few minutes, or rather seconds, she was completely recovered; and the next day was in her usual health.

This measure also is to be repeated, according to the circumstances of the case.

Some persons recommend that a constant dripping of cold water upon the patient's shaven head should be kept up. This may easily enough be managed by means of a sponge and funnel placed a little above the head. Andral mentions his attending with another physician, (M. Recamier) a young man who laboured under all the symptoms of acute inflammation of the brain. Cold water was made to drop slowly upon his head, and a complete recovery took place, although no other active treatment of any kind was adopted.

In strongly recommending this efficient remedy to your notice, you will not understand me to advise that it should supersede the use of copious blood-letting: it is to be employed as an auxiliary to the lancet; not as a substitute for it.

Purgatives.—The third remedy which I named, that is to say, purging, is also of great importance and efficacy. But it must be *hard* purging. There is a great tendency to obstinate constipation in most cases; and this must be overcome, and free and frequent evacuations from the bowels obtained: five grains of calomel and fifteen of jalap should be followed in three or four hours by a strong black dose; and after that I should give, in such cases, three or four grains of calomel every four hours, and repeat the black dose at least every morning, until the symptoms gave way. If the mercury thus exhibited affects the gums, so much the better; but we must not, in this disease, combine it with opium, to prevent its passing off by the bowels. Dr. Abercombie uses this strong language in reference to the value of purgative medicines in acute inflammation of the brain. "In all the forms of the disease active purging appears to be the remedy from which we find the most satisfactory results: and although blood-letting is never to be neglected in the earlier stages of the disease, my own experience is that more recoveries from head affections of the most alarming aspect take place under the use of very strong purging than under any other mode of treatment. In most of these cases, indeed, full and repeated bleeding had been previously employed, but without any apparent effect in arresting the symptoms." He has found the croton oil the most convenient medicine for this purpose.

Dr. Abercrombie is disposed to regard mercury as being useful in affections of the brain, chiefly in virtue of its purgative operation: and the opinions of a physician of his large experience, and observing mind, must and ought to have great weight. But I must not conceal from you my own persuasion that, in the early periods of acute inflammation of the encephalon (and it is of the early periods that I have hitherto been speaking), if the mercury comes in a short time to produce its specific influence upon the gums, a great change for the better will often be perceived: such is the result of my own observation. Recollect, however, that you are not to give calomel with the direct object

of affecting the gums, but as part of the purgative plan, and you take the chance of its specific effect. You must not combine opium with it, for two reasons; first, you would thereby shut up the bowels, and deprive yourself of the use of one of your best weapons; and, secondly, you would incur the risk of augmenting and perplexing your patient's head symptoms, and of puzzling yourself; since you would not be able to determine how much of the coma that ensued was owing to the progress of the disease, how much to your remedy.

When the second order of symptoms have arrived, those which are included under the general phrase of collapse, and which commonly result, I fancy, rather from the products of the inflammation than from the inflammation itself; from softening, that is, and from pressure exerted by effused serum, or lymph: when this order of symptoms make their appearance, I believe the time for doing much good by active bleeding has gone by. If, however, blood-letting has not yet been employed, and especially if the pulse continue hard, whether blood has been already abstracted or not, it will be right to give the patient the chance of that remedy. Of the propriety of doing so, take the following illustration:—"A girl, aged eleven, had violent headache and vomiting, with great obstinacy of the bowels; and these symptoms were followed by dilated pupils, and a degree of stupor bordering upon perfect coma; pulse 130. She had been ill five or six days: purgatives, blistering, and mercury to salivation, had been employed without benefit. One bleeding from the arm gave an immediate turn to this case: the headache was relieved, the pulse came down, the vomiting ceased, the bowels were freely acted upon by the medicines which they had formerly resisted, and in a few days she was quite well." (*Abercrombie.*) I must recite one other case—from among many which go to the same effect—to show the occasional influence of *hard purging*. "A young man who had had cough and dyspnœa, and been bled for those symptoms, appeared convalescent. One evening he became affected with headache, and some vomiting. About midnight, having got out of bed to go to stool, he fell down in a state of violent and general convulsion. The convulsion returned during the night six or seven times with such violence that one of the paroxysms continued without intermission for an hour. The pulse, during the night, varied from 60 to 120. (I should have mentioned before these great and rapid fluctuations of the pulse in respect to its frequency, as being a very common circumstance and sign, in inflammatory affections of the brain.) At first it was found impossible to bleed him, on account of the violence of the convulsions; but about seven in the morning a full bleeding was obtained, after which the convulsions ceased, except some slighter attacks during the day, which appeared to be arrested by pouring cold water over his head. The next day he was oppressed; with occasional tremors of the limbs, and some vomiting, and he had one or two threatenings of convulsion. He took repeated doses of active purgatives with little effect; and on the following morning he appeared to be sinking into a state of perfect coma, with a pulse at 50. Croton oil was now given, which operated powerfully seven or eight times. He passed a good night; and the day afterward was free from complaint.

Having this evidence of the separate efficacy of the three remedies—blood-letting, strong purgatives, and the local application of cold to the head—we have much encouragement to put them into combined operation in these very serious cases, especially when we have the opportunity of using them at an early period. Should the disorder happily yield to these measures, great care will long be required on your part, and great prudence on the part of the patient and his friends, lest the recent mischief should rekindle. A relapse is even more perilous than the first assault of the disease. Such prudence and care will consist chiefly in the avoidance and denial of all that might excite and disturb the brain; whether it be a premature return to animal food; or indiscreet and fatiguing interviews and conversations; or the too early resumption of the cares and concerns of business.

Are we to employ blisters in this disease? Not in the outset, during the period of excitement. They only add to the irritation, and make matters worse. And especially you should avoid putting them, as many are apt to do, upon the head itself, at that stage of the disease. We should not suppose, *a priori*, that they could then, and in that place, have any beneficial effect. They cannot divert the blood from the inflamed part;

but they may attract it into the external vessels of the head, and therefore towards the encephalon; and so add to the stress of the general circulation upon the arteries of the brain. If they could be supposed to do any good at all, it would be when they are placed upon the feet or legs. But this kind of revulsion is better accomplished by means of mustard poultices, or fomentations with hot water, which are often of much apparent service, in addition to the measures already spoken of. Experience confirms what reason teaches us to expect in this matter.

When, however, the patient has sunk into a state of coma, he has sometimes, in my experience, emerged from that condition after a cap of blistering plaster has been put upon the head. It is only when the violent symptoms of excitement have abated that I can venture to advise you to employ blisters: they may then be applied to the nape of the neck, or behind the ears, or to the head itself.

The symptoms which I enumerated as marking the period of collapse or sinking, are fearful symptoms; but the conditions on which they depend are not, necessarily, hopeless conditions. These symptoms do not always proceed from fatal disorganization of the brain, but sometimes (there is reason to believe) from simple exhaustion of the nervous power. And this is a point of critical importance. Patients apparently moribund are occasionally saved by the judicious administration of stimulants and restoratives; of ammonia, Hoffman's anodyne, beef-tea, wine, and, it may be, of well-timed opiates. This plan of treatment you must therefore cautiously try, when an extreme degree of collapse occurs. If the structure of the brain be already seriously injured, and the case is irretrievably mortal, no harm can be done; while in doubtful cases, and when the symptoms result from mere depression of the vital powers, the patient may be rescued: and this chance in his favour must not be thrown away.

Do you ask whether there be any mode of discriminating these opposite conditions, one of which is within, and the other beyond the range of possible recovery? I believe there is. If the tendency to *death by coma* be strong, the prospect is very discouraging: if, on the other hand, the symptoms that mark the mode of dying by *asthenia* predominate, you may hope to push the patient through. But to succeed, you must watch him hour by hour. Pallor, a feeble and flying pulse, extreme debility and tremors, coldness of the extremities, a want of power to respond to external impressions; these are alarming, but not absolutely desperate symptoms, especially if the mental faculties remain. Whereas profound stupor, partial palsy, profuse sweats, are of the worst omen; yet even these do not preclude the trial, together with blistering the head, of internal stimuli; and no other plan affords even a gleam of hope.

There is just one caution that I wish to mention before I leave the subject of acute encephalitis; and it applies to all cases of coma and insensibility, and especially when there is any paralysis mixed with the coma: it is, that you should daily ascertain that the bladder is emptied. Always make the attendants show you the urine that has been passed; and lay your hand upon the hypogastric region, and try whether there is any undue hardness and prominence there, produced by the distended bladder. I shall revert to this matter more particularly at some future time; and I content myself with merely suggesting its importance to you now, in all cases of head affection. If the patient *does not* or *cannot* empty his bladder, of course it must be emptied for him, by means of a catheter.

Delirium Tremens.—It would seem perhaps the most natural arrangement if I next proceeded to speak of inflammation of the brain when it is chronic, or partial. These forms of disease are more common, in adults, than acute and general encephalitis. I shall be obliged also to treat, separately, of inflammation of the brain as it is modified by its occurrence in young children—of what is called acute hydrocephalus. But before I touch upon any of these, I am desirous to bring under your notice at once a very singular and extremely interesting complaint, which is not, I am persuaded, in its essential nature, inflammatory, but which may easily be mistaken, and has over and over again been mistaken, for acute inflammation of the brain and its membranes, with the consideration of which we have just been occupied. And the mistake is the more serious, because the remedies that I have been recommending for encephalitis, and especially blood-letting, not

only are not required, but are in most cases positively injurious, in the disorder of which I am now about to speak; and which is best known under the appellation of *delirium tremens*. Nay this affection of the nervous system may actually be brought on, in a predisposed subject, by the abstraction of blood. I go apparently out of my way in taking notice of this disease now, but I do so that I may have the opportunity of contrasting it with encephalitis, while the phenomena of that disease are fresh in your memory. It certainly resembles it also in many respects: and it has been considered as being an inflammatory disorder by some excellent pathologists.

The symptoms which mark a decided attack of *delirium tremens*, and which have sometimes been found so equivocal, are very striking. You will be summoned to a man who is supposed to be mad, or to have brain fever. You find him with a red face, perhaps, and injected eyes, talking incessantly, fidgeting with his hands, affected often with tremors of the limbs, having a rapid pulse, and bathed in sweat. Now it is very natural that a person not on his guard should look upon these symptoms as indicating inflammation within the head. But if you look closely into the matter you will find in the state of the patient, or in his history, some things very peculiar. The delirium you will generally find to be not a fierce or mischievous delirium, but a *busy* delirium: he does whatever you desire him to do, but he does it in a hurried manner, with a sort of anxiety to perform it properly. During the approach of the malady, while he yet is able to go about, he manifests great impatience of any interference, or advice, or assistance, in his ordinary duties, which he sets about in a bustling and blundering manner. His loquacity is extreme, and he refers to matters that are not present before him: he is not altogether inattentive to the objects and proceedings that are going on around him, but his mind wanders away to other subjects. There is an odd mixture of the real and the ideal in his thoughts and language. Sometimes he is very suspicious that those who are about him intend him some injury; or that he is surrounded by enemies. You will find also that he does not sleep; that he has not slept perhaps for several nights, but been restless and rambling: and you will generally learn that he has been habitually intemperate, or subject to some great source of care, or anxiety, or excitement: and in many cases he has recently been somehow or other debarred from his customary stimulus. Very frequently, in addition to these points in his history, you will be told that having been unwell, first he has been kept upon low diet, and then, as the delirium came on, he has been freely bled; and that he has been none the better, but commonly the worse, for the bleeding. When you gather such particulars as these from his friends (for upon his own statements you cannot place any reliance) and when you find the delirium to have the characters I have been attempting to describe, and especially when there has been obstinate watchfulness, and the tongue is moist, and the skin is sweating, you may be pretty certain that your patient is affected, not with inflammation of the brain, but with delirium tremens; and that if you bleed him further you will make him worse.

But what are you to do under such a fearful state of things? Why the grand indication is to procure sleep; and the remedy which, in nine instances out of ten, you will find successful, is opium. The beneficial effects of this drug, in tolerably favourable cases of delirium tremens, are really surprising. I will give you an example or two, which will be more instructive than any abstract description.

In the year 1831, I was requested by a most respectable practitioner in this town, to visit a patient of his whom he reported to have had phrenitis, for which he had been freely bled, cupped from the back of the neck, and purged; and who, he believed, was now rapidly sinking, and not likely to survive many hours. I found the patient, a middle-aged man, with a red face, ferrety eyes, a frequent pulse, bathed in perspiration, busy with his hands, which trembled a little, and talking much and incoherently. He was particularly anxious that his legs should not be scarified, told me he was willing to do any thing I pleased, if I would not scarify his legs, nor let any one else scarify them. There was nothing the matter with his legs, nor had it entered any body's head, but his own, that they wanted scarifying. He had not slept for several nights. He had been intemperate, especially of late, drinking a good deal; and somewhat anxious about his affairs; he was a builder.

His former history was not very promising. He had brought up a good deal of blood

a few months before, and some years previously he had had jaundice; latterly he had been troubled with indigestion.

I saw him in the afternoon, and prescribed one-third of a grain of morphia: in the evening he was just in the same state. I then directed half a drachm of laudanum, to be given immediately, and twenty drops every two hours afterwards, *till he slept*. I said to the gentleman who had called me to the case, that I thought it very likely our patient might be well the next day; he smiled, and shook his head. I was obliged to leave London early the next morning, for two or three days; on my return, I learned from the medical man that the patient took five doses of the laudanum, and then fell asleep, and slept soundly and for a long time, and then awoke (to his attendant's extreme surprise and satisfaction) sane, and well.

I was asked by the apothecary of the Middlesex Hospital to see a publican in that neighbourhood. I found a large strong man, between 30 and 40 years of age. He had been without sleep for several nights, somewhat incoherent, and (what is not usual in such cases) violent, threatening and striking those about him because they refused him access to strong drink. He was joint proprietor with another in a gin shop, and for some time previously he had been a sot, and daily muddled with drink. He told me he was quite well; there was not much tremor. I found that the object of his partner and relations in sending for me was that I might sanction his removal to St. Luke's, for his strength made him altogether unmanageable, and his insane and extraordinary conduct was hurting the business of the house. I declined to take any part in consigning him to a mad-house, and recommended morphia. After one full dose he soon slept; and the next day he was quite rational, and comparatively well.

These are the broad outlines of delirium tremens; there are many other features wanted to complete the portrait of the disease, which I shall endeavour to paint at our next meeting.

LECTURE XXIV.

DELIRIUM TREMENS, CONCLUDED. CHRONIC INFLAMMATION OF THE BRAIN. SOFTENING, SUPPURATION, ABSCESS, INDURATION. TUMOURS IN THE BRAIN.

I DREW a rude outline, yesterday, of that singular and interesting malady usually denominated *delirium tremens*. The disease is very common in this country; for its causes are in common and powerful operation. You will meet with it in every walk of life: and you will be almost sure to witness several examples of it during the course of every year, in any of our metropolitian hospitals. It is not a chronic or vague complaint likely to be treated with *placebos*, or by waiting upon nature. Active measures are pretty certain to be adopted; and, in many cases, one plan of treatment, vigorously pursued, will hurry the patient to his grave; another plan will restore him to health with an almost magical celerity. It certainly bears a strong resemblance to that most formidable disease, inflammation of the brain and its membranes: but the great remedy for encephalitis acts like a poison in pure delirium tremens; and the drug, by the timely and careful administration of which we can often promise a speedy cure in delirium tremens, is one which we must carefully avoid, in the earlier treatment at least, of encephalitis. Accuracy of diagnosis, therefore, between these different disorders, with similar outward signals, becomes of the very highest importance.

Delirium—tremens.—There is delirium always; and there is generally, but not always, tremor. The name is a good enough name, in my humble opinion; yet it has been found fault with, because the trembling is not always present; and some have, therefore, christened it *delirium è potu*, or *delirium ebriositatis:* but these terms are open to just the same objection as the other; for though the disorder is most commonly connected with

intemperate habits, that is not always the case. One very curious fault has been discovered in the name; it is said that the delirium cannot *tremble;* and, therefore, that it is better to say, *delirium cum tremore*, or *tremefaciens:* and you would hardly suppose it, but there has been a sort of contention for the honour of thus mending the nomenclature of this disease. But they who object to *delirium tremens* appear to see no harm in *delirium ferox:* whereas it is just as incorrect to say *delirium* is fierce, as to say that it trembles: it is the patient who is furious, even as it is the patient who trembles; and all this dispute about a name is mere trifling. It matters not what we call a disease, so that the name conveys no erroneous theory as to its nature or treatment. No such source of error attaches itself to the term delirium tremens: and, therefore, if it be only to avoid the inconvenience of change, we will adhere to that term.

Recollect that the strong features of the complaint are sleeplessness; a busy, but not angry or violent delirium; constant chattering; trembling of the hands, and an eager and fidgety employment of them. To these are added other symptoms which, though they are not so calculated to strike a looker-on, are of not less importance, inasmuch as they help to establish the diagnosis. The tongue is moist and creamy; the pulse, though frequent, is soft, the skin is perspiring, and most commonly the patient drenched in sweat. The sweat is usually described as having an offensive or a peculiar smell: I cannot say that I have observed it to be so. The face also is said to be pale; but that, I know, is not always the case, and therefore this point cannot be relied upon as a distinguishing circumstance. In one of the instances which I related in the last lecture, the face was flushed, and the eyes red and ferrety.

Let me remind you, in a few words, of the peculiar characters of the delirium. If you question the patient about his disease, he answers quite to the purpose; describes, in an agitated manner, his feelings, puts out his tongue, and does whatever you bid him: but immediately afterwards he is wandering from the scene around him to some other that exists only in his imagination. Generally his thoughts appear to be distressful and anxious; he is giving orders that relate to his business to persons who are absent; or he is devising plans to escape from some imaginary enemy: he fancies that rats, mice, or other reptiles, are running over his bed, or that strangers are in his room. He is perpetually wanting to get out of bed, but he is readily induced to lie down again. It is very seldom that he meditates harm, either to himself or to others; there is rather a mixture of cowardice and dread in the delirium.

All the points that I have been mentioning require to be investigated in every case of this nature: and an inquiry into the previous history of the patient, into what the French call the commemorative symptoms, is equally important. In a large majority of instances you will find that he has been an habitual drunkard; and very frequently that from some reason or other this habitual stimulus has been diminished or taken away. Some accidental illness has befallen him, and he has been restricted to low diet, and as a sailor would say, "His grog has been stopped." When, with symptoms such as I have mentioned just now, you hear a history of this kind, you may be satisfied that the disease is not inflammation of the brain, but delirium tremens. I believe that habitual intoxication of any kind may lead to this disorder; but distilled spirits more so than wine; wine more than beer. I make no doubt either, that what is alleged of the habitual use of opium, in preparing a person to suffer in the same way upon its being withheld, is quite true, although I have had but few opportunities of noticing such cases.

But the disease is not confined to drunkards, although it is so commonly connected with that pitiable vice, as to have been called *mania è potu*. You meet with it occasionally in men who have overstrained their nervous system by other modes of strong excitement. Long-continued mental anxiety, that state of mind in which gamblers and great speculators (who indeed are gamblers) are accustomed to live, may cause it; any thing by which the mind is over wrought. A well-informed medical man, of temperate habits, told me a few days ago that he was on the brink of delirium tremens in the year 1825. He had foolishly entangled himself in some of the speculations which prevailed here like an epidemic at that period, and his mind was on the tenter-hooks of suspense and apprehension for some time. He could not sleep, and he found himself everlastingly chattering. It comes on in the course of certain diseases, as sometimes, for example, in apo-

plexy: and it is a very common result of bodily injuries and accidents, and of surgical operations; or, I should rather say, that it often *follows* such diseases and casualties; for it is, even then, the consequence of the treatment and regimen to which the patients are subjected, rather than of the surgicical or medical complaint. And it is certainly *more* apt to occur, under these circumstances, in old people; and in those who are younger, but have been known to be intemperate. So frequently does the delirium manifest itself upon the cessation of the accustomed spur, that the continually recurring stimulus has been regarded as the *predisposing*, and the privation of that stimulus the *exciting* cause of the affection. Sometimes, however, it comes on in men who are perpetually fuddled, even although they have not intermitted their usual indulgence in drink. We had a porter (an old soldier he had been) at the Middlesex Hospital, who was of great use to us as a subject to practice upon, and to show to the pupils. I never saw him drunk, so as to be unable to perform his duty: but I cannot conscientiously say that I ever saw him sober. Every three or four months we were sure to have him in the wards with delirium tremens. Sometimes he fell into the hands of one physician, and sometimes of another; but in one of his attacks he slipped through our fingers: I am not certain that he was not nominally my patient on that last and fatal occasion: but assuredly he was never an example of the disease coming on from the adoption of more temperate habits. We often find that the malady shows itself immediately after an unusually severe debauch, which has disturbed the stomach and bowels, and left behind it a proportional degree of exhaustion and languor.

Without knowing why it should be so, my own experience would lead me to the belief that delirium tremens is very uncommon among women. The number of beds for females in the physicians' wards of the Middlesex Hospital is somewhat greater than for males. On the men's side of the house cases of delirium tremens are very frequent; whereas I scarcely remember any on the women's. Yet each sex is obnoxious to its main causes. The gin-shops of this town are said to draw a fearful crowd of votaresses. And we might expect that the more sensitive character of the female constitution would render them especially liable to this peculiar consequence of the abuse of alcohol. My experience, however, is such as I tell you. On the other hand, Dr. Roots thinks he has seen quite as many instances of delirium tremens attacking females as males. The result of M. Rayer's observation is more in accordance with my own. Of 176 patients seen by him, 7 only (not 1 in 25) were women. A still smaller ratio is recorded by Bang, 10 in 456; less than 1 in 45. The disorder appears to be more common in the summer than in the winter months.

The peculiar nature of the complaint, and the proper method of treating it, were first brought into general notice in 1813 by a new work of Dr. Sutton's, of Greenwich. He saw a good deal of the diseases of the smugglers, and the customers of the smugglers, that frequent the coast of Kent; and he was struck by the different event of this disorder in the hands of different practitioners, according as bleeding or narcotics were adopted. It is the same disease which Dr. Abercrombie speaks of as "a dangerous modification of meningitis, which shows only increased vascularity." Dr. Bright also includes it among his cases of "Arachnitis." Both these eminent physicians had learned, however, that the complaint requires a particular method of treatment. Of late years many essays and papers on the same malady have appeared in this country, in France, and in the United States, where the disorder is common. But even now it is not so well understood, throughout the profession, as it ought to be.

You may ask me, what is the essential *nature* of the disease: and I can only state in reply that it consists in *nervous irritation*. Some persons hold that this is tantamount to no answer at all; but I do not agree with them. They seem to think that if you assign a state of the brain or nervous system which is not visible or tangible, you lose yourself in mere hypothesis. But we see a number of striking phenomena in this and in many other forms of disease, for which phenomena we can trace by our senses, in the organ affected, no physical cause; yet we are sure that they have a cause; and we call that cause irritation: if we had given it some Chinese name it would have been all the same. From certain symptoms we infer irritation; just as from certain phenomena we infer gravitation. I do not mean to put the two upon an equal footing; or to pretend to say

that the laws of irritation are established with any thing like the certainty which belongs to the ascertained laws of gravity: but we pursue the investigation of these laws in the same way in the one case as in the other: and it is quite idle to object to an arbitrary term, like irritation, because it is meant to represent something which makes itself known to us only by its effects.

Now I apprehend that we are borne out, by authentic facts, in believing that certain changes in the blood-vessels will lead to irritation, and at length to inflammation, of a part. But there are other sources of irritation; and irritation in its turn will lead to changes in the blood-vessels. In the one case we bring back the blood-vessels to their healthy condition, and the symptoms of irritation cease. In the other we calm the irritation, and the previous effect of it upon the blood-vessels stops. In other words, deviations from the natural and healthy state of the nervous system are sometimes the cause, and sometimes the consequence, of disturbances in the sanguiferous system. Whether this be good philosophy, or whether it seem to you rational and intelligible, I do not know; but it is the best explanation that I can offer you upon this subject.

I apprised you, in the last lecture, that the great remedy in delirium tremens is *sleep;* and that our most powerful means of inducing sleep are to be found in opium. The opium must be given in full doses; and it must be fearlessly repeated if its desired effect does not follow. If the patients pass many nights without sleep, they will die. I have tried various forms of opium; and I am quite satisfied with morphia. Some persons, however, have not found it so successful as solid opium, or as the common tincture, laudanum. You may try the one or the other, or the one after the other, if you please. No particular rules can be laid down that will suit all cases. After clearing out the bowels by a moderate purgative, you may give three grains of solid opium; and if the patient show no inclination to sleep after two or three hours have elapsed, you may begin to give one grain every hour till he does sleep. Or you may prescribe corresponding quantities of the acetate or muriate of morphia; or of laudanum; or of the black drop; or of Battley's liquor opii sedativus. At the same time his room should be kept dark and quiet. If he sleeps for some time he will awake calmer and more sensible; perhaps perfectly so; and you must withhold the remedy, or continue it in smaller or less frequent doses, according to the circumstances of the case.

Dupuytren found opiate enemata of great efficacy in the cases of traumatic delirium that came under his care. That mode of administering the narcotic may properly be adopted, if there be any impediment to its reception or retention by the stomach.

Now sometimes this opiate treatment alone is quite enough. Sometimes it is not. You will meet with patients who resist very large doses of the drug; but who presently sleep, or become composed, if you give some of their accustomed stimulus with it: "a hair (as the vulgar saying goes) of the dog that bit them:" if you put their opiate dose into a glass of gin, or a pint of porter. This I have continually experienced with hospital patients. And I rather think that you may get some clue to the particular cases that require this treatment, by examining into the state of the digestive functions. If you learn that, notwithstanding the intemperate habits of the patient, his appetite for food has continued unimpaired, and his digestion sound, you will, I believe, generally find that good nourishing diet, strong broths for example, and the opium, will suffice for the cure. But if the powers and natural sensations of the stomach have been injured and perverted, as is too often the fact, then a temporary recurrence to the habitual stimulus will frequently be necessary: and it is well to ascertain, in such cases, what the stimulus has been, whether spirits, or beer, or wine, and to order it accordingly. Of course this is not to be continued after the patient has recovered from his delirium; but the stimulus under these circumstances must be cautiously withdrawn. When the stomach retains its power of digestion, the bad habit of drinking ought to be broken off at once: and if, after sleep, you can get the patient to eat heartily of a beef-steak, or mutton-chop, I should always advise it.

There are some things which I find it necessary to mention, for the sake of discommending them. I know persons who in treating these cases combine calomel with the opium. And they say that they cure their patients so; and I make no doubt that they do; neither can I doubt that the same success would have followed the same quantity of

opium without the calomel. In *pure* cases of delirium tremens I advise you *not* to give calomel. I know no possible good it can answer: it is itself a source of great irritation to the nervous system in many persons: and if it comes to affect the mouth, you inflict upon your patient a superfluous discomfort; and, I believe, in many cases, a downright injury. You will be told also of digitalis, as a specific remedy for the disease; or you may read of it; but do not be led away from the standard remedies which reason recommends, and large experience has sanctioned. Knowing what we do of the power of opium generally, and of its efficacy in this complaint in particular, I should consider myself guilty of a criminal trifling with human life if I made experiments with digitalis upon the loose reports of some one or two persons, of whose credit or information I knew nothing; and whose dicta had been transferred perhaps from some foreign journal to fill a vacant corner in one of our own. Of the combination of opium and antimony, which has just been much praised by good and competent judges, I am unable to tell you any thing from my own experience.

In hospital practice it sometimes becomes necessary to confine the patient to his bed by straps, or to muffle his limbs in a strait-waistcoat: but this is a most unfortunate necessity. Physical coercion, whether manual or mechanical, should never be resorted to, in delirium tremens, when by any means it can be avoided. The angry feeling and mental fret which it produces, and the exhausting bodily struggles to escape or resist the thraldom, are always highly injurious and full of danger to the patient. A couple of strong and good-tempered attendants will not have much difficulty in persuading and managing the sick man, who is seldom either boisterous or obstinate: and if he be intractible by soft words, he will yield more patiently to their gentle restraint than to the force of manacles; while the appearance of coercion need not be continued a moment after his acquiescence.

I have drawn the line between encephalitis and delirium tremens with sufficient clearness, because I have taken well-marked forms of each. But I am sorry to add that there are mixed cases, which are very puzzling when they occur, and exceedingly difficult to treat; and which require opiates on the one hand, and moderate depletion on the other. When the indications are uncertain, or equivocal, we must carefully weigh the different symptoms, and we must cautiously *try* the remedies. The circumstances that most distinguish the one form of disease from the other are to be found in the *pulse;* which is hard and resisting in the earlier stages of inflammation of the encephalon, soft and compressible in delirium tremens: in the *tongue;* which is mostly parched and rough in the former, moist and creamy in the latter: in the *skin;* which is hot and dry in the one case, covered with sweat in the other: in the *countenance;* which is flushed in inflammation, and mostly (though not always) pale in delirium tremens: in the *tremors;* which are not common in the primary periods of inflammation of the brain: in the usual absence of *headache* in delirium tremens: and in the peculiar characters of the *delirium* in the two cases, which I need not repeat. If these symptoms contradict each other, as they sometimes will, you had better act on the *worst* supposition, and presume that there is inflammation, and employ antiphlogistic remedies: but you must not do so with a strong hand; you must use them cautiously, and watch their effects, and guide thereby your subsequent treatment. Take a moderate quantity of blood from the arm: observe whether it has the buffy coat: and note the condition of the patient afterwards. In mixed or ambiguous cases it will be proper to combine calomel with opium. You will sometimes find a state resembling delirium tremens left after the subsidence of acute inflammation of the parts within the cranium; and requiring the treatment of delirium tremens.

I do not know that there is much good to be expected from counter-irritation in this disease. But after the more decided symptoms were gone by, I have sometimes thought that the recovery has been accelerated by the application of a blister to the nape of the neck.

Inflammation of the brain, and delirium tremens, are distinct diseases. Hence, in the mixed cases, of which I just now spoke, we may expect after death to find, and we often do find, unquestionable traces of inflammatory action within the skull. But pure delirium tremens frequently leaves behind it no morbid appearance whatever in the brain or its membranes. In other cases there is serous liquid collected in the interstices of the pia mater, or in the cerebral ventricles; and I have on several occasions seen the arachnoid

thicker and less transparent than is natural, and sprinkled over with little spots or streaks of a milk-white colour. Changes of this kind we believe to be owing to chronic inflammation of the membrane. But, even in these cases, I see no reason for thinking that the fatal disorder had any connection with the morbid state of the arachnoid. We meet continually with like appearances when there has been no delirium tremens; and we have delirium tremens without any such appearances. The habitual abuse of ardent spirits leads to chronic inflammation in various parts and tissues of the body: in the blood-vessels, in the liver, in the kidneys, and in the arachnoid. We need not be surprised at finding that membrane thickened and partially opaque in the victims of delirium tremens; since they are chiefly men who have run a long course of intemperance. I believe that disease to bear the same relation, and no other, to the chronic arachnitis in such persons, as to the chronic hepatitis to which they are equally subject. There is but one morbid condition which, since my attention was first directed to it, I have found constant in persons dead of delirium tremens, and that is, a remarkably soft, pale, and flabby state of the muscular tissue of the heart.

The chemist may be more likely to detect altered conditions in the brain, in these cases, than the anatomist. Very likely Dr. Percy has obtained alcohol from the brain of a person who died from excessive drinking; and from those of various animals which had been killed by that poison. These facts are interesting, but they do not help us much in our attempts to explain the phenomena of the disorder.

Cases such as I related in the last lecture, where violent symptoms are calmed at once, and the patient is rescued in a few hours from great apparent peril, make a strong impression upon those who witness them; and the practitioner gains amazing credit, and is spoken of to all their acquaintances as a wonderful clever man. It is unfortunate that we are obliged to set off, against this advantage, a corresponding danger, when the disease ends ill, of being blamed without our deserving it. When these patients die (and they usually persist in their evil habits and die at last in one of the attacks of the disease), when they so die, they are apt to die much in the same way as patients who are poisoned by opium; and if their friends are aware that we have been giving large and repeated doses of that drug, they sometimes have the charity to lay the death at our door: and you ought to be prepared for this: and I will conclude what I have to say upon the subject of delirium tremens by relating a case, in which I have no doubt that I suffered (though quite unjustly) under that kind of imputation.

Six or seven years ago, I was asked, one morning, by a general practitioner at the west end of the town, to see a patient with him; of whom he gave me this account:—The man was about forty years old: he had been attacked some days before with sore throat, common cynanche tonsillaris; the tonsils and fauces were so much swelled that his deglutition was greatly impeded, and for four or five days he had not been able to swallow any thing. The night before I saw him he had become delirious, and then had been largely bled, and he was worse in the morning. His bowels had also been very much purged.

I found him propped up in his bed, with a coronet of leeches round his head. He was pale; there was no headache, nor affection of his breathing; his pulse was not very frequent, and it was quite soft and compressible. He was sweating profusely. He answered the few questions I put to him readily and pertinently, and then went talking on in a rambling way about his business. He was a hackney-man, or stable-keeper, in a large way. He said (I remember) that the boys were all ready to start; that there were two pair of horses going down the road, and that he must go and see after them; and much more on the same subject. His mind was busy about the execution of imaginary orders. He had not slept at all for some nights.

Upon my inquiring into his previous condition, his wife told me that without any turn for dissipation he had for some time been an habitual hard drinker; that he had continual dealings with the coachmen to the various families which he furnished with horses; and that he was obliged to drink something with each of them; so that every day he had many glasses of spirits, and a good deal of porter. She told me also that his mind had been anxious and uneasy; that the business was a large and harassing one; that he had embarked a considerable sum of money in it; and that it had not turned out so prosperously as he had expected.

Putting all these things together, there could be no doubt, either as to the character of the complaint, or as to the treatment proper to be adopted. Here was a man who had been living a life of continued mental and physical excitement: suddenly the stimulus to which he had been accustomed was taken away; he could not swallow even such nourishment as his case required or admitted: then came on delirium—a symptom not belonging to his disease in the throat—and protracted watchfulness. He is largely bled, and profusely purged, and he gets worse instead of better under these remedies. At the same time his skin is moist and perspiring, and there is no hardness in his pulse.

I recommended that the leeches should be removed from his head; that he should take immediately (for he could swallow now) two grains of opium, and afterwards twenty drops of laudanum every three hours, till he fell asleep.

Somewhat unluckily his wife's brother—a very young man—was the apprentice or assistant of a surgeon in the neighbourhood of town, and he came in to see his relative. After hearing what I had said, he went home, and probably consulted his books, and then came back again with doubts whether the complaint really was delirium tremens after all. Whether in consequence of these doubts I cannot tell, but for some reason or other only one or two doses of the medicine were taken. I had offered to see the patient again in the evening; but his friends said they would send for me if he did not get better. They did not send; and the patient did not sleep. At night, therefore, at ten o'clock, three grains of opium were administered. The result of this was, that he passed a quiet but a sleepless night. Perhaps (but I cannot be sure of that) if the opium had been persisted with, the case might have terminated otherwise. About eight o'clock the next morning I was summoned to him in a great hurry: when I got there he was dying, perfectly comatose, breathing stertorously, with blue lips and contracted pupils. He had appeared so much better at seven, that he was, for the first time, left alone for a quarter of an hour; and when they went back to him he was changed in the manner I have described.

The general practitioner with whom I had first seen the patient—a very sensible man—was much concerned at this issue of the case, and observed to me that doubtless our patient had been poisoned by the three grains of opium. I was able, however, to relieve his mind from this notion; and I have mentioned the case chiefly for the sake of guarding you against similar misgivings, under similar circumstances. The manner of dying was just such as opium will produce; but, then, death by coma is also frequently the termination of delirium tremens: effusion at length is apt to take place into the ventricles, or into the meshes of the pia mater, and stupor comes on, and the patient sinks. But in this instance I was certain that his death had nothing to do with the opium he had taken, for this reason: that so long a space of time had elapsed—nine hours—between his taking the opium and the coming on of the comatose symptoms. Dr. Christison, in his elaborate and valuable work on Toxicology, states it as the result of extensive inquiry into this subject, that when opium has been swallowed in a poisonous dose, it almost always begins to act as a poison within an hour; that very rarely indeed has its specific operation been postponed much beyond the hour, except, occasionally, when the person taking it has been intoxicated at the time. In one remarkable instance a drunken man took two ounces of laudanum, and no material stupor followed for five hours. I guess that I incurred the reproach of recommending a fatal plan of treatment in the particular case I have now related; but I am quite satisfied that the opium was innocent of the patient's death, and I even think that his chance might have been much mended if the opiate, in smaller doses perhaps, had been steadily continued.

We may be content to bear, occasionally, these unfounded imputations, when we consider the other side of the account, and call to mind the far greater number of instances in which spontaneous recoveries are credited to us as cures: and the Doctor, like Belinda's Betty, is "praised for labours not his own."

I should next wish to put you in possession of what has been ascertained in respect to *partial* and to *chronic* inflammation of the brain, as these are met with in adults; for I must speak of the head affections of children separately. But I really do not know how to bring this part of the subject before you in a practical manner. If I were first to describe symptoms, and then to state what organic changes had been discovered after death

preceded by them, I should have to tell you of different symptoms with the same morbid conditions, and of the same symptoms with different morbid conditions, in various individuals. I believe the best method, upon the whole, will be to describe the several morbid appearances which the brain is found to present; and then to mention the symptoms that have *most commonly* been observed to occur in association with such morbid conditions. I must premise, however, that the whole subject is full of uncertainty and apparent irregularity. Doubtless there *is* some constant and uniform connection of cause and effect between the altered physical states of the brain and the altered manifestation of its functions; but we have not yet been successful in our search after those settled relations; or we have but partial and imperfect glimpses of them.

Ramollissement.—One very remarkable condition of the brain has been several times mentioned in these lectures; viz. *softening*. A great deal of attention has been paid to this condition of late years, both in France and in this country: and some points in its pathology have been fairly made out. I will bring them together as concisely as I can. In the first place, the softening varies greatly in degree, from the consistence which naturally belongs to the cerebral substance, to that of thin cream. In its minor degrees it may be easily overlooked; and is more perceptible by the touch than by the eye. The cerebral matter is less coherent, but it is not yet discontinuous or broken down. It may be washed away, however, by letting a slender stream of water fall upon it; and the softened parts are thus easily distinguishable from those which retain their natural consistence. In the next stage of softening we recognize the complaint at once, for the softened parts undergo a change of form by their own weight: parts that are prominent in the healthy state, as the optic thalami, corpora striata, and convolutions, sink down, as it were, and are more or less flattened. If you make a horizontal section through a part thus diseased, a portion of the softened brain adheres to the knife, and is removed by it, and a depression is left. In a still more advanced degree, the natural texture of the organ in the softened part is entirely destroyed and confused by the change, diffluent: you may pour the softened matter out.

The *colour* of the softened portions varies also considerably. Sometimes they are unchanged in colour: sometimes they are quite white, and present a strong contrast with the tint of the neighbouring parts: sometimes they are marked with various shades of redness, from a rosy pink to an orange, or deep red, or even a mahogany brown. Often there are red spots mixed irregularly with the softened cerebral pulp, and giving it very much the appearance of a mixture of raspberries and cream. In other cases we find the softened mass of a pale yellow, or straw colour, infiltrated, as it were, with purulent matter: and sometimes it is mixed with serous fluid.

Softening of the brain is usually partial. It may occupy any part; but it is said to be more frequently met with in the gray than in the white matter: and more often in the gray matter of the convolutions than of the more central parts of the brain.

You will find softening of the septum lucidum, and of the fornix, occurring very frequently in connection with an accumulation of serous fluid in the lateral ventricles.

Now, it is well established that softening of the brain is a common result of two very different morbid conditions. It is often caused by inflammation of the softened part: it is often caused, also, if I may say so, by its starvation; by the diminished supply of arterial blood, in consequence of diseased blood-vessels.

Can we distinguish these two forms of softening from each other by their physical characters? Why, sometimes we can: and sometimes, it must be confessed, we cannot.

The same parts that are most liable to have their consistence diminished, through an inflammatory process, are also most liable to be softened from defect of nutrition. The most vascular parts of the brain, in short: the gray matter of the convolutions, and the gray matter of the thalami, and corpora striata.

It is stated, however, that softening of the corpus callosum, septum lucidum, and fornix, from obliteration of the arteries, is extremely rare.

If there be pus mixed with the softened brain, we know that there has been preceding inflammation. Again, if we find the arteries impervious, we conclude that the softening has not been inflammatory. Dr. Carswell states that the obliterated arteries may occupy the softened cerebral substance, and may often be seen ramifying through it; and that

when this substance is removed by pouring water upon it, the solidified vessels retain their situation, and feel sometimes as hard as fine wires. But we come to the same conclusion if we find the larger vessels, the carotid or vertebral arteries, obstructed by ossification; and a large portion of the brain unnaturally soft.

We have no certain test of the nature of the softening in its being red. The redness may be the result of inflammatory congestion; but cerebral hæmorrhage may occasion softening; and, on the other hand, softening may give rise to cerebral hæmorrhage. *This* may be said, however: that the redness is seldom *considerable* when the softening proceeds from an obliteration of the arteries. When the softening extends much beyond the redness, or the effused blood; or when the redness occupies several small portions only of the softened pulp: we may presume that the blood was extravasated subsequently to, and in consequence of, the softening. On the other hand, when redness and vascularity can be traced into the brain, some way beyond the softened part, we may regard the softening as the consequence of inflammation. And we adopt the same belief, with still greater confidence, when around the softened and disorganized pulp we find the cerebral substance *hardened*, and of a uniform reddish colour.

In attempting to make the diagnosis between these two forms of softening, we get some assistance by noticing the *age* of the patient. The ossification, which gives rise to the obliteration of the arteries, is almost peculiar to the advanced periods of life; whereas inflammatory softening may occur at any age; in children, in adults, or in old persons.

Some of the French pathologists have laid down this rule as the result of their experience in regard to softening of the brain—that it is attended, during the earlier part of its progress, with a permanently contracted state of the flexor muscles of one or more of the limbs. "In some cases the contraction of these muscles amounts only to a slight degree of stiffness; in others it reaches such an extent, that if the arm be the part affected, the hand is clenched, and remains pressed against the shoulder; or, if the leg, the heel is carried up to the hip." Sometimes this tonic spasm is so strong that you cannot extend the limb; and the attempt to do so gives the patient pain. After a certain time the rigidity is succeeded by complete relaxation; the contracted limb has become palsied.

I believe that this is a valuable diagnostic symptom of softening, and especially of inflammatory softening—*when it occurs*. But it is often wanting. I wish I could tell you something more certain and constant in respect to the symptoms of this interesting change; but the facts that I have myself observed, and which have been recorded by others, will not permit me to do so. Dr. Abercrombie even goes so far as to say, that judging from the cases that have fallen under his own notice, there is no foundation for the statement that *ramollissement* is distinguished by tonic contraction of one or more limbs: that the same thing is met with in connection with affections of the membranes, without any disease of the cerebral substance; and with the encysted abscess of the brain; and that it is frequently observed in cases of typhus fever, where there is much cerebral disturbance, but which terminate favourably. I will give you the general result of his experience in this matter, as being untinctured with any wish to reduce his facts into compliance with a preconceived opinion, or hasty generalization. He states that "the cases which terminate by ramollissement seem in general to be characterized by convulsion, more or less extensive, followed by paralysis and coma; the convulsion ceasing for some time before death, and being succeeded by the coma." But he saw one case in which "the convulsion continued with the utmost violence till the very time of death." In another instance "there was no convulsion at all, but a sudden attack of palsy, exactly resembling the ordinary attack of hemiplegia from other causes." In two cases he found "ramollissement of very limited extent, in connection with symptoms of long standing, both cases being at last rapidly fatal by a sudden attack of convulsion." In other cases "there was extensive destruction of the cerebral substance, without either paralysis or convulsion, and even without coma."

Suppuration and abscess of the brain.—When you find the softened substance infiltrated with purulent matter, you may call the case one of suppuration of the brain. But suppuration also occurs in another form; viz. in the form of abscess. The pus is contained in a regular well-defined cavity, surrounded by cerebral matter in a healthy or in a hardened state. Now in suppuration occurring in the brain, there is the same puzzling

diversity of symptoms as in cases of simple softening. Still, in the main, there seems an approach to the same order of symptoms; convulsions in the earlier period constituting the most prominent feature of the disease; paralysis in the latter. I will take one of Dr. Abercrombie's cases in illustration of the formation of encysted abscess in the brain.

A girl, aged eleven, thin and delicate, after having complained for some days of headache, was seized, on the 11th of January, with convulsions, which continued about half an hour: paralysis of the right arm followed the attack of convulsion. She was bled from the arm, and purged, and cold was applied to her head; and she was much benefited by this treatment. On the 13th the headache was much abated, and she had recovered a considerable degree of motion of the arm. On the 15th the headache increased again, and the arm became more paralytic, and she was again bled: and on the 16th and 17th the power of moving the arm was greatly improved. On the 18th, after being affected with increase of headache, and some vomiting, she became convulsed, the convulsion being confined entirely to the head, and to the right arm; the head was drawn towards the right side, with a rolling motion of the eyes; the arm was in constant and violent motion. She was sensible, and complained of headache. Being bled to eight ounces, the convulsion ceased instantly, and the headache was relieved; but the right arm remained in a state of complete paralysis. Her pulse, during the five following days, fell from 100 to 60; some headache continued; she had occasional vomiting; and the convulsive attacks returned several times: they were entirely confined to the right arm; which, after the 23d, was left in a state of permanent paralysis. Hitherto no other parts of the body had been affected by the convulsion; but on the 24th it attacked the right thigh and leg, and left them in a state of paralysis. The former remedies were repeated without any effect. The thigh and leg went through a course precisely similar to that described in regard to the arm, and on the 29th remained in a state of permanent paralysis.

She was now, therefore, paralytic of the whole right side; she had no return of convulsion, was perfectly sensible, and made little complaint. Gradually she became dull and oppressed, and at length fell into a state of perfect coma, and died on the 14th of February, a little more than a month after the commencement of her illness.

In the upper part of the left hemisphere of the brain there were two distinct defined abscesses, containing together from six to eight ounces of very fœtid pus. They were lined by a firm white membrane; and a thin septum of firm white matter separated them from each other. The one was in the anterior part of the hemisphere, very near the surface; and the other immediately behind it. In the posterior part of the right hemisphere there was a small abscess containing about half an ounce of pus. There was no serous effusion in any part of the brain, and no other morbid appearance.

In this very interesting case it is worth remarking how the convulsion preceded the paralysis, and how the palsy was more than once diminished by antiphlogistic measures.

It is reasonable to conclude—it can hardly be called a conjecture—that in such cases of partial disease of the brain as I have hitherto mentioned, the occurrence of convulsion, or of rigidity, marks the inflammatory stage; and the supervention of permanent paralysis denotes the period of softening or suppuration, of complete disorganization, that is, of the texture of the brain in that part.

Induration of the brain.—Partial inflammation of the brain, especially when it is chronic, sometimes produces a totally different change from any that have yet been described. Instead of becoming softer, or being converted into pus, the inflamed part is indurated; comes to resemble in consistence portions of brains that have been for a short time immersed in weak nitric acid. In this state it is often unusually vascular and injected. When the induration is greater in degree, the hardened part assumes the appearance of wax, or of boiled white of egg, or (as Andral says) of Gruyère cheese, and contains but little blood, but is, on the contrary, distinguished by its pearly whiteness. That these changes are the result of slow inflammatory action is the more probable, because they are sometimes found to exist around an old apoplectic clot or cell; the blood effused having acted as a cause of inflammation of the neighbouring part, just as any foreign substance might do. In the progress of cases in which partial induration is effected, convulsive movements are common, but paralysis does not appear to be so frequently present. The symptoms may go on for months, and often remit, and are again

aggravated by paroxysms. These cases are the more interesting, because they offer a greater probability of cure than those that are attended with an opposite condition of the cerebral mass.

Tumours of the brain.—Besides these varieties of inflammation, and their consequences, the brain is often infested with tumours, which also give rise to a great diversity of symptoms. There are fibrous tumours which grow rather *around* the nervous matter than within it, and are connected with the dura mater. They have been found at almost all parts of the surface of the brain; at its base, at its sides, and towards its summit. Scrofulous tubercles are also not uncommon: these are embedded in the nervous substance, and assume a round form, for the reason I formerly mentioned, viz. because the tubercular matter that is separated from the blood is not cast into any particular mould (as it is when it is effused iuto the small bronchial tubes), but poured forth into the homogeneous pulp, which exerts an equal degree of pressure upon it on all sides. These scrofulous tubercles of the brain are infinitely more frequent in children than in adults; and they are more commonly met with in the cerebral hemispheres than in any other part of the brain, occupying the cortical and medullary substance indifferently. They sometimes appear to originate in the pia mater. They differ from pulmonary tubercles in this respect, that they are seldom numerous in the same brain. Sometimes only one is found. They vary in magnitude from the size of a large pin's head to that of a hen's egg; and they are sometimes even bigger than that. The substance of the brain immediately surrounding these tubercles may be unchanged, in which case it is probable that the tubercles themselves give rise to no particular symptoms, the cerebral matter of the spots they occupy having been gradually absorbed to make room for them; but at length important alterations take place in the nighbouring texture; congestions of blood, or softening, or suppuration; and then the ordinary consequences of these changes declare themselves outwardly.

Cancerous tumours occur also in the substance of the brain. They usually occupy a large portion of it before they extinguish life. Hydatids are sometimes found there.

Now of the occurrence of these various local maladies of the brain it is necessary that you should be aware, for you may expect to meet with them frequently in practice. And it is right also that you should be aware that they do not disclose their precise nature by any peculiar symptoms, or succession of symptoms. They all, sooner or later, disturb the functions of the organ in which they are situated; and they may all disturb them exactly after the same fashion. We may judge, sometimes, *from other circumstances*, that the disease is of this or of that character. If we see scrofulous or cancerous disease in other parts of the body, we infer that the symptoms which denote disease of the brain are caused by scrofulous or cancerous tumours there situated; but from the symptoms themselves, we can only learn that there is some morbid condition of the brain.

I attended, recently, with Dr. Latham, a youth, whose symptoms led us to believe that he had tubercular disease of the peritoneum; a very formidable complaint, which I shall more particularly describe hereafter. We thought it probable also, although there were no *physical signs* of pulmonary disease, that his lungs contained crude tubercles. After some time, he went down to the coast; and was there attacked with a fit of general convulsions. Up to that period he had shown no symptoms whatever indicative of organic disease within the head. On being apprised of this seizure, we expressed to the physician then attending him, our opinion that it had resulted from the presence of scrofulous tumours in the patient's brain. The convulsions returned a few days afterwards with great violence, and he died. It was as we had conjectured. The peritoneum was found studded with innumerable miliary tubercles: there were a few crude tubercles, of some size, around the roots of the lungs; and two large masses of the same sort in the brain. Here, you see, we were directed to a correct special diagnosis of the cerebral disease, simply by the evidence which had satisfied us that scrofulous tubercles existed in other parts of the body.

In the case of specific tumours there is really nothing to be done by way of cure; and we must then treat symptoms, and seek to alleviate them as they arise. When it appears likely, or not unlikely, that the cerebral symptoms may be the result of cerebral inflammation, we must give the patient the chance of being benefited by some of the remedies of inflammation: we must treat the case in this instance upon the *most favourable* sup-

position. The class of remedies from which most may be hoped in equivocal cases, are local bleeding, counter-irritation, and especially the cautious and regulated employment of mercury. I have stated to you before, that I have known several obscure but threatening symptoms of brain disease clear entirely away, when the gums were made sore by mercury, and kept slightly tender for some little time. It is possible that we may sometimes do our patients harm by this mercurial treatment. We may, now and then, accelerate the arrival of death in cases which nothing could save: but we must not be deterred from giving them this chance of being rescued from a disorder which may be susceptible of cure, but which, if unchecked, will be inevitably fatal.

LECTURE XXV.

HYPERTROPHY OF THE BRAIN:—ATROPHY. ACUTE HYDROCEPHALUS. PREMONITORY SIGNS; DIFFERENT MODES OF ATTACK; STAGES OF THE DISEASE: ANATOMICAL CHARACTERS; CAUSES.

Hypertrophy of the brain.—There is a very curious morbid condition of the brain, to which I shall advert before I take up the consideration of certain cerebral diseases as they occur in *children*. The condition of which I am about to speak I was totally ignorant of till I had been some years in practice. In the spring of 1833 I admitted a young woman, 19 years old, into the Middlesex Hospital. Her countenance was sallow, and her lips pale; and she complained of pain in her chest and limbs, of great and increasing debility, and wasting, and of nightly perspiration. She had some cough, and a frequent pulse; and although no morbid sounds were audible in her lungs, I suspected that they might contain small or scattered tubercles. She had been in the hospital scarcely a week, when she had a violent fit of epilepsy; and when she was somewhat recovered, she told us, for the first time, that she was *subject* to such attacks. The convulsions recurred on the same day, and she became insensible, and remained so during the whole of the next day, and till the evening of the day after, when she died. During this period of insensibility she had many convulsive fits; the pupils were dilated, the pulse 100, small and feeble. Leeches were applied to the temples, a blister to the neck, and afterwards to the shaven head, and other measures were used; but in vain.

When the surface of the brain was exposed by the removal of the skull-cap, and of the dura mater, it was observed that the convolutions were remarkably flattened, so that the little furrows between them were nearly effaced; and the surface of the arachnoid membrane was perfectly dry. These are not very unusual appearances: I had often seen such before: and I ventured to say that we should find some cause of strong pressure in the central part of the brain; effusion of serum into the ventricles, or a large extravasation of blood. But to my great surprise, and much to the discredit of my prophecy, we found nothing of the kind; but the ventricles were smaller than natural, and contained scarcely any moisture. The skull-cap was afterwards examined, and the bone was found to be uncommonly thick, dense, and heavy; and its inner surface, without being rough, was very irregular. I regret that, in this examination, the state of the blood-vessels of the brain, and the consistence of the cerebral matter itself, were not particularly noticed. In the record made at the time by my clinical assistant, it is merely stated that the brain was otherwise healthy. There was no disease in the lungs.

This dissection interested me much, for I had never seen, nor heard of, any thing like it before. But upon looking into some modern authors, I discovered that the same phenomena had been noticed by two or three observers, who had very properly (as it seems to me) considered them as the result of *hypertrophy* of the brain. There is a very good memoir upon the subject, by M. Dance, published in the fifth volume of Breschet's *Répertoire d'Anatomie:* and Andral gives an account of the disease in his *Pathology*. It

appears that Morgagni had not overlooked it, for he speaks of instances in which the brain seemed too big for its bony enclosure. When, in these cases, the skull is sawn through, the upper loose portion of bone starts up, as if moved by a spring, and the edges of the bone remain widely apart. Laennec, also, in *Corvisart's Journal*, states that upon opening the bodies of persons whom he had thought affected with hydrocephalus, he had been surprised at finding a very small quantity only of fluid in the ventricles, while the convolutions on the surface of the brain were strangely flattened; proving that the cerebral mass had undergone strong compression, which could only have been occasioned by its preternatural volume, and undue nutrition.

Besides the characters I have mentioned, the hypertrophied and compressed brain is firmer and tougher than natural; it contains but little red blood; and sections of it are seen to be unusually dry and pale.

In several of the cases of hypertrophy of the brain recorded by authors, the patients had suffered epileptic fits, or rather paroxysms of convulsion; and in some of them the convulsions terminated in paralysis. Andral states that the intellectual faculties have been observed, in some instances, to become dull and obtuse. Many of the patients were subject to severe headaches. All these symptoms are common to various cerebral complaints. The diagnosis of this rare disorder can be no better than conjectural; and its treatment we have still to seek.

Andral remarks, what is very true, that hypertrophy of the brain, *i. e.* an undue and disproportionate development of that organ, may, and does happen, without giving rise to any morbid phenomena at all. But, in such instances, the *brain-case* is equally enlarged in capacity; so that no pressure upon the cerebral mass arises from its own preternatural growth. It is only when the brain increases faster than the bony sphere which contains it, that the hypertrophy becomes a disease. In my patient there was also, in one sense, hypertrophy of the skull; the bone was considerably thicker, and more compact and heavy, than is usual; but the capacity of the cavity had not undergone a proportional augmentation; nay it might, for any thing I know, be diminished in consequence of the increased thickness of the bone; the case may have been one of concentric hypertrophy of the bone, without any fault of the brain itself: but what makes this the less probable is, that in other cases the skull has been found of the natural thickness and density; but too small for its contents.

It is of some importance that you should be aware that the brain, and its case, may be extravagantly developed without there being any disease, or any symptoms of disease. M. Scoutetten gives an instance of this which he observed in a child five years old. Its head was as large as that of a well-grown adult person. The skull was from a line and a half to two lines in thickness. The dura mater adhered firmly to the bone, and the cerebral mass exactly filled up the cranial cavity. The superior and posterior part of the brain was developed beyond measure, so that to reach the ventricles it was necessary to make an incision nearly three inches in depth. There was nothing unusual to be remarked in any of the cerebral functions of this child; it was just like other children of the same age in respect of intellect. It died of acute inflammation of the bowels.

The late Dr. Sweatman met with just such another child a few years ago: and I refer to his description of it the rather, because cases that occur near home are always more interesting, and satisfactory, than those which we merely read of in foreign authors. Dr. Sweatman had never read of any thing of the kind: but in August 1834, a little boy, two years old, was brought to him on account of the size of his head. It had been gradually increasing from the age of six months, till it had become so large as by its weight to prevent the child from continuing long in an upright posture. The boy was active and lively, though thin. He never had had any fit or convulsion; but occasionally seemed uneasy, and then would relieve himself by laying his head upon a chair. He had never squinted, nor was he subject to drowsiness, or startings during sleep; and his pupils contracted naturally. His appetite was good, and all the animal functions were properly performed. Dr. Sweatman got Mr. Mayo to see the child with him; and they both set it down as a case of hydrocephalus, but agreed in thinking that in the absence of symptoms it would be wrong to risk disturbing his digestive organs by active medicines. In the early part of 1835 the child died of inflammation of the chest, and Dr. Sweatman and

Mr. Mayo examined the head. I here show you a cast of it. It measured, from ear to ear, over the vertex, twelve inches; from the superciliary ridges to the occipital, thirteen inches; and in circumference twenty-one inches. The anterior fontanelle, which was quite flat, measured across its opposite angles two inches and a quarter by one and a half; the posterior fontanelle was completely closed, as was the frontal suture. There was no absorption of bone at any part; on the contrary it was becoming thicker. The dura mater adhered with great firmness to the skull; and a layer of false membrane, as big as a crown-piece, was found upon its upper and anterior part. Beneath the arachnoid at that part there was slight jelly-like effusion. In all other respects the organ was sound. The convolutions were perfectly distinct, and retained their proper rounded shape. All the ventricles were found empty, and not dilated. The surfaces, however, of the medullary matter, exposed by different sections, presented very unusual vascularity. The lesson we learn from cases of this kind is, that we are not to regard every child that has a very large head as a hydrocephalic child; and especially that we are not to inflict upon such a child a course of mercury, or other active remedies, unless some morbid symptoms appear. The *nimia cura medici* may in these, as in many other cases, destroy health; produce disease where none existed before.

Atrophy of the brain.—Having told you what I know of *hypertrophy* of the brain, it is proper that I should say a word or two respecting the opposite condition; of *atrophy* of the cerebral mass. There are two forms of this affection: one is congenital, and results from imperfect development, or an arrest of development of the brain in its fœtal state. In the other the change appears to take place in consequence of disease, either in the membranes of the brain, or perhaps in its arteries; though the effect of disease in the arteries is usually softening, which *is* a species of atrophy. But in the atrophy to which I am now alluding, the *volume* of the atrophied part is diminished, not its *consistence*. And the diminution of size may extend only to a few convolutions; or it may be most manifest in the interior of the organ; in the optic thalami and corpora striata for example. There is still another alteration to which some have applied the term atrophy, though improperly I think: I allude to those cases, which I shall speak of more particularly soon, in which the form and disposition of the cerebral substance is altered, the convolutions being unfolded, and the nervous matter spread out by a large collection of fluid in the interior cavities of the brain, constituting the disease called *chronic hydrocephalus*. I have not much to say upon what may be styled atrophy *proper* of the brain: that it will give rise to symptoms we cannot doubt, but that it shows itself by any peculiar or characteristic symptoms is what I have not discovered.

I shall content myself, on this subject, with showing you Cruveilhier's representation of a strongly pronounced example of atrophy of the entire cerebrum on one side. The drawing from which this engraving was made, was painted from the body of a patient who died in the Hôtel-Dieu, dropsical, in consequence of disease of the heart. He was forty-two years old. When you look at the engraving you will perceive that the left side of the cerebrum is diminutive compared with the right. It filled up, however, a larger space than it appears to do in the plate, for the lateral ventricle on that side was distended by a large quantity of serous fluid, which ran out when the ventricle was punctured; and then the surface of that side of the brain sunk down, and collapsed. Still the convolutions on that side, and all the dimensions, are remarkably less than on the other. The anterior lobe projected half an inch farther on the right than on the left side. The frontal bone, you will observe, is much thicker; twice as thick on the atrophied as on the natural side; and the frontal sinus very wide and open. The internal parts of the brain are all diminished in proportion. There was a large quantity of serous fluid filling and distending the subarachnoid cellular tissue. The nervous matter was whiter and harder on the atrophied side. One very curious thing is, that the *left* lobe of the *cerebellum* was the bigger of the two; but there was no such marked differences between them as between the two sides of the cerebrum.

Now the patient in whom this singular disproportion between the two sides of the brain was met with, had been incompletely hemiplegic, as long as he could recollect, on the right side; and the imperfectly palsied limbs were shrunk and withered, and the fingers of the hand contracted. Yet he had managed to walk about with the help of a

stick; and there was nothing remarkable, one way or the other, in the state of his intellectual faculties.

The same condition has been seen on both sides of the brain: the organ itself existing in miniature as it were, and lying at the lower part of the vaulted cavity of the cranium; the intermediate space being filled up with serous fluid. In long-standing cases of this description you must not suppose that the nervous matter has been compressed into a smaller compass by the effused fluid; but that the fluid has been poured out to fill that part of the skull which is empty of brain, and which must be filled with something. This condition of the cerebrum is accompanied by idiotcy.

Acute hydrocephalus.—I proceed in the next place to the consideration of that disease to which the name of *acute hydrocephalus* has been given. It is the *apoplexia hydrocephalica* of Cullen, in whose time it was not looked upon as being of an inflammatory character. It is, in fact, *inflammation of the brain*, as it occurs in *children*, and especially in *scrofulous* children. The inflammatory symptoms are not always well marked; and yet they do, of all the symptoms, call for the most attention. That the essence of the disease consists in inflammation we have abundant evidence, both in the juvantia, and in the appearances presented by the brain after death.

I made some observations, in the last lecture, respecting the nomenclature of diseases, and said something in defence of the name delirium tremens. Now it must be confessed that the disease we are at present considering was unfortunately named, when it was called hydrocephalus. I repeat that it matters not at all what name we assign to a disease, provided that the denomination does not carry with it any erroneous notion in regard to the *nature* of a disease. I think hydrocephalus a bad name, because it reminds us of one circumstance only of the malady, viz. the serous effusion, which, so far from being the *cause*, is only a *frequent effect* of the disease. But hydrocephalus, or water in the head, is an appellation so established, both among ourselves and with the public, that I cannot venture to propose any change.

After what I have already stated in respect to inflammation of the brain in adults, you will be prepared to hear that acute hydrocephalus (for I shall take the liberty of restricting that term to the disease as it occurs in *children*)—I say you will not be surprised to learn that acute hydrocephalus furnishes a great variety of symptoms; and many variations in the mode of their coming on, and in their combination and succession.

It is of the greatest importance to recognize acute hydrocephalus in its *earliest* stages; and even to look out for indications of its approach. I shall, therefore, describe those changes in the state of the young patient, which have been found to be, in many cases, premonitory that the disease was impending. But such symptoms are by no means always followed by acute hydrocephalus; nor is acute hydrocephalus always preceded by such symptoms. Still, when they do occur, they should put us upon our guard.

Precursory symptoms.—The *precursory symptoms* to which I allude consist chiefly in a morbid state of the nutritive functions. The child loses his appetite; or his appetite becomes capricious: he sometimes appears to dislike his food, and sometimes devours it voraciously: his tongue is foul, his breath offensive, his belly enlarges, and sometimes is tender; his bowels are torpid, and the evacuations from them unnatural; the stools are pale, and contain but little bile; or they are dark, with vitiated bile, fœtid, sour-smelling, slimy, or scybalous; and the child loses his former healthy aspect, becomes paler, and thinner. Even already there are some indications of derangement of the cerebral functions; the child is heavy, languid, and dejected; his customary spirit and activity are gone; he gets fretful and irritable, and is manifestly uneasy; and sometimes he shows a little unsteadiness and tottering in his gait.

In very young children, when the disorder is at hand or incipient, an unnatural wakefulness is often observable; a frequent sudden cry or scream, a clenching of the little fists, and a turning in of the thumb towards the palm of the hand, give warning also of the approaching malady.

Now when this sort of alteration is observed in a child who has any hereditary title to scrofula, or who bears the marks of the scrofulous *diathesis*, or is even a precocious and particularly clever child, and still more if he present any other indication of scrofulous *disease*, there will be much ground for apprehending that mischief is brewing within his

head. I advert to these tokens of scrofula, because inflammation, in a majority of cases, if not in all, is of a scrofulous character. But there is this peculiarity in it, which distinguishes it from scrofulous inflammation in most other parts, viz. that as it occurs in an organ of delicate structure, and one which is essential to life, its progress is more rapid, and it is more necessary to treat the disease vigorously.

It has been made a question whether the derangement of the digestive organs that has just been described is or is not the *cause* of the affection of the brain; or whether both the abdominal and cerebral disorder be not common and concurrent effects of the same cause. It is said that the stomach and bowels are more in the way of being acted upon by injurious influences than the brain, and that, therefore, the complaint may be supposed often to originate in their derangement; and great good, it is alleged, is done, the disease of the brain is often *prevented*, by remedying the disordered state of the stomach and bowels. On the other hand, it may be stated that a similar derangement of the digestive organs often comes on and lasts long in children, without leading to hydrocephalus; and hydrocephalus often attacks a child in whom no such symptoms of abdominal disease have appeared. We can never be certain, therefore, that hydrocephalus has been prevented, in any given case, by remedies addressed to the digestive organs. I cannot think the question is one of much practical importance: whether the disturbances of the nutritive functions *cause* the brain disease, or merely *indicate* it, they are equally valuable in directing our attention to the head.

In these little patients any source of irritation seems to act as an *exciting* cause: surgical operations, which are sometimes necessary at that tender age—falls or injuries of any kind—painful dentition.

Modes of attack.—There are, at least, three several ways in which the disease may make its attacks; and with these it is proper that you should be acquainted.

In the first place, it may come on *gradually;* after those symptoms which have already been spoken of as being premonitory. Probably this is the way in which it most frequently commences. After a period, of uncertain duration, in which the child has complained of occasional pains in the belly and head, and signs of derangement of the stomach and bowels have been present, the pain in the head begins to be more severe and to recur more frequently; it is not mere headache, but generally a sharp shooting pain, recurring at intervals; sometimes it affects one side of the head more than the other: the little patients wake and shriek out with the pain, and this in children is a very characteristic symptom; and as coma comes on this shrieking gives place to an habitual moaning, which is scarcely less characteristic. Very often in the beginning of the disease there is pain and stiffness at the back of the neck: sometimes there is much pain of the limbs in the early periods, and in some children extreme tenderness of the scalp, so that they cannot endure to have the head shaved. The pain of the head becomes complicated with vomiting, and both these symptoms are aggravated by motion. Very often nausea is excited by the erect posture, and the patient begs to lie down. The child sighs frequently, and looks grave or sad; his eyes are pained by a strong light, so that he knits his brows. The pulse becomes rapid, and the disturbance and irregularity in the abdominal functions increase. This stage of the complaint may last ten days or a fortnight, the child becoming daily more weak, and more peevish, and looking more and more ill.

In the second form of attack there are no premonitory symptoms; or they occur for a very short while only before the disease sets in suddenly and violently, with acute pain in the head and high fever; or with convulsion: the face is flushed, the eyes are brilliant; there is intolerance of light and of sound, and there are pain and tenderness of the abdomen. In short, the disease, when it commences in this manner, is very like an attack of continued fever. You may find these varieties described in Dr. Cheyne's excellent treatise on this disease. "We are led to suspect," he says, "some deeply-seated evil from the frantic screams and complaints of the head and belly, alternating with stupor, or rather lowness, and unwillingness to be roused; and we are struck with the great irritability of the stomach, which exists in a degree beyond what we generally find it in the fevers of this country; retching and vomiting being brought on by a change of posture, and certainly by every attempt to sit up in bed: and the disordered state of the bowels, which attends this irritability of the stomach, is also remarkable: and when at any time the child

has a little respite from the violence of these symptoms, we find our suspicions confirmed by his looks: for when the features do not express pain or terror, there is not unfrequently a vacancy of look, the eyes being *set*, with an expression of dejection which is peculiar to certain diseases of the brain." The mode of attack which has now been described, although the most regular in its progress, is not so common as the first, nor as the third, which I have yet to mention. The *third* way in which the disease makes its advances is very insidious: the head symptoms supervene upon the subsidence of some other disorder: scarlet fever, small-pox, hooping-cough, or any inflammatory or febrile complaint, or even after painful dentition. In these cases the early symptoms are often but slightly marked, or do not take place at all; the occurrence of convulsions or paralysis affording the first evidence that the brain is implicated. This is the most dangerous form of hydrocephalus. It has received the appropriate title of *waterstroke*.

Stages of the disease.—In whatever way the disease makes its invasion, it is apt to be attended with many and variable symptoms; and different observers, with the view of facilitating the description of the disease, and of making it more intelligible and more easily remembered, have divided the symptoms into groups, and considered each group as characteristic of a particular *stage* of the malady. But they have not all done this in the same way. It may be of use, however, to inform you of the different classifications which have thus been proposed. Dr. Whytt, who was almost the first person in this country who wrote upon this disease (I believe Dr. Paisley, of Glasgow, was *the* first: you may see his paper in the third vol. of the Edin. Med. Essays), Dr. Whytt I say, whose description is an extremely good one, took the *pulse*—which undergoes very remarkable variations in the course of the disease—as the ground of his division. He makes three stages of it therefore; the first, in which the pulse is frequent; the second, in which it is slow and irregular; and, the third, in which it again becomes frequent and feeble. These successive fluctuations in the pulse are to be noticed in very many cases. Dr. Gölis, again, an eminent German writer on hydrocephalus, whose little work has been translated by the late Dr. Gooch, as being the best book on the subject that he was acquainted with, makes four stages, according to what he believes to be the condition of the *brain* in each. First, he has the period of *turgescence*, which corresponds with that period in which the premonitory symptoms occur; secondly, the period of *inflammation;* thirdly, the period of *effusion;* fourthly, the period of *palsy*. The two last would appear to be almost identically the same. Dr. Cheyne makes three stages; which he finds marked, not like Dr. Whytt, by the state of the circulation, but by the state of the nervous system. Thus he calls the first the period of *increased sensibility*, when every stimulus produces an impression more than proportioned to its common effects. In the second stage, that of *diminished* sensibility, the child is not easily roused, his pupil is dilated, and his pulse slow; he is lethargic, with obstinately costive bowels. The third stage with him is that of palsy and convulsions, in which there is squinting, rolling of the head, stupor, convulsions, with a rapid thready pulse.

Cases often occur, however, that baffle all these attempts at classification. Convulsions, instead of being among the *last*, are not seldom among the very *first* symptoms. The pulse is sometimes remarkably *slow* at *the outset;* sometimes *frequent through the whole disease;* and sometimes *perfectly natural*.

I do not make these statements to magnify the difficulty of distinguishing the disease; for the diagnosis is really not so difficult as it has sometimes been represented; but to show you that you cannot trust to any succession of symptoms, still less to any one symptom, as being pathognomonic of the disease.

The symptoms that occur during the first stage are very variable, as you may suppose from what I have said of the different modes in which the disease is apt to set in. Those that are most constant are, pain of the head, severe shooting pain I say it seems to be, for the child puts its hand there, and cries out frequently "Oh! my head;" restlessness; inability to sit up; very disturbed sleep, with grinding of the teeth, and from this sleep the child often starts apparently in terror, and with a scream. The head is hot externally; the little patient is annoyed by light or noise; the pupils are contracted most commonly during this stage; the child is unwilling to be disturbed, and, therefore, does not reply readily to questions; but the replies, when made, are correct and rational. This stage is

marked also by vomiting, a total loss of appetite, a white tongue, offensive breath, costive bowels, unnatural stools, green often, or black, like tar, scanty and high-coloured urine. Dr. Gölis says that the abdomen, which has been tumid and tender perhaps, sinks down and becomes flat, without any increased excretion by stool; and that this is a very characteristic symptom. The pulse in this stage is frequent and sharp. In short, the symptoms are such (in general) as indicate very plainly that inflammatory action is going on within the head. Now the symptoms that characterize this first stage of the complaint sometimes rapidly pass into those which belong to the second. They may not be present for more than a few hours; or they may last a day or two, or several days; it is very seldom, I believe, that they continue longer than a week. The period answers, in the general character of the symptoms, to the period of *excitement* in encephalitis, which, in truth, is very much the same disease, modified by its occurrence in the adult subject.

So also the *second stage* of acute hydrocephalus corresponds, in its general features, with the period of *collapse* in encephalitis. The pulse becomes irregular, extremely variable and fluctuating, and often *slow;* it is easily accelerated, however, by the smallest exertion—by taking the child out of bed, or even raising him into a sitting posture. With this slowness of the pulse comes on a diminution of sensibility, and general heaviness and stupor; the pupils dilate, the light is no longer troublesome, the vision is imperfect, often it is doubtful whether the child sees at all. If the eye be closely examined and watched, the degree of light remaining the same, the size of the pupil will frequently be seen to fluctuate, or oscillate, till at last it is wide open and immovable; while this goes on, squinting takes place, and double vision when the child can yet see any thing. One or both eyes are turned in, or more rarely outwards. Noises do not now disturb or irritate the child—who lies on his back, with the eyes half closed, in a state of drowsiness or stupor, which is occasionally interrupted by some cry or exclamation expressive of pain. Convulsions frequently occur, but not uniformly; slight and partial spasmodic twitchings; or general and long-continued convulsions; paralysis; sometimes hemiplegia. The urine and stools are passed unconsciously. Sometimes the child, with feeble and tremulous hands, is incessantly picking his lips, or boring his fingers into his ears or nostrils.

This stage may last a week or two. And what is remarkable, it is often attended with remissions, sometimes sudden and sometimes gradual—deceitful appearances of amendment, and even of convalescence. The child regains the use of its senses; recognizes those about him again; appears to its anxious parents to be recovering;—but in a day or two it relapses into a state of deeper coma than before. And these fallacious symptoms of improvement may occur more than once.

The third stage does not differ materially in the character of the symptoms that accompany it, from the second, except that the pulse again becomes frequent, nay, uncommonly rapid; beating sometimes 200 strokes in the minute, so that you can scarcely count it. Dr. Whytt, in one instance, counted more than 210 pulsations. The child rolls its head perpetually from side to side; moans continually; waves its hands in the air, or one hand, the other frequently being palsied; sometimes there is paralysis of one side, and convulsive twitchings of the other. The circulation is very unequal; one part of the body will be found hot and dry, and another covered with a cold sweat; the cheeks are alternately pale and flushed; the child is raving, or insensible; the rapid pulse gets more and more weak; and at length the patient expires. In many instances death takes place in the midst of a strong convulsion. This last period is of very uncertain duration; it may be over in a few hours, or it may last a fortnight.

For my own part, I conceive that for all practical purposes it would be quite enough to make two stages only of this disease. In the first, the symptoms are those of active inflammation of the parts within the cranium, or of some of those parts; in the second, we have the symptoms that result from the consequences and products of the inflammation, from softening, and from the effusion of serum. And frequently these sets of symptoms are, in some respects, common to both these causes, and more frequently still they are mixed up together; effusion taking place, yet the inflammation going on. And we may understand how the whole collection of symptoms may vary and fluctuate, and assume an uncertain character, according as the inflammatory process has ceased, or is still in progress, according as it exists alone, or is mingled with the further source of cere-

bral disturbance that is furnished by its own events; and according as the inflammation may have come to an end, while its events remain behind, and declare their presence by appropriate signs in proportion to their place, and extent, and their various kinds and combinations.

Anatomical characters.—What are these events? In other words, what are the morbid appearances presented after death in acute hydrocephalus?

In some cases we find traces of inflammation of the membranes of the brain; a firm adhesion of the skull-cap to the dura mater; occasionally some adhesion of the opposite surfaces of the arachnoid membrane to each other. Very commonly there is an effusion of serous fluid beneath the arachnoid, in the meshes of the pia mater, and especially in the depressions between the convolutions. You would suppose, upon looking at this collected fluid through the arachnoid, that it had the consistence of jelly, but it is not so; if you divide the arachnoid, by means of a sharp scalpel, a perfectly limpid fluid makes its escape. Not unfrequently there are layers of coagulable lymph interposed between the arachnoid and pia mater: this is a most unequivocal evidence of foregone inflammation; and it is more frequently met with in the strongly marked cases. When portions of the cerebral mass are removed by slicing it, a great number of red points are often observed, speckling its cut surface: I mention this appearance just to say, that, to the best of my belief, it does not warrant any conclusion in respect to the state of the brain before death. We find these red spots numerous in many cases, when there had been no cerebral affection manifested during life; and they are not always to be seen when we are certain that there has been inflammation. With respect to the nervous matter itself, it is said to be sometimes softer than natural, and occasionally it has been found infiltrated, as it were, with serous fluid, *wet*, and *so* rendered soft. Gölis describes an instance of this kind, in which, he says, the fluid could be expressed from the cerebral substance as from a sponge.

But the most common and characteristic change is *softening of the central parts of the brain, with an effusion of serous fluid into the ventricles.* Generally the effused fluid is thin and watery; serosity rather than serum. It contains less animal matter, perhaps, than any other animal production. Dr. Bostock found that of 100 parts, 98 6 consisted of water, 1 part of salt, and 4 only of animal matter. It is not, therefore, in common, coagulable by heat. The quantity effused is uncertain; speaking generally, it varies from two to six ounces.

But the effused fluid is not always clear and limpid; sometimes it is turbid, like whey, or even puriform, with flocculent shreds floating in it. These have been considered as flakes of coagulable lymph; but I question whether, in many cases, they are not merely fragments of the softened and broken down materials in the neighbourhood; for the septum lucidum, the fornix, and other parts forming the walls of the ventricles, are very commonly found softened, and pulpy, or entirely disorganized. The septum lucidum is perforated perhaps by a ragged irregular opening, the softened portion having fallen out: the fornix has lost its consistence, and often its figure, or falls asunder when the most gentle attempt is made to raise it. Dr. Abercrombie holds not only that this softness is the result of inflammation, which I think cannot reasonably be doubted, but that the inflammation of these central white parts constitutes the essence of the disease, in very many cases of acute hydrocephalus; and what bears him out in this opinion is the interesting fact, that this softened condition of the septum lucidum, fornix, and corpus callosum, may be fatal without any effusion of serum, and without any other morbid appearance, although with all the symptoms which are usually considered to indicate acute hydrocephalus. He relates two striking examples of this kind; one of them was as follows:—A woman became affected with violent pain in her head, shooting from temple to temple. She was extremely restless, tossing from one side of the bed to the other: her eyes were slightly suffused, and impatient of the light; pupils contracted; the pulse 60, soft and rather weak. She was repeatedly bled, both generally and topically, and used purgatives; cold applications to the head, blistering, &c. For three days she was much relieved by these measures; the violent pain was removed, and she complained of pain only when she moved her head. She was quite sensible, but oppressed, and inclined to

lie without being disturbed. At the end of four days her speech became affected, of which she was aware, for she said she felt a difficulty in getting out her words. Then came stupor, and at times incoherence, and double vision, and at last coma, and dilated pupil. She died on the eighth or ninth day of the disease.

The fornix and septum lucidum were found broken down into a soft white pulpy mass: there was no effusion in the ventricles, and no other disease in any part of the brain.

Not unfrequently scrofulous tubercles are discovered in the substance of the brain; and it is probable that these would have been *more* frequently met with, if they had always been carefully looked for. They consist almost universally of a cheesy kind of matter, like that of tubercles in the lungs.

You will find a good deal said by writers on this disease, of morbid appearances found in other parts besides the brain, and especially in the abdominal organs,—enlargement of the liver, inflammation of its peritoneal covering, a preternatural development of Peyer's glands, tuberculous matter in the glands of the mesentery. One remarkable change is very often seen, viz. intussusception of the small intestines. This probably takes place a short time only before death, and appears to be the result of spasmodic or irregular motions of the bowels, analogous to those which are observed in the voluntary muscles. The intussuscepted portions are easily pulled out, and show no marks of inflammation.

Pathology and causes of the disease.—There have been endless discussions respecting the true pathology of acute hydrocephalus, and it may be proper that I should offer you a few remarks upon this point, before I proceed to the treatment of the disease.

I need not, I conceive, take any farther pains to convince you that the disease is essentially inflammatory. We are inevitably led to that conclusion by the *symptoms*, which nearly resemble those that occur when undoubted inflammation has arisen from injuries of the head: by the *appearances on dissection*, which are *always* such as inflammation may have produced, as softening and effusion of serum; and *frequently* such as nothing but inflammation could have produced, as suppuration, and the formation of adventitious membranes; and lastly, by the unequivocal relief given (as we shall see) by blood-letting, and other evacuations, the blood drawn being sometimes also sizy.

Now, many persons, as I have already hinted, lay great stress, when discussing the pathology of acute hydrocephalus, upon the previous unhealthy state of the nutritive apparatus. They hold that the primary disease—the *fons et origo mali*—lies in the stomach, or bowels, or liver; and that the brain affection is secondary, and caused by sympathy with these distant parts: and this opinion they fortify by referring to the frequency of organic disease, met with after death, in the abdominal viscera. In accordance with these views of its origin, they propose to cure, or to prevent, hydrocephalus, by redressing the faulty condition of the digestive organs.

Now this, in my judgment, is not only an erroneous, but an unsafe doctrine: for it tends to divert our attention from the head, and to suggest a feeble and inadequate plan of treatment. The grand predisposing cause of acute hydrocephalus is certainly the scrofulous diathesis, and this is why we see the complaint run so often in families; so that one child having died of that disorder affords much ground for apprehending that others, belonging to the same family, will become victims to it. The constitutional tendency is hereditary, and children born with it are liable and likely to have strumous disease set up in various organs at once, or perhaps in succession; not, however, a succession of cause and effect, but of common relation to one pervading disposition. We need not be surprised that scrofulous inflammation should affect the brain and abdomen at the same time. When we find obvious organic disease of the brain, scrofulous tubercles for instance, which must have been antecedent to the hydrocephalus, it would be just as absurd to look to the abdomen for the cause of the hydrocephalus, as it would be to seek in the brain for an explanation of the cause of diarrhœa or dysentery, when the liver or colon were known to be diseased.

I do not mean to assert that the morbid conditions of the brain and of the abdomen are perfectly independent each of the other. The vomiting that is so constant a feature of acute hydrocephalus, the constipation that is so common a consequence of head affections,

afford familiar evidence of the influence which cerebral disorders may exercise upon the abdominal functions. Conversely, any disease in other parts of the body may react injuriously upon the brain, and may sometimes be regarded as an exciting cause of disease in that organ.

The period of life is also a strong predisposing circumstance; acute hydrocephalus being much more frequent during infancy and childhood than at any subsequent time. It is said that fifty children are attacked by it in the first five months of life, for one child that has it afterwards. But it may occur at any age up to the twelfth or fourteenth year. After that period it is comparatively rare.

Whatever tends to deepen and aggravate the scrofulous *diathesis*—improper or insufficient nutriment, exposure to cold, inadequate clothing, impure air—may be regarded as a *predisposing* cause of acute hydrocephalus. And whatever tends to call scrofulous *disease* into action, may be reckoned among the possible *exciting* causes of acute hydrocephalus. Any general irritation may bring it on. It sometimes supervenes upon the drying up or repression of eruptions, as tinea capitis, or sores behind the ears. Such eruptions, therefore, occurring in strumous children, we must not attempt to cure suddenly; and free purging should be employed when they begin to disappear. The irritation produced by difficult and painful dentition is a very frequent exciting cause; and this is a source of danger which, in many cases, may be obviated by timely and judicious management. Violent heating exercise has sometimes, apparently, kindled the cerebral inflammation. Among the exciting causes we may place all physical injuries which jar and stun the brain; blows on the head, falls from a height, although the head may not be the part struck: and all moral agencies which shock or strongly disturb the nervous system; severe bodily pain, violent fits of anger, sudden fright. Gölis goes even so far as to say that great terror and distress of mind *in the mother* during the latter months of pregnancy may lead to the occurrence of acute hydrocephalus in the child, and he brings forwards this curious fact in support of his opinion:—A large proportion of the children that were born in Vienna soon after the bombardment of that place by the French, in 1809, were seized with convulsions within a month after their birth, and died of inflammation within the cranium; effusion of coagulable lymph between the membranes, and of serum in the ventricles, being discovered on dissection.

LECTURE XXVI.

Acute hydrocephalus continued. Prognosis and mortality of the disease. Treatment: blood-letting; purgatives; cold; mercury; blisters. Prophylaxis. Spurious hydrocephalus. Chronic hydrocephalus, or dropsy of the brain. Shape of the head and face. Anatomical conditions. Symptoms.

The disease, of which I described the symptoms in the last lecture, acute hydrocephalus, is a very dangerous disease; and, when once it is fairly established, many more die of it than recover. Our chance of saving the patient's life, by active treatment, is always greater in proportion as the complaint, or the tendency to the complaint, is detected *early;* and for that reason the precursory symptoms possess so high an importance.

When our treatment commences while the symptoms are as yet rather those of a precursory state, than of the confirmed disease, it is impossible to say how many of those cases which, under such treatment, terminate favourably, would otherwise have ripened into well-marked hydrocephalus; and we must be content to have it said, without its being possible for us to refute the assertion, that not all of the disorders which we treat as acute hydrocephalus are really instances of that disease. We must act upon the worst supposition, and not wait until the nature of the symptoms *demonstrate* that the malady is

present, while they demonstrate also, at the same time, that it is well nigh hopeless. These are cases which peculiarly demand decision on the part of the medical man; and we are bound to act, in some instances, upon very slight indications; as when, for example, we perceive what we think threatenings of acute hydrocephalus in a scrofulous child, or in a child belonging to a family in which others have already been cut off by that disorder.

It has been supposed, by some, that the case is hopeless after effusion has taken place, but we cannot be sure of that; nay more, there are no symptoms by which we can ever tell *for certain* that effusion *has* taken place.

I remember to have heard it gravely maintained, in the debating societies which I sometimes attended when a student, that there are no such things as absorbents, and no absorption, in the brain; and therefore that perfect recovery from the serous effusion in that organ is impossible. But this notion is refuted by plain and well-known facts. We shall see hereafter, that blood poured forth within the nervous pulp is capable of being removed by absorption. How an opinion so palpably erroneous could ever have found credit, except with that class of men who can or will believe nothing which they cannot see, I am at a loss to guess.

The prognosis, always doubtful or bad, is a little better when the disease is violent, and occurs in tolerably healthy subjects, than when it creeps on slowly and insidiously, and in weakly, scrofulous patients. In the former case there is more room for the adoption of active measures; and the disease is more likely to be amenable to remedies, and less likely to be obstinate; it is also less likely to depend upon a permanent cause, such as the existence of a scrofulous tumour in the brain.

The probable issue of the disease is often judged of by the state of the pulse. The quick pulse belonging to the early stages of the disease will become slow; but it may become slow in two very different ways; it may diminish in frequency in a gradual and moderate manner, and then we may hope that the alteration proceeds from the progressive declension of the fever; or it may drop suddenly, which would be a reason for our fearing that the second stage of the disease was about to establish itself. We must take care, under the former circumstances, not prematurely to assert that the disease is in the decline, and the patient safe. On the other hand, if the pulse has been morbidly slow, a gradual and slight increase in its frequency must be considered as a favourable omen; while its rapid and great acceleration would show that the disease was passing into its worst and final stage.

I have already cautioned you against being misled by that deceitful truce, and apparent improvement, which is apt to take place in the course of the disease. If the signs of amendment continue, or make progress, during two or three entire days, we may venture to admit a little more hope. But the patient can never be considered secure while any approach to what are thought symptoms of effusion remains; while the pupil continues dilated for example, or even so long as it does not contract briskly under a strong light.

The prognosis is especially bad when acute hydrocephalus supervenes upon other disease; or when it is engrafted (as it sometimes is) upon the chronic form of the disorder. It is very seldom that the acute form subsides into the chronic.

To show you that we are warranted in the expectation of sometimes carrying our patient through this most perilous malady, I will mention a few statistical facts that have been recorded in respect to its mortality. Dr. Odier, of Geneva, states that, upon an average, eighteen cases of acute hydrocephalus occur every year in that place; and of these six get well: *i. e.* the recoveries are to the deaths as one to two. Dr. Gölis, to whose work I referred in the last lecture, and who had charge of a large institution for children in Vienna, gives an account of thirty-seven cases, out of which five recovered. He had seen, upon the whole, forty-one instances of recovery from acute hydrocephalus. Dr. Mills, who has also written on the disease, has narrated twenty-eight cases, all of which died but seven; and M. Bricheteau lost four out of eleven. Adding these together, and taking the average, we have seventy-six instances of the disease, and sixteen recoveries: rather more than one in five. The cases in which recovery took place were mostly those in which vigorous measures were adopted *early*.

Treatment.—The treatment resembles that which I have already recommended when

adults are attacked with encephalitis. It consists, therefore, in blood-letting, purgatives, cold to the head, mercury, and perhaps blisters. Of course the differences of age will require some modification of these remedies.

The only event of the inflammation compatible with the safety of the patient is resolution; and this we must endeavour to bring about (I cannot too often repeat it) by *early* as well as by free blood-letting: bleeding from the arm; or by leeches; or by cupping. You must bear in mind that in very young children, leeches produce the effect of general blood-letting. It is a matter of obvious importance to ascertain to what extent we may safely and beneficially carry the abstraction of blood in infants. Dr. John Clarke, a man of large experience (the elder brother of the present Sir Charles Clarke), says that very young children will very well bear the loss of blood, even to fainting, once or twice repeated: but that their powers are apt to sink if the bleeding, to that extent, be had recourse to oftener. For infants a year old, three ounces is reckoned a good bleeding. I do not like opening the jugular vein, for reasons with which you are sufficiently acquainted; and if the requisite quantity of blood cannot be obtained from the vein in the arm, cupping from the nape of the neck may be substituted; or leeches may be applied to the temples, or to the mastoid processes. It is necessary to recollect that leeches generally produce a freer discharge of blood in children than in adults, on account of the greater activity of the capillary circulation in the former. No general rule can be prescribed in respect to the number of leeches that should be applied; three will take as much blood in one case as half a dozen in another; but assuming that one leech will, on an average, cause the discharge of one ounce of blood, we may apply three of them to a strong infant of six months, when the symptoms are violent. Of course the bleeding is to be suspended if syncope occurs. In older children the quantity of blood necessary to be taken will be larger: six ounces of blood taken from a vein is a full bleeding, I should say, for a child fixe or six years old. I mention these quantities as mere approximations, as guides to what you may expect to find practically required: the true measure and test of salutary blood-letting in this, as well as in other inflammations, is to be found in the effect it has at the time. The first bleeding, in what manner soever the blood is taken, should be a sufficient one; should produce a decided and manifest impression. By attending to this rule you will break the force of the disease more surely, and more safely too, than by drawing blood in frequent driblets; a mode of using the remedy calculated to subdue the patient rather than to overcome his malady. You must afterwards go on with the leeches to the head, or you must withhold them, according to the exigency of the particular case; according to the condition of the pulse, the continuance or the cessation of the pain, the increase or diminution of the fever, and so forth. And you are not to forget that as you have to deal, in general, with scrofulous children, any *superfluous* removal of blood, the abstraction of more than is required for extinguishing the inflammation within the head, will be likely to prove injurious to the general system. After the full formation of the comatose state, a farther prosecution of the bleeding has sometimes been rapidly followed by death.

Next in rank and importance to bleeding come *purgatives*. They are to be exhibited with the threefold view of correcting depraved secretions, of clearing the alimentary canal of its irritating contents, and above all, of deriving, as the phrase is, from the head; producing a discharge of the watery parts of the blood, and taking off the stress from the cerebral arteries. The best forms of purgative medicine to be used for these purposes with children consist of calomel and jalap, or calomel and scammony; and if these do not act freely, senna and salts must be given in aid of them. I have already made you acquainted with Dr. Abercrombie's high opinion of the efficacy of purgative medicines in inflammation of the brain, whether in the child or in the adult. Dr. Whytt, again, states that he never saw even temporary relief of the symptoms produced by any other means than those which increased the evacuations. Purgatives are to be administered, therefore, at an early period. But sometimes the stomach is so irritable that it will reject them. A previous bleeding will correct this; and it is no small part of the benefit derived from the abstraction of blood, that it prepares the way for the more effectual operation of aperients and of mercury. A large clyster will often be of service, both in settling the stomach, and in procuring stools, when there is much vomiting, and a continual rejection of medicine given by the mouth. Dr. Cheyne mentions a form of

medicine by which he sometimes succeeded in quieting the irritable stomach, and procuring evacuations; he would give a drachm or two of magnesia, saturated with lemon juice, every two or three hours. You may sometimes get calomel and scammony, however, to remain in the stomach, when almost every other medicine is rejected. The purgative plan should be steadily persisted in for several days.

To show you how torpid the bowels are apt to be in this disease, and how difficult it sometimes is to procure evacuations from them, I may mention the following circumstances which I heard Dr. Alison relate as having occurred in the practice of his uncle, the late Dr. Gregory, of Edinburgh. He had one patient who took 140 grains of calomel in the course of five days, yet his bowels were not relieved, till he had also taken two doses of jalap; the first of 30, and the second of 35 grains. In another case, a child of twenty-eight months took in nine days 350 grains of calomel (nearly 40 grains a day); and in six of these days 136 grains of jalap (more than 20 grains a day): the effects were a gentle purging from the jalap, none from the previous calomel, and but slight salivation. The child recovered after having been nearly in a comatose state. Of course large doses of this kind are never to be given, until the inefficacy of smaller ones has been ascertained.

Cold applied to the head:—I have before given you examples of its power. It is especially useful in the early stages of the disease, when there is much heat, and when evacuations have been obtained. I am doubtful about the propriety of keeping *ice* in contact with the surface of the head in very young children. It will in many cases be sufficient to lay a linen rag wet with cold water (or spirit and water, to promote evaporation), upon the child's head, taking care to renew it frequently, not merely as often as it gets *dry*, but as often as it gets *hot;* or water may be poured from a pitcher upon the head, a basin being held under the chin. Dr. Darwal states that he has known cases, which seemed utterly hopeless, recover by letting water fall in a small succession of drops upon the scalp, and continuing it until the head no longer recovered its high temperature upon intermitting the dropping. I need scarcely say that under all circumstances it is expedient to keep the head somewhat elevated. The influence of this mode of applying cold to the head is increased, and, perhaps, rendered safer, by immersing the lower extremities of the patient at the same time in warm water.*

Different opinions have been held in respect to the value of mercury in this disease. Knowing how powerful an influence it has in controlling inflammatory action, and that the inflammation in acute hydrocephalus often leaves behind it traces showing that it was of the adhesive kind, I should not omit giving mercury; but at the same time (as I stated when upon the subject of encephalitis) I should not give it with the direct object of affecting the gums, of producing ptyalism. I believe the evidence respecting the efficacy of mercury carried to salivation in acute hydrocephalus is this:—that some few very desperate cases have got well, the improvement commencing at the time when the mercurial influence on the system was becoming apparent; and that in other cases, the occurrence of salivation has been followed by *no* alleviation of the symptoms, but the disease has run on, unchecked, to its fatal termination. But the truth is that it is a very difficult matter to salivate a child; there is a great reluctance in the system, at the earlier periods of life, to take on the specific mercurial action; and the disinclination seems peculiarly strong during the presence of this disease; and the younger the child, the more difficult is it to affect the gums. Perhaps this may be considered fortunate; for when salivation does take place in these little patients, it sometimes proceeds to an alarming extent. Dr. John Clarke, who employed calomel largely in a variety of diseases, never saw more than three instances in which salivation was produced in children under three years of age.

* Since this lecture was delivered, I have read Dr. D. Davis's recent publication on acute hydrocephalus. He adds the weight of his experience in attestation of the great efficacy of blood letting, early performed, and carried to deliquium animi. He mentions, likewise, a mode of applying cold, which is new to me, and likely, I should think, to be extensively useful. It forms one of the many valuable purposes to which caoutchouc has of late years been turned. A bag, or hollow cushion, made of the water proof material invented by Mr Mackintosh, is first filled with very cold water; then about half of its contents is suffered to escape, and the remainder secured by turning the stop-cock. Thus no air is included. Upon this cushion the patient's head is to be placed. The "water cushion" applied in this manner to the back of the head, does not preclude cold lotions to the forehead. It will be necessary, so long as the application appears grateful to the patient, to replenish the cushion from time to time with cold water.—T. W.

If you are desirous of taking the chance of the specific influence of mercury doing good, you had better give calomel as a part of the purgative plan, and rub in some of the mercurial ointment; you had better do this than lock up the child's bowels by combining opium with the calomel; not to mention the injurious effects of opium in young children in general, and in the early period of head affections in particular. The calomel should be given steadily, in full and equal doses, at equal intervals. Green evacuations from the bowels, resembling wet tea-leaves or chopped spinach, usually follow its continued administration: and this appearance (like the rising of the gums in adults) is generally regarded as a proof that the influence of the mineral is felt by the constitution, and that it is doing all the good of which it is capable.

Of blisters I may repeat the substance of what I stated when we were considering encephalitis. I should abstain from them at the commencement of the disease. Even when applied at a distance from the head, they are apt to prove a source of hurtful irritation in these young and susceptible subjects. But in the second stage of the malady, I believe blisters are often of good service. They may be applied to the nape of the neck, or to the head: and several may be applied in succession; or the ulcerated surface may be kept open by the help of irritating ointment, such as the unguentum cantharidis, or the ceratum sabinæ.

These are the main remedies to which we trust in the treatment of acute hydrocephalus: bleeding, purgatives, cold, in the outset; mercury and blisters, of more equivocal efficacy than the former, in the more advanced stages of the disease. When there is much irritability towards the decline of the disorder, or in its latest period, opiates may cautiously be tried; they sometimes have appeared to be extremely beneficial: two or three grains of Dover's powder is a very eligible form of opiate in such cases.

I do not feel called upon to say any thing, in addition to what I stated in a former lecture, about other remedies that have been proposed in acute hydrocephalus; digitalis, colchicum, squills, antimony. I have told you the remedies which *I* believe to be the best; and which will save the patient, when judiciously used, if the case be within the compass of our cure: and you will do well to learn how to manage these powerful means. I am confident you will find *that* more to your purpose than trying now this and now the other remedy because it is new, or because some persons tell you they have been wonderfully successful with it.

Let me say a word in reference to the *prevention* of this disease: concerning which your advice will be sure to be asked again and again. In families, in which acute hydrocephalus has occurred, or that show decided marks of the scrofulous diathesis, the earliest attention should be paid to any deviation from the healthy condition of any of the functions. Children in such families should be kept upon a nourishing but light and unstimulating diet; consisting of well-dressed vegetables, farinaceous substances, and a moderate proportion of animal food. Particular care should be taken to keep the bowels regular; not that weakening purges should be given, but the bowels should be fairly relieved at least once every day. Any disturbance of the digestive organs should be immediately corrected; by antacids, laxatives, change of diet, and sometimes by mercurials, as the hydrargyrum cum cretâ. Such children should also, if possible, be brought up in the country, and freely exposed to mild and dry air; and in winter great care should be taken to have them sufficiently clothed. During the hazardous period of dentition, the state of the teeth and the gums must be sedulously attended to. There is good reason for believing that a seton or an issue in the neck or arm has been very serviceable in warding off and preventing attacks of the disease. Dr. Cheyne mentions some striking instances of the good effect of establishing an artificial irritation at some distance from the brain, when there has been a disposition to disease in that organ.

There is another caution, too, which you will often find reason for suggesting; and that is, not to press or encourage the development of the mental faculties in children who are quick and intelligent beyond their years. Parents are apt to be proud of the early acquirements of their little ones: they are not aware that such precocity of the mind implies danger to the health of the body; and they provide them with instructors, and to a certain extent abridge their hours of exercise and amusement, that they may do justice to their cleverness. But it is our duty to admonish them of the risks they are thus run-

ning: and to advise them to think only, for the present, of corroborating the corporeal strength of the child; and to avoid over cultivation of his intellect until this dangerous period of his existence is got over.

Spurious hydrocephalus.—There is still one point remaining, and one of great importance, in relation to the acute hydrocephalus of children. I told you in the last lecture, that the diagnosis was generally not difficult; and I have told you in the present lecture, that it will be your duty to act sometimes upon a conjectural diagnosis, as if you *knew* the disease was present, or at hand, although, in truth, you only *suspected* it to be so. But there is a form of disorder very apt to be mistaken and treated for acute hydrocephalus, and one which may be rendered fatal, if the *remedies* of acute hydrocephalus be applied to it. Encephalitis, whether it occur in the child or in the adult, has its spurious double. As in morals, every virtue has its corresponding vice, which apes its actions and assumes its garb, so is it also with many opposite bodily disorders; and it is of great moment that we should be capable of discerning the essential difference of character that lurks beneath external similarity of feature. It is a most curious, but unquestionable fact, that *anæmia* of the brain, a diminution of its natural supply of red blood, will produce symptoms very much resembling those which result from the diametrically opposite condition. If you pay no regard to the state of the general circulation, as indicated by the temperature and by the pulse, you will find the actual symptoms of syncope, and of apoplectic fulness, to be identically the same. When a human being bleeds to death,—as many do from wounds, from uterine hæmorrhage, and so on,—what do we see? Why the patients may have nervous delirium, become convulsed, and then insensible, with a wide and fixed pupil. The outward visible signs of concussion and of compression of the brain are very much alike. The vulgar always confound them, and are clamorous that a vein should be opened: a measure which would be proper and useful in the one case, but most mischievous in the other. It is the same with the functions of other parts: we have palpitation of the heart when that organ is insufficiently supplied with blood; palpitation when it is over supplied: dyspnœa, or hurried breathing, when the lungs are congested; hurried breathing when blood does not arrive in them plentifully enough. You must see that the importance of distinguishing between the causes of these analogous phenomena is immense. Several authors in modern times have noticed the condition of the brain to which I now wish you to attend: Dr. Abercrombie, Dr. Marshall Hall, and the late Dr. Gooch. Their observations were made and published each, I believe, independently of the other's. Dr. Gooch's paper is entitled, "Of some Symptoms in Children erroneously attributed to congestion of the Brain." His description of the state in question is very graphic; it is chiefly indicated by heaviness of the head, and drowsiness. The age of the little patients whom he had seen so affected was from a few months to two or three years; they were generally small of their age, and of delicate health, or had been exposed to debilitating causes. Dr. M. Hall has found this condition to succeed the diarrhœa often produced by weaning; or the ill timed administration of purgative medicine; or the application of leeches for some previous complaint. The physician finds the child lying on its nurse's lap, unable or unwilling to raise its head; half asleep; one moment opening its eyes, and the next closing them again, with a remarkable expression of languor. The tongue is slightly white, the skin is not hot, at times the nurse remarks that it is colder than natural; in some instances there is at times a slight and transient flush. In all the cases that Dr. Gooch saw, the bowels had been already disturbed by purgatives; the symptoms had invariably been attributed to congestion of the brain; and the remedies employed had been leeches and cold lotions to the head, and purgatives—especially calomel. Under this treatment they had gradually got worse, the languor had increased, the pulse become quicker and weaker, and at the end of a certain number of days the little patients had died: in two instances he had known, during the last few hours, coma to come on, stertorous breathing, and dilated and motionless pupils. Dr. M. Hall describes a precisely similar set of symptoms.

I will take one of Dr. Gooch's cases in illustration, and give it you in his own words. "I was going out of town (he says) one afternoon, when a gentleman drove up to my door in a coach, and entreated me to go and see his child, which he said had something the matter with its head, and that the medical attendant of the family was in the house,

and just going to apply leeches. I went with him immediately, and when I entered the nursery I found a child ten months old, lying in its nurse's lap, exactly in the state which I have already described; the same unwillingness to hold its head up, the same drowsiness, languor, absence of heat and all symptoms of fever. The child was not small of its age, and had not been weak; but it had been *weaned* about two months, since which it had never thriven. The leeches had not been put on. I took the medical gentleman into another room, related the foregoing case (*i. e.* a case in which a child had been leeched out of its life), and several similar to it, which had been treated in the same way, and had *died* in the same way. Then I related to him a similar case which I had seen in the neighbouring square, which had been treated with ammonia and decoction of bark, and good diet, and which had recovered; not slowly, so as to make it doubtful whether the treatment was the cause of the recovery, but so speedily that at the third visit I took my leave. He consented to postpone the leeches, and to pursue the plan which I recommended. We directed the gruel diet to be left off, and no other to be given than ass's milk, of which the child was to take at least a pint and a half, and at most a quart, in the twenty-four hours. Its medicine was ten minims of the aromatic spirit of ammonia in a small draught every four hours. When we met the next day the appearance of the child proved that our measures had been right; the nurse was walking about the nursery with it upright in her arms. It looked happy and laughing. The same plan was continued another day; the next day it was so well that I took my leave, merely directing the ammonia to be given at longer intervals, and thus gradually withdrawn; the ass's milk to be continued, which kept the bowels sufficiently open without aperient medicine." This case contains both a picture of the morbid state, and a summary account of the treatment it requires. Instead of the sal volatile, you may occasionally substitute with advantage from five to ten drops of brandy mixed with arrowroot. You are to restrain diarrhœa if it exists; give the child plain nourishing diet—there is none so good for it as that furnished from a mother's breast; keep its extremities warm with flannel, and if the season permit, let a current of mild fresh air blow freely over it.

When the symptoms are more ambiguous, you will derive great assistance in judging of the true nature of the case, by tracing the manner in which it came on, and the causes to which it seems to be attributable: and in very young children—in respect to whom the question is most likely to arise—you may often determine between congestion or exhaustion, between fulness or emptiness, by a very simple and easy test, which is not adverted to, as far as I remember, by any of the three writers whom I have mentioned. I mean, by taking notice of the state of the unclosed fontanelle. If the symptoms proceed from plethora, or inflammation, or an approach to inflammation, you will find the surface of the fontanelle convex and prominent, and you may safely employ, and expect benefit from, depletion. If, on the other hand, the symptoms originate in emptiness and want of support, the surface of the fontanelle will be concave and depressed; and in that case leeches, or other evacuants, will do harm, and you must take to a better diet, ammonia, and so forth. It may be as well to remark, that in the unclosed state of the fontanelle, the observations formerly made respecting the peculiarity of the circulation in the brain, do not apply.

Chronic hydrocephalus.—All that has hitherto been said has relation to *acute* hydrocephalus, which is an *inflammation*. I have next to speak of *chronic* hydrocephalus, which is a *dropsy*. From some cause, not well understood, a watery fluid collects within the skull, most commonly in the ventricles of the brain; and this occurring at the earlier periods of life, before the whole of the brain case has become solid, the containing parts yield to the increasing pressure, and the size of the head is augmented in various degrees; at the same time the cerebral functions are more or less deranged. This dropsy of the cranial cavity often commences before the period of intra-uterine life is completed, and the head of the fœtus becomes so large, that it cannot pass with safety into the world. Accordingly, many of these infants perish at the moment their separate existence commences:—*nascentes moriuntur*. The pressure of the maternal pelvis is fatal to them; or the diseased head bursts; or is crushed by the accoucheur, to preserve the life of the

mother. The skull is emptied of its contents, and the shell, if I may so call it, collapsing, passes through the natural outlets.

In many cases, however, the dropsical skull is expelled entire and unhurt, and the infant lives for a shorter or longer period. Sometimes the fluid does not begin to accumulate till after birth: in a few days, however, or after some weeks, or some months even, the head is perceived to enlarge with a rapidity quite disproportioned to the growth of the other parts of the body; and enlarging, it becomes misshapen also. The intervention of the membranous partitions called fontanelles and open sutures, between the ununited bones, allows the centrifugal pressure of the gradually accumulating water to modify the shape of the head. These membranous interspaces are unnaturally wide, and more numerous than in healthy children. Nevertheless the process of ossification goes on, but the bones are extremely thin. We see but little islands of bone in seas (as it were) of membrane. By degrees, if the child survives, the proportion of membrane to bone becomes less and less, and at length the whole brain case is hard, and firmly closed up, its surface exhibiting an unusual number of joinings; there are many *ossa triquetra*.

Shape of the head.—In the meantime the direction and relations of the loose and yielding bones, are altered. The os frontis is tilted forwards, so that the forehead, instead of slanting a little back, rises perpendicularly, or even juts out at its upper part, and overhangs the brow. The parietal bones bulge above towards the sides; the occiput is pushed back; and the head becomes long, broad, and deep, but flattened on the top. This, at least, is the most ordinary result. In some instances, however, the skull rises up in a conical form, like a sugar loaf. Not unfrequently the whole head is irregularly deformed, the two sides being unsymmetrical. Some of these rarer varieties of form are fixed and connate; others are owing, probably, to the kind of external pressure to which the head has been subjected.

While the *skull* may be rapidly enlarging, the bones of the *face* grow no faster than usual, perhaps not even so fast; and the disproportion that results gives an odd and peculiar physiognomy to the unhappy beings who are the subjects of this calamity. They have not the usual round or oval face of childhood. The forehead is broad, and the outline of the features tapers towards the chin. The visage is triangular. This great disproportion of size between the head and the face is diagnostic of the disease, and would serve to distinguish the head of a hydrocephalic child from that of a giant. Heartless parents sometimes make a wretched profit of the deformity. A penny show of that kind existed very recently in the immediate vicinity of this College.

Anatomical characters.—When, after death, we explore the physical causes of these singular deviations from the natural figure and bulk of the cranium, we find that they proceed from the pressure of accumulated water: the complaint is manifestly a dropsy. But the situation of the water, and the condition of the brain itself, are subject to some curious varieties.

In a certain number of cases the brain is incompletely formed; deficient in some of its parts, or even altogether wanting. That portion of the cranial cavity which should contain the nervous pulp is filled up by a thin pellucid fluid. From some unknown cause, operating during the period of intra-uterine life, the progressive formation of the brain has been arrested. Marks of imperfect development are often visible in other parts of the same infants; they have a hare-lip, a bifid spine, or a fissured palate. It is in cases of this kind generally that the skull, unnaturally small, perhaps, is pinched up into a conical peak, and has considerable thickness. They are obviously hopeless cases. To the physiologist they are subjects of much interest: for the practical physician they have none.

But in the majority of instances, when the infants survive their birth, the liquid is contained in the central cavities or ventricles of the brain, which are expanded into one. The convolutions are unfolded, and the cerebral matter is spread out into a hollow sphere; the irregularities of the surface have disappeared; the whole of the brain is smoothly extended in a thin layer, immediately beneath the bones and the membranes that connect them, and surrounds the inclosed liquid like a bag. Less frequently a different state of matters is seen. The liquid, instead of being included within the cerebral substance, lies in contact with the dura mater; while the brain, perfect in all its essential parts, is at the

bottom of the cavity. The difference, however, is more apparent than real: the two conditions are substantially the same, only that, in the one case, the solid parts that lie around the ventricles gradually expand as the fluid slowly collects, much as an air-balloon dilates in proportion as gas is introduced within it; while in the other case the seams, or commissures (as they are technically called), that unite the hemispheres of the brain, give way, or are deficient, so that the ventricles and the general sac of the arachnoid form together one huge cavity; the hemispheres are turned aside, or folded back, the surfaces that naturally have a *central* aspect look *upwards*, and seem to constitute the summit of the cerebrum. This was the state of the parts within the immense skull from which the largest of the casts before you was taken. It belonged to a man named Cardinal, who died in Guy's Hospital, in 1825, and of whom Dr. Bright has given a very interesting account.

Symptoms.—Now some of the consequences of this distension of the brain and skull with watery fluid are simply mechanical. The child is top-heavy. His large unwieldy head is too much for the muscles of his head to sustain without fatigue; or even, when they are unassisted, to sustain at all. He walks gently and carefully, like a person balancing a heavy load upon his head; or he holds and partly carries his head with his hands, as a milkmaid steadies and supports a pail; or he reclines the weight of his burden upon the chair, or table, as he sits.

But far more important effects of the disease are those which relate to the three great functions of the brain. The child is soon found to be deaf, or blind; or palsied in one or more of its limbs; or idiotic; or all these. In other words, the special senses, the power of voluntary motion, and the mental faculties, are apt to be defective or perverted. Instances, however, do occur, in which these functions are, for some time, but little deranged. The greater number of those who are afflicted with dropsy of the brain either recover or die during their infancy. Still, a few survive, bearing their complaint to the adult period, and even to old age; and in some of these individuals who, with excessively large heads, have yet numbered many years of existence, the intellect and the senses, if not entire and perfect, have been sufficiently effective to answer the common wants and purposes of social life: the moral emotions strong, the feelings lively and correct, the memory tolerably retentive, the reasoning powers respectable. Dr. David Monro relates the case of a hydrocephalic girl, six years old, whose head measured two feet four inches in circumference. She is described by him as being "as lively and sensible as most of her age," and as "having a strong memory." Dr. Bright's patient, Cardinal, was nearly thirty years of age when he died. He was born in 1795. At the time of his birth, his head was only a little larger than natural; but it had a pulpy feel, as if it were almost destitute of bony matter. A fortnight afterwards, it began to increase rapidly; and when he was five years old, it was but little less, according to his mother's account, than when he died. He could not walk alone till he was nearly six, and then only on level ground; if he attempted to run, or to stoop, he fell down. He was sent to school when he was about six, and soon learned to read well and to write tolerably; but writing he soon gave up, because, as he was near-sighted, it obliged him to stoop, which he could not conveniently do. When a candle was held behind his head, or when his head happened to be between a spectator and the sun, the cranium appeared semi-transparent; and this was more or less the case till he was 14 years old. About the age of 23, epileptic fits began to occur; and after that, his health, which previously had been very good, failed somewhat. The ossification of the skull was not complete till two years before his death, the anterior fontanelle being the last part that closed. It has been mentioned that he was near-sighted; but he was very quick of hearing, his taste was perfect, and his digestion good. Dr. Bright states that his mental faculties were very fair, and his memory tolerable; but it was not retentive of dates. It was said that he was never known to dream. There was something childish and irritable in his manner, and he was easily provoked. He died, at last, of fever and diarrhœa. There were seven or eight pints of fluid within the cranium, in contact with the dura mater. On the base, or floor, of the skull lay the brain, with its hemispheres opened outwards, like the leaves of a book.

How comes it that the cerebral functions are thus sometimes fulfilled, or go on so well, when the machinery through which the mental powers are manifested—the instrument

whereon and whereby the immaterial principle mysteriously operates—is so palpably and greatly deranged? How comes it that life, and especially the life of the mind, subsists at all? These questions open very interesting considerations. It would appear, from such cases as I have been referring to, that the curious arrangement and collocation of the several parts of the brain is rather a matter of convenient package than of necessary relation. The pulp which furnishes the medium of sense, and thought, and volition, is there, but it is disposed in an unusual shape. In neither of the two varieties of the malady that have been described as being compatible with prolonged existence, is there any necessary diminution of the cerebral mass. The brain itself, which forms a bag in the one case, and is split in halves in the other, has been found to weigh quite as much as a healthy brain at the same period of life. There has been no loss, therefore, of substance; the pressure has been gradual, and it has not been made to act injuriously by counter-pressure; no countervailing resistance has been afforded by the rigidity of the brain case: and thus the unopposed distending force neither causes absorption of the cerebral pulp on the one hand, nor, on the other, induces coma, or convulsions, or idiocy, by its compression.

Most commonly, however, the mental and voluntary functions are maimed or perverted; and these serious calamities make parents look at a large head in a young child with anxious solicitude. But you are aware, after what I stated on this subject in the last lecture, that the head *may* be extravagantly large without dropsy of the brain and without disease.

We have just seen, that while the brain itself is gradually unfolded, or its hemispheres are parted and turned aside by the liquid accumulating within the cranium, the functions of the organ may suffer but little, so long as the yielding brain-case permits the expansion or separation of the nervous substance, without inordinate pressure. But as soon as undue pressure begins to be exercised, then morbid symptoms arise, or the defects that have previously shown themselves are aggravated. Hence that period of life becomes a perilous period, at which the skull, by the closure of its fontanelles and sutures, loses its capability of farther expansion. In some rare cases the closed sutures reopen under the augmenting pressure, and a respite is thus obtained. Dr. Baillie has recorded an instance in which this happened in a boy seven years old. A similar case is mentioned in Dr. Yeats' work on hydrocephalus. The patient was a boy nine years of age. The sutures of his skull separated again after having been united; and it was remarked that the teeth in the jagged edges, whereby the bones interlock with each other, were much fewer than is common. If this be always so when the sutures give way, it will serve to facilitate our understanding how such a phenomenon can take place. The skull may, however, go on expanding, although the sutures are permanently closed; there still being left intervals between the several points of ossification, which intervals are covered by membrane only. The beautiful preparation on the table, showing this remarkable state of the cranium, I have borrowed for your inspection, from Dr. Sweatman's museum.

Indeed, although I have spoken of this complaint as being especially a disease of childhood, it does occasionally commence long after the skull has become a complete case of bone. Enlargement of the head, in these cases, is impossible; but this circumstance, and the symptoms it is apt mechanically to produce, form the only differences between the disorder as it affects the child and the adult. In both cases disturbance of the cerebral functions arises, and at length convulsions and coma close the scene. In both a dropsical state of the ventricles of the brain constitutes, often, the only morbid change presented after death. A young and distinguished lawyer of my acquaintance had one or two attacks of rather sudden loss of consciousness, while engaged in the Court of Chancery; by degrees he became dull, stupid, forgetful, and, at length insensible. In this condition he died. A large quantity of serous liquid was found distending the ventricles of his brain. No other alteration could be detected.

Dr. Baillie describes a case of chronic hydrocephalus that occurred in a man fifty years old. Six ounces of fluid were contained in the lateral ventricles. He had been paralytic on the right side of the body; and for eleven months before his death had lost the recollection of his own language, with the exception of four or five words; which he employed, with different intonations, to express his various wants.

The celebrated Dean of St. Patrick's afforded another instance of the same disease, attended with a similar interruption of the power of discoursing. The case, as related in Sir Walter Scott's *Life of Dr. Swift*, is curious, and contains an early suggestion of a piece of practice which in our own day has met with more favour. "A few days afterwards he sunk into a state of total insensibility, slept much, and could not without great difficulty be prevailed on to walk across the room. This was the effect of another bodily disease, his brain being loaded with water. Mr. Stevens, an ingenious clergyman of his chapter, pronounced this to be the case during his illness, and upon opening his head it appeared that he was not mistaken; but though he often entreated the Dean's friends and physicians that his skull might be trepanned, and the water discharged, no regard was paid to his opinion or advice."

He remained from October 1742, to October 1745, in a state of silence, with few and slight exceptions; and died in the 78th year of his age.

Gölis also mentions three instances in which this disease began in advanced life: two of the patients were above seventy years old; the third, who was a physician at Vienna, likewise died in the decline of life, having suffered under the disorder for ten years.

Now, what can we do in these wretched cases? Seldom much good, I am afraid. Yet something we must try, for parents will flatter themselves with hopes of a cure; and to say the truth, there have been, under judicious management, a sufficient number of recoveries to forbid our despairing in any case, and to make it incumbent upon us to employ carefully all those measures which have occasionally brought the disease to a favourable termination. Gölis even affirms, that of the cases which began after birth, and which he saw and treated early, he was fortunate enough to save the majority.

LECTURE XXVII.

TREATMENT OF CHRONIC HYDROCEPHALUS: INTERNAL REMEDIES; MECHANICAL EXPEDIENTS; BANDAGES, TAPPING. SYMPTOMS OF SPINAL DISEASE. INFLAMMATORY CONDITIONS OF THE SPINAL MARROW.

THE cure of chronic hydrocephalus may be attempted by internal remedies, or by external mechanical expedients, or by both.

Internal remedies.—The internal remedies by which most good appears to have been effected, and from which, therefore, most is to be hoped, are diuretics, purgatives, and above all, mercury, which is believed by many to have a special and powerful influence in promoting absorption. Conjointly with these, the abstraction of small quantities of blood from the head, by means of leeches, has been found beneficial.

Gölis advises that calomel should be given in half-grain doses, twice a day; or if that should purge too much, in doses containing only one-fourth of a grain. At the same time he would rub a scruple or two of mercurial ointment, mixed with ointment of juniper berries, upon the scalp, every night. He recommends that the head should be kept constantly covered also by a woollen cap. Infants require, he says, no other nutriment than good breast milk; while older patients should take a moderate quantity of meat. In mild weather they should be as much as possible in the open air. Under this plan of treatment he affirms that he has known the circumference of the head decrease by half an inch or an inch in a period of six weeks or three months; and that perseverance in this method has frequently, in his experience, been followed by the perfect recovery both of the mental and of the bodily powers. If no improvement should be perceptible in two months, he advises that diuretics should be given, *with* the former remedies; the acetate of potash, or squills, or both: that an issue should be made in the neck, or in each arm, and be kept discharging for several months. And he thinks that when convalescence has once begun,

it is often much accelerated by minute doses of quina; the fourth of a grain, for example, thrice daily.

In a disease so unpromising as chronic hydrocephalus, we are warranted in trying any plan that has been found or supposed to be useful. An apothecary of considerable experience—now dead—once took the pains to write out and send to me the particulars of two cases in which he had seen a peculiar mode of administering mercury successful. I will give you them nearly in his own words.

He had a lad, fourteen years old, whose name was Scott, under his care, with chronic hydrocephalus, in the year 1817. He had been ill two or three years. He was nearly blind, had very little use of his lower extremities, and could not walk across the room without support. He suffered violent pains in his head, and was unable to bear the least pressure on his scalp. His bowels were constipated, and his pulse "oppressed." Cupping and blistering, and the blue pill, and drastic purgatives, and ordinary diuretics, tried in combination and succession, gave him temporary relief; but no permanent benefit was obtained. Dr. Gower then suggested a plan which he had himself found successful in such cases, and which had first been used by Dr. Carmichael Smyth, who had recorded ten cases of recovery under its adoption. Dr. Gower's plan was to rub down ten grains of crude mercury with about a scruple of manna, and five grains of *fresh* squills: this was to be one dose: and it was to be repeated every eight hours.

My informant rubbed the quicksilver down with conserve of roses, and then added the fresh squills, making the whole into the consistence proper for pills, with liquorice powder. The patient took this dose three times a day, for nearly three weeks, without any ptyalism being produced. Its effects were great prostration of strength, and loss of flesh, with gradual relief of all the boy's sufferings. It operated profusely by the kidneys. The medicine was continued twice a day, and at length once, for another fortnight; when all the symptoms of the disease had disappeared. The boy was greatly emaciated: he was then ordered an ounce and a half of Griffith's mixture thrice daily; and soon regained his health and strength, and got quite well. And he remained well eight years afterwards.

The success obtained in this case led to the pursuance of a similar course in that of the son of a well-known fishmonger in Old Bond Street. He was about twelve years old, and afflicted in nearly the same manner as Scott, except that the pain in the head was more acute, and caused violent screaming: *relief* had been repeatedly given for a time, by cupping. The physician in attendance was unwilling to try the plan, when it was proposed to him, but said that he would give what was equivalent—small doses of blue pill, with squills in powder. The result was salivation in a few days, without any amendment. In about three weeks, the effects of the mercury having subsided, and the patient then suffering extreme pain in the head, loss of sight, and want of power over the lower extremities, my informant was desired to adopt any measures he thought fitting. The medicine was given as in the former case, and with the same happy consequence; it acted, as before, without producing ptyalism, but with a great reduction of strength and flesh: health was restored by steel, after the symptoms of hydrocephalus had disappeared. This cure was also permanent.

I think you will give me credit for not being over fond of recommending what I may call *conundrums*, instead of well-tried and approved means of cure; but I say that in such a complaint as chronic hydrocephalus, we have generally the opportunity of testing the virtues of many reputed remedies, one after another; and we are not to despise or neglect any measures that have been found beneficial, merely because they are out of the way, or because we cannot see in what manner they can excel the more common formulæ.

You will observe that these were cases in which the disease came on some time after the sutures of the skull had closed.

Mechanical remedies.—The mechanical remedies of chronic hydrocephalus are two: and they have a totally opposite mode of action. By the one, the brain is compressed; by the other, it is lightened of its pressure; yet both of them have proved successful What does this show? what, but a confirmation of the doctrine that there are different states of the encephalon, very dissimilar in their essential character, yet having some symptoms in common; and those the most likely of all to catch our attention. Such

common symptoms resemble an algebraical symbol, which derives its value from the plus or minus sign prefixed. Surely it is of vital importance to study, and if we can to settle, the differences whereby these inverse conditions, requiring contrary remedies, may be discriminated.

Bandages.—Bandaging the head is one of these two expedients; *puncturing* it the other. Neither of them is practically applicable after the bones of the skull have united.

Bandages appear to have been suggested by the notion that the increase of the fluid within the head, and probably some of the symptoms too, might depend, more or less, upon the want of firmness and proper resistance in the outer containing parts; in the feeble and half solid skull. A certain amount of support and pressure seems requisite for the due exercise of the cerebral functions. Beyond this amount all increase of pressure is hurtful. The middle point of safety it may be hard to hit. It is certain that the easy yielding of the bony walls of the head, by reason of the membranous interspaces that exist in the early periods of life, proves oftentimes the safety of these patients. If the skull did not expand as the water gathered, morbid symptoms would ensue. Great nicety must therefore be required in the use of this remedy. While the head is palpably enlarging, compression by means of plasters or bandages would probably be mischievous. When the disease is stationary, and the unconnected bones of the skull are loose and fluctuating, and the child is pale and languid, much benefit may be expected from moderate and well-regulated support. The late Sir Gilbert Blane was the first, I believe, to suggest this mode of treatment; but its safety and efficacy have been more recently demonstrated by Mr. Barnard, of Bath, who has related several examples of complete success from the employment of bandages. In these cases the children were pale, bloated, and feeble, with flabby muscles: the bones of their heads were movable and floating, and the functions of the brain more or less impaired. Mr. Barnard applies strips of adhesive plaster, about three quarters of an inch wide, completely round the head from before backwards; covering the forehead from the eyebrows to the hair of the head, as low down on the sides as the ears will permit, and lapping over each other behind. Then, cross strips are carried from one side of the head to the other over the crown; and lastly, one long slip, reaching from the forehead, within half an inch of the root of the nose, over the vertex to the nape of the neck. In his first trial of this plan, but never afterwards, Mr. Barnard laid pieces of linen, wetted with cold water, ever the plasters. The only internal medicine given was castor oil, to regulate the bowels. The effects, in all this gentleman's cases, were these: a gradual diminution of the size of the head; mitigation, and ultimate disappearance, of all head symptoms, such as strabismus, rolling of the eyes, starting of the muscles, and convulsions; and at the same time, increased tone of the muscular system, an improved appearance of the skin, and of the secretions from the bowels. These are striking results: they show that, in certain conditions of chronic hydrocephalus, a part of the danger arises from a lack of due support and confinement of the brain; and they prove that compression alone may be equal to the cure.

But in children who are not of this pale and feeble habit, and in whom ossification of the skull goes on, the period when the walls cease to yield is the period of danger. The water continuing to accumulate, inordinate pressure begins to take place. To such heads the application of bandages or plasters must, if nothing else be done, be insufficient or unsafe. The brain-case being no longer capable of expansion, there remains to be attempted a reduction of the quantity of the liquid which it contains.

Tapping the head.—Now any considerable diminution of the accumulated fluid, through the agency of mere absorption, is scarcely to be expected; even although we endeavour to aid that process by applying leeches and cold to the head, and by purgatives, or diuretics, or diaphoretics. Some mode, more certain and effectual, of emptying the distended cavity, has therefore been earnestly sought after; and the second mechanical expedient of which I have spoken offers a very sure method of attaining this object. He must have been a bold physician who first proposed to decant the water from the brain, by means of a perforation, made with a trocar, through the membrane of the fontanelle, the membranes of the brain, and even the expanded cerebral substance itself. But the success of the project has amply vindicated his happy audacity. It is not a very new suggestion, but it has received particular attention in this country of late years: and though tapping the brain

in chronic hydrocephalus has been denounced as useless and cruel by some high continental authorities, by Gölis and Richter especially, it furnishes one of the best of the few chances of safety to the patient. Of course I mean ultimate safety, for the operation itself is attended with the present risk of accelerating the patient's death. Other means, however, failing, we are justified in advising that hazard. We have to consider that by performing the operation we incur the danger of abbreviating the existence of a being, whose life, without it, could scarcely be long continued, or capable of enjoyment: but then we afford *some* chance of a perfect cure. A speedy death, or an uncertain life of mental and bodily imbecility, or complete restoration: these are the three events to be looked at. Of the three, the second is, in my judgment, incomparably the worst; and if the case were my own, if I had to decide the painful question in reference to one of my own children, I would accept the alternative of probable speedy death on the one hand; possible complete recovery on the other.

To say the truth, the *immediate* danger is not so very great as you might suppose; provided that the operation be skilfully and cautiously performed, and only a moderate quantity of water drawn off at a time. That even a very rough operation is not necessarily fatal we learn from a singular case related by Mr. Greatwood. A child, 15 months old, afflicted with chronic hydrocephalus, fell down, and struck the back part of its head against a nail, which penetrated the skull. Above three pints of water gradually flowed out at the orifice thus made, and the child was cured.

I will mention a few instances in which tapping the brain has been performed; for I know no better mode of showing you the manner in which the operation should be done, the cautions to be attended to in doing it, and what kind of succes it has had.

There is an account of the performance of this operation by Lecat, in the *Philosophical Transactions* for the year 1751. This date is subsequent to the period when the Rev. Mr. Stevens suggested the propriety of trepanning Dean Swift's cranium. In 1778, Dr. Remmett, of Plymouth, punctured the head of a hydrocephalic child on five several occasions, with a lancet, and took away, in all, no less than 80 ounces of fluid; five pints, as pints were measured in that day. The child died 17 days after the last tapping. A very interesting case of the same kind is related by Dr. Vose, of Liverpool. His patient was an infant seven months old. Its head was more than twice the ordinary size. Three operations were performed; the first with a couching needle. Upwards of three ounces were on that occasion evacuated; and it was estimated that about the same quantity dribbled away afterwards. The child thereupon became very weak, but was presently revived by some cordial medicine. About six weeks afterwards, the liquid having collected again, an opening was made with a bistoury, and 8 ounces were removed; and 9 days after that 12 ounces more, without any bad consequences. The head diminished in size, the patient got apparently well, and the case was published as a successful one. Unfortunately, however, the complaint afterwards returned, and the child died of it.

Mr. Lizars, of Edinburgh, operated upon a little patient of his twenty times in the course of three months; using a small trocar. Upon the escape of the water, squinting, and dilatation of the pupils, which previously existed, ceased immediately. The child recovered. Another very striking and instructive instance is recorded by Mr. Russell, of Edinburgh. The patient was an infant three months old, with an enormous head: twenty-three inches in circumference, and fifteen inches and a half from one ear to the other. The child was affected with strabismus, and a perpetual rolling of the eyes. The usual routine measures, compression among the rest, had been employed without any success. By four operations performed at intervals of about ten days, the size of the head was considerably diminished: but, the fluid continuing to collect, calomel was given in small and frequent doses, and the gums became sore, and the child got well. At eight months old the dimensions of the head were less, by four inches in circumference, and by two inches and a half across the vertex, than they had been previously to the first tapping; and the sutures had entirely closed.

But Dr. Conquest, of Finsbury Square, has, more than any other person, given authority to these operations. In a paper published in the Medical Gazette in March 1838, he tells us that he had then tapped the heads of 19 children for this complaint, and in 10 of the 19 cases the children survived. He introduces a small trocar through the

coronal suture below the anterior fontanelle, and cautiously makes pressure upon the head afterwards by means of strips of adhesive plaster; and he closes the wound in the integuments carefully after each time of puncturing. The greatest quantity of liquid withdrawn by him, at any one time, has been twenty ounces and a half; and the greatest number of operations on any one child has been five, performed at intervals of from two to six weeks. The largest total quantity of water removed was 57 or 58 ounces, by five successive tappings.

This expedient, therefore, though doubtless hazardous, is really a valuable one. The rules relating to its performance may briefly be summed up. The operation should scarcely be had recourse to until other means have failed. The trocar should be small, and it should be introduced perpendicularly to the surface, at the edge of the anterior fontanelle; so as to be as much as possible out of the way of the longitudinal sinus, and of the great veins that empty themselves therein. The fluid should be allowed to issue very slowly; and a part only of it should be evacuated at once. The instant that the pulse becomes weak, or the dilated pupil contracts, or the expression of the child's countenance manifestly alters, the canula should be withdrawn, and the aperture in the skull closed. Gentle compression should be carefully made, to compensate, in some degree at least, the pressure that has been removed with the fluid. Should the infant become pale and faint, it must be placed in the horizontal posture; and a few drops of sal volatile, or of brandy, mixed with water, should be given. Sometimes slight inflammatory action comes on in the course of a day or two after the tapping. When this happens, we must apply cold lotions, and leeches, and use the other remedies which I mentioned before, as proper to subdue such inflammation.

I once got a surgeon to perform the operation upon the infant of a poor woman, after I had tried in vain all the other measures that I have spoken of. To our horror, when the trocar was withdrawn from the canula, instead of clear serosity, a fine stream of purple blood spouted forth. The opening was at a considerable distance from the longitudinal sinus; but the trocar was not so delicate as it might have been, and I presume that one of the larger superficial veins had been pierced. I do not think, either, that the instrument was introduced in a sufficiently perpendicular direction. Of course the risk of hitting a vein is increased when the trocar is carried obliquely inwards: and a large portion of the cerebral mass is also wounded. We naturally thought it was all over with the child, which presently became deadly pale and faint. A verdict of *infanticide by misadventure* stared us in the face. But under the use of stimulants the infant revived again; no hæmorrhage went on internally, as we apprehended it would; but the child, after a day or two, was very much the better for the loss of blood. This amendment, however, did not last; and the mother, who had been terrified by the immediate consequences of the operation, feared to come near me, lest I should wish to have it repeated: and at length our patient died. I had no opportunity of examining the condition of the head internally, which I had much desired to do.

On one subsequent occasion I have witnessed the operation. The subject of it was an infant about eight months old. Four months after its birth, its head was observed to grow inordinately large. At the time of the operation the fontanelles were exceedingly tense; the child screamed frequently, occasionally vomited, and was slightly convulsed; the features were pinched, and the eyeballs distorted downwards; but the pupils were not dilated. Four ounces of transparent liquid were let out by puncturing the anterior fontanelle. A few hours afterwards the child was tranquil, and much improved in aspect; the distortion of the eyeballs had disappeared. Three ounces more were taken away the next day. For two days thereafter all the symptoms appeared to be mitigated; but the skull was flaccid; yielding, like a broken egg, to the gentlest pressure. On the evening of the fourth day after the first tapping, the respiration became hurried, the child grew dull, and, before midnight, expired. In this case it appeared to me that the chance of success was baulked by the want of external support subsequently to the tapping.

You will not expect me to draw any comparison between the merits of compression and of paracentesis, as substantive remedies. They are opposite measures, and adapted to different and opposite conditions of the brain. The one supplies defect of pressure; the other relieves its excess. To hold the balance even requires much care, a steady and

gentle hand, an accurate judgment, and incessant vigilance. Either expedient may suffice, alone. Both may be (and have been) profitably employed in the same case, in succession, according to its varying circumstances. If the walls of the head be tight and firm, the trocar should precede the bandage; if lax and movable, compression should be cautiously tried, and followed, if need be, by the puncture.

I have now done with the *inflammatory* affections of the *brain:* in conjunction with which I have also considered some other morbid conditions, that are either connected with inflammation of the contents of the cranium, or resemble it in some of their phenomena. Thus, I have spoken of *delirium tremens*, which is apt to be mistaken for inflammation of the brain: of *softening from disease of the cerebral arteries*, which is liable to be confounded with inflammatory softening: of *tumours* of different kinds, which tend to produce inflammation, or symptoms like those belonging to inflammation: and of *chronic hydrocephalus*, which sometimes is the sequel, sometimes the precursor, of acute hydrocephalus; and has other points of analogy with that disease, the encephalitis of children.

Spinal cord.—Before I take up the subject of apoplexy, and of cerebral hæmorrhage, I wish to direct your attention to the inflammatory conditions of the spinal cord.

The whole pathology of this portion of the nervous system is extremely interesting; but it has not yet been so thoroughly made out as to enable any one to give a very systematic or satisfactory account of it. In addition to those numerous difficulties with which I showed you in a former lecture that the entire subject of the diseases of the nervous apparatus is beset, there is this farther obstacle to our studying diligently the disorders of the spinal marrow—that much labour and expense of time are required for exposing the interior of the vertebral canal; which is, therefore, too often neglected in examining the dead body.

There are certain points in the anatomy and physiology of the spinal cord which it is necessary that you should bear in mind, if you would have any clear notions even of what has been learned in respect to its pathology.

1. In the first place the spinal cord (including the medulla oblongata) is the seat and centre of that remarkable property, the reflex function; by which so many of the automatic movements of the body are governed.

2. In order that we may feel, or be conscious of, what occurs in any part of the trunk or limbs, and in order that our will to move any such part should be successful and obeyed, it is necessary that there should be a continuity of nervous matter between the part in question and the brain. If the cord be cut across at any point, or so crushed as to be thoroughly disorganized at that point, a complete abolition of sensation and of *voluntary* motion ensues in all those parts of the body that receive their sentient and motor nerves from that portion of the cord which lies beyond the place of the injury, reckoning from the brain; and what is true in this respect of the mechanical division of the cord, is equally true of such disease as pervades and spoils the nervous matter composing it.

Now it follows from this, that the effect of disorganizing forms of disease—as well as the effects of injury—vary greatly according to the part of the cord they occupy.

Thus any injury or disease situated in the spinal marrow, and pervading its whole thickness, where it is contained within the upper cervical vertebræ, is inevitably fatal at once; producing suffocation by paralyzing those muscles by the play of which the motions of respiration are performed. You know that the intercostal muscles and the diaphragm have at all times the main share in carrying on the mechanical actions of respiration; and probably they execute the whole action in every case of ordinary breathing. Now the intercostal muscles are supplied with nerves from the spinal cord all along the dorsal vertebræ; and the diaphragm is principally supplied by the phrenic nerves, which are chiefly derived from the third and fourth cervical nerves. These muscles obey the will; but they act also independently of the will. The pneumogastric nerve, with respect to them, is an excito-motory nerve, and calls into play a reflex power which is transmitted from the medulla oblongata. Hence any profound injury of the spinal cord, above the origin of the phrenic nerves, stops both the voluntary and the involuntary movements of the respiratory muscles, and the individual perishes by apnœa in as strict a sense as

though the access of air to the lungs had been suddenly prevented by a ligature drawn tightly round his windpipe.

Again, when a segment of the cord, however small, is disorganized in its cervical part, between the origin of the phrenic and the origin of the upper intercostal nerves, the breathing is not instantly suspended; but is performed entirely by means of the diaphragm, the intercostal muscles having no share in it. The ribs cease to rise and fall; and the abdomen is alternately protruded, and sinks back again. In each case I suppose the disease of the cord to be such as suffices to paralyze the parts supplied with nerves from it, below the seat of the disease. If disease of this kind occur below the giving out of the intercostal nerves, the breathing is not affected; we have *paraplegia* only, palsy and loss of feeling in the lower extremities, and, perhaps, in the hips, or even higher. Now a person in this condition *may* live a long time. When the disease is situated between the origin of the intercostal nerves and the origin of the phrenic, he may live a few days, but he seldom lives a week, and he never survives a month: and when the disease is higher still, in the very upper part of the cord, above the origin of the cervical nerves, he perishes outright. The *kind* and *degree* of disease, therefore, being the same, the character of the symptoms, and the amount of danger, differ remarkably according to the *seat* of the disease.

3. Although sense and voluntary motion cease upon the disruption of the communication with the brain, the excito-motory functions of the separated portion of the cord are not necessarily suspended. On the contrary, they seem to acquire increased activity. The automatic power is apt to run riot, as it were, when the controlling influence of the sensorium is lost. Each of you has probably seen the limbs of a recently decapitated frog thrown into violent action by the stimulus of galvanism. I have witnessed the same thing in the human body after death by hanging. What is still more curious, you may have unequivocal manifestations of similar phenomena in the *living* body. I have lately been informed, by Dr. William Budd, of a case in which a man was afflicted with paraplegia, in consequence of disease of the vertebral column. He was totally deprived of the power of moving his lower extremities. Sensation in them was almost, yet not entirely extinct. A sharp pinch, or the prick of a pin, he could feel; but slight friction he was quite unconscious of: yet (as he himself said) his limbs were not; for when the inner edge of the foot was brushed or tickled by the hand of another person, the corresponding leg, over which he had no voluntary control, would start up, and be briskly convulsed. The same thing took place, in both limbs, whenever he passed his urine or fæces; so that he was obliged to have an apparatus of straps and ligatures to keep the legs down on such occasions. I have seen something like this myself.*

Separate and different filaments of the spinal cord connect themselves with, or help to form, different nerves which emerge from the cranio-spinal axis. A knowledge of this fact enables us to understand how it happens (as it sometimes does happen) that the upper extremities are deprived of sensibility, or of voluntary motion, or of both, by disease of the cord, while the same functions remain perfect in the lower and more distant limbs. Here the disease must have spared those strands or filaments of the cord which pass down to connect themselves with the nerves given off at the lower part of the spine; while it has affected those strands or filaments only which belong to certain nerves from the upper part.

4. You must bear in mind also the important discovery of Sir C. Bell, that the two roots by which each spinal nerve arises have distinct and different functions; the anterior roots being composed of motor fibrils, the posterior of sensiferous.

It is not so clear, although that opinion is, I believe, a prevalent one, that the anterior *columns* of the spinal *cord* are subservient to the purposes of motion, and the posterior to the faculty of sensation. This has been inferred, too hastily perhaps, from the ascertained endowments of the anterior and posterior *roots* of the *nerves;* and cases are cited which appear to favour such a notion: but then other cases go completely to contradict it. Thus Mr. Stanley has recently published an account of a patient who died in St. Bar-

* This very interesting case has since been published, in detail, with several others resembling it, in the 22d volume of the Medico Chirurgical Transactions.

tholomew's Hospital. For some time before his death he had been completely unable to move his lower limbs, throughout their entire extent; while there was no discoverable impairment of sensation in any part of either limb. The spinal cord was the only part found diseased; and the disease was strictly limited to its posterior half or column. This portion of the cord, in its whole length, from the pons to its lower end, was of a dark brown colour, and extremely soft and tenacious. The anterior half, in its entire length, exhibited its natural whiteness and firm consistence. The roots of the spinal nerves were unaltered.

It was remarkable, and illustrative of the difficulty of these subjects, that with the change of structure which the cervical portion of the cord had undergone in this instance, there was no impairment either of motion or sensation in the upper limbs.

5. We must not forget that the brain, and the spinal cord, which are distinct from, but yet continuous with each other, sympathize largely and mutually under disease. This circumstance throws an additional obscurity over the study of their morbid conditions. It is one, however, which we cannot avoid, but which we must estimate and allow for, in our observation of diseases, as we best may.

6. There are a few remarks made by Dr. Abercrombie in relation to some of the anatomical dispositions of the cord and its investing membranes, which may help us to comprehend better some of their morbid contingencies. Thus, with respect to the dura mater of the cord, it is practically of importance to recollect "that it adheres very slightly to the canal of the vertebræ by a very loose cellular texture; and that it adheres very intimately to the margin of the foramen magnum. In this manner a cavity is produced betwixt the membrane and the inner surface of the spinal canal (external, *i. e.* to the membrane), which cavity may be the seat of effusion, and which has no communication with the cavity of the cranium. On the other hand, the space between the dura mater and the pia mater (or membrane immediately covering the cord), communicates freely with the cavity of the cranium; so that fluid may pass easily from one to the other, according to the position of the body."

Inflammation of the Spinal Cord.—I shall pursue the same order, in speaking of the inflammatory affections of the spinal cord, as I followed in respect to the analogous conditions of the encephalon. And, first, let us inquire what has been noticed of inflammation of the *membranes* of the cord. They may undergo inflammation, independently of the substance of the cord, and independently of the brain; but this is not very common. Usually, when we have meningitis of the cord, we have the same disease also within the cranium: usually also, with meningitis of the cord, we have more or less inflammation of the nervous matter composing it. The commonest symptoms of inflammation of the meninges of the cord (for I do not pretend to speak of the several membranes separately) appear to be pains, often intense, extending along the spine, and stretching into the limbs, and aggravated usually by motion, and simulating therefore rheumatic pains: rigidity or tetanic contraction, and sometimes violent spasms of the muscles of the back and neck, amounting in some instances to perfect opisthotonos: a similar affection of other muscles also, as those of the upper or lower extremities: a sense of constriction in various parts, in the neck, back, and abdomen, as if those parts were girt by a tight string: a feeling of suffocation: retention of urine: obstinate constipation: and with these symptoms, rigors often.

You are not to expect all the symptoms which I have been enumerating in every case: they will vary according to the seat and extent of the inflammation. We need not wonder at the spasmodic symptoms, when we recollect that the nerves which issue from the body of the cord receive a covering from its pia-mater. The pain felt along the course of the spine itself is said to be aggravated by percussion of the spine, but not by simple pressure; and this seems very likely.

I know of no way in which I can so well hope to awaken an interest in you about these diseases, or to offer you instruction respecting them, as by instances. The following I take, abridging it somewhat, from Dr. Abercrombie. A man, twenty-six years old, had for several years been subject to suppuration of the laft ear; suffering occasional attacks of pain on that side of the head, which were followed by a more copious discharge from the ear. In the first week of April he became ill, with pain of the forehead

and occiput, disturbed sleep, and loss of appetite; but no fever. At the end of the week he complained of pain extending along the neck. This pain gradually passed downwards in the course of the spine, and deserted the head; and at last, after many days, it fixed itself with intense severity at the lower part of the spine; shooting thence round the body towards the crests of the ilia. He became affected also with great uneasiness over the whole of the abdomen, and with great pain and difficulty in passing his urine. About the end of the second week in April his sufferings had become extreme. He could not lie in bed for five minutes at a time, but was generally walking about the house in a state of great agitation, grasping the lower part of his back with both his hands, and gnashing his teeth with the intensity of the pain. He had no interval of ease, and was sometimes incoherent and unmanageable. On the 18th he went to take a warm bath, walking down three stairs, and into an adjoining street, with little assistance. His speech afterwards became somewhat affected: there were convulsive twitches of his face, and difficulty of swallowing. Some transient squinting also was observed. The pulse was now very frequent. On the 11th, while sitting in a chair, he suddenly threw his head backwards with great violence, and immediately fell into a state of coma, in which he remained for two hours, when he died. During the whole disease there had been no paralysis, except the slight affection of his speech; no difficulty of breathing; no vomiting; and no convulsion except the twitching of his face the day before his death. The pulse was small and irregular. The bowels were easily kept open, but the pain in his back was much increased by going to stool. Two days before his death he had several attacks of shivering; and much purulent matter was discharged from his left ear during his illness.

Upon a very careful examination of his body, every part of the brain was found to be in the most healthy state. Some gelatinous deposition was found under the *medulla oblongata;* and purulent matter flowed, in considerable quantity, out of the spinal canal. The spine being entirely laid open, the cord was found covered with a coating of purulent matter, which lay betwixt it and its membranes. The matter was most abundant at three places; at the upper part, near the foramen magnum—about the middle of the dorsal region—and at the top of the sacrum; but it was also distributed over the other parts with much uniformity. The substance of the cord was soft, and separated into filaments in some places. All the other viscera were healthy.

You may find several interesting examples of this form of disease in *Ollivier's Treatise on the Spinal Marrow.* The prominent symptom was generally *pain*, referred to some part of the spine, and *increased by motion;* and what is curious, sometimes little complained of except upon motion. In general, also, it extended along some of the limbs, and was accompanied by muscular rigidity, or tetanic spasms. Palsy occurred in one case; but this seemed to have been owing to *softening* of the cord itself. Constantly there was increased sensibility; a circumstance which Ollivier thinks calculated to distinguish inflammation of the membranes from inflammation of the substance of the cord, which is usually attended with *diminished* sensibility. In the case that I have quoted from Dr. Abercrombie, the intense pain underwent no remission or abatement. In one of Ollivier's examples, there was, at the commencement of the disease, a striking intermittence of the pain; it came on with intense severity at ten at night, and lasted till three in the morning.

The causes of spinal meningitis are not always to be discovered. It sometimes extends from within the cranium. It may be excited by external violence to the spine, of which a good speciman has been recorded by Sir Charles Bell:—A wagoner sitting on the shafts of his cart, was thrown off by a sudden jerk, and pitched upon the back of his neck and shoulders. He was taken to the Middlesex Hospital, where he lay for a week, without complaining of any thing except stiffness of the back part of the neck. He could move all his limbs with freedom. On the eighth day after his admission he was seized with general convulsions and locked jaw. After a few hours he was affected with a singular convulsive motion of the jaw, which continued in violent and incessant movement for about five minutes. This was followed by maniacal delirium. He then sunk into a state resembling typhus fever; and after four days was found to be palsied and insensible in his lower extremities. The day before his death he recovered sensation in his legs.

On dissection, a great quantity of purulent matter was found within the spinal canal. It appeared to have formed about the last cervical and the first dorsal vertebræ, and to have dropped down, by its own gravity, to the lower part of the canal; where it produced palsy and anæsthesia of the inferior limbs by the pressure it occasioned.

Inflammation of the *substance* of the spinal cord leads to the same changes in its texture which have been already spoken of as being often results, in the brain, of inflammation of the *cerebral* matter. Softening—induration—suppuration. I need not, therefore, again describe the physical characters of these alterations.

The symptoms which flow from inflammation of the nervous pulp of which the spinal marrow is composed, are by no means uniform; nor can we expect that they should be so, when we recollect what has been already stated of the different effects that must ensue according as different parts of the cord happen to be implicated. The phenomena will vary likewise, according as the inflammation is acute or chronic. If we bear in mind how many parts of the body depend for their power of motion, and for their sensibility, upon the integrity of the spinal cord, we shall not be surprised at the diversity and multiplicity of the symptoms that flow from disease of the cord. We shall expect to find, and we actually do find, some such an arrangement of those symptoms as the following. When the upper portion of the spinal marrow is inflamed, or partly disorganized, we look for convulsive affections of the head and face; inarticulate speech, loss of voice, trismus, difficult deglutition: as we go somewhat lower we should anticipate difficulty of breathing, irregular action of the heart, constriction of the chest: and proceeding lower still, vomiting, pain of the belly, sensation of a cord tied round the abdomen, dysuria, retention of urine, incontinence of urine, constipation, tenesmus, involuntary stools. And with respect to the voluntary muscles corresponding to these parts of the spinal marrow, convulsions, or palsy; or palsy succeeding to convulsions.

I must again have recourse to examples, to put you more fully than any attempted abstract picture could put you in possession of such forms of inflammation of the cord as you may expect to meet with in practice.

A man, 56 years old, was exposed to severe cold, while travelling on the outside of a coach. After this he was attacked with pain in the right arm and leg, most severe about the shoulder, but affecting the whole side, and he had also considerable headache. He soon perceived some loss of power in the affected limbs; and the progress of this was very curious. It began at the upper part of the arm, and extended downwards so gradually, that he was able to *write* distinctly, after he had lost the power of raising the arm, or bending the elbow. Then the leg became affected in the same gradual manner, and after ten or twelve days from the commencement of the disease, the whole leg and arm had become completely paralytic. Some pain continued in the parts, and it was occasionally severe, especially in the leg. Repeated blood-letting, and purgatives, and blistering, were employed. His mind remained quite entire. His pulse was 84, and rather weak. After some time the *left* arm became paralytic, rather suddenly; but it was not so completely motionless as the limb on the right side: the left leg was not at all affected. Slight delirium occurred, but passed off again. At the end of two months, after the exposure to cold, he again became delirious, and his pulse got feebler and rapid: he then fell into a state of stupor, muttering incoherently, but answering questions distinctly when he was roused. He lost his speech a few hours before death. For the last eight or ten days there had been considerable sloughing of the sacrum.

The brain was found to be healthy throughout. Much bloody fluid was discharged from the spinal canal into the cavity of the cranium before the spine was laid open. On displaying the spinal cavity itself, the cord was found in a state of complete softening, from the second to the last cervical vertebra. The portions above and below that part were quite healthy.—(*Abercrombie.*)

Comparing this case with the one I detailed of meningitis, we find that pain was present in both, but more severely so in the case of inflammation of the membranes: we find, also, that *stiffness and spasm of the muscles* marked the *meningitis: palsy* the *inflammation of the substance of the spinal cord.* In neither of them were the intel-

lectual functions disturbed till towards the last. I believe that the characters now pointed out belong to these forms of disease respectively.

I shall take, from the same store-house, one more case, in which both the membranes and the cord were simultaneously inflamed; and which, therefore, was analogous to encephalitis. And I quote it the rather because it possesses one or two points of peculiar interest. A young man, of unhealthy constitution, 18 years old, had suffered for some time from ulcers in various parts of the body, accompanied by exfoliations of bone from the leg, thigh, and sacrum. For several months before his fatal attack he had a sore on his head, as big as a shilling, with caries of the bone beneath it. At length he began to complain of pain in the loins, without fever. On the 2d of October this pain had increased; it was chiefly seated among the lower dorsal vertebræ, and extended downwards in the course of the ureter, with a frequent desire to pass urine. Then the pain descended lower, into the sacrum, and the symptoms referable to the bladder ceased. But soon afterwards pain in the belly came on, and numbness of the inner side of the thighs, and retention of urine; and in two days after this there was *perfect palsy* of both thighs and legs, *without loss of feeling;* retention of urine, and involuntary stools. He had still some pain in the lower part of the dorsal region. He died at length, on the 14th of October, having continued quite sensible till about six hours before. There had not been the smallest approach to a renewal of power of the lower extremities, but their sensibility remained. There was *palsy*, but no *anæsthesia*.

All was quite sound in the brain, except some old thickening of the dura mater in the neighbourhood of the diseased bone. In opening the spinal canal, some purulent matter flowed out, during the sawing, from about the middle of the dorsal region; and one of the vertebræ at that place was found carious. There was an extensive deposit of flocculent matter, having a purulent appearance, upon the outside of the membranes of the cord. Bloody sanious fluid was discharged from beneath its dura mater, and its pia mater was highly vascular. The substance of the cord was found most extensively disorganized along nearly the whole extent of the dorsal portion. The *anterior columns* of this part were completely broken down into a soft diffluent pulp; on the *posterior part* the cord was *more entire*. When the whole cord was taken out, and suspended, it hung together by the posterior columns of the dorsal portion, while the anterior part of it fell off entirely, in a soft half fluid state. The parts above and below the diseased portion were quite firm and healthy.

The complete palsy in this instance going along with the destruction of the *anterior* columns; and the persistence of the sensibility, the *posterior* columns being comparatively entire; invest the case with a remarkable degree of interest. We might infer from it that the anterior half of the cord is the channel through which the power to move the limbs is transmitted, while their sensations are carried along its posterior half; but we are checked from so concluding, by such cases as the one recorded by Mr. Stanley. These apparent inconsistencies may puzzle, but they ought not to discourage you. That time, and our advancing science, will at length explain and reconcile them, I cannot doubt. Meanwhile they teach a lesson which many practitioners much need—viz. that it is unphilosophical and unsafe to draw general conclusions from single cases of disease.

We are much instructed in regard to the effects of inflammation, or any other cause of disorganization, *confined to a limited portion of the cord*, by observing what takes place in those injuries in which the bones of the vertebral column are broken, or displaced. Of course I do not dwell upon these accidents, for they belong to surgery; but I have seen a good many of them, and watched them with much interest. The symptoms are much more uniform than when inflammation occurs within the vertebral canal, independently of external injury; simply because the injury to the cord is more definite and local. But such cases are very valuable objects of study to the physician. I remember several that occurred when I was a dresser at St. Bartholomew's hospital; and I will state very briefly the particulars of one, as an exemplar. In the year 1820, a man was brought there who had been thrown out of a tilt cart, in consequence of a dray running foul of it. He had pitched upon his head, which showed however no trace of injury. He became powerless, both in the upper and lower extremities, immediately on the receipt of the injury. His stools passed from him without his being aware of it, and it was

necessary to use the catheter to empty his bladder. He breathed entirely by the diaphragm—that is, his thorax was motionless, and his abdomen rose and fell with every alternate act of inspiration and expiration. These symptoms are perfectly distinctive of injury to the cord between the origins of the phrenic and intercostal nerves. He suffered pain about the middle part of the neck behind. He went on exceedingly well for four or five days, and then the nurse very foolishly acceded to his request to be turned on his side, which caused his death in a very few minutes. This is not the only instance, by the way, in which life has been suddenly extinguished by similar imprudence. The lesson may be useful. There was another patient in the same hospital, who had fractured the spinal column about the cervical vertebræ. Among other remedial measures, the surgeon had directed that his head should be shaved. The barber had performed half his task, and was turning, with his hands, the unfortunate man's head into a more convenient position for completing it, when he suddenly expired. The twist was fatal to him.

On the examination of the body of the patient whose case I was mentioning, a very remarkable state of the spinal column was found. The fifth and sixth cervical vertebræ were dislocated from each other *without any fracture:* a thing which has sometimes been pronounced impossible. The articular processes were fairly separated; and the vertebræ were also forced asunder, by the detachment of one of them from the intervertebral substance. The nervous matter of the cord opposite the point of dislocation was quite soft.

There is one very common and distressing consequence of such disease of the spinal marrow as produces paraplegia, not particularly noted in any of the cases which I have related, but always to be looked for. The muscles, by means of which the bladder empties itself, are apt to participate in the palsy; and then the bladder empties itself no longer. The urine accumulates in it, and distends it, and the ureter even becomes distended; and in this way not only the present but the prospective danger is increased. For the foundation of future disease in the kidneys is often thus laid, even when such distension of the bladder by its retained contents occurs independently of any disease of the spine; as it may do from stricture; from enlargement of the prostate; or even from the voluntary retention of the urine beyond a certain period, through feelings of delicacy. You are to look out, I say, for this distension of the bladder, and relieve or prevent it by the introduction of a catheter through the urethra. You must not be deceived by being told that the patient passes plenty of water; that it even runs from him. Incontinence of urine is, in fact, in these cases, though it may sound paradoxical, a sign of retention of urine. The urine dribbles away because the bladder admits of no further distension; it overflows, and runs out at the natural orifice, but the bladder remains constantly full and stretched. You must make an examination, therefore, of the hypogastric region with your hand. If you find that part of the belly hard and resisting, and giving out a dull sound on percussion, you may be sure, in these cases (where there is paralysis of the lower extremities, and the water dribbles away), that the bladder is full, and has lost the power of expelling its contents. Sometimes you may recognize the fluctuation of the urine in the distended bladder, and ascertain the globular shape of that organ. It will rise even beyond the umbilicus. But what I chiefly wished to point out to you is, the circumstance that the bladder becomes diseased, and the urine altered in quality, under this state of palsy. The urine becomes thick, ropy, and alkaline, and exhales a very offensive ammoniacal smell; and the inner surface of the bladder is found, after death, to be thickened, red, and covered with adhesive mucus—in a state of chronic inflammation, in short.

LECTURE XXVIII.

INFLAMMATORY AND STRUCTURAL DISEASES OF THE SPINAL CORD, CONTINUED. TREATMENT. APOPLEXY. ITS GENERAL SYMPTOMS AND DIAGNOSIS. DIFFERENT FORMS OF THE ATTACK. PREDISPOSITION TO APOPLEXY — NATURAL, AND ACCIDENTAL. PRECURSORY SYMPTOMS.

ALLOW me to repeat that the structural diseases of the spinal cord will most clearly reveal themselves, by their symptoms, to him who most distinctly perceives, and most accurately bears in mind, the *physiology* of that part of the nervous system. But to the best informed, and the most sagacious, they are too frequently obscure and perplexing.

Disease occupying a portion only of the cord, but affecting the *whole thickness* of that portion, from centre to circumference, will be likely to disturb, or suspend, the functions of sensation and voluntary motion in all the parts supplied with motor or sentient nerves by that portion of the cord, and by the portion beyond it. So that a great variety of symptoms depend, when the *amount* of disease is the same, upon the *place* of the disease. A total interruption of the conducting function of the cord, in the neck, above the origin of the phrenic nerves, extinguishes life by stopping the actions of respiration. A similar interruption in the cervical part of the cord, above the origin of the intercostals, but below the origin of the phrenic nerves, destroys life as certainly, but not so rapidly, nor in exactly the same manner. We find the lungs loaded with frothy serous fluid in such cases; we find the bladder inflamed; and, often, sloughing of the integuments and muscles of the nates and hips. A similar interruption below the dorsal vertebræ is not *necessarily* fatal, even when it is attended with permanent paralysis: but it *usually* is so, sooner or later.

It is commonly believed that disease affecting the *anterior* columns only of the cord, will be likely to disturb, or to suspend, the *power of voluntary motion* in the corresponding parts; to produce *spasm* or *palsy:* and that disease affecting the *posterior* columns alone will be likely to alter or abolish the *faculty of sensation* in the corresponding parts; to cause *pain*, *tingling*, *numbness*, or *complete anæsthesia*. But I have mentioned certain facts which contravene this opinion. Suspend your judgment respecting it. Neither the minute anatomy nor the physiology belonging to the question are yet conclusively settled.

There seems no reason to doubt that disease affecting the *lateral* half only of the cord will be likely to derange *both* the sensibility and the power of movement, in the corresponding parts on the *same side of the body alone*.

If you impress upon your recollection the facts thus summarily stated, you will find in them, I think, a key to many of the phenomena which accompany, and denote, more or less plainly, disease of the spinal marrow.

Inflammation of the *membranes* of the spine is most apt to declare itself by *pain*, increased on motion, of the spine and of the limbs; and by *rigidity and spasm* of the muscles of the neck and back. Inflammation of the *cord* itself, which readily passes into, or rather produces, softening of its substance, is most commonly marked, first, with convulsive movements of some parts of the body; secondly, by palsy of those parts, with or without anæsthesia. The same may be said of *suppuration* when it occurs as an event of inflammation; and the pus may be collected into an abscess in the nervous matter of the cord, or it may be diffused and mixed with softening.

Now I need not dwell upon the *treatment* proper to be adopted in inflammation of the

spinal cord and its membranes. *Mutatis mutandis*, it is the treatment already recommended in inflammation of the *brain* and *its* membranes. When the inflammation is acute, we must take blood freely; from the arm, or by cupping glasses along the sides of the spine. Blood enough may be taken by cupping along this tract to produce the effect of general bleeding as well as of local. Perfect rest in the horizontal posture must be strictly enjoined. Mercury will generally be proper.

In more chronic forms of inflammation within the spinal canal, we still have a capital remedy in cupping: and counter-irritation in various ways, but more especially by means of issues made on one or both sides of the spinous ridge, is also, in many cases, of most essential and unquestionable service.

Great care must be taken, when there is palsy of the bladder, not only (as I admonished you in the last lecture) that the urine be regularly drawn off, but also that the patient be kept *dry* and *clean:* for if great attention be not paid to this point, sores will form where the urine remains in contact with the skin, to the great increase of his suffering and of his danger. Indeed, take what pains we may, there is always a strong disposition to the formation of sloughs upon the sacrum and hips in cases of paraplegia. They result from the perpetual pressure made upon those projecting points; from the feeble state of the circulation in the palsied parts; and (often) from the irritation of the urine and fæces, which are passed without the patient's consciousness.

When the patient is kept clean and dry, and the surfaces on which the weight of his body has been supported begin to be red or angry, you may protect them by a plaster: or by rubbing them with brandy you may sometimes prevent the skin from breaking: or, what is best of all, you may put your patient upon one of Dr. Arnott's hydrostatic beds; and then the pressure will be equally distributed over all that portion of the body which comes in contact with the waterproof material of the bed.

To bring this outline of the diseased states of the spinal cord up to that point in which we left those of the encephalon, I may state that, like the brain, the spinal marrow may become hardened by chronic inflammation; and, like the brain, it may be encroached upon by tumours; fibrous, scrofulous, or malignant. With respect to these, all that I can now say likely to be of any practical benefit to you, is that the symptoms they occasion are those of slowly increasing paralysis, without fever or what is called reaction; and that the locality, and extent, and effects of the paralysis, will vary according to the part of the cord in which these changes occur, and the depth to which they affect it.

Apoplexy.—I proceed, in the next place, to a perfectly distinct class of diseases of the brain and spinal cord; to the apoplectic affections: and especially to cerebral hæmorrhage, and spinal hæmorrhage.

When a person falls down suddenly, and lies without sense or motion, except that his pulse goes on beating, and his breathing continues, he is said to have been attacked with *apoplexy*. He appears to be in a deep sleep; but this is not all, for you cannot awaken him by the same measures which would rouse a healthy man. He is not in a state of syncope, for his pulse beats, perhaps with unnatural force; and often his face, instead of being pale, is flushed and turgid; and his respiration goes on, though it may be laboured and stertorous. What I now denominate apoplexy, is the very same state which has so frequently been mentioned already in these lectures: it is *coma* occurring suddenly, or coming on (at least) with rapidity. What is coma? it is that condition in which the functions of animal life are suspended, with the exception of the mixed function of respiration; while the functions of organic life, and especially of the circulation, continue in action. There is neither thought, nor the power of voluntary motion, nor sensation. But the pulmonary branches of the par vagum continue to excite, through the medulla oblongata, the involuntary movements of the thorax. When this upper part of the cranio-spinal axis becomes involved in the disease, and its reflex power ceases, the breathing ceases also, and the patient is presently dead.

It is a common question—how would you distinguish apoplexy from the effects of a narcotic poison? If you were summoned to a person in the state I have been describing, how could you tell whether he was affected with apoplexy, or labouring under the influence of a large dose of opium, or merely dead-drunk? Why, so far as the condition of

the cerebral functions is concerned, you cannot discriminate the one from the other. In each case there is profound coma; but the cause of the coma is different in each, and you must seek to ascertain that cause in the history and other circumstances of the patient: you inquire whether he is known to have been drinking, you try whether you can perceive the odour of spirits, or of wine, in his breath; or you endeavour to make out whether he has been low-spirited, or in known difficulties; in short, whether it is likely that he may have swallowed poison. But from the actual condition of his sensorial functions, you cannot solve the question.

Yet let me say, thus in the outset of our remarks upon apoplexy, that it is often of great importance that the diagnosis should be determined. A man was found lying in Smithfield in a state of total insensibility, except that he still breathed. He was carried into St. Bartholomew's Hospital. The house-surgeon thought he smelt the smell of gin in his mouth; and thereupon very properly made use of the stomach-pump: by means of it he discharged a large quantity of ardent spirit; and in the course of a few minutes the man revived, shook his ears, and walked off. If the gin had been suffered to remain in his stomach, and if the remedies of apoplexy had been vigorously put in force, the absorption of the poison would have been thereby accelerated; and the debauch would probably have had a fatal termination. The same remarks apply still more urgently to the case in which opium, or any other strong narcotic poison, is lying in the stomach. Even when there is no great danger, either in the person's state, or in the remedies used for it, it is not a very pleasant or creditable thing to make a false point of this kind. If we do err, however, we had better err on the safe side. The father of the late Professor James Gregory, of Edinburgh (who used to relate the case in his lectures) was once called out very late in the evening to visit an old gentleman of that place. He found him in a complete comatose condition; his wife crying, and his household all plunged in grief and distress. They told him that the patient, whom he now saw in a fit, had come home, and upon the servant's opening the door to him, had fallen into the passage, on his back, in a state of insensibility. Dr. Gregory learned, however, that he had been at the "Club," and he knew well enough that this Club was composed of jovial spirits, fond of their cups; although the gentleman's wife did not know as much. Therefore he ventured to express his "hopes" to the wife that her husband was drunk: a charitable view of the case, at which she was extremely affronted and indignant. He persisted, however, in his opinion, and not long afterwards the patient began to recover his senses. It turned out that he had partaken more liberally than the rest of the Club, and was the *first* to be drunk. Two of his companions carried him home quite incapable of motion: but not liking to introduce him themselves to his wife in that predicament, they placed him with his back against the door, rang the bell, and walked away. Of course when the servant came to open the door, his master tumbled senseless on the floor. I need not point out to you the ridicule which the physician would have brought upon himself, and the damage he might have inflicted upon his patient, had he busily applied, in this case, the ordinary remedies of apoplexy.

The state of coma, such as I have described as being characteristic of apoplexy, may terminate in one of three ways. It may cease, more or less rapidly, and leave the patient in perfect health. What is the exact condition of the encephalon during the continuance of the coma, in such cases, no one can positively tell. But the occurrence of temporary coma, under the influence of a narcotic poison, and the perfect disappearance of the coma as the effects of the drug pass off, teach us that the functions of the brain may be almost totally suspended for a time by causes which do not injure its texture. It is possible that the coma may depend upon that presumed disturbance of the balance of the arterial and venous circulation within the cranium, which I spoke of in a former lecture. It may be that the force and rapidity of the circulation in the cerebral vessels undergo some great alteration. It is still more probable (to my mind) that a temporary stress upon the cerebral blood-vessels (produced by a determination of blood towards the head, through the arteries, or by a detention of blood in the obstructed veins) may really exercise pressure enough to cause transient coma. But these are mere conjectures.

In the second place, the apoplectic coma may terminate, more or less quickly, in *death*. And on examining the brain we may find a large quantity of extravasated *blood;* or a

considerable effusion of *serous fluid* in its ventricles, or beneath the arachnoid; or we may detect *no* deviation whatever from the healthy structure and natural appearance of the organ. The congestive pressure (if it indeed existed) has left no prints of its action.

The last is comparatively rare. Dr. Abercrombie has given to this form of apoplexy, which destroys life, but leaves no traces behind it, the name of *simple* apoplexy. And this name, for its convenience, I shall retain. Of the other two kinds of quickly fatal apoplexy, that in which *blood* is found extravasated is more common than that in which there is effusion of *serum* only. The one has been called *sanguineous apoplexy;* a better term is *cerebral hæmorrhage:* the other has been called *serous apoplexy.*

Thirdly, the apoplectic coma may terminate in *partial* or *imperfect* recovery. One, or all, of the cerebral functions may be left impaired; the mind enfeebled; the power of motion limited, or lost, in some parts of the frame; the faculty of sensation benumbed or extinguished: the unhappy subject of the attack remaining more or less crippled in body, and more or less maimed in intellect. In these cases, when at length we have an opportunity of examining the brain, we almost always find that there has been extravasation of blood, to a small or moderate extent. I say *almost* always, because I have myself, in more than one instance, carefully looked for such appearances, after such a series of symptoms, without finding them. Occasionally, instead of a clot of blood, we meet with circumscribed softening of the brain.

Modes of attack.—The *attack* of apoplexy does not always occur in the same manner: and Dr. Abercrombie has pointed out three several ways in which it is apt to come on. I am confident, from the result of my own observation, that the distinctions laid down by Dr. Abercrombie are just and true; and it is of importance that you should be aware of them. "In the *first* form of the attack, the patient falls down suddenly, deprived of sense and motion, and lies like a person in a deep sleep; his face generally flushed, his breathing stertorous, his pulse full and not frequent, sometimes below the natural standard. In some of these cases convulsions occur; in others rigidity and contraction of the muscles of the limbs, sometimes on one side only."

Now of persons seen in this condition, the immediate prognosis is *uncertain.* Some die in a short time, and much blood is found extravasated within the cranium. Some die after a rather longer interval, and then we often find serous effusion only, and that of no great amount. And in some that die early, no effusion either of blood or of serum can be detected. Some recover altogether, without any ill effect of the attack remaining. Others recover from the coma, but are left paralytic of one side, and with some imperfection of speech, or of one or more of the senses. And this paralysis and imperfection may disappear in a few days, or gradually subside, or remain for life.

In the second form of attack, the coma is not the earliest symptom. The disease generally begins with a sudden attack of pain in the head. The patient becomes pale, faint, and sick, and usually vomits; and sometimes, but not always, falls down in a state of syncope, or resembling syncope, with a bloodless and cold skin, and a feeble pulse. This also is occasionally accompanied by some degree of convulsion. Sometimes he does not fall down, the sudden attack of pain being accompanied only by slight and transient confusion. In either case he commonly recovers in a short time from these symptoms, and is quite sensible, and able to walk; but the headache does not leave him: after a certain interval, which may vary from a few minutes to several hours—and Dr. Abercrombie records cases in which it was even much longer—the patient becomes heavy, forgetful, incoherent, and sinks into coma, from which he never rises again. In some instances paralysis of one side occurs; but perhaps more often, there is no palsy observed.

The disease, when it comes on in this way, is much more uniform, and of much worse omen, than when it commences after the former fashion. It is of great use to know this; for to an unexperienced eye the cases do not *seem* so terrible as those in which the patient becomes profoundly comatose from the very first. The apparent amendment is fallacious, and apt to lead one into giving a false prognosis. Very few persons come out of the coma, and a large quantity of *blood* is usually found extravasated in the brain. These cases are not, as Dr. Abercrombie well observes, apoplectic in the outset. They differ remarkably from the first set of cases. If there be at the very beginning some loss of sense or motion, it goes off again in a very few minutes, or perhaps in a few seconds: the pro-

minent symptom, at the commencement, is sudden and violent pain of the head, with faintness, sickness, and often with vomiting. The pain continues, and is sometimes confined to one side of the head; the face is pale and ghastly, the pulse weak and often frequent or irregular; but the patient is quite conscious, and in full possession of his intellect. At length he recovers his natural temperature, his countenance improves, and the pulse becomes stronger and steadier: then his face gets flushed, he feels oppressed, answers questions slowly, and at last sinks into stupor and fatal coma. The period between the first attack and the commencement of the coma is variable. Sometimes the stupor succeeds the pain and faintness so rapidly, that the case comes greatly to resemble those in which coma is the first symptom, and takes place suddenly; but still a short period of sense, commonly with complaint of great pain, may be observed. But the interval may be a quarter of an hour, or many hours, or even two or three days. "Upon inspection," says Dr. Abercrombie, "we find none of those varieties and ambiguities, which occur in the apoplectic cases, but uniform and extensive extravasation of blood. [I should state that he calls the first class of cases *apoplectic* cases, the coma being present from the first: and the second class, which we are now considering, he calls cases *not primarily apoplectic*.] The symptoms in this form of attack depend, no doubt, upon the giving way of some one of the cerebral vessels. At the moment when the vessel is ruptured, a shock is given to the brain; a temporary derangement of its functions occurs; but this passes off. The circulation then goes on as before, until such a quantity of blood has escaped from the ruptured vessel as is sufficient to produce coma. There is no part of Dr. Abercrombie's book more admirable and clearly put than that which is occupied with these important distinctions, which I give you very much in his own words. He points out the close analogy which exists between this variety of apoplexy, and the result of external injuries, when they occasion extravasation of blood on the surface of the brain. The hurt person recovers from the immediate effects of the accident, walks home perhaps, and after some time becomes stupid, and at last comatose. The surgeon trephines the skull, and discovers blood upon the dura mater; and the blood being removed, the coma goes off. We cannot help *our* patients by a similar expedient; though the opinion has been broached that trepanning the skull will, at some future period, be a common practice in apoplexy. Dr. Abercrombie conjectures that after the rupture has taken place, the hæmorrhage is sometimes stopped by the formation of a clot at the orifice in the vessel, but at length the blood bursts out again, and proves fatal. He relates two cases in which this probably happened; in one of them an interval of three days, and in the other an interval of a fortnight, elasped between the first attack, and the supervention of coma. The portions of blood extravasated at the distinct periods may sometimes be distinguished by their appearance—their colour and consistence.

The *third* form of attack is characterized by sudden loss of power on one side of the body, and frequently by loss of speech, without loss of consciousness; or at most with a very temporary suspension of consciousness. The patient is sensible, listens to and comprehends your questions, and answers them as well as he is able, either by words, which in most cases he articulates imperfectly, or by gestures. The farther progress of the cases that commence in this way is marked by considerable variety. Sometimes the hemiplegia passes gradually in a short time into apoplexy. Sometimes the patient soon gets well, the palsy leaving him entirely. Or a gradual recovery takes place, which is not complete for some weeks or months. Or the patient recovers up to a certain point, and there the improvement stops; he regains the power of moving his leg, but it drags somewhat after him; or the leg recovers, but the arm remains feeble, or his speech continues to be inarticulate. And in another variety of this form the patient neither recovers on the one hand, nor becomes apoplectic on the other, but is confined to his bed, paralytic, and perhaps speechless, but in possession of his faculties in other respects, and dies at last worn out and exhausted, some weeks, or months it may be, after the attack. In the outset of these cases there is not always complete hemiplegia, sometimes the arm only is affected, sometimes (but much more rarely) the leg only. Or some other voluntary muscles are the first to lose their power.

Now the appearances on dissection after death, in cases that have thus commenced (Dr. Abercrombie calls them the class of *paralytic* cases) are, as in the *apoplectic* cases, in-

constant. Much the most common of all—according to my own experience—is the extravasation of blood, to a moderate or small amount, and definite extent, in the substance of the brain. But sometimes nothing is found upon dissection to account for the symptoms, or slight serous effusion only. The same symptoms attend some cases of softening of the brain also; or inflammation and its consequences. In a vast majority of cases, I repeat, this sudden hemiplegia marks an attack of cerebral hæmorrhage.

You will not find that all cases of apoplexy commence exactly in the one or the other of the three ways which I have been describing. But most attacks range themselves in one of these classes, and by attending to the points of distinction, I make no doubt that you will often derive much assistance from them in regard to diagnosis and prognosis; and that the distinctions themselves will give a higher interest to your study of this complaint, than it would possess if all the forms of attack were lumped together in one common description.

Predisposition.—In treating of this large subject, this multiform disease, the main points will best be made intelligible by my breaking what I have to say into separate heads. I have told you the different ways in which the disease may make its assault. I will next say something of the persons who are most liable, *cæteris paribus*, to attacks of apoplexy: and afterwards of the symptoms which in many cases, though not in all, precede the seizure; and lead us to fear that it may be impending. It is of great importance to attend to these threatenings; for, as you will readily conceive, the chief good that medicine can do in such cases, is in the way of prevention. After the attack has taken place, the effect of our treatment must be very uncertain. A large effusion of blood upon the brain will be fatal in spite of us: and a smaller amount of extravasation we cannot remove; and the best that the patient can expect in too many cases, is long continued or permanent palsy, a weakening of the mental powers, and sometimes a state nearly approaching to idiocy. Also, when once an apoplectic fit has happened, it is the more likely, on that very account, to happen again. These are quite sufficient reasons why we should not neglect the *warnings;* the symptoms which are apt to precede and herald the attack of apoplexy.

The classes of persons in whom, *cæteris paribus*, attacks of apoplexy are especially to be apprehended, are those whose *ancestors* have suffered the same disease; those who possess a *particular conformation of body;* and, above all, those who have reached a *certain period of life*. No doubt apoplexy may and does occur in persons whose progenitors have escaped it; in persons of every conceivable conformation; and in persons of all ages. But it is *much more* frequent in the classes I have specified, than it is among persons not comprehended in those classes.

The first and second class sometimes concur, *i. e.* a particular conformation of the body is transmitted from parent to child, and with it is transmitted a proclivity to apoplectic disease. But even when there is nothing peculiar in their bodily form, or in their habits of life, practical men of large experience declare that they who come of an apoplectic stock are themselves more than ordinarily liable to apoplexy.

The pattern of body which is most prone to apoplexy is denoted by a large head and red face, shortness and thickness of the neck, and a short, stout, squat build. This remark is as old as the time of Hippocrates. However, apoplexy is common enough in men and women who are thin, and pale, and tall. *Cæteris paribus*, corpulent people are more in danger of apoplexy than spare people; but it attacks both the one and the other.

Advanced life is certainly a very strong predisposing cause. And the reason of this will be apparent when we come to inquire more particularly into the morbid appearances presented after death by apoplexy. The disease begins to be *common* after 50: but it does sometimes occur even in young children. I am speaking principally of that form of apoplexy which depends upon cerebral hæmorrhage, which is by far the most frequent of all its forms.

Of sixty-three examples of cerebral hæmorrhage collected, and carefully examined by Rochoux, (who has written a very good treatise on this affection), two only happened between the ages of 20 and 30; eight between 30 and 40; seven between 40 and 50; ten between 50 and 60; twenty-three (or more than one-third of the whole) between 60 and 70; twelve between 70 and 80; and one between 80 and 90. To analyze this table a

little farther: it appears that of the 63 cases seventeen only took place before 50; forty-six after that age. There are also twice as many victims to the disease between the ages of 60 and 70, as between 70 and 80. And from this fact Rochoux has drawn, I conceive, an erroneous conclusion; and I mention it that, in case his treatise falls in your way, you may not be led by it into what I imagine would be a mistake. There being 23 cases between the ages of 60 and 70, and only 12 between the ages of 70 and 80, Rochoux infers from this that the disposition to cerebral hæmorrhage *decreases* after the age of 70: which would be a most unaccountable thing, and quite inconsistent with what I believe to be the true pathology of the disease. But I make no doubt that the difference in the actual numbers observed in these two decennial periods depended upon the number of persons alive, at the same time, of the ages of 60 and 70 respectively. There are always more persons living whose age ranges from 60 to 70, than from 70 to 80; and therefore more persons die of apoplexy in the former period. In all probability, if the exact truth could be ascertained, of a *given number* of persons, there are *more* attacked with apoplexy between 70 and 80 than between 60 and 70.

All these three kinds of predisposition are beyond our power. We cannot eradicate the hereditary tendency; nor remodel the plan upon which the body is constructed; nor arrest, or put back, the clockwork of human life: but we may guard and caution persons, thus predisposed by nature towards apoplexy, against many of its exciting causes.

A strong predisposition to apoplexy is, moreover, engendered by certain other diseased conditions; and over some of these conditions our art enables us to exercise more or less control.

Disease of the cerebral blood-vessels is a very common and a very pregnant circumstance of predisposition. I shall revert to this when I describe more particularly the anatomical characters of cerebral hæmorrhage.

Diseases of the chest influence very materially and injuriously the circulation in the head. Without going into detail respecting complaints with which I am obliged to suppose that you are as yet unacquainted, I may state, by anticipation, that impediments to the free transmission of blood through the heart and lungs constitute the mode in which thoracic disorders predispose to apoplexy. The *plethora capitis* produced by such impediments is frequently visible in the turgid and livid features, and in the distended jugular veins.

A very remarkable disease of the kidneys, of which also I must postpone any particular account, renders the system unusually liable to apoplectic affections. I mentioned this renal disease when speaking of the general pathology of dropsy. It is connected, on the one hand, with a morbid state of the urine, which contains albumen, and is deficient in urea; and, on the other, with a morbid state of the blood, which circulates unpurified from the latter excrementitious substance.

The cessation of habitual discharges, of the catamenia, of bleeding piles; the drying up of old sores; the healing of long established issues and setons; all have an unquestionable tendency, by causing or augmenting plethora, to generate a predisposition to apoplexy.

And large observation of the habits of those who fall victims to this terrible malady, leaves no room for doubting that intemperance often paves the way for its invasion. The continued abuse of ardent spirits, in particular, lays the foundation of many of those morbid conditions of the sanguiferous system, and of the viscera, which constitute the predisposition we are now considering.

Precursory warnings.—Among the *premonitory* symptoms, *headache* is of frequent occurrence: but the same symptom is abundantly common in persons who are in no danger of apoplexy: it derives its minatory character from the concurrent circumstances. Headaches awaken our fears when they *begin* to be troublesome in advanced life. They are, then, still more formidable if they are accompanied by vertigo; or, without any other evidence of gastric derangement, by nausea and retching. Sometimes, as I just now told you, severe headache ushers in, and almost forms a part of, the apoplectic attack.

Vertigo itself, even without headache, is a very common precursor or warning of an approaching seizure. It is sometimes slight and transient; sometimes almost habitual. Although vertigo may depend upon other causes than mischief within the head, we cannot regard it without apprehension when it often occurs in old persons. It should teach

us to obviate as nearly as we can all the known exciting causes of apoplexy. The principal of these I shall by-and-by describe to you.

Transient deafness, or transient blindness, blindness or deafness for a few seconds or minutes, is another of these warning symptoms. The late Dr. Gregory, of Edinburgh, used always to mention in his lectures the case of Dr. Adam Ferguson, the celebrated historian, as affording one of the strongest illustrations he ever met with of the benefit that may be derived from timely attention to the avoidance of those circumstances which tend to produce plethora and apoplexy. It is, perhaps, the most striking case of the kind on record. Dr. Ferguson experienced several attacks of temporary blindness some time before he had a stroke of palsy; and he did not take these hints so readily as he should have done. He observed that while he was delivering a lecture, his class, and the papers before him, would disappear, vanish from his sight, and reappear again in a few seconds. He was a man of full habit; at one time corpulent and very ruddy, and, though by no means intemperate, he lived fully. I say he did not attend to these admonitions; and at length, in the sixtieth year of his life, he suffered a decided shock of paralysis. He recovered, however, and from that period, under the advice of his friend, Dr. Black, became a strict Pythagorean in his diet, eating nothing but vegetables, and drinking only water or milk. He got rid of every paralytic symptom, became even robust and muscular for a man of his time of life, and died in full possession of his mental faculties at the advanced age of ninety-three; upwards of thirty years after his first attack. Sir Walter Scott describes him as having been, "long after his eightieth year, one of the most striking old men it was possible to look at. His firm step and ruddy cheek contrasted agreeably and unexpectedly with his silver locks; and the dress which he usually wore, much resembling that of the Flemish peasant, gave an air of peculiarity to his whole figure. In his conversation, the mixture of original thinking with high moral feeling and extensive learning, his love of country, contempt of luxury, and especially the strong subjection of his passions and feelings to the dominion of his reason, made him, perhaps, the most striking example of the Stoic philosopher which could be seen in modern days."

This anecdote, which I have made use of as a wrapper for some medical instruction, will not be the less acceptable to you when I add that the remarkable man to whom it relates was the great uncle of my friend and present colleague in this school, Dr. Robert Ferguson.

Very frequently slight and partial paralysis is the forerunner of an attack of apoplexy. Double vision is one form in which such limited palsy is apt to show itself. It is evidently connected with some degree of squinting; *i. e.* some one or more of the muscles that move the eyeball are paralyzed; the person cannot direct each eye to the same object at the same time. This is a very suspicious symptom. Dr. Gregory was acquainted with a sportsman who one day, when out shooting, disputed with his game-keeper as to the number of dogs they had in the field. He asked how he came to bring so many as eight dogs with him. The servant assured him there were but four; and then the gentleman became at once aware of his situation, mounted his horse, and rode home. He had not been long in the house when he was attacked with apoplexy, and died.

Sometimes the slight and local paralysis shows itself in a faltering or inarticulate mode of speaking. The rapidity of the movements of the tongue requisite for distinct utterance is so great, that the slightest weakness of any one of its muscles is rendered obvious. We see this in one very common form of what may in truth be considered a kind of apoplexy; viz. in drunkenness. In many persons the very first symptom of their becoming intoxicated is their inability to speak plainly. "Clipping the King's English," is the slang expression for it; and the same thing often takes place in respect to the more proper forms of apoplexy.

It is a curious circumstance, by the way, and one which is analogous to what we meet with in disease, that different sets of muscles are chiefly affected by intoxication in different persons; the same sets being always the first affected in the same persons. Thus some persons who are drunk lose (as I have just stated) the proper command over the muscles of the tongue, and falter in speech, while they can walk very well: others reel and stagger, having lost, in a greater or less degree, the power of moving and governing their limbs, and of balancing themselves, who yet can speak quite fluently and distinctly;

and in a few cases, drunken persons become delirious, who still retain the power of distinct articulation, and of directing their steps aright. This being so, we need the less wonder at the variety in the nature of the warnings that precede the apoplectic attack.

In many cases there is numbness or debility, or total palsy of one limb, or of a single finger, or even of a solitary muscle, as of the levator palpebræ. The patient cannot grasp your hand with firmness, or sign his name in his usual way, or pick up a pin, or snuff a candle, or manage an obstinate button, or tie a knot in a thread cleverly: or, perhaps, one of his eyelids droops, and the eye is half closed. Sometimes, on the contrary, the patient stares at you, frightfully, with one eye, which he cannot shut.

The numbness also assumes various characters, according to its place and degree. One patient will tell you that he feels as if one limb was always muffled in flannel; another, that he is uncertain whether, in walking, his foot has reached the ground or not. One gentleman, since dead of apoplexy, assured me that, when seated, he did not know how far his breech extended beyond the edge of the chair.

All these symptoms are modifications of the function of voluntary motion; or of the function of sensation. Nor are manifestations wanting, among these precursory circumstances, of a derangement of the other and nobler function, of which the brain and nervous system form the material instrument. I mean the function of *thought*.

Thus one deplorable warning is the loss of *memory*. All persons, I believe, as they grow older, find that they do not retain so tenaciously in their recollection things which have recently occurred, as things which happened when they were young. This depends upon the degree of *attention* which we pay to different circumstances. Those events which strongly excite the curiosity, and rivet the attention of the boy, become familiar to the man, and he gives them but little notice, and is very apt to forget them. But the loss of memory that threatens apoplexy is sometimes more than this. It is sometimes partial, and extends to certain sets of things only. For example, some persons entirely forget certain words, while they recollect others perfectly. Common words are often thus forgotten, while unusual or remarkable words are remembered: or a wrong word is chosen. One word is used for another that sounds something like it. Thus one of my patients, meaning to accuse a certain individual of *perjury*, always called it *purging;* and many other words he changed after the same fashion. But in truth the modifications of a partial loss of memory that have been known to precede apoplexy are both odd and endless: some people forget their own names, or the names of their children. Dr. Gregory, who had paid particular attention to these precursory symptoms, and who had a large practice for a great number of years to furnish them, used to mention a case of this kind. After some efforts his patient could recall to recollection what his christian name was, but he could not think of his surname. About twelve months after his memory began to fail in this strange manner he was found dead in his bed. Another gentleman for some time before his death could never recollect the name of the street in which he lived. Upon one occasion of his visiting Edinburgh, he called on Dr. Gregory, and partook of a hearty breakfast, having forgotten that he had breakfasted before he came out. On the same day he attended, with Dr. Gregory, the funeral of a young lady who had been his ward; the funeral took place in the country; and when they returned together in the carriage, the doctor found that his friend had forgotten all that he had been doing. Next day he met him in the street, and saluted him with all the kindness of an old acquaintance at first meeting; saying he was happy to have fallen in with him now that he was in town, and totally forgetful of their former interviews.

Connected with this failure of memory, there is often an unnatural degree of drowsiness. Sometimes, without any other affection of the memory, there is a temporary confusion or suspension of thought: the patient suddenly loses the train of ideas in which his mind had been occupied; stops short in the middle of a sentence, and endeavours in vain to recover the broken thread of his discourse.

Among the mental conditions that bespeak a tendency to apoplectic disease, I have several times noticed a strange and vague *dread*, of which the person can give no reasonable explanation; a sense of apprehension and insecurity not accounted for by the apparent state of his general powers and functions; a painful degree of indecision and irritability; with a dislike and fear of being left alone. One patient of mine described his "nervousness"

of this kind, by telling me that in descending a staircase, especially a winding one, he was obliged to turn round, and come down backwards, as one descends a ladder; or even to sit down, and so slip, stair by stair, from the top to the bottom. Yet with the assurance given him by a friend's arm, or by a convenient bannister, he could walk down stairs without difficulty. He had no actual vertigo.

I say, all these, and many other signs that indicate a disposition to apoplexy, are well worth your study; because a knowledge of them may enable you to ward off the threatened attack, by medicine, by regimen, and by admonition to the patient on the subject of such exciting causes of the disease as are within his own control. They show that, even before the stroke descends, there is some morbid process going on within the head.

LECTURE XXIX.

APOPLEXY CONTINUED. SYMPTOMS CHARACTERIZING THE APOPLECTIC STATE. PRESSURE THE ORDINARY PHYSICAL CAUSE. HEMIPLEGIA. AFFECTION OF INVOLUNTARY MUSCLES. ANATOMICAL CHARACTERS. SITUATION OF THE CLOT OF BLOOD. DISEASE OF THE CEREBRAL BLOOD-VESSELS.

WE were engaged with the subject of apoplexy. I requested your particular attention to the threefold mode in which that fearful disorder has been observed to make its attack. In the first, the coma is sudden, and deep; the condition of the patient, thus struck in an instant senseless and motionless, warranting those epithets which the ancients applied to the victims of this disease, of *attoniti*, and *siderati*, as if they were thunder-smitten, or planet-stricken. In the second form of the attack, the earliest symptom is acute pain of the head, with sickness and faintness; the coma *supervening* usually in no long time. The third form is ushered in by sudden hemiplegia, which may or may not lead to loss of consciousness, or stupor. The cases which range themselves under the one or the other of these three forms of attack are called respectively by Dr. Abercrombie *apoplectic* cases; cases *not primarily apoplectic;* and *paralytic* cases: and so as you bear in mind what these terms really imply, they appear unobjectionable.

I next pointed out the classes of persons in whom an attack of apoplexy is chiefly to be apprehended: those, namely, in whose families that disease has been known to be common: those who have large heads, thick necks, red faces, square shoulders, and a short stature; although persons of quite the opposite conformation are by no means exempt from it: and lastly, and above all, those who have passed the middle period of life, and are advancing towards old age: and more particularly is apoplexy to be apprehended in people of this description when they have already suffered what are called head symptoms: which symptoms have reference to the three great functions of the brain and nervous system; voluntary motion, sensation, and thought. Such symptoms consist, therefore, in slight and often transient paralytic affections, double vision, a dropping of one eyelid, occasional inarticulate speech, weakness perhaps of a single finger; headache, giddiness, unnatural sounds in the ears, numbness or tingling of the extremities, which last are all modifications of sensibility; some impairment of the intellect, marked, most commonly, by partial and strange defects of memory, and temporary confusion of thought. When several or any of these symptoms occur, and especially when they become habitual in persons in the decline of life, we have reason to dread the supervention of apoplexy; and to exhort and protect our patients against its ascertained exciting causes.

Symptoms characterizing the apoplectic condition.—When the apoplectic state is *fully* formed, in what manner soever the attack may have commenced, it is marked by most or all of the following circumstances. The patient lies totally unconscious as to all

that is going on about him: he replies to no questions, he is unmoved by the cries and lamentations of his family; in fact, he does not hear them: his pulse is infrequent, often full, perhaps intermitting: his breathing is peculiar, being slow, sometimes interrupted or irregular, attended with snoring or stertor during *in*spiration, and a puffing out of the cheeks, like the action of one who smokes a pipe, during *ex*piration. Both these peculiarities are referable to the same principle, and both denote a profound insensibility to all external impressions. There is no longer any voluntary attempt to breathe, yet the involuntary movements of respiration subsist: the medulla oblongata still responds to the impressions which reach it from the lungs, still prompts contraction of the muscles that enlarge the capacity of the thorax; but the loose curtain of the palate, and the lips and cheeks, are passive: by the vibrations of the one the stertor is occasioned; the mouth is closed by the mere elasticity of the others, and the flaccid cheeks flap outwards with the explosion of the air, as it escapes when the chest again collapses. The countenance is frequently turgid, and livid; the blood which tinges it is already but half arterialized; the pupils are commonly contracted. The limbs lie motionless: either they are all absolutely palsied; or (what probably is often the case) the capacity of motion remaining, the *will* to move them is wanting. If you raise one of them it falls passively down again, when you leave hold of it, like a *dead* limb. Sometimes, however, they are rigid and stiff. Sometimes one is stiff, and the others limber. And sometimes one or more of them, or those of one side, tremble, or are distinctly convulsed. You find the patient is unable to swallow; if you put fluids into his mouth, they appear to choke him, or they run out again at the corners of his lips. His bowels are usually torpid; but if they act, the evacuations are passed in the bed without his knowledge or concern; his urine also flows involuntarily; or is retained in the distended bladder until it fairly *over*flows, and dribbles away perpetually.

When the attack terminates in death, that event is preceded, I believe in almost every case, by profuse perspiration, which bursts forth from every part of the surface, and is often cold and clammy. The pupils are sometimes at this period dilated: and I have more than once seen them of unequal size. The pulse becomes more frequent, the breathing more rare, and at last it ceases altogether.

Pressure the ordinary physical cause.—This state, so appalling and painful to look upon, but fortunately so devoid of suffering for the patient—this suspension of the functions of animal life—depends, we have reason to believe, upon *pressure* applied to the brain, the organ subservient to those functions.

That excess of pressure is a *vera causa* is obvious, and that it is adequate to the production of coma is capable of demonstrative proof. It is not enough to show that they often exist together, for the coincidence might be casual. Neither does their occasional disjunction, real or apparent, furnish any conclusive argument against the general proposition, that coma, in many and in most cases, is the result of pressure upon the encephalon. Coma may exist without pressure. In other words, coma acknowledges *other* causes also, *besides* pressure. It is produced by many narcotic poisons; by the circulation of venous blood through the arteries. In these cases we have no proof of any compression of the cerebral substance.

The other disjunctive condition is much more puzzling, and has led some persons to question or deny the general proposition. Can there be unnatural pressure, yet no coma? It would seem so. Serum, pus, blood, have been met with in the brain, foreign matters have penetrated the cranium, and coma has not occurred.

The force of this difficulty is lessened by the consideration that foreign substances may be present within the skull, without occasioning any preternatural degree of pressure. We read of bullets being carried about for some time in the brain. In such instances it is probable that a portion of the contents of the skull was forced out at the time of the injury; or that coma has come on, and gone off again, in consequence of the gradual absorption of the cerebral matter to make room for the foreign body. The same explanation may be applied to the chronic accumulation of water within the cranium, and to the slow growth of tumours.

Farther, it is open to conjecture that it is not on every part of the brain that the same degree of pressure made, will produce the effect ascribed to it. It is stated in Mr. Mayo's

Physiology, as the result of actual experiments on animals, that *lateral* pressure against the hemispheres of the brain produces no observable ill consequence; but that vertical pressure, pressure downwards, occasions stupor, which is attributable to the compression of the medulla oblongata. Now it is obvious that some injuries of the brain may tend more than others to cause pressure in that direction.

I confess that the difficulty is not wholly relieved by these considerations. But it is a difficulty which cannot invalidate the evidence of numerous facts that attest the agency of pressure, as, at least, one cause of coma. The presumption of such agency arises whenever coma immediately succeeds to pressure; and it is converted into certainty if, upon the removal of the pressure, the coma immediately departs. Now the annals of physic are full of instances of that kind. In experiments upon animals, stupor has been brought on, and made to cease, at the pleasure of the operator, by applying pressure to the exposed brain, and by remitting that pressure. Nay, the experiment has been tried on the human brain itself. A man who had undergone the operation of trepanning, and had recovered, was in the habit of exhibiting himself for money in Paris, where Haller saw him. He suffered the spectators to make pressure upon his brain, where it was covered by the integuments only. This always put him into a state of coma or deep sleep; but sensibility and the power of voluntary motion returned at once when the pressure was taken off.

A most remarkable example of the occurrence of coma from pressure upon the brain, and of the removal of the coma by removing the pressure, was afforded by a patient who was in St. Thomas's Hospital under the care of Mr. Cline. Mr. Green, who was Mr. Cline's nephew, was in the habit of relating the case in his lectures here. It is quite pertinent to my present purpose. One of Mr. Cline's apprentices was visiting the depôt at Deptford, and discovered there a man who had been for some time in a state of unconsciousness: and he had him removed to St. Thomas's. His main symptoms were apparent insensibility to all surrounding objects, and a total incapacity to make any communication to those about him; except that his attendants learned to infer, from certain instinctive movements or gestures, that he felt hunger, or thirst, or a want to relieve his bowels. His fingers were permanently bent towards the palm of the hand, and his eyes were turned upwards, so that the corneæ were completely concealed beneath the upper lids.

Upon examining this man's head, Mr. Cline found that there had been fracture with depression of one of the parietal bones. He trepanned that part, and elevated the bone. The patient seemed to feel the operation; and as soon as it was concluded, his eyes and fingers were restored to their natural position. On the evening of the same day, he sat up in bed, and though at first stupid and incoherent, soon became rational and well.

When he had entirely recovered his senses, it was ascertained that the last thing he remembered was his serving on board a vessel which made a capture off Minorca. He was wounded in the engagement, and carried afterwards to the hospital at Gibraltar. All this happened upwards of twelve months before the operation. So that one whole year of this patient's life was a complete blank, because, during that period, a little piece of bone was pressing upon his brain.

Cases of this kind show, very convincingly, the connection that subsists between pressure on the brain and coma, and their relation to each other as cause and effect. The pressure and the coma begin together; the coma continues as long as the pressure continues; and it ceases when the pressure is removed. The old definition of the *cause* of a morbid condition is completely satisfied. "Prasens morbum facit, mutata mutat, sublata tollit."

From this digression—not altogether foreign to our subject—I return to the consideration of the pathology of apoplexy.

Hemiplegia.—If the patient recovers from the coma, he may live a few hours, or days, or he may live for many years. Sometimes, as the coma departs, all the natural functions are gradually restored; but much more commonly paralysis remains. You already know that it is apt to affect one moiety of the body only. If a line be drawn from the vertex to the perinæum, dividing the body into two halves, which, as far as the exterior is concerned, are symmetrical, all the *voluntary* muscles that lie on one side will be found powerless; or, if they are not *all* so, those which are palsied are situated on the same side of the line. And this state of things is called *hemiplegia*. *Paraplegia*, that condition in

which all the parts below a transverse line are palsied, though it sometimes results from cerebral disease, is much more commonly the consequence of mischief in the spine.

Now, of this hemiplegia, when it is complete, there are several particulars worthy of your notice; and there are many things worthy of your notice when it is incomplete. But we will take one of these predicaments at a time. By complete hemiplegia I mean palsy of all the voluntary muscles of one side. The patient may *will* the motion of his leg, or arm, but they no longer obey the act of volition; if they are lifted by another, and then let go, they drop down like logs of wood. You will find that, in well-marked cases, the *intercostal* muscles of the palsied side do not contract. The muscles of the face, also, are some of them inert on the same side. I have known many persons who have thought that the muscles of the *face*, in hemiplegia, when they were affected at all, were affected on the opposite side of the body from that to which the palsied *limbs* belonged. But they never could have examined actual cases of hemiplegia with any attention. How the error arose I cannot tell, but I have known a professed anatomist make it. I guess that it has arisen from one of two causes. An anatomist who had not looked closely upon disease, would expect, and not unnaturally, that the face and limbs would be affected on opposite sides of the body; seeing that the nerves which supply the muscles of the face are given off above the place where those fasciculi of nervous matter which are called the anterior pyramids, decussate each other. And a vulgar observer, who was not an anatomist, would be apt to conclude that the side towards which the mouth was drawn was the affected side: whereas it is just the reverse. The face is drawn to the healthy side, because the muscles on that side are no longer counteracted and balanced by the corresponding muscles of the palsied side. The *blank* half of the face is that which answers to the paralyzed limbs: on that side the patient cannot frown, or smile. He presents a singular spectacle, which I do not dwell upon now, because I shall return to it again when I have to speak of certain important varieties of *local palsy*. What I wish you to bear in mind at present is, that when the muscles of the face are affected in hemiplegia, the *rule* is, that they are palsied on the same side with the limbs. But there is no rule, they say, without an exception: certainly the exceptions to this rule are very uncommon. I have not had leisure to look over the records of the very many cases of this disease which my position as physician to a hospital has brought under my observation; but I do not recollect more than two exceptions; and one of them, as it happens, is now exhibited in the person of one of my patients in the Middlesex hospital. Some of you have seen the woman. It is a well-marked exception: but in this instance the hemiplegia followed a blow on the head, and I suspect that a double injury was inflicted; that the palsy of the face results from mischief on one side of the brain, and the palsy of the limbs from mischief on the other. This I only conjecture; because the phenomenon is so rare.*

Then, again, with respect to the *tongue:* when put out beyond the lips, its point is commonly turned to one side. To which side? Why *towards* the palsied side. For what reason? Why because the muscles that protrude the tongue are powerless on that side, and in full vigour on the other. That half of the tongue which corresponds with the sound side is pushed farther out than the other half, and therefore the tongue bends to the other side. Such is the usual fact, and such the explanation of it. But there are more numerous exceptions to this than to the paralysis of the external facial muscles. Sometimes the tongue comes out straight; sometimes the patient cannot protrude it at all; and sometimes, even, it bends towards the sound side. But the *rule* is as I have stated it.

This also has been noticed of the tongue in such cases; that the patient has been able, after some effort, to thrust it suddenly out, and then has required a certain interval of time before he could do so again; as if the spent nervous power was slowly regenerated. With these different affections of the tongue, the patient's speech is variously altered. His voice is thick, muttering, inarticulate, or unintelligible. Sometimes, even though he may be quite conscious and rational, he is unable to utter a syllable; and seems vexed at finding that his attempts to speak are fruitless.

Supposing the patient to recover, wholly or partially, from the paralysis, it is the *leg*, in

* This patient died afterwards, at her own home; and no opportunity was given of inspecting the body.

nine cases out of ten, aye and in a much larger proportion than that, which recovers first and fastest: sooner and faster than the *arm*, I mean. And another fact, quite analogous to this, is, that when one of the extremities alone is affected with paralysis, it is, in nineteen cases out of twenty, the *arm* that is so affected. I give you again the *rules;* they are liable to occasional exceptions. The reasons that have been assigned in explanation of this curious circumstance I shall lay before you by and by; after I have had an opportunity of describing the morbid appearances met with in the brain in these cases.

This, then, is one way in which the hemiplegia may become, or be from the first, incomplete: viz. in *extent*. One limb may be powerless and the other strong. But the palsy may be also incomplete in *degree*. The patient may be able to move and use his limbs, but they are feeble. He cannot bend his fist firmly: or lift his arm beyond a certain height. Or his leg feels heavy to him, and trails a little behind as he walks: he is unable to stand upon that limb; or to plant his foot securely, or with the usual precision. In short, there are innumerable gradations of paralysis, from slight weakness of the affected muscles to perfect immobility.

Besides the *palsy*, there is often *anæsthesia* also. But this is by no means so constant a symptom as the paralysis. The function of sensation (wherefore I cannot tell) is less frequently abolished or perverted than the function of voluntary motion. When the sensibility is lost, or blunted, or any how modified, it is so, commonly, in the same parts that are affected with paralysis. But sometimes there is anæsthesia, and no palsy; and, more strange still, there has been sometimes anæsthesia of one side, and palsy of the other. As a general rule, the anæsthesia is less common, and less intense than the palsy; and is much sooner recovered from.

The mental faculties are, in some few instances, quite unhurt by the attack: too frequently, however, they suffer irreparable damage. Of many persons, a striking alteration is evident in the whole character and temper. The brave man has become timid; the prudent man foolish; the calm and cheerful man peevish and impatient. There is no longer the same power of attention, the same capacity for business, the same clearness and comprehension of thought. And, whatever other changes may be observable, there are two ways, especially, in which the patient, after he has emerged from the coma, is very apt to be affected: viz. by a defection of memory, more or less partial; and by a peculiar tendency to emotion, especially the emotion of grief: he will weep for very slight causes, sometimes long after the attack of apoplexy has passed over. This is very curious. I should have stated before that the same readiness to shed tears, and to be immoderately affected by trifling causes of emotion, is sometimes noticed among the *precursory* symptoms of apoplexy.

Tracing these cases onwards still farther—such cases, I mean, as do not perfectly recover—we find that the palsied limb wastes: inaction of the muscles, according to the principle which I explained to you in a very early part of these lectures, leads to a lessened nutrition, and a consequent diminution of bulk; in one word, to atrophy. Sometimes, indeed, the size of the helpless limb is maintained, or even augmented, by the supervention of œdema. The motion of the blood in its veins is not aided by the play of its muscles, and the cellular tissue becomes infiltered with serous liquid.

Again, these palsied limbs are usually colder than their fellows. This probably is owing to the diminished circulation of blood through the capillaries: there is not so much blood converted into venous from arterial; and less animal heat is developed. This has been observed even when the main artery of the part has beat as forcibly as in the corresponding part on the other side.

It is necessary to be aware that these palsied parts do not resist the influence of cold or heat so well as the sound parts. When the sensibility is blunted, we can readily understand how the limb may become burned, from the absence of any warning pain that an injurious degree of heat is applied: but this is not all. A lower degree of temperature than would injure a sound part has often been found prejudicial to a palsied part: and if these palsied parts get chilled by frost, they more readily vesicate and inflame, on the return of heat, than other parts: merely *warm* water will sometimes act upon them like *scalding* water. I say a knowledge of this fact is of practical moment. That degree of warmth which the palsied limb fails to generate for itself, we must accumulate for it by

warm clothing; and we must take care that it is never exposed to any artificial temperature which exceeds a certain point. We sometimes see mischief done by applying hot bottles or bricks—*too* hot—to such limbs.

In speaking of the palsy, I have dwelt especially on the loss of action and power observed in the *voluntary* muscles; but the strictly *involuntary* muscles do not altogether or always escape. The pulse, as I have stated, will often become slow or irregular in the apoplectic attack; and the bowels are usually very obstinately costive; their peristaltic motion, which results from the contraction of involuntary muscles, is suspended or diminished. Now the old writers on apoplexy puzzled themselves with devising explanations of the fact that the involuntary muscles are so little affected in this disease. Boerhaave, and others, fancied that the voluntary muscles were influenced by affections of the cerebellum; the involuntary by those of the cerebrum, of the brain proper: but this is quite in opposition to well-ascertained facts. Le Gallois entertained the notion that the movements of the heart and alimentary canal are under the dominion, not of the brain, but of the spinal cord; and this doctrine was received and sanctioned by the French Institute: but it has been refuted by the experiments of later observers. We are enabled, I think, to explain, now-a-days, why the muscles of involuntary motion often are *not* affected in apoplexy. I have before had occasion to show you that the functions of organic life are not necessarily dependent upon any influence derived from the brain: they might go on, if a due supply of arterial blood were kept up, even though there was no brain at all. Some fœtuses have arrived, in other respects, at their full growth, in which there was no brain; nay, in which there was neither brain nor spinal marrow.

In truth it seems to be more difficult to explain why the organs of involuntary motion *are* sometimes affected, in apopletic and paralytic diseases, than why, in general, they are *not*. But some elucidation of this matter I have also attempted to give in a former part of the course. The organs of involuntary motion are not dependent upon the brain and nervous system; yet they are liable to be influenced through their medium; as we know by the effect produced upon those organs by certain emotions of mind. Dr. Wilson Philip has shown clearly, by his experiments, that the way to affect the action of the heart, and of the involuntary muscles, through the brain and nervous system, is to act upon a *large portion* of that system at once. Hence any *disease* which inflicts extensive damage upon the encephalon will be likely to disturb and weaken the functions of the heart and alimentary canal.

Conversely, when we find, in a case of apoplexy, the involuntary muscles sensibly affected, we may infer, I believe, that the injury done to the nervous matter is great and serious.

Anatomical characters.—Let us next, with the view of farther explaining the pathology of this disease, direct our attention to the appearances which are met with after death in the most common forms of the complaint.

I shall pass over those cases in which no morbid condition is detected, simply reminding you that the altered relation of the venous to the arterial circulation in the brain may perhaps account for the symptoms, and for the extinction of life: or the altered velocity of the blood circulating in the brain may account for them: or, what is more probable still, a determination of blood towards the head, or a detention of blood in the head, sufficient, by tightening the full vessels, to occasion extraordinary pressure upon the nervous pulp, may account for them.

I pass over likewise those cases in which serum only is found effused beneath the arachnoid, or into the ventricles. A moderate quantity of serous fluid poured out rapidly during life would certainly occasion a degree of pressure adequate to the production of fatal coma. How the serum comes to be so effused, it is not always easy to say. Yet there is one condition of the blood-vessels of the brain which, when it can be proved to exist in a given case, is sufficient to account for the effusion. Any real or virtual retardation of the blood in the cerebral veins would lead to what is tantamount to dropsy, *there*, as well as in any other part of the body; and the intelligible causes of such retardation are known sometimes to be in operation.

But I wish to consider more particularly the appearances that are met with in the brain after death by *cerebral hæmorrhage*, which, after all, is the most common source of apoplectic and paralytic disorders.

In the first place (as I have more than once stated before) the popular notion that hæmorrhage is owing to the giving way of a considerable blood-vessel—although this notion seldom has reference to the brain, because the blood cannot reach the external surface of the body, and therefore does not strike the popular sense—I say this notion is more true of cerebral than of any other hæmorrhage. Much more true, especially, as regards the brain than as regards the lungs, to which latter organ the bursting of a blood-vessel is, in vulgar parlance, most commonly ascribed.

This comparative frequency of hæmorrhage from the actual rupture of vessels may, in some measure, be accounted for by their peculiarities of texture and relation. The blood-vessels distributed within the cranium are long and slender; excepting the sinuses, the coats of both arteries and veins are thinner and weaker than in other parts of the body: the middle tunic of the arteries has not more than one-half its ordinary thickness; and the outer or cellular coat is of such extreme tenuity that doubts have been entertained concerning its existence. These vessels, moreover, are not protected, as elsewhere, by investing sheaths of cellular membrane, and receive but slight support from the soft and delicate substance by which they are immediately surrounded. They are likewise very subject to a particular form of disease, by which their natural fragility is much increased, and lying near to the heart, and in the primary direction of the blood as it is driven from the left ventricle, they have been thought especially liable to sustain the additional momentum arising from the more forcible contractions of that chamber; whether these are determined by occasional and transient causes, or depend upon permanent organic disease of the heart itself. On this point, however, I shall have something more to say hereafter.

Still there is no reason, I think, for doubting, that hæmorrhage by *exhalation* may take place, though rarely, from the free surface of the brain, or rather of its investing membranes. But the *rule* is, that it proceeds from the mechanical rupture of a blood-vessel.

In cerebral hæmorrhage, the blood may be effused in one or other of three different situations; viz. upon the external surface of the brain, *i. e. upon* or *between* the membranes; or into one or more of its ventricles; or into the very substance of the brain itself. In the two former situations it is sometimes, perhaps, poured out by exhalation; in the latter, which is infinitely the most common, it always proceeds from the rupture of one or more blood-vessels.

It is necessary to remember that even when blood is found spread over the surface, or distending the ventricles of the brain, it frequently has not been originally poured out in those situations. If the hæmorrhage into the substance of the brain be considerable in amount, the blood generally forces a passage, by laceration of the cerebral tissue, either into the ventricles, or (less frequently) to the surface; or even in both these directions at once.

In some rare cases blood is found effused beneath or into the pia mater, over a small space only, as between one or two of the convolutions, and nowhere else. Sometimes it is spread in a thin layer over the whole of one hemisphere, and is found nowhere else. Sometimes blood is discovered in one or more of the ventricles, and nowhere else: but all these are comparatively unfrequent events.

When the blood is effused into the substance of the brain, and does not break a passage out, either in one direction or the other, its pressure is not necessarily or immediately *mortal*. The patient, as I have already explained, may survive for weeks, or months, or years; and the clot of blood will, in the meantime, undergo very remarkable changes.

The cell, or cavity, in which the extravasated blood is contained, varies much as to its *size*. It is sometimes scarcely of sufficient capacity to receive a large pea: sometimes it occupies nearly the whole of one hemisphere. It is seldom, however, I repeat, that a communication is not formed between the original cavity and the surface in the one direction, or the ventricles in the other, when *much* blood has been shed. Frequently a direct opening is made by the blood from one lateral ventricle to the other through the septum lucidum; sometimes it passes from the one to the other through the foramen of Monro; and even the chamber of the septum lucidum itself has been found distended by a certain quantity of blood.

If the clot of blood in an isolated cavity be examined soon after its effusion, it is found

to be of a soft gelatinous consistence, and of a dark colour, much like black currant jelly; the sides of the cavity are irregular and ragged; and the cerebral substance of which they are formed is generally, to the depth of a line or two, moist, soft, and as if stained of a reddish or yellowish colour, which is fainter in proportion as it is more distant from the coagulum, and gradually loses itself in the natural tint of the surrounding parts. This latter condition would seem to depend upon a slow imbibition of the serous portion of the effused blood mixed with some of its colouring matter. It begins to manifest itself about the third day from the attack, and is most apparent from the eighth to the twelfth day; at which period, under ordinary circumstances, the whole of the serum has been removed, and the process of absorption seems to be in active operation. By degrees this stain disappears; the coagulum becomes more and more compact; assumes first a brownish, and subsequently a pale red or even yellowish hue; diminishes continually in magnitude; and at length may be entirely reabsorbed.

In the meantime the walls of the cavity are becoming less uneven, and clothe themselves, by degrees, as they contract upon the shrinking coagulum, with a distinct membrane of a yellowish colour, sometimes of extreme delicacy, and resembling the serous membranes; sometimes thick, and apparently fibrous. When the opposite sides of the cell at length meet, they adhere together, and a true cicatrix ensues, the place of which is marked by a sort of fibrous knot, forming a remarkable contrast with the softer texture around it; or less frequently by a similar induration of a *linear* form. In this case, the sides of the collapsed cavity are sometimes found to be merely applied to each other, without actual adhesion. When, from the great extent of the original cavity, or from some other cause, its parietes are not ultimately brought into mutual contact, there remains a kind of *cyst*, lined by a smooth yellowish membrane; sometimes traversed by a few slender threads of cellular tissue which cross each other in various directions; sometimes filled with a soft, fine, orange-coloured spongy tissue, in which a number of minute blood-vessels ramify; sometimes containing a gelatinous or serous liquid; and sometimes apparently empty, having been occupied by some kind of aeriform fluid.

It is impossible to assign the precise period within which these remarkable changes may be accomplished. Dr. Abercrombie has detailed an instance in which a coagulum, that must have been of very considerable size, had entirely disappeared in less than five months. In another of his cases it was seen to be partially absorbed at the end of three months. On the other hand, Moulin found a small coagulum not quite gone at the end of a year: and Riobé observed some of the blood still remaining in a cavity of small extent after twenty months. In two cases Serres found a hard coagulum of blood remaining; in the one at the end of two, and in the other at the end of three years.

It has been said that the cicatrization of the cavity takes place much more slowly and imperfectly when the effused blood has passed *across*, and *torn*, the *fibres* of the brain, than when it has been poured out in a direction *parallel* to those fibres, so as to separate without breaking them.

Dr. Abercrombie states that he had never seen any thing to satisfy him that the cysts are capable of being obliterated by cicatrization. Neither have I. But Dr. Sims, Dr. Bright, and several of the French pathologists of approved credit and accuracy, agree in their description of this obliteration of the cells. And you should bear in mind that a small cicatrix in the brain may very easily escape notice, if not expressly sought for; especially as the examination of that organ is often conducted, viz. by cutting away thick slices from the hemispheres in rapid succession, in order to arrive as soon as possible at the lateral ventricles, and the base of the brain.

It frequently happens that a patient has suffered, during life, several distinct attacks of apoplexy or of cerebral hæmorrhage; and that as many cells are met with after death, exhibiting respectively various stages of that process of repair which has just been described.

These are the changes that mostly take place in the coagulum, and its containing cell, when the hæmorrhage does not prove fatal, and the patient recovers more or less completely. But the same changes do not always, or necessarily occur. Instead of being gradually removed by absorption, the extravasated blood appears occasionally to become a solid, organized, and consequently living mass, deriving its nourishment from the

arteries of the brain. A man, whose case is related by Andral, was smitten with apoplexy, and remained thenceforward, for many years, hemiplegic. At length he died, of some other complaint, in La Charité. When his brain was examined there was found, in one of the hemispheres, a mass of a pale red colour and fibrous appearance, traversed by numerous small blood-vessels which anastomosed with those of the brain: the surrounding nervous matter retained its natural aspect; and there was no appearance of any cyst.

Situation of the hæmorrhage.—It has long been known that hæmorrhage does not occur in all parts of the *substance* of the brain indifferently. Morgagni had remarked the frequency of sanguineous effusions in or near the corpora striata and optic thalami; and more extensive subsequent research has amply verified the general correctness of his observation. Rochoux, in the treatise on apoplexy which I mentioned before, published in 1814, has given a tabular account of the morbid appearances observed by himself in the heads of 41 persons, dead after attacks of cerebral hæmorrhage.

In so many cases as 24 of these, *i. e.* in three-fifths of the whole number, the blood was extravasated in the corpus striatum; in two others in the optic thalamus; in one it was effused into the substance of both the corpus striatum and the optic thalamus of the same side; and in another, beneath the corpus striatum: so that altogether there were 28 cases out of 41, or seven-tenths of the whole number, in which the clot was confined to the corpora striata, optic thalami, and their immediate neighbourhood. In the remaining three-tenths the blood was found collected in several other parts of the cerebral mass; five times in the middle of one of the hemispheres; twice towards the posterior part of the ventricles; twice in the inner and *anterior*, and three times in the inner and *posterior* portion of the hemispheres; and once in the middle lobe.

In Andral's *Pathological Anatomy* you will find a much more extensive table relating to the same subject, and constructed by him from various authentic sources. It leads to the same general conclusions. Thus, among 392 cases of hæmorrhage into the nervous substance, there were 202 (or more than one-half) in which the blood was extravasated at once into the corpora striata, the optic thalami, and that part of the hemispheres of the brain which is on a level with those bodies. In 61 cases (or about one-seventh of the whole number) it was confined to the corpus striatum. In 35 (or one-eleventh of the whole) it was limited to the optic thalamus; making, in all, 298 instances (or more than three-fourths of the whole number) in which the sanguine effusion occupied the corpora striata, optic thalami, and their immediate vicinity.

The result of my own observation coincides entirely with this, although I cannot, at present, reduce it to a numerical statement.

From the same table we may infer also the comparative infrequency of hæmorrhage into the cerebellum. It is mentioned as having occurred in 21 of the 392 cases; or in about 1 in 19.

Dr. Craigie states that the parts which are the seat of the hæmorrhage may be arranged, in the order of frequency, as follows:—the corpus striatum; the optic thalamus; the hemispheres; the pons varolii; the crura of the brain; the medulla oblongata; and the cerebellum.

It is natural to seek for some physical explanation of the cause which determines the extravasation of blood in certain parts of the brain more frequently than in others. Some light may, perhaps, be thrown upon this inquiry, by a consideration of the sources of the hæmorrhage, in the various forms of its occurrence.

I stated before that the blood *may sometimes* be poured out by *exhalation*, in those less frequent forms of cerebral hæmorrhage to which M. Serres has applied the term *meningeal* apoplexy, and in which the blood is found distending the ventricles, or spread, like a cap, over the surface of the hemispheres, without any laceration of the cerebral matter. This supposition rests, however, rather upon the analogy drawn from what is known to occur in other parts of the body, than upon any decisive and unequivocal evidence. Blood has not unfrequently been discovered in each of these situations, when the most careful scrutiny has failed to trace its source to any ruptured vessel. Yet we cannot doubt that such rupture may have existed—either in some one or more of the numerous vessels of

the pia mater, in the one case, or of the plexus choroides in the other—and yet have escaped detection by the most vigilant eye. In Dr. Abercrombie's book there are two interesting examples of extravasation upon the surface of the brain, without any obvious source of the hæmorrhage; the one detailed by Dr. Hunter, of Edinburgh, the other by Dr. Barlow, of Bath.

That the hæmorrhage proceeds from rupture of some of the vessels composing the choroid plexus, rather than from the membrane that lines the inner surface of the ventricles, when the effused blood is confined to those cavities, is the more probable, because the vessels have been actually found broken (as in cases of ventricular hæmorrhage, described by De Haen and Cruveilhier), and because they are liable to well-marked disease of a nature to render them more than usually fragile. The arteries, for example, which belong to that plexus, are subject to a peculiar kind of alteration that I shall presently mention, as frequently pervading the whole arterial system of the brain; and its veins are often partially enlarged and varicose. This latter condition has sometimes been mistaken for a collection of small hydatids.

But hæmorrhage into the *substance of the brain* depends *always* upon rupture of some one or more of its blood-vessels: and it is to this fact of the rupture of vessels that we must chiefly look for an explanation of the peculiar liability to hæmorrhage of certain portions of the brain; the corpora striata, namely, the optic thalami, and the parts immediately adjacent to these. The corpora striata are not only of much softer consistence than most other parts of the brain, but they are also traversed by more numerous as well as by larger blood-vessels than are other parts. These facts, and the conclusions to which they point, did not escape the sagacity of Morgagni. "On some occasions (says he) when I have cut the corpora striata into pieces horizontally, I remember to have observed in the external anterior part of each, a little pit as it were, across which lay a very conspicuous blood-vessel. And on other occasions, upon cutting obliquely and slowly, I have remarked in the same situation many red lines, like threads, which were in fact blood-vessels running parallel to one another, and of a larger size than elsewhere." In truth you may often notice the open mouths of a cluster of such vessels that have been divided. Morgagni saw in this anatomical fact a probable solution of the pathological fact that the parts in question are the most common seats of extravasation. In corroboration of these views it is worth remarking that the corpora striata are especially subject to laceration and sanguine effusion, while the surrounding parts remain unhurt, in violent concussion of the brain. And when injections are forced into the cerebral blood-vessels in the dead body, it is in the very same parts, the corpora striata above all others, that a sort of factitious hæmorrhage is produced by the rupture of vessels, and the escape of their contents.

Disease of the cerebral blood-vessels.—I have mentioned some original peculiarities of texture and relation, which may be thought to predispose the blood-vessels of the brain, more than others, to laceration. But the *main* predisposing cause of that event is, doubtless, their great liability to *disease*. Except the commencing portion of the aorta itself, there are no arteries in the body so frequently found in a morbid state as the *cerebral* arteries. And the change to which they are most subject is that deposition, between their tunics, sometimes of a substance resembling albumen or soft cartilage, sometimes of actual phosphate of lime, to which we commonly apply the term *ossification*. This earthy or cartilaginous deposit exists usually in whitish patches of a roundish or oblong form, disposed at various distances from each other: sometimes in a succession of bony rings, with healthier portions of the artery between them. One effect of this morbid condition is to diminish the *bore* of the affected artery, and to make it of unequal capacity. And as this variation of calibre impedes the free passage of the blood, it tends indirectly to increase the pressure of that fluid against the sides of the vessel. Another effect is to deprive the coats of the artery of their natural elasticity, and to diminish their power of cohesion: and thus to render them weak and frangible, and at length unable to sustain the increased impulse of the blood. This condition occurs in the smaller ramifications as well as in the larger trunks of the cerebral arteries.

There is yet another occasional cause of hæmorrhage. The arteries at the base of the brain are subject to *aneurism*, and to consequent rupture. Morgagni has reported cases of aneurism affecting the internal carotid and basilar arteries. Serres has described a case

of apoplexy resulting from perforation of the basilar artery, which was dilated, not far from its superior bifurcation, into an aneurismal pouch as big as a hen's egg. Dr. Baillie records an instance where both the internal carotids, on the side of the sella turcica, were distended into little aneurisms, one of the aneurisms being about the size of a cherry, the other somewhat smaller: and similar examples are related by other writers. I have seen two such myself; and a beautiful preparation of one of them is preserved in the museum of the College of Physicians.

LECTURE XXX.

APOPLEXY CONTINUED. RELATIONS BETWEEN THE SYMPTOMS AND THE APPEARANCES FOUND IN THE BRAIN AFTER DEATH. EXCITING CAUSES. TREATMENT.

I LEFT off in the last lecture, after having described the appearances met with, in the head, at different periods after an attack of cerebral hæmorrhage; and pointed out the various sources of the hæmorrhage; and endeavoured to explain why it happens that the blood so much more commonly proceeds from a ruptured vessel in or near the corpus striatum and optic thalamus, than in any other part of the brain.

Relations between the symptoms and the morbid appearances.—I have already anticipated, in some degree, the account that may be given of the connection which subsists between the morbid appearances, and the symptoms. I shall proceed to touch upon certain points, relative to that connection, which have not yet been noticed.

One of the most remarkable circumstances which dissection teaches us, when there has been partial palsy, is, that the palsy is on *one* side of the body, and the hæmorrhage of the brain on the *other*. This is a very general law. But exceptions to it are said to have been observed. Morgagni mentions such. Dr. Bright has recorded a somewhat doubtful case of exception. I have never met with any: and I cannot help suspecting that in some of those which are said to have occurred, mistakes have been made: that either they have been incorrectly observed, or inaccurately described. You may consider the rule as almost, if not altogether, universal.

This crossing over of the morbid effect of the extravasated blood, or of any other diseased state, has long been supposed to be satisfactorily explained by that crossing over of nervous fibres which takes place at the upper part of the spinal cord. Just where the medulla oblongata and the medulla spinalis unite, the anterior periods decussate each other, and send their fibres mutually to the opposite side of the body. All this of course you know. The right anterior pyramid is continued into the centre of the left half of the spinal cord; and the left anterior pyramid into the centre of the right half of the cord. Now supposing, as we have every reason to suppose, that the nervous influence, whatever may be its nature, travels in the course of the fibres of the brain, we see in this decussation of the anterior pyramids an easy and pleasing solution of the phenomenon in question. But then comes this serious difficulty. How does it happen that the muscles of the *face* and *tongue*—which are supplied by nerves that arise from the brain *above* the place of decussation—how does it come to pass that *these* muscles sustain the same cross injury, and are paralyzed on the same side on which the limbs are paralyzed? And again, how does it happen (as it certainly does) that hæmorrhage into the *cerebellum* should have a similar cross influence?

I am not aware that any one has undertaken to explain the anomaly, except Mr. Mayo; and his conjectures upon the subject are so recent, and some of them so ingenious, and some of them, in my opinion, so erroneous, that I cannot do less than touch upon them here.

He takes some pains, in the first place, to show that the morbid influence is communicated from one side of the brain to the limbs of the other side of the body, by means of

the fibres of decussation already described. This point did not, I think, require any laboured demonstration; but he has made a happy use of two facts previously ascertained, which, taken together, afford a very neat proof that the transference of the morbid influence from one side to the other actually takes place in that very part of the nervous system where the decussating fibres cross. The *facts* are stated by Dr. Yelloly, in the *Medico-Chirurgical Transactions*. Sir A. Cooper divided the right half of the spinal cord of a dog in the space between the occiput and the atlas; immediately, that is, after the cord has emerged from the skull through the foramen magnum: the result of this division of the cord was hemiplegia, paralysis of the limbs, on the *same side* with the injury. The bridge by which the morbid influence crosses over must therefore be *above* that point. We have got a limit on one side. And a case observed by Dr. Yelloly gives us a limit on the other. He examined the head of a man who had died hemiplegic; and he found a tumour, as big as a filbert, imbedded in and pressing upon the *right* side of the *annular protuberance*. The palsy had existed on the *left* side. The bridge of communication must consequently lie *below* that point. It must lie, therefore, between the two points now indicated, *i. e.* it must be either in the medulla oblongata, or just at the junction of the medulla oblongata with the medulla spinalis. Now in this very interval, *and here alone*, a decussation of the nervous filaments is found to exist. There can be no doubt that the decussating fibres form the channel of communication.

Supposing (what I think very questionable) that the prevalent notion respecting the uses of the anterior and posterior columns is correct, Mr. Mayo next explains, more clearly than I have found it explained by any previous writer, that the decussation of the anterior pyramids accounts both for the palsy and for the anæsthesia, which are apt to accompany cerebral hæmorrhage into the opposite side of the brain; inasmuch as the decussating fasciculi, on plunging into the opposite column of the spinal marrow, strike into its *centre;* and implicate themselves nearly as much with the posterior, as with the anterior, *i. e.* nearly as much with what is thought the sentient, as with what is thought the motor portion of the cord. The wonder seems to be why the numbness is not more constant; why it is infrequent in comparison with the affection of the voluntary muscles.

Mr. Mayo holds that palsy does not result from the "interruption of the ordinary supply of nervous stimulation furnished by the brain"—for this reason, that in living animals the brain has been gradually removed, sliced away, yet sensation, and the power of voluntary motion, have subsisted; and that instances of acephalous human infants, which have survived their birth, show the medulla oblongata and spinal cord to be sufficient, *without the brain*, for the production of sensation and voluntary motion. He conjectures therefore that the immediate cause of the hemiplegia is "*a depressing influence*, or *shock*, (a *withering influence* he elsewhere calls it) originating in the brain when in certain states of lesion, and propagated from it to the medulla oblongata and spinal marrow."

Now it can scarcely be doubted, at least I cannot doubt, that the inference here drawn from the movements observed in acephalous monsters, and in animals after amputation of their brain, is a wrong inference. They are purely automatic movements, independent of sensation and of the will, and derived from the reflex endowment of the cord: and if this be so, the main foundation of Mr. Mayo's argument is cut away. For my own part, I have no faith in any *shock* except *pressure*. But mere pressure Mr. Mayo repudiates, asserting that in many cases of hemiplegia from cerebral disease there is *no* pressure. And this may be granted: although even in cases of softening, such as he refers to, the mere absence of support in some parts of the brain may lead to the subsidence or settling down of other parts, so as to cause pressure upon the medulla oblongata. I took some pains, in the course of the last lecture, to show you that pressure is adequate to the production of coma and *general* paralysis; and pressure on a nerve in its course we are sure is capable of occasioning *local* paralysis; and upon the whole, my doctrine of the production of palsy by cerebral hæmorrhage or cerebral disease is, that it is frequently (not always) owing to the benumbing influence of *pressure*. I do not ask you to adopt that opinion; nor do I think the solution of the question is of any primary im-

portance; yet I place the question before you, as the best way of impressing upon your memories the *facts* which it involves.

Now whether Mr. Mayo's notion, that some "shock" or "withering influence" is transmitted from the injured brain, be true; or whether that which I have proposed to your notice, that this influence is no other than the benumbing influence of pressure, be true; either supposition will plausibly account for these facts, viz. that "in general hemiplegia from cerebral lesion, the palsy of the leg is (commonly) less complete, and is sooner recovered from, than palsy of the arm," and that when one of these limbs only is affected, it is (commonly) the *arm* alone. The shock, or the pressure, will be most felt in proportion as the part is nearer the origin of the pressure; and less and less felt in proportion as we recede from the source of the injurious influence.

But, unfortunately, I was obliged to insert the word (*commonly*) in the statement just made of the *facts;* which word Mr. Mayo does not employ. To make either his theory, or the theory of pressure, perfectly satisfactory, either the arm alone should be affected; or the affection of the arm should always accompany and be more intense than, or at any rate not less intense than, the affection of the leg. But this is not the case. Since Mr. Mayo's observations were published, I have met with two or three cases, and pointed them out to him, in which the leg alone, or the leg first, has been palsied, from cerebral disease. And Andral, among 75 cases of cerebral hæmorrhage collected for another purpose, which I shall presently advert to, met with 12, in which the leg only was affected. It is a great pity that these stubborn facts should thus cross and thwart a very pretty theory. Perhaps they may hereafter be found not in reality inconsistent with that theory; but at present they are a sad stumbling-block in the way of our adopting it.

I will venture, in my turn, to suggest a conjecture by which the apparent inconsistencies may perhaps be reconciled. I think the excepted cases may be probably accounted for upon the very theory which Mr. Mayo rejects; viz. that the paralysis proceeds, *in these cases*, from a simple interruption of the nervous influence, a breaking up of the road by which the changes leading to sensation travel in one direction, and the mandates of volition in the other. We may easily conceive that, without any pressure, the conducting fibres of communication between the sensorium and the muscles of the leg may *alone* be torn across, or severed by a process of softening, while the residue of the conducting apparatus is entire: and I think it may be worth your while to note, as future opportunities for observation occur, whether the cases that fall within the general rule are not cases of hæmorrhage, in which there is a palpable cause of pressure; and whether what are now called anomalous cases are not cases of mere *softening*, without any obvious compressing agency.

I must not omit to give you Mr. Mayo's explanation of the *other* difficulties to which I referred—viz. of the paralysis of the muscles of the *face* being on the opposite side from the injury of the brain; and of the hemiplegia being on the opposite side from an injury of the *cerebellum*. And having given you it, I shall leave it, without farther comment, to your consideration.

Mr. Mayo's words are: "Where the decussating fasciculi of the anterior pyramid plunge into the opposite half of the spinal marrow, they are implicated, in a wonderful closeness of intertexture, with fibres, which, *in their upward course*, bend towards the places of origin of the ninth and seventh, and of the eighth and fifth nerves of the palsied side. May it not be supposed that this interlacement may be a sufficient means of communicating the palsying influence to the *ascending* fibres, which are in close relation to the affected cerebral nerves?"

Again, "How is the fact to be accounted for, that hemiplegia of the opposite side is produced by lesion of one hemisphere of the *cerebellum?* I have little doubt that the following explanation of the phenomenon will eventually be proved to be correct. The fibres of the anterior pyramids pass through the pons varolii. The pons varolii consists in great part of filaments which issue from each hemisphere of the cerebellum. These filaments may easily be supposed to convey a depressing influence from the diseased hemisphere. But in their course they come immediately upon the filaments of the anterior pyramid of the same side; and they are so implicated with the latter, with such a singular closeness of reticulation, and often with so much that looks like an actual inter-

change of filament, that it is far from unlikely that they may transmit to the descending fasciculi of the pyramid a shock (*qu.* a degree of pressure), which may thence be communicated to the same part at which a *cerebral* lesion exerts its paralyzing force."

I will only say farther of this hypothesis, that if the explanation it furnishes of the facts in question be not the best and most satisfactory in the world, it is the best and most satisfactory that we *possess:* and that, at any rate, we may make use of it to bind those facts to our recollection, until some better theory shall be devised.

There is one very curious law asserted, by Andral, in respect to hæmorrhage of the *cerebellum*. If the blood is effused into one side of the cerebellum, and nowhere else, the palsy that ensues follows the general rule: it takes place in the limbs of the opposite side of the body. But supposing hæmorrhage to take place on one side of the cerebrum, and on the other side of the cerebellum, simultaneously: what then, think you, happens? Doubtless you would expect that there should be palsy on *both* sides of the body. Hemiplegia on the one side, from the effusion into the brain proper; hemiplegia on the other side, from the effusion into the cerebellum: double hemiplegia; that is to say, general palsy. But it is not always so, in fact. The cerebral affection seems to overpower and master that of the cerebellum. Whatever the explanation may be, the palsy has been found to occur on the side opposite to the lesion in the brain proper; and not to occur on the side opposite to the lesion in the little brain. This is a very singular fact, of which Andral relates four or five examples. But I suspect that they will ultimately take their place among the "anomalous" cases. As facts multiply, the law will, I conjecture, be found to be a different one.

The complex structure of the brain, and the dissimilar consequences that ensue, in different cases, from its injury or disease, lead directly to the belief not only that the organ subserves several distinct functions, but also that separate parts or sections of it hold peculiar and definite relations with other portions of the body. Ingenious men have even attempted to settle these points experimentally. By wounding or removing various portions in succession of the cerebral mass in living animals, and comparing the results, they have endeavoured to assign to each portion its particular province and function. But, to say nothing of the remarkable differences which exist between the cerebral functions in man and in the inferior animals, there is an unavoidable source of fallacy common to all such experiments. We cannot reach the particular spot in the brain upon which the contrived injury is to be inflicted, without penetrating and hurting various other parts; and from these combined injuries (dangerous, indeed, and often fatal in themselves) arise symptoms which the experimenter may erroneously conclude to be characteristic of the lesion originally in his contemplation.

Much more accurate and satisfactory data for the determination of this interesting class of questions, would seem to be furnished by the spontaneous operation of disease, and especially of the disease we are now considering. The injury done to the cerebral substance by the eruption of blood is not less sudden, nor less mechanical, than in the experiments or contrived observations to which I have alluded. It is capable also, in many instances, of exact appreciation in regard to its extent; the parts which lie round the seat of the effusion remain undisturbed; and above all, the organ that is the subject of our observation is the *human* brain itself.

Attempts have accordingly been made to connect particular symptoms with the disorganization of particular parts of the brain. These attempts can boast, as yet, it must be confessed, but little success. Very few, if any, of the conclusions hitherto advanced upon this intricate subject can be relied on. Yet it is proper that you should be informed of them.

Because palsy of the arm is, in general, more complete, and more persistent, than palsy of the leg, it has been maintained that the former, the paralysis of the arm, is to be ascribed to hæmorrhage of the *corpus striatum*, which seems to be *more common* than any other; and upon similar grounds hæmorrhage of the *optic thalamus* has been supposed to determine paralysis of the *leg*. So much have these distinctions been confided in, that the honour of having first pointed them out has actually, in France, been made a subject of dispute. Now it is plain that one example of the contrary effect of these particular lesions, would suffice to upset the whole theory: but *many* such exceptions have,

in fact, been noticed. It was with the view of settling this question that Andral collected and collated the 75 cases of cerebral hæmorrhage to which I lately referred. In each of these 75 cases the clot of blood was sufficiently limited to allow of that case being applied towards the solution of the controverted points.

In 40 of the 75, both the leg and the arm were paralyzed together. And where was the place of the hæmorrhage in these 40 cases? Why, in 21 of them the corpus striatum was the only part injured; and in 19 of them the optic thalamus was the only part injured. Thus you see, according to the theory just explained, in about one-half of these cases the arm alone *should* have been palsied, and in about half the leg alone: whereas both leg and arm were palsied in them all.

Again, in 23 of the 75 cases the palsy was confined to the arm. Therefore, according to the theory, the injury should have been confined to the corpus striatum. What was the fact? Why, in this class of cases also there was as nearly as possible an equal sharing of injury between the two parts: in 11 of the 23 the corpus striatum alone suffered; in 10 the optic thalamus alone; in two the space between them.

Once more: there were, as I stated before, 12 out of the 75 cases in which the leg alone was palsied. Consequently, in all of these 12, if the theory were sound, there should have been damage of the optic thalamus only. But in 10 of them the mischief was confined to the corpus striatum; in 2 only to the optic thalamus.

Gall had conjectured that the faculty of speech was placed under the governance of the anterior lobe of the brain: and Bouillaud has endeavoured to support that opinion by a number of facts observed in connection with cerebral hæmorrhage; but Cruveilhier has brought forward several curious instances in which the loss of speech was a prominent symptom, while the disease was *not* found in the anterior lobe, but in some other part of the brain.

Andral, with his accustomed industry, has accumulated evidence upon this point also.

In 37 cases of cerebral hæmorrhage observed by himself or by others, in which the morbid condition occupied one or both of the anterior lobes, the power of speech was abolished 21 times, and unaffected 16 times.

On the other hand, he has collected 13 cases, in which the power of speech was lost, yet no alteration had taken place in the anterior lobes. In 7 of these 14 cases the lesion was situated in the middle lobes; and in the other 7 in the posterior lobes of the brain.

There can be no doubt that there *are* certain distinct parts of the brain which influence respectively the upper and lower limbs; inasmuch as they are often separately palsied: and since the loss of speech is occasionally the only, or the most prominent symptom, while in other cases the speech is not affected at all, we cannot but believe that this faculty is under the special guidance of some definite part within the cranium. But the facts that I have just been quoting, show, in the most convincing manner, that we are not able, as yet, to allot these separate functions to their proper spots in the cerebral mass.

Exciting causes.—I dwelt some little time, in a previous lecture, upon the circumstances that give *warning* to the patient, or to his physician, that the former is in danger of being smitten with apoplexy. And the great use of being acquainted with these circumstances, and of looking out for them, consists in the opportunity and the *authority*, which they furnish, for enforcing, upon the person in whom they manifest themselves, the absolute necessity of avoiding all the avoidable *exciting* causes of the disease. But our means of advising him will be very imperfect if we have not carefully considered what these exciting causes are. I propose to devote a few minutes, therefore, to the consideration of the circumstances that are apt to *bring on* the attack. There are many cases of apoplexy in which we cannot trace the operation of *any* such causes: but in many other cases their influence is decidedly marked; and the avoidance of them, while it is important to all who show a disposition to apoplectic disease, is especially so to those who having once suffered an attack, have reason to dread a *repetition* of it.

In the first place, any thing which is calculated to hurry the circulation, and to increase the force of the heart's action, is likely to operate as an exciting cause of apoplexy: simply by augmenting the momentum of the blood against the sides of the cerebral vessels, which in advanced life are so often diseased and weak. Strong bodily exercise therefore

is a thing to be avoided by all persons in whom the predisposition to apoplexy has declared itself. It is of much importance to make patients aware of this; for many persons think, when they labour under uncomfortable bodily feelings of any kind, they may get rid of them by a brisk walk; or by galloping some miles over the country on horseback.

Another dangerous state for such persons arises whenever the free escape of the blood from the head is suddenly obstructed. I have adverted to this before. Certain diseases, chiefly thoracic, which tend to keep the veins of the head inordinately full, rank among the *predisposing* causes of apoplexy. But, upon the very same principle, various conditions, which are temporary only, may operate as *exciting* causes. By what is called "holding the breath," whether upon an inspiration or expiration, the transit of the blood through the lungs is impeded: and the check is felt (through the pulmonary artery, right chambers of the heart, and great veins) in the vessels of the head. And this effect is increased when *straining* is at the same time performed; that is, when a deep breath is taken and retained, while some muscular forcing effort is made.

Under this principle fall a number of bodily acts, which, however harmless in a healthy frame, are not without peril to a person having a predisposition to apoplexy. The motion of the blood in the lungs, and therefore in the head, is checked in the acts of coughing, vomiting, sneezing, laughing, crying, shouting, and so forth. You cannot have looked at a person in a violent paroxysm of coughing without seeing that it produced a determination of blood to the head, or rather a congested state of the veins of the head. The jarring pain in the head which is apt to follow each succussion of the cough depends upon this principle: which is often strikingly exemplified in young children labouring under hooping-cough. They turn purple in the face, and become giddy; and not uncommonly ecchymosis of the conjunctiva occurs, giving fearful evidence of what might just as well take place *within* the cranium. It is not very unusual for the whole of the white part of the eye to become suddenly blood-shotten in these violent fits of coughing; and convulsions even happen under the like circumstances.

Straining at stool is a common exciting cause of apoplexy in those who are predisposed to it. And this is one of the worst dangers attending costiveness of the bowels in old people: but it is one which it is often in our power effectually to obviate. It is more within our control than a bad cough could be. Any kind of straining indeed is equally perilous. A very good illustration of this danger was recently afforded by a patient of my own. He was attacked with apoplexy on his way to Ascot races; and upon recovering somewhat, was found to be paralytic on one side of the body. He was brought back to town, when I saw him. After some time he regained the power of using the affected limbs to a very considerable extent; so as to be able to walk about, and follow his business, which was that of a job-master, or proprietor of a livery stable. I cautioned him seriously, *inter alia*, against straining: but I suppose he forgot my caution. For, when getting up one morning, he tugged violently in attempting to pull on a damp boot, and in the midst of his efforts fell back insensible: and from this relapse he never fairly recovered.

To the same principle are to be referred a variety of things, which a patient, in danger of this disease, must most carefully avoid. Lifting heavy weights; leaping; striking a hard blow; playing on wind instruments; even long and loud talking. Dr. Abercrombie relates two instances of fatal apoplexy brought on (as it would seem) by a sustained exertion of the voice: one of the attacks happened to a clergyman during the delivery of his sermon; the other to a literary man while speaking in a public assembly. In both cases a large quantity of blood was found extravasated within and upon the brain. Dr. James Gregory used to mention a patient of his, an officer in the army, who had apoplexy, and in whom the attack had been preceded by pains of the head, and giddiness, upon his giving the word of command, and particularly when dwelling upon the last sound; that is, when he made a long expiration. Precisely of the same kind is a case told by Van Swieten, of a singer who was obliged at length to abandon her vocation by reason of gradually increasing vertigo whenever she had to hold a high note. Violent emotion is another exciting cause. Large fires, crowded rooms, the heat even of the sun, will tend to produce apoplexy, and therefore ought to be shunned by those who have a

tendency to that disease. The warm bath is not without hazard to such persons. This is so well known, I understand, at Bath, that the physicians there will not allow paralytic patients, in whom the paralysis has been connected with apoplexy—hemiplegic patients, for example—to go into their hot baths. The excitement of drunkenness, and the venereal excitement, are not uncommon causes of apoplexy, especially in old persons. I had a man of middle age under my care during the spring of 1837, in whom a most awful attack of apoplexy came on under circumstances just as I have now referred to. He had dined at a large festive party, and afterwards accompanied a woman with whom he was acquainted to a brothel; and he was struck with palsy during the act of intercourse. He was long unable to speak; and he still remains, and probably will ever remain, a cripple; imperfectly hemiplegic.

I have been since consulted upon the case of an old gentleman residing in France, in whom an attempt at sexual connection was attended with similar consequences.

> "The Gods are just, and of our pleasant vices
> Make instruments to scourge us."

Posture again has no small effect upon apoplectic people. Giddiness, and some degree of confusion of thought, are apt to be occasioned in most persons by long stooping. There is one peculiar posture or position mentioned by Dr. Fothergill as being very unsafe, especially for short-necked persons—viz. that position which is assumed when we turn the head to look backwards for any length of time without turning the rest of the body; in fact, a twisting of the neck. In this action the jugular veins are more or less obstructed. He gives an account of a man who was seized with apoplexy as he was crossing the Thames in an open boat; he having kept his eye fixed upon a particular ship until, and after, he had been rowed past her. On the very same principle tight ligatures worn about the neck, and compressing the jugular veins, may bring on apoplexy, the wearing a tight neckcloth, for example. A continental writer informs us that a Swedish officer, who was desirous that his men should look well in the face, caused them to wear tight stocks; and the consequence was that in a short time a great many in that regiment died of apoplexy. Dr. Abercrombie quotes from Zitzilius the case of a boy who had drawn his neckcloth remarkably tight, and was whipping a top, stooping and rising alternately. After a short time he fell down apoplectic. The neckcloth being loosened, and blood drawn from the jugular vein, he speedily recovered.

There is one very powerful exciting cause of apoplexy, in those predisposed to it, which I need only refer to now, because the facts that have been observed in proof of its agency were fully detailed in a former part of the course; I mean exposure to cold. You will recollect my telling you that the number of deaths in London from apoplexy and palsy in the month of January 1795, which was a bitterly cold month, very much exceeded the number in the month of January 1796, which was a remarkably mild month. The cold operates in two ways, in the production of apoplexy. In the first place it drives the blood from the surface, and accumulates it in the large vessels of the interior of the body, and so increases the stress upon the cerebral arteries. And in the second place, the cold has a great influence in causing or aggravating affections of the *chest;* and the return of the venous blood from the head is impeded, in the manner just now explained, by fits of coughing and obstructed respiration.

This influence of external cold, and probably certain barometric conditions also of the atmosphere, help to explain, what I am sure I have several times had experience of, namely, the epidemic prevalence, now and then, of apoplectic seizures.

There is an alleged *exciting* cause of cerebral hæmorrhage, which I think it the more necessary to consider, because I believe that very erroneous notions prevail about it, even among pathologists of eminence. I allude to the imputed dependence of cerebral hæmorrhage upon hypertrophy of the left ventricle of the heart. It has been supposed that the powerful contractions of a ventricle thus morbidly strong may drive forwards the blood with such unusual force, as to strain and burst the cerebral arteries. Dr. Hope, in his very complete work upon Diseases of the Heart, uses these words:—"Instances of apoplexy supervening upon hypertrophy have been so frequently noticed, that the relation of the

two, *as cause and effect*, is one of the best established doctrines of modern pathology." And similar opinions are entertained by the most distinguished of the French writers on this subject; Andral, Bouillaud, Cruveilhier. I think they are all wrong: or that at least they state their proposition much too broadly, and generally.

I fully admit, no less from my own observation than upon the testimony of others, the frequent coincidence of hæmorrhage of the brain and hypertrophy of the left ventricle of the heart; but I distrust the reasoning which would always connect these events with each other as cause and effect. They may, sometimes, have that relation; but I have long thought that in most cases, if not in all, the coincidence is capable of being explained upon other and more satisfactory principles.

In the first place, hypertrophy of the left ventricle of the heart is very frequently, far more frequently than not, accompanied by other structural changes of that organ: changes which imply some impediment to the circulation; changes which involve or influence its right chambers also. In fact, disease of the right heart is not very often seen, without disease of the left; and one of the commonest forms of alteration to which the left side is liable, is hypertrophy of its ventricle. Now I have already pointed out to you the connection which sometimes subsists between cerebral hæmorrhage and such disease of the heart as obstructs the ready and regular descent of the blood from the head through the veins. Many of the cases of apoplexy occurring in persons who have previously had cardiac hypertrophy are, I really believe, cases of this kind. The brain affection is dependent, in part, upon disease of the heart, but not upon the preternatural strength of its left ventricle. The heart acts morbidly upon the brain through the veins, and not through the arteries.

But there is another reason for the coincidence; and here the arteries are concerned.

No one can doubt that the momentum, with which the blood reaches the cerebral arteries, in *healthy* persons, under violent bodily exercise or mental excitement, must often exceed the momentum produced by a hypertrophic heart in the cerebral arteries of persons who are tranquil and at rest. But apoplectic seizures are frequent under the latter circumstances, infrequent under the former. We must look, therefore, for something more than the mere hypertrophy to explain the coincidence. Now (supposing the absence of any check to the flow of blood from the head through the veins) that something is to be found in *disease* of the arterial system.

When the arteries of the brain are ossified, or changed, and rendered brittle in the way I yesterday described, the *commencement of the aorta* also is found, in a great majority of cases, to be the seat of similar alterations; and, often, to be sensibly dilated. Now the mere albuminous deposit beneath its inner tunic must seriously impair the elasticity of the vessel; and in this way the free passage of the blood out of the heart will be impeded; and the dilatation of the aorta at that part will produce the same hindrance more certainly and in a greater measure. Still more effectually and obviously will any contraction of the outlet prove an impediment. It is in consequence of these mechanical obstacles to the free exit of the blood from the left ventricle, that the walls of that chamber, urged to more vigorous contraction, become thicker and more powerful. The hypertrophy is the natural compensation for the morbid state of the aorta; without it the heart would much sooner become unable to propel its contents at all: and the hypertrophy does not often, I fancy, become greater than is needful for its purpose. The strength of the left ventricle, therefore, in such cases, is not a true measure of the force with which the blood is driven into the distant arteries. Quite the contrary. It is a measure of the *difficulty* with which the blood is circulated through the *primary branches*, and therefore through the entire system of the arteries. It indicates the *diminished* force with which the blood is likely to reach the cerebral vessels. And in point of fact you will find in *many* cases of hypertrophy of the left ventricle—I do not say in all, but certainly very many—you will find the pulse at the wrist to be disproportionately small and feeble. So that, in these cases, instead of regarding the cerebral hæmorrhage as the *effect* of the hypertrophy (acknowledging, as I do, the frequent coexistence of these morbid conditions) I have been accustomed to look upon the apoplexy and hypertrophy as *concomitant effects of the same cause;* viz. of disease pervading the arterial tree. The hypertrophy of the left ventricle is the effect of the diseased condition of the aorta at its mouth; the cerebral hæmorrhage

is the effect of the same diseased condition of the arteries in the brain. When you find each of these lesions, and nothing to retard the venous current, you may, I believe, safely apply this explanation of the occurrence of apoplexy.

Having again referred to the frequent existence of disease in the cerebral arteries as a predisposing cause of hæmorrhage within the brain, I will just point out, before I leave this part of the subject, the light which that fact throws upon the circumstance that sanguineous apoplexy is so peculiarly a disease of advanced life. Earthy concretions in the coats of the arteries are so frequent in the later periods of existence, that they are met with, according to Bichat, in seven individuals out of ten of those who die beyond the age of sixty: and Dr. Baillie considered ossification to be much more common in old persons than a healthy state of the arteries.

Prognosis.—In the account which I have endeavoured to give you of the symptoms of apoplexy, of the different modes in which the attack may commence, and of the various morbid appearances discovered within the cranium in the fatal cases, I have already embodied almost all that can be stated, with any confidence, respecting the special diagnosis and the prognosis of the disease. The one of these follows the other: the exact diagnosis being known, the prognosis is seldom difficult. By the diagnosis, however, I do not now mean simply the recognition of the disease as a case of apoplexy: of that *general* diagnosis, of the means of distinguishing the coma of apoplexy from the coma caused by opium or alcohol, I told you all that I know in a former lecture. But I use the term diagnosis now in a stricter sense, and in reference to the distinctions that exist between *one case* of apoplexy and *another;* and I say that in proportion to the accuracy with which we may be capable of determining the precise condition of the contents of the skull, will be the facility of predicting the issue of the complaint. Let me remind you, then, that when a patient suddenly becomes apoplectic, we cannot tell whether there be effusion of blood or effusion of serum, or no effusion at all within the cranium: and therefore the diagnosis must be precarious and uncertain. If, after the use of suitable remedies, the coma persists for many hours, the prognosis becomes worse. In those cases which begin with pain of head, faintness and nausea, and which pass on to coma, the prognosis is positively bad; for the diagnosis is easy, and we are tolerably certain that a blood-vessel has given way, and that a large quantity of blood has ploughed up the substance of the brain. In the paralytic cases also, if coma supervenes, the prognosis is gloomy: but frequently coma does not supervene, and then our prognosis, as far as life is concerned, may be pronounced favourable.

Among the symptoms that belong to the apoplectic condition itself, there are some which experience has selected as being most especially of evil omen: and it is well worth your while to remark that these discouraging signs relate, almost all of them, to the automatic functions of the cranio-spinal axis. The open, fixed, unwinking eye; the explosive flapping of the cheeks in expiration; the inability to swallow; the slow, sighing, interrupted breathing; the loosening of the sphincter muscles of the bladder and anus; these are fatal symptoms, and these all depend upon the excito-motory portion of the nervous system. Perhaps the profuse sweat that so often attends the process of dissolution may be referred to the same source; the whole tone of the various tissues being lost or relaxed. I would not say that no one of these symptoms is ever recovered from: but I may say that of twenty patients in whom such phenomena occur nineteen will die.

Now symptoms of this kind may be expected to arise, if there be hæmorrhage in or near the medulla oblongata; or if there be mischief so extensive in the brain as to cause pressure upon the medulla oblongata. We should reason out the likelihood that such symptoms would be of bad augury. But the fact that they are so was ascertained long before the theory which accounts for them was devised. The fact is independent of the theory, and for that reason helps wonderfully to confirm it.

Treatment of apoplexy.—The older writers entertained some very false notions in respect to the distinction between sanguineous and serous apoplexy. They laid it down that apoplexy resulting from extravasation of *blood* within the cranium was denoted by flushing of the face, and strength of the pulse; and that it was a disease of persons in the

vigour of life: while apoplexy resulting from the effusion of serum was marked by paleness of the countenance and weakness of the pulse; and occurred in the old and the infirm: and they directed their practice according to this distinction. 'After what has already been said, I need not tell you that this classification of apoplexies could not have been founded upon the actual observation of disease: and that our treatment, now-a-days, is not regulated by any such erroneous theory.

Nevertheless, I do not mean altogether to praise the modern practice in apoplexy; for it is often one of mere routine. Practitioners are very apt, in this as in other instances, to be guided in their choice of remedies by the *name* of the disease, and to treat all cases of apoplexy alike. I remember being much amused by the perplexity which a friend of mine once told me he had felt on being summoned by letter many miles into the country to see a gentleman who had been struck with apoplexy. As he posted down he earnestly revolved in his mind what he might be able to advise when he should reach the house of sickness. He felt confident that the patient must already have been copiously bled; cupped, or leeched; blistered; and thoroughly dosed with calomel, senna, and croton oil. Mustard poultices had doubtless been applied to his legs. My friend was distressed to think that while much would be expected, nothing would be left for him to do worthy of so long a journey, and so heavy an expense to the patient. A clyster of turpentine might yet, perhaps, be an untried expedient. His cogitations were cut short, however, and his cares relieved, by an express which met him half-way on the road to announce that the patient was dead. Now this is the routine of which I speak: most proper in many cases; unnecessary in others; pernicious in some. There are persons who seem to think that they have not done their patient justice if any part of this active intermeddling has been omitted. Others regard depletion as being worse than useless, and trust entirely to stimulants and cordials. These are still more dangerous *routiniers* than the others; but they are fewer in number.

Our practice would indeed be much easier than it is, if we could thus make one plan fit all cases which are, nominally, the same. But I need not, now, tell you that diseases alike in name—aye, and alike in their essential nature—are often widely different in their circumstances. I formerly explained to you that certain symptoms tell us what the disease is; but that we are often obliged to look to other symptoms, which may tell us what we are to do. I know of no rule so likely to guide you aright as that laid down generally by Cullen, of *obviating the tendency to death*. You must examine and judge to which of the several modes of dying there may be any obvious approach. If the tendency be, as in cases of apoplexy it mostly is, to death by *coma*, then blood-letting and the evacuating system will be requisite. If, on the other hand, the tendency be to death by *syncope*, you must withhold the lancet, and even have recourse to stimulating and restorative measures. Now the distinction between these modes of dying is to be made by attending to the state, not so much of the nervous, as of the sanguiferous system. Insensibility and unconsciousness are common both to syncope and to coma: and cases which fall under the class of apoplexies, and which we cannot separate from that class, are sometimes really more like cases of concussion than any thing else; the shock having been of internal instead of external origin. If the pulse be full, or hard, or thrilling (sometimes it feels like a tense vibrating rope), or if there are obvious external signs of plethora of the head, you must abstract blood. You are not to refrain from bleeding the patient because he is pale, if his pulse warrants it: nor may you omit taking blood if the head and face be turgid, although the pulse be small; for that smallness may depend upon organic disease of the heart.

On the contrary, if his skin is pale and cold, and his pulse feeble or flickering, you would probably ensure your patient's death, or determine the accession of palsy, if you withdrew from the failing heart and blood-vessels a portion of their natural stimulus. I can only invite your attention to these broad features of distinction. Being once taught to look for and attend to them, your own judgment must instruct you as to what may be needful in particular cases. To this, as to most other diseases, the remark of Boerhaave is strictly applicable, who declares that he knows of nothing which can be called a *remedy*, "quin solo tempestivo usu tale fiat."

Having made up your minds as to the general indications of treatment, you will pursue

them steadily in detail. If the patient to whom you are summoned be stupid and drowsy rather than faint, and his pulse and appearance warrant the conclusion of plethora capitis, the first thing to be done is to place him in a semi-recumbent position, with his head raised; to loosen any tight parts of his dress, especially his neckcloth and shirt collar, and whatever might press upon the *neck;* and then as quickly as possible to bleed him from the arm. We know that in some cases the apoplectic state occurs, when as yet no injury has been done to the brain; no effusion, no laceration of its texture; and we may hope, by timely and vigorous measures, to *prevent* these terrible evils. We never can be sure that there is blood extravasated in such cases, and we must act, in the first instance, upon the presumption that there is not. We are especially encouraged to take away a considerable quantity of blood by venesection when we perceive external signs that the vessels of the head are full; redness and turgescence of the face, throbbing and prominence of the temporal arteries, distension of the superficial veins of the neck and forehead. Our object is to take off the strain upon the internal vessels, by bleeding in such a manner and to such an amount as shall produce a decided effect upon the general circulation. Sometimes the good effect of the bleeding is very marked indeed, so that no doubt of its propriety can be entertained; the patient being so insensible as not to feel the puncture of the lancet, and yet emerging from his coma while the blood is still flowing. It is seldom, however, that we can expect such manifest and immediate melioration as this.

After one *sufficient* bleeding from the arm, the vessels of the head may be farther relieved by cupping the nape of the neck, or the temples; and venesection may be repeated if the condition of the pulse, and of the symptoms generally, should require its repetition. It is seldomer, however, in cases of apoplexy than in cases of acute inflammation, that a second or third recourse to the lancet becomes advisable: unless, indeed, the first blood-letting has been mismanaged. Enough blood must be taken, in the first instance, to produce some evident effect; and therefore no precise rules can be laid down in regard to the absolute quantity to be drawn: nor can we make any estimate beforehand as to the whole amount of blood which it may be necessary to remove.

Even if we could be sure that a blood-vessel had given way, and blood was already poured out upon the brain, there are good reasons why (no adverse circumstances withstanding) we ought to bleed our patient largely, and at once. I will enumerate briefly the benefits we seek to obtain by the abstraction of blood in such cases.

1. The effusion from the ruptured artery may be slowly going on. Bleeding from a vein, so as to make a sensible impression on the general circulation, will diminish the stress upon the cerebral arteries, and so tend to put a stop to the hæmorrharge. Both of these two objects are of primary importance.

2. By early and free bleeding we lessen the chance of inflammation supervening upon the mechanical injury done to the brain by the sudden tearing and contusion of its texture by the effused blood; and

3. We thereby bring the system into the most favourable condition for the rapid absorption of the extravasated blood, and for expediting the patient's recovery from those symptoms which depend upon the presence of the clot in the brain.

But although, in that form of disease which we are now considering, bleeding is our sheet-anchor, it may be carried too far, or repeated too often. We must not lose sight of the fact that many of these patients are old, and will not survive over depletion; and that if they survive at all they will need all the strength that we dare suffer them to retain for carrying on the vital actions, when the chief instrument of the most important of the animal functions is so greatly damaged: nor of the fact that if there be blood extravasated, we cannot touch it, except indirectly, by the abstraction of more blood from the arm: nor of the fact that a patient may be bled into convulsions, and fatal syncope. In short, after the first free bleeding, you must be guided by the special circumstances of the case, and particularly by the pulse. The woman at present in the Middlesex Hospital, with paralysis of the limbs on one side, and of the face on the other, attributes her palsy (erroneously most likely) to her having been cupped. She had had a blow some weeks before, and suffered headache from that time. At length she was cupped, from the neighbourhood of the head; and the next morning she was paralytic. This might have been an accidental coincidence. But I remember being sent for a few years ago to see a patient,

at Greenwich, who had already three physicians about him, and was apparently in danger of apoplexy, of which he had for some time experienced distinct warnings. The three physicians had agreed that he ought to be cupped from the back of the neck; to which I assented; and while blood was being rapidly extracted in that manner, he became all at once hemiplegic. Similar cases have been noticed by other persons. Therefore we are not to bleed without measure or discretion.

The pulse may be small, and the arterial action feeble, while yet the veins are turgid, and the capillaries of the head and face loaded with blood. Changes may have occurred in the heart, such as to obstruct the stream which it is its healthy office to transmit. These are cases to which the local abstraction of blood from the head by leeches and cupping glasses is peculiarly adapted.

Again, the whole state of the patient may approximate more or less nearly to the state of syncope; the pulse being weak, the aspect pinched and bloodless, and the skin cool. In this condition, no good, but much harm, is to be expected from blood-letting of any kind. You will do better to apply warmth, cautiously, to the surface, and cautiously to administer what are called diffusible stimuli, of which the preparations of ammonia afford the most eligible forms. Five grains of the sesquicarbonate, or half a drachm of sal volatile, mixed with camphor julep, are ordinary doses. Meanwhile you must carefully watch for symptoms of reaction.

In more ambiguous cases, when you scarcely can tell which way the balance inclines, I would advise you to wait the effect of the next remedy I have to mention; viz. purgatives, about giving which you need not entertain the same doubt and hesitation.

Purgative medicines are of signal service in apoplexy. They empty the intestines, which are sometimes loaded, and which by distending the abdomen have perhaps occasioned undue pressure against the diaphragm, embarrassed the breathing, and through it the cerebral circulation. Another very important purpose of hard purging, which I have frequently pointed out before, is the producing of copious watery discharges from the bowels; whereby the blood-vessels are drained, and the tendency of blood to the head especially relieved. If the patient can still swallow, you may give him half a scruple of calomel, and follow it up by a black dose. If the power of deglutition be lost, the croton oil becomes a most valuable remedy. Dr. Abercrombie suggests that it may be conveniently introduced into the stomach, suspended in thick gruel or mucilage, by means of an elastic gum tube. But really this is not necessary. If two or three drops of the oil be put upon the tongue, as far back as is possible, it will produce its specific effect very readily and well. But we are not to wait for the operation of aperients given by the mouth. Strong purgative and stimulating enemata must be thrown into the rectum: half an ounce, or six drachms, of turpentine, suspended, by the help of the yolk of an egg, in gruel or warm water. We very often witness decided signs of amendment upon the free operation of a purgative. I may mention one instance of this while it is fresh in my recollection. I was asked, a few evenings ago by a medical friend, to see an old gentleman, a patient of his. I found him in bed, comatose, but capable of being roused when loudly spoken to: but he presently fell off again into stupor. His respiration was peculiar. For a minute or two he would breathe, snoring strongly; then the breathing would cease altogether for half a minute or thereabouts; and then the stertorous respiration recommenced: and so on alternately.

He had been found by his servant on the floor, nearly insensible, in the morning, having fallen either out of, or upon rising from, his bed. He had very properly been cupped; and calomel and aperient medicine had been given: but the coma had been growing more profound all the afternoon. His bowels had been but scantily moved; and the fæces and urine were passed as he lay. His extremities were coldish. The pulse was neither full nor strong.

I learned that for four or five years he had had some very significant warnings; and within that period had suffered one or two slight apoplectic seizures, which had left him with impaired mind and memory.

I recommended blisters behind the ears, and two drops of croton oil with two drachms of castor oil in a draught. The next morning I expected to hear that he was dead; but I found him quite conscious, speaking somewhat inarticulately, with the right side of his

face chapfallen and inexpressive. There seemed no particular weakness of the corresponding extremities. The oils had been followed by copious evacuations from the bowels. The day afterwards he was sitting up, and so well that I took my leave.

In combination with blood-letting and purgatives, cold lotions to the head are often found useful in this disease, especially if its surface is hot. I need not trouble you by rehearsing the modes in which the application of this remedy may be managed. Blisters near or upon, the head, are also frequently of service, after due abstraction of blood, in rousing the patient from his state of coma.

Formerly, at the suggestion, I fancy, of Dr. Fothergill, it was much the fashion to give an *emetic* in the outset of the treatment of apoplexy. But we can scarcely imagine that this was not a hazardous measure; and almost sure to do harm if there was already any extravasation of blood. I believe this practice to be now obsolete; and that very circumstance is a proof that it could not have been attended with much success. I merely mention it to protest against it.

When the immediate danger has passed by, and paralysis remains, we are not to be over busy. If the palsy is to get gradually well, it must be by virtue of time, and the resources of nature. To young and strong persons I should, under such circumstances, give small and repeated doses of mercury: and in all cases I should prescribe aperient medicines, so as to keep the bowels freely open twice or thrice a day; enjoin perfect quiet; and put the patient upon very short commons. Diuretics are also very proper when the urine is not plentiful without them.

You will often have to contend against the ignorance and impatience of patients, and their friends, on these occasions. They think that *weakness* is to be remedied by *strengthening* food; by meat and drink, and tonic medicines: or if they are not so foolish as this, they will want to be electrified, or to be put into a warm bath. Certainly in the earlier states of the palsy that remains after apoplexy, none of those measures ought to be *permitted*. If after some time, when all febrile action has ceased, the palsy seems stationary, it may be warrantable and right to attempt to stimulate the torpid nerves, and to accelerate the acquirement of power by the mind over the muscular contractions: but when any means for attaining these objects are employed at all, they must be used with the utmost caution; for they are much more likely to stimulate the vascular system, and so to do harm; or even to renew the apoplectic attack. Electricity, and strychnia, are the remedies most relied upon. In cases of long-standing palsy, it may also, no doubt, be of use to try to awaken, by stimulating frictions, the dormant powers of the muscles, which by protracted inaction become *forgetful*, as it were, rather than *incapable*, of their natural office: and in these old cases we sometimes are able to benefit our patient's condition by the cautious exhibition of some of the preparations of iron.

LECTURE XXXI.

SPINAL HÆMORRHAGE. PARAPLEGIA. FACIAL PALSY AND FACIAL ANÆSTHESIA; THEIR SYMPTOMS, PROGNOSIS, AND TREATMENT. OTHER FORMS OF LOCAL PARALYSIS, AND LOCAL ANÆSTHESIA.

I HAVE done with apoplexy as it respects the brain; which is the same thing as to say that I have done with apoplexy. You will find the same term applied, indeed, to effusions of blood in other organs of the body: but this use of the word is a perversion of language. Apoplexy, as I have frequently observed before, is the abolition of the functions proper to the *brain;* of sensation, voluntary motion, and thought. In short, it is coma, coming on under certain circumstances.

Spinal hæmorrhage.—I shall not speak therefore of *spinal apoplexy* (though that would be less improper than *pulmonary* apoplexy, or *hepatic* apoplexy), but of *spinal*

hæmorrhage. Of this I really have little to say, except that it is well known occasionally to occur; and that the symptoms to which it gives rise are by no means peculiar or distinctive. They consist in *pain* in some part of the spine; *convulsions; palsy:* that is, they are the very same symptoms which inflammation, softening, mechanical violence, and other disorders of the same part, may give rise to. Spinal hæmorrhage is much more rare than cerebral hæmorrhage. Dr. Abercrombie had met with only one case of it. He gives the heads of seven others which have been recorded by different authors. Dr. Bright has never seen it: but he publishes the particulars of one case, which was communicated to him by Dr. Stroud.

I will read you one or two short examples of spinal hæmorrhage, as specimens. A girl, fourteen years old, was attacked with headache, *pain in the back*, and a tendency to sickness when in the erect posture. At the end of a week the pain in the back became suddenly and very greatly aggravated; and this was followed by general convulsions, which proved fatal in five or six hours. The spinal canal was found filled with extravasated blood, in the lumbar region, where she had felt the pain. The brain and all the other viscera were sound. The case is detailed by Mr. Chevalier in the third volume of the *Medico-Chirurgical Transactions*.

Take one more instance from Ollivier, whose work on the spinal marrow you may read hereafter, when you have leisure, with advantage.

A gentleman, aged sixty-one, had just arrived in Paris after a long journey, when he was seized with *pain of the back*, all the way down from the cervical vertebræ to the sacrum. In the course of a few hours he became paraplegic, and was unable to retain his urine or fæces. He then sent for a physician, and died while talking to him. A very extensive extravasation of blood was found in the spinal canal, beneath the membranes of the cord. The lower part of the canal was filled with a bloody mass, in which the substance of the cord could not be distinguished. Above the third dorsal vertebra the cord was entire, but of a deep-red colour, and very soft.

The *suddenness* of the symptoms may lead you to suspect the true nature of these cases; but I cannot pretend to point out any other feature by which they may be distinguished from other morbid conditions of the spine, already spoken of. I show you one preparation; of which, however, I do not know the history.

I have nothing to add, to what I have already said, respecting that species of palsy which is called *hemiplegia:* and I have only a very few farther observations to make in regard to *paraplegia*.

Paraplegia.—The cause of this kind of palsy is sometimes obvious; sometimes most obscure. If we find, in the spinal canal, blood effused, softening of the substance of the cord, traces of inflammation of its investing membranes, tumours pressing upon it, pressure from disease or displacement of the bones, we have a sufficient explanation of the paralysis of those parts of the body, the nerves of which come from the spinal marrow at or below the place of the disease. There are three preparations on the table of tumours that pressed upon the cord; scrofulous tumours I believe they are: each of the three persons from whose bodies they were respectively taken was more or less completely paraplegic.

But in very many cases we detect no alteration that seems adequate to explain the paraplegia. The palsy creeps on slowly and insidiously, without any particular pain, or violent symptoms; there is no tenderness or bending of the vertebræ. The weakness commences mostly in the legs, which appear to the patient heavier than usual, and of which the healthy sensations are often perverted. The toes tingle, or are numb: he experiences a feeling in them as if a number of ants were crawling on the skin. This is so common a circumstance as to have given a name to the symptom, *formication*. The patient straddles as he walks. His legs are lifted awkwardly, the toes being often the last part to quit the ground: they are then flung obliquely forwards and outwards, and the feet flap down heavily and uncertainly at every step. By degrees the weakness of the lower limbs increases: the palsy creeps upwards, affects the bladder and rectum, at length invades the arms, and ultimately the patient dies: yet very faint traces of disease, or no traces at all, may be visible upon inspecting the brain and spinal cord.

Very recently I had a patient with these symptoms in the Middlesex Hospital. He went out contrary to my wishes; he was discharged, in fact, by mistake: he took refuge in St. Bartholomew's, where he died after a few days. I have been informed that serum was found beneath the arachnoid, and that the whole brain had a wasted appearance; but nothing satisfactory was made out. The palsy had by slow degrees ascended from his legs to his arms; he had had no head symptoms beyond some occasional confusion and vague uneasy feelings there. At this present time a patient in a similar condition comes to the hospital every Thursday, dragged by a friend in a hand-chair. He looks very well in the face, but is palsied below.

It is in these cases that you may expect to witness the very remarkable phenomena which I mentioned before as evincing the separate existence of a "true spinal marrow," distinct from the brain and its prolongations into the spinal canal, endowed with special and peculiar properties, and performing functions that are independent of sensation, consciousness, or the will. If you pinch or tickle the surface of the paralytic limbs, particularly about the feet, they will, in many cases, start up and move strongly, not only without any voluntary effort on the part of the patient, but in spite of him, and even (in those instances in which there is anæsthesia as well as palsy) without his knowing it. The legs often spring up *of their own accord*, as it *seems;* but, no doubt, the apparently spontaneous movement is frequently an excited movement, and takes place in obedience to the law that governs the automatic motions of the body. Some impression, made first upon the peripheral extremities of afferent nerves, runs through the nervous arc of communication, and exhibits its ultimate effect at the extremities of the corresponding efferent motor nerves. We can imagine many such accidental and unsuspected sources of excitement; a casual touch, the varying contact of the bed-clothes, the bite of a flea for aught I know to the contrary, may suffice. Even the passage of fæces or flatus along the lower bowel, or of the urine through the urinary passages, may be enough (as we are taught by unquestionable facts) to produce these movements. They are more readily excited, *cæteris paribus*, in proportion as the interfering influence of the will is more completely cut off.

I knew a gentlemen, who had retired from the medical profession, and who, though not paraplegic, laboured, I believe, under some morbid condition of the spine. He had been, in early life, a hard drinker, and had suffered delirium tremens. Every night, sometimes more than once or twice, the trunk of his body, and all his limbs, became for a while fixed and stiff, from rigidity of the muscles. He told me this curious fact. Whenever he scraped his shoes on the scraper at the door, his leg flew up, with a spasmodic suddenness, from the iron, notwithstanding his endeavour to prevent it. I believe he was found dead in his bed.

In some cases of paraplegia involuntary retractions of the palsied limbs can be excited; in some cases they cannot. When the influence of the cerebrum is quite excluded by the operation of disease affecting the spinal cord itself, then is the susceptibility of excited movements the most lively. But the increased susceptibility, which has this inverse relation to the voluntary power, is limited to that portion of the body, the nervous arcs belonging to which lie beyond the seat of the disease; more distant, I mean, from the brain. Hence it follows that we may determine, approximately, the place of the disease, by the test of these reflex actions. The mischief may be situated, or may extend, so low down, that there are no uninterrupted nervous arcs below it. Supposing it to lie as low as, or to reach, the commencement of the cauda equina, we should have no involuntary movements. Conversely, when no involuntary movements can be excited, the spinal disease is, at least, as low as the upper lumbar vertebræ. Thus, I say, we have another mode, in addition to those pointed out in a former lecture, of determining, in a given case of spinal palsy, whereabouts, or to what extent, the cord is implicated in the disease.

We do not so often observe these reflex movements in cases of hemiplegia; apparently for this reason, that in hemiplegia the sensorial influence is not, usually, so completely shut out as it is apt to be in paraplegia. Yet I have seen some of these phenomena in hemiplegic patients. One of them, whose right hand and arm were quite passive under the strongest efforts of his will to stir them, took notice himself, as did his nurse, that whenever he yawned and stretched himself the fingers of the palsied hand participated in

the action, and were thoroughly extended: and I could, by tickling the sole of his foot, excite some starting of the leg, long before any power of voluntary movement returned.

Do not forget the important fact that, in these cases of paraplegia, the urine generally becomes ropy, alkaline, and stinking; and that the bladder, after death, presents appearances such as chronic inflammation might produce; roughness and redness of its inner surface, and thickening of its coats. What is the order and relation of these changes, I confess I do not know: whether the quality of the urine is first altered, and then the bladder suffers from the perpetual contact of this unnatural secretion; or whether the bladder becomes diseased in consequence of the palsy, and pours forth unhealthy mucus, whereby the quality of the urine is affected; has not, I believe, been clearly ascertained. The first of these two suppositions seems to be the most probable. I fancy, indeed, (but I am not sure) that the urine remaining in the pelvis of the kidney in some of these cases has been found to be alkaline. Perhaps the disorganization of the bladder, and the alkaline state of the urine, may both be common results of the interruption of the nervous influence.

We have reason to believe that the defect in some of these cases of paraplegia is merely functional: independent, I mean, of any such change in the nervous matter as is cognizable by our senses. It may be brought on by various causes: by cold; by intemperance in drinking; by excessive sexual intercourse; or, still more surely, by self-abuse. I have had the latter cause assigned to me voluntarily by patients themselves. In such cases we may presume that the loss of function is confined to the spinal marrow. But there is another way in which paraplegia may be accounted for, although its physical cause is very liable to be overlooked. It may result from serous effusion into the spinal canal; which effusion may have originated *there;* or, what seems sometimes to be more probable, may have been poured out *within the cranium*, and descended by the force of gravity to the lower part of the cavity of the spine. Dr. Baillie read a paper on this subject before the College of Physicians: it is contained in the sixth volume of the *Medical Transactions*. He was not the first person to whom this mode of explaining certain obscure cases of paraplegia suggested itself; but he was the first, I believe, who published upon it. This effusion may very readily be overlooked. Commonly the brain is examined first; and no great attention is paid to the escape of fluid from the vertebral canal. It would be better to lay open the spinal cavity first, at its lowest part, and to puncture the theca, and then to observe what quantity of fluid runs out when the body is placed upright. There *should* be a *certain* quantity: but if *much* serum so escaped, we might conclude that it had existed in hurtful abundance during life, and had caused the paraplegia. In most of these obscure cases you may trace *some* head symptoms; giddiness, transient confusion of thought, loss of memory; and it really will be worth your while to make the examination in the way I have pointed out, whenever you have occasion to inspect the body of a patient who has died paraplegic.

I regret very much that I had not the opportunity of doing this in the case of the man whom I had been for some time watching in the hospital.

Cases, such as I have now been describing, are by no means uncommon. They are usually slow and tedious; and you will be called upon to administer to their relief. I need not repeat the caution which I have several times given, in respect to the condition of the bladder; you must take care that it does not become over distended with urine; and you must enjoin strict attention on the part of the nurse to keeping the patient clean and dry. Friction along the course of the spine; blisters to the loins or sacrum, frequently repeated; issues; and electricity: all these means you will generally have opportunity enough for trying, and for regretting their inutility. In such cases it is warrantable and right to give strychnia; a poison which mainly affects the spinal cord; causing, when given in an over dose, tetanic spasms of the limbs, with very little affection of the sensorium. I have heard of some striking instances of recovery from paraplegia under the use of this drug. I wish I could tell you that I had even *seen* such. I should not advise you to begin with a stronger dose of strychnia, or of the sulphate of strychnia, than the twelfth part of a grain every six hours: this may be gradually and cautiously increased, until it gives rise to twichings of the limbs, or to some other obvious effect. The twitchings is usually confined to the palsied limbs. This shows that it results from the agency of the

remedy upon the excito-motory system, or true spinal marrow; of which the reflex function is always more readily exercised when the sensorium has lost its customary controlling power. When this symptom occurs, you had better go on with the same dose; it would be unsafe to increase it: and the progress of the case will soon inform you whether any benefit is likely to accrue from the continuance of the medicine. The out-patient now attending has taken the strychnia. It made the palsied limbs start and extend themselves; but no permanent power has been gained.

There is one other drug which I should recommend you to try in such cases; viz. the tincture of cantharides. It certainly has sometimes a very beneficial effect. Generally, when it does good, it acts as a diuretic; and Dr. Seymour has thrown out a suggestion that it is most likely to be useful in those cases of serous effusion into the spinal canal, of *spinal dropsy*, which I just now described. He recommends the tincture as a good diuretic in several forms of dropsy; and supposes that it benefits paraplegia by tending to produce absorption of the serum effused within the vertebral canal.

Moreover, there is another principle upon which this medicine may be sometimes advisable. Cantharides are well known to have a peculiar effect upon the bladder; which effect is doubtless produced by the corresponding part of the spinal cord. If, by means of the Spanish fly, we can excite, though but from time to time, the function of that part, we may obviate, in a great degree, the distressing consequences of incontinence of urine, arising from paralysis of the *sphincter vesicæ*. Dr. Marshall Hall relates a very interesting fact, bearing directly upon this point. A young lady had a tumour within the tenth and eleventh dorsal vertebræ. It gradually, but completely, severed the spinal marrow, and induced perfect paraplegia. The bladder lost its power of retention. But on giving a dose of the tincture of cantharides the power of retaining the urine was always restored *for the time*. That power would cease, and again be restored, on suspending or repeating the medicine.

Dr. Hall remarks that the cantharides obviously acted through the segment of the excito-motory system left below the division of the spinal marrow.

The tincture may be given in half-drachm doses.

Facial palsy and facial anæsthesia.—The forms of paralysis that have hitherto been noticed are forms of *partial* paralysis. When the palsy is still more limited, although the epithet partial would be equally applicable, the term *local* palsy is more commonly used. There is one of these local palsies which is exceedingly interesting, and of much importance: I mean palsy as it affects exclusively one side of the visage; *facial* palsy. It is sometimes called, not very correctly, paralysis of the portio dura of the seventh nerve. The most common kind of facial palsy is indeed paralysis of the muscles supplied by that nerve. But the word paralysis is misused when it is intended to express any other loss of function than that of the faculty of motion in muscular parts. It is incorrect to speak, as some authors do, of palsy of the kidney; it is equally inexact to speak of palsy of a nerve.

I say that facial *palsy*, and facial *anæsthesia*, (for the two should be considered together,) are very *interesting* affections, because they elucidate, in the human subject, some of the most curious speculations of modern physiology: and they are *important* affections for you to study and understand, inasmuch as, though always distressful and alarming to the patient and his friends, they sometimes are merely inconvenient and disfiguring, and sometimes bespeak a condition of extreme danger.

Let us first consider that affection in which the majority of the muscles on one side of the face alone are palsied. I have already briefly touched upon this form of palsy when it constitutes a part of hemiplegia. But it is of more consequence to attend to it when it occurs without any similar affection of the limbs. When the arm, or leg, or both, are paralyzed at the same time with one side of the face, we know that the whole results from cerebral disease; but it is not necessarily so when the face alone is palsied; and it is often of great moment to the comfort and the safety of the patient, that we should be able to tell whether the palsy does imply disease within the skull or not.

The appearance presented by patients affected with facial palsy is peculiar, and very striking. From one half of the countenance all power of expression is gone; the fea-

tures are blank, still, and unmeaning. The other half retains its natural cast, except that, in some cases, the angle of the mouth on that side seems drawn a little awry. This is apt to be mistaken for proof of a spasmodic condition of that part; but it is owing simply, as I stated before, to the want of the usual balance or counterpoise from the corresponding muscular fibres of the palsied side. The patient cannot laugh, or weep, or frown, or express any feeling or emotion with one side of his face, while the features of the other may be in full play. One half of the aspect is that of a sleeping, or of a dead person; the other half is alive and merry. The incongruity would be ludicrously droll, were it not so frightful also, and distressing. To the vulgar, who do not comprehend the possible extent of the misfortune, the odd appearance of such a patient is always a matter of laughter and merriment. On the other hand, his friends and relations imagine that he has had a fit, and are in great alarm for his life. In the majority of these cases there is not, however, any real danger of that kind to be apprehended; a circumstance which, of itself, would render the exact diagnosis of the complaint peculiarly interesting. In general there is no deficiency of sensation. And we sometimes have loss of sensibility in the same parts, without any diminution of the power of motion. The best way, I believe, to place the phenomena of these curious affections plainly before you, will be by examples.

A house-maid, Jane Smith by name, twenty-eight years old, became one of my out-patients at the Middlesex Hospital, with the following symptoms. She had lost all power of moving the right side of her face. When she endeavoured to raise her eyebrows, the right side of the forehead remained smooth, and the left was wrinkled. When she attempted to close her eyes, the right eye was but partially covered, the eyeball rolling upwards, and carrying the cornea within the curtain of the upper lid, which descended a little to meet it. When she tried to snuff in air through the nose, not being able to keep the right nostril stiff and open, its sides came together, and no air passed up on that side. When she smiled, the right side of the face remained perfectly still, like a mask; and it wore at all times a vacant and inanimate character. When she was told to perform the action of blowing, her right cheek was puffed out like a loose bag, and the breath issued, whether she would or no, at the right angle of her mouth. The same thing happened with her food and drink; she could not prevent their escaping at the right corner of her mouth: nor could she convey morsels of food from the right side to the left jaw, without the aid of her hand applied externally in support of the paralyzed cheek. The masseter and temporal muscles, however, acted as strongly on the one side as on the other; she could chew perfectly well on the palsied side, and the sensation of the palsied parts remained perfect; and there was no paralysis of any other part of the body.

All these phenomena are invariably met with in all well-marked cases of this kind. I will contrast them with the phenomena presented by another of my patients, who was in the hospital, and whose name was Ann Church. I give their names, that I may the more readily distinguish the one from the other. When this woman, Church, applied for admission, she complained of intense pain, with some swelling, of the right temple, and extending thence generally over the right side of the face and head. It was soon discovered, however, that although she complained of most severe pain in these parts, they had entirely lost their ordinary sensibility to external impressions. She felt nothing when her forehead, or cheek, or nose, or chin, was touched on that side. In short, there was complete *anæsthesia* of the right half of the face; just as in Smith's case there was complete *palsy*. The insensibility was very exactly limited to the right half, and terminated abruptly at the mesial line. It was remarkably evident in a part in respect to which the bystanders could scarcely be deceived, even if there had been any reason (which there was not) for distrusting the patient's own statement. The surface of the eyeball is proverbially sensitive, even to slight impressions. But you might place your finger upon this woman's right eye, or you might brush it with a feather, without giving her the smallest pain, or producing any sensation at all: whereas, on the left side, the lightest touch caused involuntary shrinking, and closure of the eyelids, and a gush of tears. She declared also that she had no feeling in the right half of her mouth; she neither tasted sapid substances, nor was at all conscious, from any sensation produced by them, that they were placed there. Her lips, on the same side, were equally destitute of sensibility;

so that when she drank, having no perception of the contact of the cup with her lips beyond their middle point, she felt as if she were drinking from a vessel with a broken rim. This is a circumstance which all persons who are thus affected are much struck with; and it almost always forms a part of their voluntary account of themselves.

Besides this defect of sensibility, the power of contracting the masseter and temporal muscles on the right side was entirely abolished in this patient. You may deceive yourselves on this point, if you do not investigate it carefully, and with certain precautions. At least I have known persons doubt, because, having directed the patient to open and shut his mouth, they have confounded the movement of the whole jaw with the action of the masseter muscle. But if you tell the patient first to close his mouth, and then to perform the action of grinding with his teeth, placing your fingers at the same time on the corresponding muscles on each side, the difference, when it exists, will be very striking. In the woman of whom I speak, no swelling of the masseter or temporal muscle on the affected side took place when she forcibly closed her jaws. There was no other paralysis.

Now we cannot separate the physiology from the pathology of such affections as these. Nor ought we. The morbid conditions of which the two cases just described furnish samples, illustrate in a very beautiful manner the modern doctrine respecting the special uses of particular nerves. In the first of the two cases the palsy resulted from suppression of the function of the hard portion of the seventh pair of cerebral nerves; and the anæsthesia, in the last of the cases, depended upon suspension of the function of the fifth pair. You know that experiments performed upon living animals have proved that the division, by the scalpel, of the portio dura, before it spreads out into that remarkable nervous network on the side of the face, paralyzes all the muscles the combined play of which gives variety and significant expression to the countenance; and that, on the other hand, the division of the fifth nerve deprives the same parts of their sensibility. In these two cases, and in such as these, for they are by no means infrequent, a similar set of experiments upon the same nerves, in the *human* living body, is performed before our eyes by the agency of disease, or accident: and the result justifies most completely those conclusions which had been deduced in the first instance, from contrived observations made upon the lower animals.

There is one point in the history of these cases upon which I must dwell a moment longer; for it is a most interesting point. That the condition of the temporal and masseter muscles should be reversed in two patients so oppositely situated, was no more than might have been expected. But in each these muscles were affected in a manner the very contrary of that which the general circumstances of the case would, *à priori*, have prepared us to anticipate. Where the superficial muscles were paralyzed, and the principal movements of the face suspended, there the masseter and temporal muscles were in full power and action; and where the loss of sensation was the predominant phenomenon, and the ordinary motion and expression of the countenance remained, there these muscles were in a state of complete palsy.

A few years only ago, this difference and apparent inconsistency would have been quite inexplicable. The progress of modern science has removed the difficulty, by establishing a general agreement between the *functions* of different nerves, and certain observed peculiarities in their *anatomical relations* and *arrangements*.

Suffer me to *remind* you (for I know that these interesting points of physiology must have already been taught you) that the nerves which proceed from the spinal column on each side are connected with it by two fasciculi of nervous fibrils—two *roots*, as they are metaphorically called—of unequal size; that when the larger of these, which is situated posteriorly, and is furnished with a ganglion, is divided in a living animal, the parts to which the nerve is distributed lose the faculty of sensation, while the power of voluntary motion remains unimpaired; and that when the smaller and anterior, which has no ganglion, is alone cut, the same parts are instantly palsied, but retain their sensibility. In other words, the posterior fasciculi minister to the faculty of sensation, the anterior to that of motion.

Now the fifth pair of nerves was observed to have a similar origin; to be composed, that is to say, of two fasciculi or roots, one larger than the other, and invested with a

ganglion; the other smaller, and having no ganglion. It was natural to infer that the functions of these roots would be analogous to those of the corresponding portions of the spinal nerves; that the ganglionic fasciculus would relate to sensation, and the other to motion. And such is found to be the case; and the arrangement here is really very curious. The smaller portion of the fifth nerve is exclusively expended upon a very few muscles; viz. the masseter, the temporal, the two pterygoid muscles, the circumflexus palati, and the tensor tympani. The action of the two first of these, of the masseter and temporal muscles, is obvious to common observation; and therefore their condition is noticed in such cases as I have related. Again, these very same muscles have been shown, by careful dissection, to receive no nervous branches from the seventh nerve, which is a motor nerve, and which ramifies so abundantly upon the superficial muscles of the face.

It was to be expected, therefore, that any diseased state confined to the *portio dura* of the seventh nerve, would leave the temporal and masseter muscles fully effective: and that disease involving the fifth nerve, but leaving the seventh untouched, would destroy, not only the general sensibility of the face on that side, but also the power of contracting these particular muscles. And this was thoroughly exemplified in the two cases that I have detailed. The girl Smith had total palsy of the superficial muscles; but sensation, and the action of the deeper-seated muscles, continued perfect: while in the woman Church there was default of sensibility, and paralysis of the temporal and masseter muscles; but the movements of the superficial muscles were unimpeded.

Every interruption of the function of the portio dura will paralyze these superficial muscles of the face: and such interruption may be occasioned either by *sudden injury* done to the trunk of the nerve; or by *disease* affecting its proper structure; or by *pressure*, the consequence of disease in the parts contiguous to it. And it is of great importance to observe that the morbid condition which causes the interruption may be situated in any part of the course of the trunk of the nerve: while it is yet within the cranium; or during its passage through the petrous portion of the temporal bone; or after it emerges upon the face, through the stylo-mastoid foramen, to be ultimately spread in meshes over the cheek and temple. The nerve is often compressed or hurt while still within the skull; but in *most* cases of this kind other portions also of the nervous matter are involved in the mischief, and other sets of voluntary muscles testify this by their immobility or their irregular action. This is sometimes the case when facial palsy occurs as a part of hemiplegia. In many instances, however, of hemiplegia, there is but slight distortion of the countenance, a mere hanging of the cheek, with no paralysis of the orbicularis muscle of the eye. In these cases, it has been suggested to me by my colleague, Dr. Todd, that the seventh nerve is probably not affected at all, but the motor branch of the fifth nerve only. When the *facial muscles alone* are paralyzed, it happens in a great majority of instances that the nervous function is interrupted in that part of the portio dura which lies incased in the bone, or in the more exposed part which issues in front of the ear: and hence it arises that this particular form of palsy is, in general, unattended with any danger to life.

The physical cause of this remarkable disfigurement, and the true explanation of its prevailing immunity from danger, were first pointed out by Sir Charles Bell; but both the existence of the malady as a distinct form of disease, and its comparative harmlessness of character, had been observed and described some years previously: although the reason of neither the one nor of the other was at that time understood. Dr. Powell had narrated, in the fifth volume of the *Transactions of the College of Physicians*, three marked instances of this form of local palsy; and had noticed at the same time its apparent independence of any apoplectic tendency, or cerebral disease.

The exciting causes of the complaint are various. Sometimes it is the consequence of mechanical violence, by which it is plain that the nerve has been lacerated, or otherwise injured. Sir Charles Bell, to whom we are indebted for much information on this subject, mentions several examples of this kind. In one a man was shot by a pistol ball, which entered the ear and tore the portio dura across at its root. In another, the patient was gored by an ox; the horn of the animal entered beneath the angle of the jaw, and came out in front of the ear, tearing the nerve across. In a third, the nerve was

divided by the surgeon's scalpel, in an operation for the removal of a tumour which lay above and around its course. In all these cases the injury was external and obvious. In a fourth the palsy followed a blow on the ear which caused hæmorrhage from that part; here probably the nerve was hurt in its passage through the bone. Some time ago, a man was brought into the Middlesex Hospital who had fallen from a height, upon his head. The muscles of the left side of the face were paralyzed. He died in a few days; and examination of the head showed a fracture in the base of the skull, passing through the petrous portion of the temporal bone, and rending the seventh nerve at its entrance into the meatus auditorius internus. In the year 1832 I had a patient (Richard Hills) in the hospital with the same kind of paralysis, which seemed, in him, to have been occasioned by a mere shock, or jar. He was a coachman, and one day, when he was off his box, his horses started away, and he ran to their heads to stop them, but he was thrown down in the attempt, striking his hip and elbow. He received no blow on the head at all. Three hours afterwards he found that he could not *spit* properly. The affection is not unfrequently discovered by that circumstance. He could not avoid spitting on his clothes on one side; and he could not whistle. Another circumstance worthy of notice took place in this man, which often, though not always, happens in these cases, and which I did not mention before. He remained for about two months in the hospital; and regained during that time in some degree the power of exercising the affected muscles; but he still was unable to close the right eyelid. The eye itself was unharmed. After he was made an out-patient he resumed his functions on the coach-box; and his eye, permanently half open and unprotected, was more exposed to cold and to currents of wind than it had been when he was an in-patient. Moreover he got drunk; and he soon presented himself again with universal redness and inflammation of the conjunctiva. Sometimes the inflammation in such cases produces opacity of the cornea and a total loss of vision. This is one of the worst consequences of facial palsy. Fortunately it is only an *occasional* consequence: and it will occur or not, according to the quantity of motion which remains to the eyelids, and the degree of exposure to the ordinary causes of inflammation.

Sometimes the palsy depends upon manifest *external disease;* sometimes upon disease which is hidden, and probably *internal*, in the bony canal. Sir C. Bell describes an instance in which it accompanied the disorder called the mumps. Dr. Malden, of Worcester, witnessed another in which a fixed, hard, indolent tumour, had formed between the ramus of the lower jaw and the mastoid process of the temporal bone. As this tumour gradually subsided, the palsy disappeared. In each of Dr. Powell's three cases the affection was apparently caused by exposure of the side of the head for some time to a stream of cold air. A medical acquaintance of mine, residing in London, had a patient at Greenwich whom he visited daily. It was cold weather; and on one occasion, as he was returning in the cabin of a steamboat, he was sensibly incommoded by a keen east wind, which blew through an open window directly upon his ear. The next day he presented himself to me with that side of his face fixed in the manner I have been describing. Exactly the same mishap befel a Scottish physician while travelling to London by a coach; and sent him in great alarm to Sir C. Bell. Six years ago a marked example of facial palsy occurred in one of my hospital patients; it appeared to be owing to his having been constantly in the streets for some days without shoes or stockings, during a cold thaw. It may be presumed that in these instances some swelling was produced in the soft parts around the nerve, compressing it where it lies within the unyielding bone. Exposure to cold in this way is the commonest of all the exciting causes of the complaint, and cases thus arising are more obedient to treatment than most others. Probably some of you saw a female patient who came under my care in the hospital in May last (1838), in whom facial palsy had existed on one side for eighteen years. When about three years old she had the measles; and a scrofulous tumour formed behind the ear, and broke; and after some time, a portion of carious bone came away. Then the wound healed (of which deep traces are still visible); and the peculiarity of her features was observed. There are still other cases in which we fail to discover any direct explanation of the paralysis, either in the history of the patient, or in his physical condition. In the girl Smith, whose symptoms I stated in detail as an example of the appearances uniformly present,

the malady came on without any obvious cause, and it resisted all the means employed for its removal.

That the greater number of cases of this kind are free from serious peril, is a fact of great practical importance. It enables us to quiet the alarm of the patient and his friends; and it regulates in many instances the *treatment;* rendering it less severe than it might and ought to be, if the palsy were really the harbinger of apoplexy. At the same time you should not be ignorant that a similar limitation of paralysis to the particular muscles supplied by the portio dura is sometimes (though rarely) observed, when the disease has a more inward origin; when it affects and involves the brain itself. The following case caused me much anxiety, for the subject of it was a personal friend of mine:—I was summoned to his house in the autumn of 1829, and found him with complete palsy of the left side of the face, which had existed a day or two. I shall not describe the appearances and symptoms that resulted from the paralysis; for they were precisely the same as were presented by the girl Smith; and they are always, and necessarily, very much alike. But though the *palsy* was strictly limited to this set of muscles, there were other symptoms present which indicated that the interruption of the functions of the portio dura was connected with some morbid condition within the cranium: nausea and vomiting, twitching of the muscles of the *other* side of the face, great drowsiness, and a slow pulse, 48 only in a minute. He lurched also, and staggered as he walked; but he distinguished this from the reeling of vertigo, and denied the latter sensation altogether. He was deaf, too, on the affected side.

His previous history did not tend to diminish the fears which his actual state excited.

In the preceding February he had been attacked, rather suddenly, with intense pain just above the right eyebrow, and became extremely drowsy. Being desirous, on account of these feelings, to excuse himself from a dinner engagement, he found that he was unable to write a proper note: he could not remember how he ought to express himself.

All these symptoms soon passed off; after the operation, I believe, of an emetic. But he had another attack of the same kind in the subsequent May: the same severe pain over the right brow, with great drowsiness and confusion of mind. He could not recollect the first line of the Æneid. He wished a friend to look at the *signatures* of some letters that had arrived: and although he knew the root, he could not tell how the word he wished to use was formed; whether it was signition, or signation, or signature. The digestive organs on this occasion were made the object of treatment; and he soon got well.

There was another instructive part of his history; and therefore I mention it. Before these attacks he was in the habit of eating and drinking freely; and his power of digestion was supposed to be enormous. After the attack in May he commenced a strict course of temperance. He drank no wine *till three or four days before* the occurrence of the facial palsy: he had then taken it again, and had about four glasses daily; and on one of the days he drank two glasses of Champagne.

It was of some moment to this gentleman, not only that he should recover, but that he should recover quickly. He had been appointed by Government to a mission to Ceylon, and all his equipment was already on board a vessel, which would sail in a fortnight.

Cupping behind the ears, blistering, purgatives, and small doses of calomel continued till the gums were slightly sore, removed the paralysis, and all the other symptoms, in about ten days. He went to Ceylon, and performed his mission so ably that after his return the Government appointed him to one of far greater importance in India, where he now is. He has remained perfectly well; and possesses one of the clearest and strongest intellects that I am acquainted with.

I must trouble you with one more case, to complete the history of this disease: a case in which the cause of the facial palsy was situated within the cranium, and proved fatal, and became visible after death.

Samuel Dovey, a tailor, 57 years old, was admitted under my care into the hospital, in February 1833, with complete palsy of the muscles supplied by the portio dura on the right side; and of no others. There were symptoms enough, however, to show that some serious mischief was going on within the skull. He suffered intense headache, more on

the left than the right side; was dizzy and staggering; and could not get to the ward without being led.

The palsy had come on about ten days before, in the night. He found when he came down stairs the next morning that he could not spit as usual; and his friends observed the unnatural state of his features. He had had no fit, nor loss of consciousness; but he thought his memory was failing. At the time when the paralysis was first noticed he had some numbness and tingling of the right arm, extending to the last two fingers. He was quite deaf in the right ear. This is a point deserving attention in such cases. The deafness, when it occurs, marks an affection of *both the portions* of the seventh nerve; and therefore indicates the probability of an *internal* cause.

The whole progress of this case was very interesting; but I must confine myself to those circumstances which bear upon our present topic. He lived about a month after his admission, and during that interval he suffered great pain in the head, was delirious at times, and at other times in a state of coma: at one period he suddenly presented the ordinary symptoms of apoplexy, from which he partly recovered.

I found a cancerous tumour occupying the right hemisphere of the brain; at its under part was an apoplectic clot, as big as a hazel-nut. I found also a very satisfactory explanation of the deafness and the facial palsy which had been noticed during his lifetime. The portio dura and portio mollis, where they emerge as distinct cords from the medulla oblongata on the right side, were adherent to each other. The portio dura was both harder and larger than the corresponding nerve on the opposite side, while the portio mollis was wasted and diffluent. The same change was traced up to their entrance into the petrous portion of the temporal bone. Immediately over the medulla oblongata, and in a vertical line above the point of emergence of the seventh pair of nerves, a nipple-like portion of brain projected downwards, and had apparently communicated pressure to these nerves; and this projection from the lower surface of the brain seemed to have been produced by the general pressure resulting from the growth of the tumour.

The remarks which I have been applying to *palsy* of these parts hold true also in respect to their *loss of sensibility*. The anæsthesia may or may not portend danger to life, according as the interruption of nervous function on which it depends is situated more or less near to the origin of the fifth pair of nerves in the brain. The patient, Church, whose case I have several times referred to in this lecture, left the hospital with the sensibility of her face nearly as perfect as ever. The treatment consisted in local blood-letting and counter-irritation. She had erysipelas of the head while in the hospital, and was in some danger from that complaint, which was attended with a good deal of fever and delirium. With the exception of the delirium, which belonged no doubt to the erysipelas, there was no reason to suspect any affection of her brain.

Treatment of facial palsy.—I have incidentally adverted to the plan of treatment to be pursued in these cases of facial palsy. When the complaint is recent, and has an obvious cause, the appropriate remedies will readily suggest themselves. When, for example, it has come on after exposure to a current of cold air, or after a blow, or any circumstance likely to give rise to inflammation, you must treat the case as you would treat inflammation; bearing always in mind that a small amount of disorganization, a little thickening or induration of the parts around the nerve, may render the deformity and the inconvenience *permanent*. If there be inflammatory fever, bleed from the arm: if there be not, take blood from the neighbourhood of the affected nerve by cupping: apply fomentations; or, what is better in these cases, conduct the steam of hot water against and into the ear: and administer mercury so as just to touch the gums. I should always take this latter precaution, lest any effusion of lymph should cause abiding pressure on the nerve. If the palsy gives way before the gums become tender, the mercury need not be pressed farther.

Where there is any ground to suspect that the brain is implicated, the treatment just described must be pursued with greater diligence, and with such modifications as the nature of the case may requrie. If there be evidence of chronic disease in the petrous portion of the temporal bone, such as tenderness of the mastoid process, deafness, a protracted discharge from the ear, and an imperfect state of the membrana tympani, we can scarcely expect much good from very active treatment. We must then have recourse to

counter-irritation, and such other measures as I spoke of when the subject of otitis was briefly considered.

Other forms of local palsy and anæsthesia.—The examples which are met with of local palsy, and local anæsthesia, are numberless; but those which I have mentioned are the moat common and the most important. They are always deserving of attention; but more so when any suspicion arises that they may be connected with cerebral disease. Sometimes they evidently have no such connection. In the month of November, 1834, a coachman became my patient in the hospital with imperfect paralysis affecting some of the muscles of the right foot alone, with numbness of the foot. He could both stand and walk; but on advancing that leg, his foot flapped suddenly down, and he could not deliberately direct and plant it, like the other. His general health was quite good; he had no headache, or giddiness, or palsy of any other part. But a month before he had been sitting with the right leg thrown over the opposite knee; and he continued in that position until the foot felt numb, tingling, and was (what is called) asleep; and it had remained in the same condition from that time. After some general treatment (cupping and purgatives) before he came to the hospital—treatment which was quite proper in the way of precaution, but which was probably, in truth, unnecessary—I had his leg electrified; and in about ten days the sensation and the power of the limb were almost restored. Mr. Swan mentions a somewhat similar case, in which anæsthesia of the hand was produced by strong pressure made upon the wrist.

There are some very curious facts, connected with anæsthesia, showing that the voluntary exercise of the muscles is regulated in some measure by the sensations of the limb that is employed. The sense of resistance prompts to such contraction of the muscles as is required to balance that resistance; reminding the will (so to speak) of the necessity that exists for its perpetual and vigilant operation. Continued volition is essential to the continuance of the muscular tension. Thus Dr. Yelloly describes a woman who had no power of feeling in her hand and fingers, although the power of moving them, and of grasping any objects, was entire. This woman found that she could carry glasses or plates in that hand very well and safely, if she continued to look at and attend to them; but if her eyes were turned another way, as she did not feel what she held, she was very apt to drop it. Dr. Ley met with just such another case. A woman had defective sensibility on one side of the body: she could hold her child in the arm of that side so long as her attention was directed to it; but if surrounding objects diverted her from taking notice of the state of her arm, the flexor muscles soon began to slacken, and the child was in danger of falling. All this is exceedingly curious.

Andral has recorded a most singular example of local anæsthesia, which preceded an attack of apoplexy. The patient lost, from time to time, all sensation in certain isolated parts of the skin upon the thorax; there were five or six of these insensible spots, each about the size of a five-franc piece. You might pinch the skin in these places without producing the slightest feeling in the patient. In all other parts the sensibility was perfect and lively.

There are other cases also on record, more remarkable still; in which the patients have lost both the power of motion and the faculty of sensation in almost every part of the body, and yet have survived for a considerable time. Thus one person (whose case is related in the *Bulletin des Sciences Médicales* for Jan. 1828) became first amaurotic, then deaf, and then by degrees lost all power of sensation and motion except in the tongue and in the muscles of deglutition and respiration. His speech and intellects were unimpaired. It was accidentally discovered that a small patch on the right cheek retained its sensibility; and by tracing letters on this sensible spot, his wife and children were enabled to interchange ideas with him. He died at length, and his body was not examined.

I shall finish what I have to say on this head, by relating a case of the same kind, which occurred under Dr. Abercrombie's notice; and which we are sure therefore would be observed with care, and recorded with fidelity.

A servant girl, about 20 years old, sprained her back in lifting some heavy article of furniture. She felt no great inconvenience at the time; but some little while after, weakness of the legs came on, and gradually increased to perfect paraplegia. After an interval, the affection extended to the arms, and she then had not a vestige of motion of any of the

parts below the head, except a very slight movement of one of the fingers: but the internal functions were all perfect, and her speech was distinct, except that in speaking she was sometimes seized with spasmodic twitches of the lips and lower jaw. She lived in that state, without any change of the symptoms, and her general health continuing good, for about twenty years. In the morning she was taken out of bed, and placed in a chair, so contrived as to support her in a sitting posture. Her arms rested on a cross board which passed before her; and if by any accident one of them slipped from this support, she had no resource but to call the assistance of another person to replace it. Having been on one occasion left alone for about two hours after one of her arms had thus slipped down, the hand had become extensively œdematous. In the same manner, if her head fell forward upon the thorax, it remained in that position until raised by an attendant. Her mind was entire. She died after four days illness with symptoms of low typhus fever. You may suppose that Dr. Abercrombie looked with the greatest interest for the cause of these most remarkable symptoms. "I examined the body with the utmost care, (says he) along with Dr. Pitcairn, who had been in the habit of seeing her for several years; and we could not discover any disease, either in the brain or in the spinal cord."

It is much to be regretted that when this case was under observation, the excito-motory functions were not understood, nor attended to.

I shall next proceed to consider those diseases (and there are several of them) which are marked by definite symptoms, which consist essentially in some disturbance or disorder of the nervous system, but which are not accounted for by any physical changes that we can appreciate in any part of that system. After some of these diseases we do, to be sure, sometimes meet with morbid appearances in the brain and spinal marrow: but none that are constant, or uniform.

LECTURE XXXII.

TETANUS. ITS SYMPTOMS AND VARIETIES. CAUSES. DIAGNOSIS. PATHOLOGY. TREATMENT: OPIUM; BLOOD-LETTING; THE WARM BATH; THE COLD BATH.

IN those diseases of the nervous system which have hitherto engaged our attention, the function of voluntary motion, when it has been affected at all, has mostly suffered in the way of diminution, or suspension; the power of moving has been impaired, or lost; there has been complete or incomplete *palsy*. Sometimes, indeed, convulsion, or an irregular and involuntary action of the muscles, has also occurred. But, distinct from the paralytic affections, there is a class of *spasmodic* diseases, of which it is the main and leading feature, that the function of voluntary motion is (not morbidly heightened, as in the preternatural strength of a madman; nor lowered, as in palsy; but) *perverted:* performed in an irregular and unnatural matter.

There are two sorts of spasm. One of these is marked by a long-continued contraction of the affected muscles, not rapidly alternating with relaxation: the relaxation taking place slowly, after some time; and then, perhaps, the contraction, after another interval, coming on again. This is called *tonic* spasm; and, by Cullen, spastic rigidity. A very familiar example of it is the common cramp of the leg. In the other form of spasm, the contraction of the affected muscles takes place repeatedly, forcibly, and in quick succession; and their relaxation is as sudden and frequent. This has been named *clonic* spasm. We find illustrations of it in convulsions.

Sometimes the two are mixed together in the same disease; certain muscles undergoing convulsions or clonic spasm, and certain other muscles being affected with rigidity or tonic spasm. But it is convenient to keep the distinction in mind.

We recognize these disorders by the unnatural conditions of the *muscles;* but you will please to remember that the fault lies in the *nervous* system.

With regard to spasmodic diseases generally, I may say that some of them constitute the most appalling and fatal maladies to which the human body is liable; and some of them, though frightful to look upon, and productive of extreme distress to patients and their friends, are trivial in their consequences, and scarcely ever attended with any peril to life.

I propose first of all to consider one of the most formidable and worst of these spasmodic diseases, viz. *tetanus;* of which tonic spasm is essentially characteristic. Its name is derived from τείνω, to stretch.

In respect to all those diseases concerning the exact pathology of which we are ignorant, and which we identify by the group of symptoms they present, rather than by any organic changes of structure in any part of the body, the most convenient mode of proceeding will be, first to describe the distinctive symptoms.

Tetanus, then, is characterized by an involuntary, long-continued, violent, and painful contraction—in one word, by *cramp*—of the voluntary muscles of various parts, or of nearly the whole body.

There is no difficulty in recognizing the disease when it is fully formed. But it is of much importance to be aware of the marks of its approach, and of its earliest symptoms; in respect of the treatment to be then adopted.

In general, the muscles that seem to be the earliest affected are those of the neck, jaws, and throat. The patient feels a difficulty and uneasiness in bending or turning his head; and supposes that he has got what is called a stiff neck. He finds also that he is unable to open his mouth with the customary facility. At length the jaws close: sometimes gradually, but with great firmness; sometimes (it is said) suddenly, and with a snap. In four cases, perhaps, out of of five, the disease begins in this way, with *trismus*, or *locked jaw;* so that this last is the vulgar name for the complaint. Along with this symptom, or very soon after it, the muscles concerned in swallowing become affected; and in a short time there comes on, what is often the most distressing part of the disorder, an acute pain at the lower part of the sternum, piercing through to the back. This depends, it can scarcely be doubted, upon cramp of the diaphragm. The pain is subject to aggravation in paroxysms; and each paroxysm of pain is attended with increased contraction of the other parts also that are implicated. The spasm extends to the muscles of the *trunk;* to the *large* muscles of the *extremities;* the muscles of the *face;* and last of all, in general, to the muscles of the tongue, and of the hands and fingers, which often remain movable at the will of the patient, after all the other voluntary muscles of the body have become fixed; and frequently the muscles of the wrists and hands escape altogether.

With respect to *all* the muscles involved, from the time when they are first affected to the time when the disease is relieved, or the patient dies, they *continue* in a state of contraction, and are swelled and hard in their centres. The jaw, for instance, can never be completely opened; and the muscles of the abdomen are so rigid as to make it as hard as a board. But, besides this, they are all subject to aggravations or exacerbations of the spasm, which occur, perhaps every ten minutes, or quarter of an hour, and last for two or three minutes at a time; and then the muscles fall back into the minor degree of contraction in which they were prior to the exacerbation. In a very few instances only has a perfect remission of the spasm been observed. The exacerbations usually begin by an increase of the pain felt at the sternum. Sometimes there is no obvious exciting cause of their occurrence; but frequently it is evident that they are brought on by exertions of the body; even by slight movements, such as belong to a change of posture, to the attempt at swallowing, or speaking. As the disease advances, these paroxysms of aggravation become more frequent, and a rapid increase in the frequency of their recurrence is one of the most unequivocal signs that the case is severe and dangerous. The more speedily the intervals between the paroxysms shorten, the worse.

It is a curious thing, that the spasm is observed to give way, sometimes at least, and the muscles to be relaxed, during sleep. To be sure, in the severer cases, the patient is seldom able to sleep; and it may be that in the less violent instances, the spasm abates or ceases, and the exhausted sufferer sinks into repose, in consequence of this abatement. However, a similar phenomenon occurs in at least another of these spasmodic diseases, as we shall see hereafter. Mr. Mayo had a boy afflicted with tetanus, in the Middlesex Hos-

pital. On visiting him one day, he found him asleep, and remarked that he lay perfectly relaxed. The abdominal muscles were soft and yielding, and had not the least tension. The boy was awakened, and at the instant the full tension of the muscles returned: not being farther disturbed, he fell asleep again in a few minutes, when the muscles again slackened; and again, upon his being a second time roused, resumed the state of spasm.

In most cases the strong muscles of the body are the *most* affected, and they overcome those on the anterior part of the body; so that sometimes the patient during the paroxysm rests only upon his head and his heels, while his body is raised into the shape of an arch. This form of the complaint is called *opisthotonos*, a bending backwards. The sterno-cleido-mastoid muscles of the neck have been so stretched and misplaced as to become powerful *extensors* of the head. In a few instances the body is bent forwards, so that the head and knees are in contact, and the patient is rolled together like a ball. This is called *emprosthotonos*. In the only example of emprosthotonos which I ever saw, these two conditions alternated with one another. The patient was a girl, in Edinburgh, under the care of a friend of mine, who took me to see her. It was rather a case of hysteria than of tetanus; but all at once she would be drawn into a position such, that the top of her head, and her feet, were alone supported on the bed, while her body was bent like a bow; then, after a time, with equal suddenness, the opposite posture was assumed, her forehead and knees being brought together. Still more rarely the body is bent to one side. This is *pleurosthotonos*, or *tetanus lateralis;* and this I never saw. Sometimes again, in the height of the spasm, the antagonist muscles counteract each other exactly; and the head and trunk are rigidly extended: and the term *tetanus* is by some writers confined to this form of the disease. It is called *trismus* when the jaw only is affected.

It is well to know that these varieties occur, and may be looked for; but in all of them —trismus, opisthotonos, emprosthotonos, or pleurosthotonos—it is the same disease: and the prognosis is not altered, any more than the diagnosis, by the variety that happens.

During the fit of exacerbation, the aspect of the sufferer is often frightful. The forehead is corrugated and the brow knit, the orbicularis muscle of the eye rigid, the eye-ball motionless, and staring, the nostril spread, the corners of the mouth are drawn back, the set teeth exposed, and all the features fixed in a ghastly grin—the true *risus sardonicus*. The tongue is apt to get between the teeth, and to be severely bitten.

All the contractions are attended with intense pain. You may form some notion of the severity of this pain, if you have ever been troubled by spasms of the gastrocnemius, or cramp of the leg, and if you can bring your mind to conceive that the same sensation which you then felt in the calf, involves nearly all the voluntary muscles of the body. The pain is worst during the exacerbations, and that which is experienced at the sternum is commonly the most complained of. Even to this, however, there are occasional, though very rare, exceptions. Sir Gilbert Blane has described a case of tetanus, which ran the usual course, and terminated fatally, yet the patient suffered no pain: the sensation excited by the violent muscular contractions was a sort of tingling, of rather a pleasurable kind.

So *violent* are the contractions sometimes, that the teeth have been broken by them. There is one case related in which the thigh-bones were fractured by the forcible action of the femoral muscles; and another in which the psoas muscles were found, after death, to have been torn across. Dr. Latham tells me that he once saw one of the recti muscles, in front of the abdomen, thus rent asunder.

With all this disturbance of the muscular system, there is commonly very little derangement of the other functions of the body. The disorder is almost always attended with obstinate costiveness; partly, perhaps, from the spasmodic closure of the anus, partly, perhaps, in some cases, from the medicines that are given. When stools are obtained they are usually very offensive and unnatural. There is no fever. The pulse and respiration are quickened, and a sweat frequently breaks out, during the exacerbations, from the pain and anxiety then experienced; but this is not the case during the intervals between the paroxysms. In the last stages of the fatal cases, the pulse becomes quick and feeble, and the sweat is cold, as in other instances of approaching dissolution.

What is still more worthy of observation is that the mental functions are unaffected. There is seldom any delirium, or coma, or disturbance of the intellect. These symptoms

only appear (if they appear at all) when other indications of the failure of the powers of life come on.

The mode of death, in this disease, seems to be of a mixed nature. Partly it appears to result from apnœa; the thorax being held as in a vise by the spasm of its muscles, and the breathing for a time suspended, or much embarrassed: partly, and chiefly, it occurs from asthenia: the power of the heart flags and is exhausted by the continuance of the suffering, by the fatigue and expenditure consequent upon the muscular action, and by the patient's inability, in many cases, to take sufficient nourishment. When death occurs suddenly, as it sometimes does, in a paroxysm, it is owing, in all probability, to spasm of the respiratory muscles, and perhaps of those of the glottis among the rest.

Most cases of tetanus may be traced to one of two causes: which are, *exposure to cold*, especially to sudden alternations of temperature, and *bodily injuries*. In many instances both these causes cooperate in producing the disease. When it supervenes upon some bodily hurt, it is called *traumatic* tetanus; when it arises spontaneously, or after exposure to cold, it is held to be *idiopathic*. In this country, and I believe in every other, the traumatic variety of the disease is much more common than the spontaneous. But in what manner soever it may originate, tetanus is far more frequent in hot than in temperate climates and seasons. In this case, however, as in so many others, the heat appears to act as a *predisposing* cause only; the exciting cause, in addition to the wound in the traumatic species, being the application of cold (and particularly, according to Hennen, of *cold air in motion*) after the heat, or during the prevalence of hot weather. Thus it is stated that after the battle of Muskau, in the midst of great heats, very few of the French troops were affected with tetanus: whereas those who were wounded in the battle of Dresden, when the weather was cold and wet, just after a very hot season, were decimated by that complaint; which did not spare even those who underwent immediate amputation.

Idiopathic tetanus is extremely rare in this country. Dr. Gregory, of Edinburgh, used to mention in his lectures the case, seen and treated by himself, of a man who, having fallen asleep in moist grass, awoke with a stiff neck, which afterwards went on into regular tetanus. A good example of well-marked tetanus, arising from exposure to cold, is narrated, in the *Edinburgh Medical and Surgical Journal*, by Dr. Hall, of Berwick.

The history of that species of tetanus which occurs in connection with wounds and injuries presents nothing constant or uniform. The disease is liable to follow hurts of any parts of the body, and of every kind, degree, and extent; from a slight cut or scratch, to a compound fracture, or a severe surgical operation. It comes on also in various stages and conditions of the injury. Sir James M'Grigor tells us (in the sixth volume of the *Medico-Chirurgical Transactions*) that in the Peninsular war the complaint supervened "in every description, and in every stage of wounds, from the slightest to the most formidable; the healthy and the sloughing, the incised and the lacerated; the most simple and the most complicated." Sometimes, however, the discharge from the wound has been observed to have been remarkably diminished, or suppressed, at the coming on of the tetanic symptoms: and sometimes the wound has healed completely before the commencement of the attack of tetanus. To show you how very trivial the injury may be, how various in kind and in place, I may mention a few instances that have been collected, in illustration of the manner in which this terrible disorder may originate. It has been known to arise from the sticking of a fish-bone in the fauces; from a slight wound of the ear by a musket-shot; from the mere stroke of a whip-lash under the eye, although the skin was not broken; from cutting a corn; from a bite on the finger by a tame sparrow; from the blow of a stick on the neck and on the hand; from a seton in the chest; from the extraction of a tooth; from the injection of a hydrocele; from the operation of cupping.

Nevertheless there are some sorts of injury, and some parts of the body, more frequently than others concerned in the pathogeny of tetanus. The disorder more often supervenes upon injuries of the extremities, than of the trunk, head, or neck; and upon wounds made by puncture, than upon most other hurts. Penetrating wounds in the sole of the foot, such as are not seldom inflicted by treading upon a nail, or a splinter; and laceration, or other violence done to the muscles that constitute the ball of the thumb, are very apt to be followed by tetanic spasm. Some have supposed that the disease has some special con-

nection with injuries of tendinous parts; but there can be no doubt that it is essentially a malady of the nervous tissue.

The tetanic symptoms occur at no fixed period after the reception of the injury. Professor Robinson, of Edinburgh, was once at table when a negro servant lacerated his thumb by the fracture of a china dish. He was seized with convulsions almost instantly, and died, with tetanic symptoms, in a quarter of an hour. Such a rapid progress as this is, however, quite out of the usual course of the disease: probably fright had something to do with it. Hennen, in his work on *Military Surgery*, states that terror is frequently the immediate antecedent of the attack. In general the tetanus supervenes between the fourth and the fourteenth day after the infliction of the injury: some time in the second week is the most common period of all. In the Peninsular war it did not commence later than the twenty-second day. In some rare instances its accession is still longer deferred. "Of the nature of the changes that take place in the interval (justly remarks Dr. Alison) we have no information whatever." The longer, however, that the disease delays its assault, in these traumatic cases, after the reception of the local injury, the milder in general does it prove, and the more room is there for hoping that it will end favourably.

When the disorder arises from exposore to cold and damp, it comes on much earlier; often in a few hours. If, for example, the exposure takes place during the night, the complaint may begin to declare itself the next morning.

Although tetanus may be excited by a wound, independently of any exposure to cold, or by cold without any injury, there is good reason for thinking that, in many instances, one of these causes alone would fail to produce it, while both together call it into action.

After the disease has set in, its rate of progress is various. Almost all writers divide it into acute and chronic tetanus. But the difference is merely in the degree of severity. When the spasms come on suddenly, recur often from the beginning, and increase in frequency and violence, the chance of recovery is but small. The patient, in these cases, sometimes dies on the second, and generally before the fifth day. If he lives to the ninth day of the disease, his prospect is somewhat better, and the spasmodic symptoms may gradually abate and disappear. Some, however, have died as late as the sixteenth, the twentieth, and even the thirty-fifth day: but this last is very rare.

The idiopathic tetanus, or that which is produced by cold, although it commences earlier, is more generally of a chronic character than the traumatic: that is to say, the spasmodic contractions take place more slowly, and the paroxysms do not increase in violence, and in rapidity of recurrence, as they are apt to do in the symptomatic variety: and accordingly this form of the malady is much oftener, I dare not say cured, but recovered from, than the other.

Diagnosis.—With respect to the diagnosis of tetanus, there is only one point in which it is at all ambiguous or important. There is no other *disease* that is likely to be confounded with it, except perhaps that extraordinary disease hysteria, which sometimes mimics its phenomena. I have already alluded to one example of this kind that I myself saw. But there is a form of *poisoning* that may easily be mistaken for tetanus. The symptoms produced by a poisonous dose of strychnia, or its salts, or the vegetables from which it is procured, are the symptoms of tetanus. And as this drug is now readily obtained, and its noxious qualities are well known, it is not unlikely to be made an instrument of suicide, or of murder. It is necessary therefore that you should be acquainted with the effects of this poison, which constitutes the active principle of the nux vomica, the faba St. Ignatii, and the upas tieuté. Dr. Christison has excellently well described these effects as they are observable in animals; and I have once, by accident, had an opportunity of witnessing them in the human body. I shall not be wandering from our present subject if I enumerate the symptoms to be expected from a large dose of strychnia; especially as I have lately been advising you to make trial of it as a remedy in certain forms of disease. Dr. Christison, who had made experiments with it upon animals, tells us that the creature "becomes agitated, and trembles, and is then seized with stiffness and starting of the limbs. These symptoms increase, until at length it is attacked with a fit of general spasm, in which the head is bent back, and the spine stiffened, the limbs extended and

rigid, and the respiration checked by the fixing of the chest. The fit is then succeeded by an interval of calm, during which the senses are quite entire, or unnaturally acute. But another paroxysm soon sets in, and then another and another, till at length a fit takes place more violent than any before it, and the animal perishes suffocated."

Some time ago I had occasion to prescribe the strychnia for two patients in the Middlesex Hospital, both of whom had paraplegia. I directed one grain to be intimately mixed with crumb of bread, so that it might be divided into twelve pills: and one of these pills, or one-twelfth of a grain of strychnia, was to be taken by each patient every six hours. Unluckily, through mistake or negligence in the person who was at that time the dispenser, a grain of the poison was administered at once to each patient. It was given about seven in the evening. At half-past seven it began to produce its characteristic effect upon one of the patients. He was suddenly seized with tetanic spasms; his legs were separated widely from each other, and rigidly extended: and his head and trunk bent backwards. He was, in fact, in a state of opisthotonos. His abdomen was quite hard, and his limbs stiff, even when the violence of the paroxysms abated. He cried out with the pain at the coming on of these spasms. Any attempt at movement, even the touching him by another person, brought them on. This is just what happens in the *disease*. The opening of a door, a sudden current of air, the smallest bodily effort, the act of swallowing, nay, even the imagination of these influences, will be sufficient to renew the spasmodic tightening of the affected muscles. My patient spoke of a particular sense of constriction all over the abdomen, as if it were drawn in. His intellect was quite unaffected. He had two extremely violent attacks of the kind I have been describing, in which he thought he should have died: and to say the truth I was myself horribly afraid of the same catastrophe. Afterwards, from half-past eight o'clock to between eleven and twelve, he had several slighter and shorter fits. He was left weak and exhausted by them: but he soon recovered. I may as well tell you that his paraplegia was not a whit benefited by the violent action of the remedy.

You may suppose that when I found one of my patients in this alarming state, I became very anxious to ascertain the condition of the other, who had taken the same quantity of the strychnia, and lay in another ward. He told me that he had been for a short period very dizzy, and had trembled all over; and at the time when I saw him, he had a weight or uneasy sensation at the nape of his neck, which drew his head backwards; and he experienced some difficulty in opening his mouth, and in articulating his words. But he thought these symptoms were diminishing rather than increasing. He was perspiring profusely. It is stated by Dr. Christison that if the spasms do not come on within two hours after the poison was swallowed, the patient is safe. It was more than two hours since this patient had taken the strychnia. I gave him a full dose of purgative medicine, which acted as an emetic: and, after he had vomited, the unpleasant sensations about his head and neck left him.

I scarcely knew what to do with the other patient, in whom the spasms had commenced. There is nothing satisfactorily made out, that I know of, concerning the mode of treating such cases. Of course, if one saw the patient early and knew what he had swallowed, the first thing to be done would be to procure its evacuation from the stomach. But here it had full time to get into the circulation: and no emetic could have withdrawn that part of it, at least, which had already found its way into the blood-vessels. When I reached him, though the spasms were strong, they were less violent than they had been, and their violence seemed upon the wane: but they were brought on by any almost the slightest muscular effort, or change of position. I hoped therefore that the most dangerous period was passing off (and so it turned out), and I was fearful of doing harm by exciting those movements of the body which accompany the act of vomiting. I recollected too that another patient in the hospital, under the care of one of my colleagues, had once been attacked with opisthotonos after taking half a grain of strychnia; and that brandy and water had been given to him; and that he got well from that time, without having another paroxysm. So I gave my patient some brandy and water; and he seemed the better for it: but whether or no it contributed much to his recovery, I cannot be sure.

Now how are we to tell, when we meet with such symptoms as these, whether they are the result of disease, or of poisoning? The symptoms are the symptoms of tetanus;

I know of no means of distinguishing them from the symptoms of tetanus caused by exposure to cold, or supervening upon a wound. Dr. Christison has suggested that the cases of fatal poisoning by strychnia that are quickly fatal, are fatal in a shorter time than the disease ever is. But if the case related by Professor Robinson, in which the negro was dead in fifteen minutes, is to be regarded as a genuine instance of tetanus, this distinction, drawn from the rapidity with which the poison kills, will scarcely hold. Again, persons who have taken an overdose of strychnia sometimes survive the tetanic symptoms, but die afterwards from the irritant effects of the poison upon the mucous membrane of the alimentary canal. This we do not observe in the disease. In suspicious or questionable cases, we must look into the history of the patient; inquire whether he were likely to wish to destroy himself; what he last swallowed, and when it was taken; whether he has lately been exposed to the injurious influence of cold, especially to a stream of cold air while he was perspiring; and whether he has recently received any bodily hurt. By a careful investigation of all the circumstances, we shall generally be enabled to decide the true nature of the case; but it is clearly necessary that our eyes should be open to the possibility of a case of poisoning by some of the preparations of strychnia being palmed upon us as a case of natural disease.

The *pathology* of tetanus is undoubtedly obscure: but not more so, I conceive, than that of those nervous diseases in general which produce violent symptoms, and even death itself, without leaving any traces of their operation inscribed upon the dead materials of the body. Nay, it is not *so* obscure as several others. I think we may fairly come to the conclusion that the symptoms result from irritation of the *spinal cord*, or of its afferent nerves; and that the *brain* is not involved in the disease. The French (at least some of the most modern writers on tetanus) hold that it is always an inflammatory complaint; and that it consists essentially in inflammation of the spinal marrow: and some of them have sought to remedy it by enormous blood-lettings; from fourteen to fifteen pounds of blood being taken in the course of a few days by one practitioner; and another bleeding his patient eight times, and applying 792 leeches along the course of the spine, and to the epigastrium. But this doctrine of inflammation being at the bottom of every case of tetanus is contradicted by the plainest facts; and the practice founded upon it has been pushed to a most extravagant and absurd extent. Numberless instances occur of inflammation of the spinal cord and its membranes without any tetanus; and equally numerous examples of tetanus have been met with, when no unnatural appearance at all could be discovered within the vertebral canal. I say we must content ourselves with referring the phenomena of the disease to *irritation*, direct or indirect, of the spinal cord; or its nervous appendages: and I am quite proof against all sneers against the alleged vagueness of that term.

If you *irritate*, mechanically, by means of a pair of forceps, the exposed spinal cord of a recently decapitated animal, a turtle for example, you produce spasmodic contraction of the limbs: what difficulty is there in supposing that some mechanical irritation existing within the spinal canal of a living man may have a similar effect? It may be, and probably is, sometimes, the mechanical irritation caused by the altered state of the blood-vessels under inflammation; for we sometimes find traces of such inflammation in the spinal marrow after death by tetanus.

Again, if you irritate, by pinching, one of the spinal nerves of a turtle whose head has just been cut off—if your irritate one of these nerves in any part of its course, what happens? why the muscles of the limbs contract spasmodically; those on the side to which the nerve belongs, and those on the other side also. The property of the cord comes into play which I have so often mentioned: a property which it possesses independently of the brain; which it evinces when all communication with the brain is cut off; a property, therefore, which may be manifested without any exercise of volition, and even in spite of efforts made by the will to restrain its manifestation: I mean, of course, the property whereby it is capable of receiving impressions through the medium of its afferent nerves, from parts at a distance, and of originating motion in the muscles of the trunk and limbs through the medium of its efferent nerves. By the courtesy of Dr. Marshall Hall I have been afforded the opportunity of witnessing, in the headless turtle, the phenomena that I have been describing to you. Surely they throw a broad light upon the pathology of

tetanus, and of some other affections. We infer from them, most legitimately as it seems to me, that the tonic spasm which characterizes the disease we are considering may be caused by a diseased state of the spinal marrow itself; or by a morbid condition of the nerves that belong to it. In the latter case, irritation is set up at the free extremity, or somewhere in the course, of incident nerves; along these nerves an influence is conducted to the cranio-spinal axis, in which a process or change takes place, whereby an answering influence is reflected to the muscles along motor nerves: and the whole circle of action and reaction is run through with the suddenness and swiftness of lightning, or of thought. You cannot expect that visible marks of the irritating cause should, in all cases, be left upon the body; any more than you could discern the pinch made by the forceps after they were withdrawn.

When, in the experiments to which I have referred, Dr. Hall plucked or compressed one of the denuded spinal nerves, spasmodic motions were excited in the muscles of *both sides;* and *above*, as well as *below*, the junction of that nerve with the cord. This shows that the change (whatever it be) that is wrought in the cord by impressions made upon one of its afferent nerves, is not necessarily confined to the corresponding *segment* of the cord; but may be instantly communicated, in both directions, throughout its entire course: the whole of this centre of the excito-motory system responding to the influence conveyed by a single nerve, as completely as a tight string vibrates from end to end, when struck at any one point. We frequently, indeed, find that the excited motions are more limited: but it is important to mark this ready consent of the whole cord, under sufficient excitement.

Dr. Hall has given certain distinguishing epithets to tetanus, according to the supposed source and locality of the irritation. When the irritating cause operates directly upon the spinal cord itself, he calls the disease *centric* tetanus: when it resides in some part of the body, distant from the spinal cord, he calls the disease *eccentric* tetanus. These are good and intelligible names; and I shall take leave to adopt them.

Observe now how well this explanation meets the facts of the case. We sometimes find the spinal cord or its membranes inflamed, when there has been tetanic spasm. We then refer the spasm to the centric irritation. But in a far greater number of cases we can detect no marks whatever of disease in the spinal canal, but we know that an irritating cause has been applied to parts at a distance: often we have evidence which is visible that a nerve has been injured, torn across perhaps, or half torn, or compressed in some way or the other; just as we might compress a nerve, with a pair of forceps, in a decapitated turtle. That experiment shows us that very slight irritation may be enough to produce the spasmodic action; and we find that slight injuries, as well as severe, will bring on the disease, when, by the operation of certain injurious agencies, the frame has been predisposed, and rendered morbidly susceptible. There is no part of the trunk or limbs which is not supplied with nerves from the spinal cord; and we find that injuries of various parts, or of almost any part, in an individual predisposed to take on the disordered action, will produce it. The exciting cause may be a wound irritating a particular nerve: it may be exposure to cold, acting upon the extremities of various nerves that proceed from the surface: it may be a bundle of worms, irritating the nerves spread upon the mucous tissue of the alimentary canal; for I omitted to state before that some writers, especially MM. Laurent and Lombard, have maintained that tetanus is almost always, even when it supervenes after wounds, the result of the presence of worms in the digestive organs. They have founded this opinion upon the *fact*, that worms have been very frequently indeed found in the stomach or intestines of persons dead of this disorder. I think this is a point well worth attending to. It is objected that worms infest the human body without causing tetanus: but the very same thing may be said of the operation of cold; and of external injuries. Any of these may probably excite the disorder, when the body is preternaturally susceptible of it. The real mystery lies in this predisposition. We have reason to suppose that a high atmospheric temperature continued for some time is *one* predisposing cause; but how it operates, or what is that state of system in which the increased susceptibility consists, these are points concerning which we are really in the dark.

The disease is common enough in the lower animals: and it is frequently *eccentric* in them; brought on by injuries, mostly of the extremities. Locked-jaw is well known in

the nosology of farriers. It is not uncommon in the horse after castration. I remember a mare belonging to my father dying of that disease a few days after foaling. Dr. Parry noticed eccentric tetanus in lambs. "I have often seen lambs," says he, "whose ears, for the purpose of marking them, have been bored with a red-hot iron too near the root, so rigid all over with tetanus, alternating with convulsions, that their bodies would project in a right line with their hind legs, when one held them out horizontally by the hind feet." Dr. Mason Good tells us that parrots also are frequently affected with trismus: a calamity which, supposing the bird to be within ear-shot, it would be difficult to commiserate.

We are not advancing any wild theory, then, respecting the controverted pathology of this disease, when we lay down the following propositions: that it is essentially a disorder of the excito-motory apparatus; that it results from irritation of a peculiar kind, affecting that part of the nervous system; that the irritating cause may be centric, or within the spinal canal itself; and that it may also be, and often is, eccentric, or situated at the extremity or somewhere in the course of one or more of the afferent spinal nerves; and that a certain predisposition of the body is for the most part necessary, to render it susceptible of the disease under the operation of the exciting irritation.

At one time it was supposed that the physical cause of the disease was detected, in the presence of more or fewer thin scales of bony matter, lying in or upon the arachnoid of the cord. I have myself seen these after death preceded by tetanic symptoms. But tetanus often happens and proves fatal without them: and they are often met with when there has been no tetanus. If, therefore, there be any connection between these thin plates of ossification and the occurrence of tetanus (which may well be questioned) it must be of this kind; that the scales of bony matter predispose the spinal cord, somehow, to be affected by the exciting causes of the disease.

The doctrines recently propounded by Dr. Marshall Hall, of which the importance becomes daily more apparent, and by which his name will be enduringly connected with the physiology of the nervous system, acquire a strong confirmation from the phenomena of tetanus. They furnish a key to many problems which had previously perplexed the pathologist; and they do this simply by distinguishing the proper functions of the two distinct nervous centres; the brain and the spinal cord. But the practical application of these new views is yet in its infancy.

Treatment.—The *treatment* of tetanus is a mortifying subject. The disease is and has always been a lamentably fatal one. Hippocrates says, επι τρωματι σπασμος επιγενομενος, θανασιον, tetanus supervening on a wound, is mortal: and the aphorism holds true, with very few exceptions, in the present day. Almost all the acute and severe traumatic cases are fatal. Hennen declares that he never saw a case of "acute symptomatic tetanus" recover. Dr. Dickson found all curative measures followed by "unqualified disappointment." Mr. Morgan uses these words: "I have never yet seen or heard of an instance of recovery from acute tetanus. Another of Hippocrates's aphorisms is, ὁκοσοι υπο τετανου ἁλισκονται εκ τεσσαρσιν ἡμερησιν απολλυνται; they who are seized with tetanus, die within four days: but he adds ην δε ταυτας διαφυγωσιν ὑγιεες γινονται; if they get over this period they recover. And to this we can only add now, that those who survive the first few days, and ultimately get well, recover in a variety of different ways, and under various modes of treatment. But as to the mode of treatment which is to be preferred, or even as to the real efficacy of any mode, there is much room for doubt. Under every plan of management a vast majority die.

Let us briefly pass in review the principal remedies that have been tried, and inquire what degree of success has followed their employment.

One drug from which much benefit has been hoped for, is *opium*. In some spasmodic disorders it is of unquestionable service. Very large doses of it have been given and borne in tetanus; and some have recovered under its use, and more have died.

It is well known that pain fortifies the nervous system against the peculiar influence of narcotic substances. We need not, therefore, be surprised that opium, administered in enormous quantities, in this painful disease, has had but little effect. I was assured by a physician, with whom I formed an acquaintance in Edinburgh some years ago, and who is known, I find, to a student now present, that his own wife, while labouring under a tetanic affection, swallowed, in twenty successive days, upwards of 40,000 drops of lau-

danum, which is at the rate of more than two ounces a day; in all, more than an imperial quart. The lady recovered. A case is recorded in the 2d volume of the *Medico-Chirurgical Transactions*, in which an ounce of *solid* opium was taken, in divided doses, every day, for 22 days. This appears a more astounding instance than the former; but I am not sure that it was so; for, in this complaint, solid opium does not always dissolve in the stomach. I have heard the late Mr. Abnernethy say that he had found enough undissolved pills of opium in the stomach after death, to poison a dozen healthy persons. This fact should teach you, if you resolve on trying opium at all, to exhibit it in a liquid form; laudanum, or a solution of the acetate, or of the muriate of morphia. And it would be well, I think, to combine the external use with the internal administration of opium; to blister the spine, and strew the blistered surface with powdered acetate of morphia, while you give it in solution by the mouth.

It is sometimes a difficult matter to introduce medicine by the mouth, so strong is the spasmodic contraction of the muscles that close the jaws. You cannot get the mouth open. Some persons set to work to heave it open, by levers; and it has been proposed, and I believed practised, to break off or extract a tooth or two, to make a passage for the introduction of medicine and of nourishment; but I hope you will never be guilty of such clumsy barbarity as this. Food, and physic, may be carried into the fauces or into the stomach by means of a flexible tube: and this may be inserted through the nostril; or through the mouth, by passing it between the jaws, behind the back teeth, where there is always an aperture that will admit a sufficiently large tube.

After all, in respect to the cures that have been ascribed to the opiate treatment, they have all (so far as I know) occurred in cases of the milder or more chronic tetanus; and mostly in the idiopathic form of the disease; and this circumstance makes it a question whether they were *cures* at all; whether they were not simply recoveries.

Dr. William Budd (in the paper already referred to) challenges the propriety, on physiological principles, of giving any opium in this disease. He says "it has been ascertained that the effect of that drug is to excite, and not to quiet, the motor function of the spinal cord: indeed, it is well known that the motor acts of the cord may be rendered much more active and powerful, by giving, before decapitation, opium to animals that are to be subjects of experiment." He considers "these objections, furnished by theory, to be motives sufficient for the future exclusion of opium from the treatment of tetanus."

I had long been aware that the effect of opium upon frogs was to produce tetanic spasms. But in no case of poisoning by opium in the human subject (and I have seen a great many) have I ever witnessed any approach to tetanus: and I much question the safety of arguing, in such matters, from what we know to happen in the inferior animals, to what we suppose would happen in man.

The failure, however, of opium in the severer forms of the malady, and its equivocal utility in any, taken together with these theoretical objections, prevent my *recommending* opium as a remedy for tetanus.

Blood-letting.—What is the result of experience in regard to blood-letting in tetanus? I am afraid that, as a curative agent, it has very little power over the disease. Yet it may be, and probably is, of considerable use, as an auxiliary to other measures. When the disorder bears any aspect of inflammation—when, for instance, fever is lighted up, and pain is felt along the course of the spine, or when the approach of the spasm is marked by the supervention or the increase of pain in the wound—our chance of doing good by venesection is the greatest. Some of the cases that happened in the Peninsular war, were decidedly benefited by blood-letting practised under such circumstances. I need scarcely say that though the bleeding, when adopted, should be early, free, and full, so as to produce some sensible impression upon the system, yet we must always use this remedy with caution. The tendency of the disease is to exhaust the power of the heart: and if by one over-bleeding we bring that organ to a stand-still, it may refuse to begin again.

In a complaint which depends so much on irritation, and so often on manifest irritation of external parts, we look naturally to the *warm bath* for help. And it has been fairly tried: and some persons have found it useful; and others have found it useless, doing neither good nor harm; and some have condemned it as actually hurtful.

The *cold bath* has been extolled as a much more powerful agent than the warm; and

so, doubtless, it is. But it is more potent for harm as well as for good. For example: a tetanic patient, in St. Thomas's Hospital, was plunged into a cold bath, at his own request. "All the symptoms disappeared (says Mr. Morgan) in a moment; and he was almost immediately taken out of the bath: but he was taken out lifeless." Sir James M'Grigor says that, during the campaign in Spain, "the warm bath gave only momentary relief; and the cold bath was worse than useless."

However, the application of cold water to the surface has, in many recorded instances, been of at least temporary benefit and comfort: and, in the West Indies, where the disease is common, the cold affusion still continues, I believe, to be the most favourite expedient. After it, the patient is rubbed dry, put to bed, and has laudanum administered. I have again to observe, of this remedy also, that it is chiefly serviceable in the idiopathic form of tetanus. It has been tried upon animals. Dr. Parry says that it was quite unavailing in the case of certain lambs that had the disease. In a note which I made at the time of Mr. Abernethy's lecture on Tetanus, I find the following statement. "The effect of cold in diminishing excessive muscular action was strikingly shown in the case of a horse belonging to Professor Coleman, which had tetanus. The animal was slung, and carried out of the stable, and laid on the snow, which was then on the ground: and he was covered over with snow also. A horse affected with tetanus is a curious sight. His legs straddle, and become stiff; his ears are pricked up; and his tail sticks out. In this case, on the application of the snow, his ears sunk, his tail became pliant, and the rigidity of his muscles was removed. He was again taken into the stable, and the spasms returned." Mr. Abernethy said, that were he himself the subject of tetanus, he would desire to have the cold affusion tried. If you are willing to assay the same remedy, do not plunge your patient into a cold bath, but take him out of his bed on an extended sheet, splash him well with cold water, wipe him dry, and place him in another dry bed. This will often, for a time at least, diminish the spasmodic action; and the patient will sometimes sleep comfortably after it.

LECTURE XXXIII.

TREATMENT OF TETANUS, CONTINUED. WINE; MERCURY; PURGATIVES; DIGITALIS; TOBACCO; MUSK; PRUSSIC ACID; BELLADONNA; CARBONATE OF IRON; OIL OF TURPENTINE; STRYCHNIA; SURGICAL EXPEDIENTS; GENERAL RULES. HYDROPHOBIA.

IN the last lecture we considered the symptoms, the nature, the causes, and to a certain extent, the treatment, of that terrible malady, *tetanus*. There is good reason for believing that it is essentially a disorder of the excito-motory apparatus; that it is caused by irritation of a peculiar kind, affecting that part of the nervous system, and producing tonic spasm of the voluntary muscles; that the irritating cause may be centric, situated within the spinal canal, and applied directly to the cord; or eccentric, situated out of the spinal canal, applied to some part of one or more of its afferent nerves directly, and thus influencing indirectly the cord itself, and through it the reflex motor nerves; and that a certain ill-understood state of the system is necessary, a certain aptitude to take on the disease, before the exciting cause can be efficient; and that one circumstance which has been ascertained to tend to the production of such an aptitude, is a long-continued high temperature of the atmosphere.

I mentioned several remedies and plans of treatment which have been fairly tried; and mostly tried in vain, for the removal of this disease. The severe cases, and especially the severe traumatic cases, almost all prove fatal; the less severe cases, those in which the paroxysms are less violent and less frequent, and which run on for several days, sometimes terminate in health: whether in consequence of the measures employed, or whether in spite of them, it is not easy to say. The idiopathic cases, as they are called, those

which appear to be produced by exposure to cold and wet, are usually the least severe, and the more hopeful. The remedies that have been tried, and which were mentioned in the last lecture, are opium; blood-letting; the warm bath, the cold bath. I showed you that, under each of these remedies, a great number of patients died, and some recovered; and that the recoveries had been almost exclusively among those patients in whom the disease appeared originally in its milder form. So that whether the complaint was actually cured in any of these cases, whether, *i. e.* any single patient recovered, or recovered sooner, from using any of these remedies, who would have died, or in whom the disease would have been protracted, if he had *not* used them, is a matter of uncertainty.

The celebrated American physician, Dr. Rush, regarding the disease as essentially a disease of debility, and looking probably at its common tendency to death by asthenia, wrote a paper to recommend the employment of bark, and wine, and spirits, in full doses. It is curious enough, but quite in agreement with what has been already stated of opium, that how much wine soever may be swallowed by the patient, nothing like intoxication is produced by it. The system resists the ordinary influence of the alcohol. In one instance related by Dr. Currie, the disease lasted six weeks, and in that space of time the patient drank 110 bottles of port wine. The same author mentions a remarkable case, in which a horse, which was attacked by tetanus, and happened to be a great favourite with its master, was treated with wine, and got well, after swallowing more port wine than he was worth. Whenever *this* plan has appeared to do good, it has been in the more chronic variety of the complaint.

Mercury, you may be sure, has not been left untried. It is said that the system is slow in submitting to its influence, in this malady. The specific effect of mercury upon the gums is not, however, so strongly resisted as that of wine or opium upon the nerves. Nor can we be surprised at this, when we consider that in tetanus the functions of organic life are, comparatively, but little involved. It is clear that there is not time for any effectual exhibition of mercury in those severe cases that are early fatal. In its more chronic form the disorder has been known to yield upon the mouth becoming affected. This happened, if I mistake not, in Mr. Mayo's patient, mentioned in the last lecture. Tetanus has sometimes, however, commenced while the patient was in a state of salivation. Dr. Wells has recorded three instances of that kind. The experience of the military surgeons who were in Spain is, upon the whole, against the reputed efficacy of mercury. We must take care not to conclude too hastily that because a patient uses a certain remedy and recovers, he recovers through the operation of that remedy: any more than we should conclude, if he recovered during a general election, that the election had cured him. Yet this absurd and unsafe mode of reasoning is for ever employed in respect to disease, by the public; and too often, I fear, by ourselves.

Purgatives have been much given in tetanus; and often with manifest advantage: I mean in the less severe cases. But very large doses are commonly required to produce evacuations from the bowels. Whether the torpor of the intestines be always the effect of the disease, or whether it may not sometimes be, in part at least, the consequence of the opium that is given, I am not sure. When they do act, very unnatural motions are frequently produced: Mr. Abernethy tells us of a hospital patient of his who recovered under the use of purgatives: they were long before they had any effect, and when they did at last operate, such fœtid stuff came from him that no one who could crawl out of the ward would remain in it. He says also that the nurses, in other cases, have reported the stools to be more like sloughs than fæces. Enormous quantities of drastic purgatives have been given. You may read an instance of this in the second volume of the *Medico-Chirurgical Transactions*. It is related by Mr. Harkness. There is a still more extraordinary case detailed by Dr. Briggs, in the fifth volume of the *Edinburgh Medical and Surgical Journal*. In little more than 48 hours, the patient in that case took 210 grains of scammony, 89 of gamboge, 80 of calomel, an ounce and four scruples of jalap, and 2½ pints of what we call *black dose*, the infusion and tincture of senna: and all this without either sickness or griping; but on the contrary with the most decided benefit. In the first week of his disease, the patient swallowed—of calomel, 280 grains; scammony, 260; gamboge, 126; jalap, 3 ounces and 10 grains; infusion of senna, 5⅚ pints. And altogether in the first 25 days—of calomel, 320 grains; scammony, 340; gamboge, 126; jalap,

5 ounces and 7½ drachms; infusion of senna, 10⅔ pints; besides an ounce and a half and 35 grains of the colocynth pill. I mention all this to show what the system will bear, under the bondage of the disease; not as an encouragement to you to prescribe such doses.

It is certainly proper and necessary to clear out the bowels, and to endeavour to correct unhealthy secretions; yet numerous evacuations, the act of going to stool often repeated, should be avoided. Under such obstinacy of the bowels, the croton oil would perhaps be the most eligible purgative.

Foxglove and *tobacco* are two medicines, or rather poisons, which have been used; both, probably, upon the same principle. Their effects, when full doses have been given, are much alike: sickness, faintness, feebleness, and fluttering of the pulse, coldness of the surface, with that slack and passive state of the muscles which belongs to syncope. But if we consider that the influence of these substances upon the involuntary muscles, especially upon the heart, is more certain and decided than upon the muscles of voluntary motion, which are the muscles involved in the tetanic spasm, and if we take also into account the strong disposition observable in tetanus towards death by asthenia, we shall scarcely be prepared to expect any good, but the contrary, from digitalis, or tobacco: especially in the later periods, when, so far from obviating the tendency to death, they would seem to co-operate with the disease in extinguishing life. However, if the result of experience were clearly in their favour, we should not be warranted, by mere theoretical views, in withholding these drugs. The army surgeons, some of them, have fancied digitalis useful. Sir James M'Grigor mentions a case in which it caused a relaxation of the spasms; but the man died afterwards, apparently from the effects of the remedy. And this is just what I find with digitalis. When given in large doses (and small ones here must be useless) it becomes unmanageable. Certainly we have no such accounts of its salutary power as would induce me to give it with much expectation of success, or to give it at all.

The tobacco is not given by the mouth, but thrown up into the rectum: either the smoke of its burning leaves, or (what is probably as efficacious, and I should think more uniform and less unsafe) an infusion of them in water. Mr. Curling, after analysing a large number of cases of tetanus, thinks tobacco the best remedy we at present possess. Mr. Travers is of the same opinion. However, I should recommend great caution in the use of this ticklish remedy. You ought to know that, when injected in other emergencies, in strangulated hernia, for example, mortal syncope has followed such enemata.

Musk, in large doses, has been strongly recommended by a Frenchman, Fournier-Pescay, who has written on this disease. He gave ten or twenty grains, at intervals; so that a drachm, or even two drachms, were taken in the course of the day; and he declares that he found it more efficacious than any thing else that he had tried.

Prussic acid and belladonna are said by Dr. Elliotson to have been freely employed, and to have failed: whether in his own hands, or in those of others, I do not know.

There is another remedy which the same physician has employed; and employed not without success: *the carbonate of iron*. Reflecting, he tells us, upon the good effect of this medicine in another complaint which has some points of analogy with tetanus, viz. chorea, of which I shall soon speak, and considering how miserably narcotics had failed, he determined to give the carbonate of iron a fair trial, upon the first opportunity. He has published some account of its effects, in tetanus, in the Medico-Chirurgical Transactions. In the first case in which he used it, the tetanus supervened upon a compound dislocation of the great toe. The method in which the remedy was administered was this: the carbonate was made into an electuary by mixing it with twice its weight of treacle. The electuary thus made was well mixed with beef-tea just as the patient was about to drink it. He took this every two hours, as much as he could swallow: and he got well. The next case is described as being a very severe one; it resulted from a contusion of the thumb. Dr. Elliotson says that he never saw a case *which did well*, that was more severe. This patient also took the carbonate of iron, as much as could be got down; and that was about two pounds a day. He had injections twice daily, to keep the bowels unloaded: and the iron is described as having come away in large red lumps, in shape like horse-dung. This man recovered. In a third case, where a chilblain above the heel

was the exciting cause, the boy died within 24 hours of the time when the remedy was first prescribed. To produce its effect upon the system (Dr. Elliotson observes, very truly) iron must be given *for a few days:* nay he holds that months sometimes elapse before it has any effect. So that if it really be useful in tetanus, we cannot expect much good from it in the more acute cases: and these are the cases for which we want a remedy.

Oil of turpentine is one of the many substances that have been praised as useful in tetanus. Now bearing in mind its power (which I shall hereafter describe, but which you must at present take for granted) as a worm-killer, and also the frequency with which worms are met with in the stomach and bowels after death by tetanus, this is one of the drugs which I should employ as a *purgative*, taking my chance of whatever good might possibly arise from its specific or anthelmintic qualities. It may be given in such cases either by the mouth or in an enema, or at both ends together: but it must be given in large doses, not less than an ounce at a time; and it may be mixed with an equal quantity of castor oil. The one oil dissolves or becomes incorporated in the other.

Strychnia has been suggested as a remedy for severe tetanus: not in infinitesimal doses, as Hahnemann would, I suppose, prescribe it, but in sufficient quantity to produce a sensible effect. The principle upon which this has been recommended is the same with that on which the nitrate of silver ointment is applied to the inflamed conjunctiva in purulent ophthalmia. We know that strychnia acts upon the spinal cord, affecting apparently those parts and those functions of the cord which are affected in tetanus: and in so fatal a malady, it would be justifiable, I conceive, to give the strychnia, in the hope that it might occasion a morbid action which would supersede the morbid action of the disease, and yet be less perilous and more manageable than it. But it would be right to try such a remedy as this, in the first instance, *in corpore vili;* upon one of the lower animals. This, were it successful, would be a cure, according to the Hahnemannic doctrine—similia similibus curantur —a doctrine much older, however, than Hahnemann. But the opposite maxim, contraria contrariis, has been suggested. Mr. Morgan proposes to give such poisons as are known to cause paralysis, with the view of countervailing the undue action of the muscles in tetanus. He produces artificial tetanus by inserting a poison brought from Java, called "chatic," into a wound, and then relieves the tetanic symptoms by a North American poison, the ticunas. Professor Sewell, of the Veterinary College, has tried this principle in one case at least, where the tetanus was the result, not of any poison, but of disease. Not having had an opportunity of getting the particulars of this case from Mr. Sewell himself, I give you Mr. Mayo's account of it. "A horse, suffering from a severe attack of tetanus and locked-jaw, the mouth being too firmly closed to admit the introduction of either food or medicine, was inoculated on the fleshy part of the shoulder with an arrow point coated with the wourali poison. In ten minutes apparent death was produced. Artificial respiration was immediately commenced, and kept up about four hours, when reanimation took place. The animal rose up, apparently perfectly recovered, and eagerly partook of corn and hay. He was unluckily too abundantly supplied with food during the night. The consequence was over-distension of the stomach, of which the animal died the following day, without, however, having the slightest recurrence of tetanic symptoms." I had fancied that the death had resulted from some injurious effect upon the lungs, produced by the artificial breathing. But I have little doubt that Mr. Mayo derived his statement from Mr. Sewell himself. The experiment deserves to be carefully repeated.

I have but little to say concerning what may be called the surgical treatment of traumatic tetanus. It was a natural thing, the source of the irritation being supposed to be the wound, to expect relief from amputation of the limb. But that will not arrest the morbid action after it has once been fairly established. Dr. Elliotson says he has searched scores of books, and found only one case in which the limb and the disease were lopped away together. However, Mr. Blizard Curling, in his *Essay on Tetanus*, refers to seven instances of recovery, after the injured part had been amputated. Yet he states that "it is almost impossible to ascertain with certainty how far the amputation, in these cases, was of service." I believe I cannot offer you better advice on this subject than may be gathered from the concluding remarks of a very distinguished and philosophical surgeon, in his lectures on the disease. I allude to the late Mr. Abernethy, whose pupil I had the good

fortune to be. He said, "The state of the part injured is not the sole cause of tetanus. In cases of tetanus I have often amputated injured fingers; and though I did not thereby save my patients, yet I think that the symptoms were mitigated after such amputations. In such cases then, I would not amputate any considerable member; nor even a small one, unless I thought that, from the injury sustained, it would prove useless to its possessor, even though the case should terminate favourably."

The tourniquet has been applied to the hurt limb; but not, so far as I know, with any good effect. The most promising expedient which surgery offers is the division of the principal nerve proceeding to, or rather from, the seat of the injury. This, supposing the nerve to be known, and accessible, is less formidable, less severe, less hazardous, less maiming, and, if we may judge from past experience, more effectual too, than amputation of the part. Dr. Murray has recorded (in the 11th volume of the MEDICAL GAZETTE) a very interesting case in which the operation was followed by most decided and instant relief. The patient was a young midshipman, who having trodden on a rusty nail, which pierced the sole of the left foot, had kept watch the same night upon deck, the weather being very cold. The disease began the next day, and the symptoms ran high. It was a case, therefore, of severe or acute tetanus. Without loss of time the posterior tibial nerve was divided. The limb was previously cold, and as the patient said, dead, and he had little power of moving it. He could not articulate distinctly, on account of the closed state of his jaws. The nerve was cut through by one stroke of the scalpel; and "immediately (says Dr. Murray) he opened his mouth with an exclamation; and on looking at his countenance I was astonished at the striking improvement in it. I asked him how he felt, and he said he was already much better, and that his leg had come to life again." Some stiffness of the jaws and neck remained for a day or two; but he soon recovered. Dr. Murray refers to another case mentioned by Baron Larrey, in which division of the nerve had a similar result.

Probably, to be successful, the operation must be *early;* before the morbid condition peculiar to the disorder has had time to root itself in the nervous system.

Although, in the present state of our knowledge, there is no one remedy or plan on which we can rely for the cure of this fearful malady, we may with much confidence lay down certain general rules, the observance of which will secure to the patient the best chance of a favourable result.

Since any the smallest movement, or impression made upon the surface, or upon the senses, will bring on the severer degrees of spasm, it is of primary importance to protect the patient against these sources of trouble, so sure to aggravate his sufferings, and so likely to augment his danger. Hence if blood-letting should be thought advisable, it should be done early, sufficiently, and once for all. There should be no repetition of venesection or of cupping, or of leeches, unless the circumstances and progress of the case plainly demand them. The same remark applies to the frequent use of purgatives. The bowels should be well cleared in the outset, and then let alone. The patient should lie in a darkened room; from which noise also should, as far as may be possible, excluded. He should not be surrounded by a multitude of friends or attendants. He should be enjoined to speak, to move, to swallow, as seldom as he can. In the severe traumatic cases, the nerve (in my judgment) should be promptly divided. And in all cases, there being no special indications to the contrary, I should be more inclined to administer wine and support, in large doses, than any particular drug. If the tendency to mortal asthenia can be staved off, the disturbance of the excito-motory apparatus may, perchance, subside or pass away.

There is a form of this complaint called *trismus nascentium*. As the name implies, it occurs in newly-born children. It is very frequent, and very fatal, in the West Indies; coming on usually in the second week after birth. Hence it has been called, "the ninth-day disease." Another of its names in the British settlements there, is the "jaw-fall;" from the circumstance that shortly before death the lower jaw, which had previously been firmly pressed against the upper, drops on the breast. It has been said that a fourth of the infant negroes in Jamaica used to die of this disorder. Some persons refer it to the irritation produced by the retention of the meconium in the intestines; others to irritation from the wound made by dividing the navel-string. A dose of purgative medicine appears to be the most hopeful remedy.

Tetanic symptoms sometimes occur (but I should think very rarely) in ague. Or paroxysms of tetanus return at regular intervals, and terminate by profuse perspiration; the patient being well during the intermission. When such phenomena arise, the treatment proper in severe forms of ague must be adopted: what that treatment is, I shall, in no long time, be able, I hope, to lay before you.

Again, tetanus is occasionally a symptom in hysteria: and then the treatment applicable to hysteria must be had recourse to; especially enemata of oil of turpentine, or the same medicine given by the mouth; and the cold affusion.

If the disease of which I have been speaking be dangerous, and very often fatal, in spite of all remedial measures, that which I propose to bring next under your attention is still more appalling; for I believe that, hitherto, it has been uniformly mortal. I know not that any one has ever been rescued by art, or saved by the efforts of nature, from *Hydrophobia*, after that frightful disease has once declared itself by its characteristic symptoms. The nature of those symptoms, and the absence of all constant or satisfactory traces of organic change in the dead body, sufficiently mark the disease as belonging essentially to the nervous system, and as being essentially a *spasmodic* disease also.

What are the symptoms, stated in broad outline? These:—Excessive nervous irritability and apprehension; spasmodic contractions of the muscles of the fauces, excited by various external influences, and especially by the sight or sound of liquids, and by attempts to swallow them; and extreme difficulty, amounting sometimes to impossibility, of drinking.

This is one of the diseases which are produced by animal poisons; and its course will be most conveniently traced if we include in our description of it the very first step towards the complaint—the application of the specific poison to the body. A man is bitten by a dog. After a time the symptoms proper to hydrophobia come on. After another interval the man is dead. Before we advert to the many very interesting points of inquiry which arise out of the contemplation of this malady, let us follow the tragedy from its commencement to its closing scene.

A person is bitten, then, by a mad dog. Does the existence of rabies in the animal modify in any way the injury thus inflicted? No; the wound that is made behaves just the same, to all appearance, as it would have behaved if the dog had not been rabid; and it gradually heals. After an uncertain interval—which lies, *for the most part*, between six weeks and eighteen months, and which has been called the period of *incubation*—these symptoms begin to be noticeable. The patient experiences pain, or some uneasy or unnatural sensation, in the situation of the bite. If it is healed up, the cicatrix tingles, or aches, or feels cold, or stiff, or numb; sometimes it becomes visibly red, swelled, or livid; on one occasion a papular eruption took place around it; sometimes it opens afresh, and discharges a peculiar ichor. The pain or uneasiness extends from the sore or scar towards the central parts of the body: *i. e.* if the bite has been inflicted on a limb, the morbid sensations extend towards the trunk. All this gives fearful notice of what is about to happen. This period is called the period of recrudescence. I believe it seldom fails to occur, although it sometimes is not noticed; the attention of the patient, and of his medical advisers, being absorbed by the horrible sequel. Very soon after this renewal of local irritation—within a few hours perhaps, but certainly within a very few days, during which the patient feels uncomfortable and ill—the specific constitutional symptoms begin: he is hurried and irritable; speaks of pain and stiffness, perhaps, about his neck and throat; unexpectedly he finds himself unable to swallow fluids, and every attempt to do so brings on a paroxysm of choking and sobbing, of a very distressful kind to behold; and this continues for two or three days, till the patient dies exhausted; in the way of asthenia.

I have seen only two examples of this terrific malady; one in St. Bartholomew's Hospital, in the year 1826; one much more recently in the Middlesex Hospital. As they constitute the whole of my personal experience in the matter, I shall relate these cases.

The first occurred in a coachman, the back of whose right hand had been struck, ten weeks previously, by the teeth of a terrier dog: but, as both the patient and his fellow servants declared, there was no wound made, no blood drawn, no breach or lifting of the

skin; but merely an indentation, showing where the animal's teeth had pressed. He was brought to the hospital on a Tuesday. On the preceding Thursday his hand had become painful, and swelled a little. On Friday the pain extended into the arm, and became more severe. His wife stated that he had been in the habit of sponging his head and body every morning with cold water, but that, on this morning, he refrained from doing so, on account of some feeling of spasm about the throat. His own remark on this was, that "he could not think how he could be so silly."

On Saturday the extent and the severity of the pain had still farther increased. On this and the preceding night he got no sleep. He felt ill and drowsy on the Sunday, but drove the carriage to Kensington Gardens: he was, however, obliged to hold both whip and reins in his left hand. The pain extended to the shoulder. He was then bled. A slop-basin full of blood was taken, with much relief to the pain; and purgative medicine was given, which operated well.

The next day he complained of "feeling ill all over," and he told his medical attendant that he could not take his draughts, because of the spasm in his throat. That gentleman (Mr. Macdonald), concealing his own suspicions as to the true nature of the disease, said, "Oh, you don't like the taste of your physic! drink some water." But he declared he had the same difficulty with water. The next day he came to the hospital. When there, water was brought and placed before him in a basin, for the alleged purpose of allowing him to wash his hands. It did not seem to disturb him, nor to excite any particular attention. Water was then offered him to drink, which he took, and carried to his mouth, but drew his head from it with a convulsive shudder. After this, on the same morning, he was much questioned by several persons about the supposed cause of his illness; and water was again brought him, which agitated him, and he became exceedingly distressed and unquiet, complaining of the air which blew upon him.

I first saw him myself soon after this. He was then, to all outward appearance, well; lying on his back, without spasm, without anxiety; his face was somewhat flushed. He said he had a little headache, but no pain in the arm. His pulse was 132, full, and strong; his tongue moist, and slightly furred. He appeared to be a very quiet good-tempered man; and smiled generally when he was spoken to.

I was naturally much interested in this case, and at nine in the evening I visited the patient again. He was composed and quiet. Gruel was mentioned, and then he sighed two or three times deeply; then sat up, and, after a moment's look of serious terror, took half a spoonful of the gruel in a hurried gasping manner; and said he would not take more at a time, lest *the sensation* should come on. He was desired to drink the last portion of the gruel from the basin. He accordingly seized it with hurry, carried it to his mouth with an air of determination, and then a violent choking spasm of the muscles about the throat ensued, the sterno-cleido-mastoidei starting strongly forwards. Most of the gruel was spilled over his chin; and he observed that he had been too much in a hurry, or he should have managed it.

The treatment consisted in full doses of opium, repeated at frequent intervals. On this visit to him I noticed that while attempting to take some of the gruel with a spoon, he seemed inclined to doze as he sat. Otherwise there were no signs of his being overwhelmed, or even sensibly affected by the opium; except that his general quietness might have been the consequence of it. He was quite calm and rational, except when attempting to take fluids.

On the Wednesday, at noon, he was nearly in the same state, but said he was better. In the course of the night some bits of ice had been given him: with considerable effort he swallowed two or three of these; the third or fourth caused so much spasm, however, that he was obliged to throw it out of his mouth: but so great was his resolution that he seized it again, and, by a strong exertion, succeeded in swallowing it. He complained now that his mouth was and had been clammy; and he champed much, and spat out a good deal of tough mucus. At his own request, and (as he said) that he might injure no one, a straight waistcoat was brought, which he assisted in putting on. But he was perfectly calm then.

I now had an opportunity of seeing him take some arrow-root. He sat up in bed to eat it; and before attempting to do so, he made hurried inspirations, and sobbings pre-

cisely resembling those which occur when one gradually wades into cold water. He took small quantities of arrow-root eight or nine times, with hurry and difficulty, and with sighs that succeeded each other rapidly. He said that he felt the upper part of his throat narrower than it should be. He continued to take laudanum mixed with sugar and bread into a kind of pulp.

By the evening of that day the disease had not made much farther progress. He again sat up and tried to take some thinnish gruel. While taking the basin into his hand, he drew back his head to a distance from it, apparently involuntarily. He took one half-spoonful with effort and difficulty, then sighed deeply and rapidly, or rather his breathing consisted of a succession of sighs at short intervals: he gave up the basin, and sank back on his pillow still sighing. In the course of that night he ceased to take the laudanum; he could no longer attempt it. The next day he was still composed, though more easily irritated; and it was found that he had lost the power of moving the left arm. His pulse was 140, and much weaker than before, and his mental powers were failing. He gradually sank, and died in the evening, having repeated the Lord's Prayer an hour previously. During the last hours of life he had been moaning, and tossing from side to side: his bowels were purged; fluid stools ran from him, and distressed him greatly. His lower extremities first became cold, and the coldness extended by degrees up to his chest. He hawked up in the course of the day a considerable quantity of ropy mucus, and much frothy saliva came from his mouth towards the close. As his wife was wiping this away, whether by convulsive accident or otherwise, his teeth came in contact with her finger, and drew blood. The part was cut out; and no bad consequence followed that I know of.

The examination of the body threw no satisfactory light upon the essential nature of the disease. Blood and serous fluid escaped on the removal of the calvarium. The vessels of the membranes were full, and the brain itself was somewhat mottled by its vascularity. There were a few spots of ecchymosis on the heart. The back part of the tongue was very vascular. The stomach presented the most notable appearance. There was a quantity of brownish-coloured mucus on its inner surface, and the mucous membrane had disappeared from a space about four inches in diameter at its larger extremity. That space alone was diaphanous; its edges sloped inwards; and a segment of this thin place looked exactly like a piece of china: on a white ground there were inosculating vessels, some of them blue, and some of them of a coffee-coloured brown. I conclude that this appearance was produced by the action of the gastric juice after death.

This was in some respects a remarkable case. It was remarkable for its duration. Dr. Bardsley, in the article on Hydrophobia in the *Cyclopædia of Practical Medicine*, states that the patients "invariably go on from bad to worse, and finally die before the sixth day." Now if we reckon that stage of the complaint here referred to by Dr. Barlow to have begun on the morning of Friday, when he was obliged to omit his sponging because of the spasm about his throat, this patient did not die till the middle of the seventh day. In fact it was a very protracted case; and the symptoms were less violent than usual; whether this was owing to the opium he took or not, it would be difficult to determine.

In the second of the two cases which it has been my lot to witness, the characteristic symptoms of hydrophobia were more faintly pronounced than is usual.

On going to the Middlesex Hospital on Thursday, the 5th of October, 1837, I was told that a patient had been admitted (under one of my colleagues) labouring probably under hydrophobia. He had applied at the hospital in the middle of the night; but was then sent away, after receiving some aperient pills, with assurances that he was only feverish and nervous. On his reapplication in the morning he had been admitted.

I found him in the ward: a man twenty-five years old, of dark complexion and hair. He expressed his conviction that he was afflicted with hydrophobia; and said he was prepared for his fate. I observed that every now and then he suddenly sighed in a very peculiar manner; just as I had seen the former patient sigh. This would happen sometimes in the middle of a sentence, while he was speaking. He told us he had been bitten by a dog in the latter end of July; the dog was swimming, and like to drown, in a canal, and upon his reaching over to lift him out of the water, the animal seized upon his hand. After dragging the dog out, he beat him for his ingratitude; and then the dog ran off, and was pursued by a mob of boys who had previously been pelting him as a mad dog. There

was a scar on the middle finger of the right hand; the nail of that finger had (he said) been torn through, and each of the two adjacent fingers had been more slightly bitten. His pulse was 84; but varied in frequency at short intervals.

He acknowledged that after receiving the bite he was uneasy as to its possible effects, and read books about hydrophobia at the time: but he affirmed that he had afterwards ceased entirely to think about it. He had persuaded himself that the dog could not be mad, from its being in the water. On Tuesday, if not earlier, he had been uncomfortable and restless; and on Wednesday he found he could not swallow liquids. On one of these days he experienced a slight pricking sensation, without any redness or tenderness, in the site of the scar; his right arm and leg seemed to himself hotter than the opposite limbs; and the arm, though not tender, felt raw, and he could not bear the light contact of his clothes upon it. He became feverish also. From time to time a slight expression of terror passed across his features, and then he made a sudden, deep, sighing, inspiration: at other times his breathing and appearance were perfectly natural. It was said that when some water was brought him he drew himself back from it with horror. He talked a good deal.

I saw him eat rice, made pulpy with milk. He took it without looking at the spoon, from which he averted his eyes, and ate several mouthfuls, in a gulping manner, and with evident effort. His bowels had been purged by the pills, and he declared that the noise of the water in the water-closet had distressed him. The sound of some water poured from one vessel into another by the patient in the next bed, had also agitated him. So did the contact of my cold hand on his arm; and currents of air, even the breath of any one speaking to him; so that he insisted on conversing with the apothecary in such a position that the chin of each was on the other's shoulder. But there was no actual or apparent spasm.

At this time he affirmed that the presence of company cheered him, and did him good; and begged that he might not be removed into a separate room. And he wished for some amusing book that he might read.

In the evening I again went to see him. He did not seem worse, though he said "his symptoms were increasing." He had taken a dose of musk, and some morphia.

The next day I found the hospital in some confusion. Between eleven and twelve o'clock in the preceding night some of the officers of the hospital had gone to his bed, while he was apparently asleep, and certainly very quiet. They asked him if he would like some water. This seems to have greatly excited him; and immediately after their departure he rushed out of bed, (terrified, he said,) became furious and unmanageable, and was never again quiet till he died, about the same time the next night. He was now put into a room by himself; and, taking advantage of the momentary absence of the nurse, he bolted himself in alone; and declared he would admit no person but her. The door was at length forced, and a straight-waistcoat was put on him. He then became quieter in his manner; begged that no unnecessary violence might be used; asked to be poisoned: spat at some of the bystanders, and reproached them, talking rapidly and wildly like an insane person; yet loudly and angrily imposing silence on every one who addressed him. He said he could not bear to hear any one speak; that he did not like my bass voice. Then he would sneer at the students, and say they showed bravery enough now he was confined: "was it right for young gentlemen of education to stand there gazing with curiosity on a dying man?" asked for bread soaked in water, and when it was held towards him, snatched it in his mouth in a savage manner; spoke of his "poisoned tooth," and talked perpetually. He took a fancy to one of the students, and begged that he might remain with him.

About this time he vomited some yellow fluid, and thought he felt the better for it, and asked for an emetic; and some tartarized antimony was exhibited. He was now pale, and his lips were livid; but none of the distinctive spasmodic attacks occurred: indeed water was not at this time suffered to be brought near him. This circumstance it was, this absence of the peculiar spasmodic paroxysms which characterize hydrophobia, that induced several medical men of much sagacity and experience to doubt, and even with some positiveness to deny, that the patient was suffering under that disease at all. They supposed him to be hysterical, half-crazy, or on the brink of delirium tremens. But though slightly

expressed, the symptoms were unlike any thing I had ever seen, except in the previous instance. And the closing scene was quite distinctive.

It appeared, and he spoke of it as a thing which distressed him, that when he was most excited, his urine passed involuntarily.

In the evening I found his father with him. He had recognized him, and kissed his mother-in-law; but soon began again to rave, and to be apparently occupied with absent persons. He was pale and weak, and lay with his head over the bed, spitting continually upon the floor, which was thus made quite wet. He wished to have his hands at liberty that he might "clear his mouth." He was soliloquizing when I went into the room, in this way. "Monsters—monsters—see that monster Susan—take her away." (It appeared that he was now speaking of a young woman who had had a child by him.) "I thought they would do much for science, but never supposed they would inflict such agony as this;" and so on.

A little later Mr. Arnott visited him. He had then no pulse at the wrist. The waistcoat was removed. He sat up, and used some water, brought to wash his hands, without apparent distress. Soon after he sunk back exhausted; and expired.

His father corroborated what the patient had said of the dog: and told us his son was clever, and better educated than many of his rank (he was a tailor), and always exceedingly nervous.

The body was examined the next day. Its posterior and undermost surface was very livid. The blood everywhere quite fluid. The veins of the spinal cord, on its posterior part, were turgid; not at all so on the anterior. The substance of the cord was quite natural. There was some fluid in the theca. The brain appeared to me, in every part, quite sound and healthy.

The head and face, which had been hanging over the table while the spinal canal was opened from behind, were deeply purple, as though universally bruised. This colour diminished rapidly after the corpse was placed supine, and the head raised somewhat above the level of the body. The papillæ at the back part of the tongue were greatly exaggerated, and looked like large vesicles. The cartilage of the epiglottis, at its lower part, was red. At about the middle portion of the œsophagus there was an appearance as if the cuticle was abraded. The mucous membrane of the stomach was soft, and red here and there, with a dotted injection resembling ecchymosis, especially on its rugæ. The air-passages were apparently healthy.

Generally, the disease, when it has once set in, and shown the peculiar hydrophobic symptoms, runs a short and fierce course. The nervous irritability becomes extreme; the peculiar paroxysms of choking spasm, and sobbing, are excited, not only by attempts to swallow liquids, but by the very sight or sound of them; Dr. Elliotson mentions a boy who was thrown into a violent state of agitation by hearing a dresser who sat up with him make water: the passage of a gust of wind across his face, the waving of a polished surface, as of a mirror, before his eyes, the crawling of an insect over his skin, is often sufficient to excite great irritation, and the peculiar strangling sensation about the fauces, in a hydrophobic patient. These circumstances were but little observable in the men whose cases I have related; the first of them indeed was remarkably calm and tranquil under the disease. In general the patient is dreadfully irritable, and apprehensive and suspicious: and in most cases there is a degree of mania or delirium mixed up with the irritability; the sufferer is very garrulous and excited. In this respect there is a marked difference between hydrophobia and tetanus. In the latter disorder the mental faculties are clear, and the patients serene, and what is called heartwhole, to the last. The two diseases differ in another striking particular: the spasm in the one case is tonic, in the other clonic. In tetanus again, there is no thirst, and seldom any accumulation of tough and stringy mucus in the fauces and about the angles of the mouth: in hydrophobia both these symptoms are almost always present. So also is vomiting; but vomiting in tetanus is rare. The nervous irritability in hydrophobia is doubtless a part of the disease, and is very rarely absent even now-a-days. Some time ago it might perhaps have been plausibly attributed to the treatment adopted. I allude to that period in which it was believed that these miserable persons had both the power, and the inclination, to impart the disease to

others by biting them; and when, under pretence of shortening *his* sufferings, but really, I am afraid, with the cowardly view of protecting *themselves*, his friends were accustomed to smother the unhappy patient between two feather beds, or to open a vein, and to leave him to bleed to death. Any person, suspecting what was the matter, and foreseeing such a termination to his disease, might well be nervous and irritable. But now that this barbarous practice has been exploded, and the dread of being smothered does not occur to the mind of the patient, he is still found to be exquisitely irritable and timorous. The foam and sticky mucus that gather in the throat and mouth, these patients make great efforts, by spitting and blowing, to get rid of; and the sounds they thus produce have been exaggerated, by ignorance and credulity, into the barking and foaming of a dog. In the same way the paraplegia which sometimes takes place, rendering the patient unable to stand upright, has been misconstrued into a desire on his part to go on all fours like a dog. The pulse, though it may be strong and hard at the outset, becomes, in a short time, frequent and feeble, and the general strength declines with great rapidity. Death occasionally takes place within twenty-four hours after the commencement of the specific symptoms. Most commonly of all it happens on the second or third day; now and then it is postponed to the fifth day; and in still rarer instances, of which my first case was one, death does not occur till the seventh, or eighth, or ninth day. In most cases, the paroxysms, becoming more violent and frequent, exhaust the patient; but in a few instances the symptoms undergo a marked alteration before death. The paroxysms cease, the nervous irritability disappears, the patient is able to eat and drink, and converse with ease; those sights and sounds which so annoyed and distressed him before, no longer cause him any disquiet. In this state he often sinks into a sleep, and suddenly wakes from it to die: sometimes his existence is put an end to by a sudden and violent convulsion.

It is needless for me to go into a minute account of the morbid appearances that have been met with in persons dead of hydrophobia. They are various, uncertain, unsatisfactory. In some bodies, the most careful examination has discovered nothing amiss. In others, vascularity of the brain, or of the spinal cord, has been noticed. And in not a few instances the mucous membrane of the fauces, œsophagus, and stomach—or of the larynx and trachea—or of both these tracts—has been found red, and covered with adhesive mucus. But we must take care not to attribute undue importance to these last appearances—not to conclude that they have been the cause of the symptoms, when in truth they may have been the effect of the disease. That we should find the parts in the throat red and congested is what we might naturally expect, when we consider the violent straining spasmodic action of these parts for some time before death. The morbid anatomy of this disease throws but little light upon its nature, or its proper treatment.

There are many very interesting questions connected with hydrophobia. I will state the principal of these, as shortly as I can.

1. You will be surprised when I tell you that some persons have made it a question whether there is any such disease at all. I have known such. The late Sir Isaac Pennington, who was Regius Professor of Physic at Cambridge, had never seen a case of hydrophobia, and nothing could persuade him that any one else had seen any thing more than a nervous complaint produced by the alarmed imagination of the patient, who having been bitten by a dog reputed to be mad, and having the fear of feather-beds before his eyes, was frightened into a belief that he had hydrophobia, and ultimately scared out of his very existence. Now if you meet with such incredulous persons, and think it worth your while to argue the point with them, you may object to their unbelief, the improbability that so many persons who have been bitten by mad dogs should have suffered so precisely the same train of symptoms, and at last have died, from the mere force of a morbid imagination; you may urge them with the fact that many of these persons have been under no apprehension at all, until the disease has seized upon them; that many also have been men of natural strong and firm minds, not at all likely to be frightened into believing that they were seriously ill unless they really were so, and still less likely to be terrified into their graves. And if this has no weight with such reasoners, you may bring forward the conclusive facts that the disease has befallen infants and idiots, who had never heard or understood a word about mad dogs or hydrophobia, and in whom the

imagination could have had no power in calling forth the complaint. And if they are proof against this, you must give them up: I can suggest nothing more.

2. Allowing that the disease exists as a real, and not a merely imaginary disease, and also that it is caused by the bite of a rabid animal: this important question arises—has it any *other* cause?

Setting aside that quibbling application of the term hydrophobia which some writers have chosen to make, to diseases in which, from some painful affection of the throat, the patients have been unwilling to attempt to swallow fluids, there are cases recorded, exactly resembling hydrophobia in their symptoms, and occurring in persons who were never known to have been bitten by, or ever to have been in the presence of, a rabid animal. The celebrated and accurate Pinel has given the history of such a case. There is another by Savirotte, in the Journal des Savans (August 1757). Now it is just possible that this disease may sometimes develope itself in the human body without any contagion having been applied: and it is possible, and much more probable, in my judgment, that the poison may have been applied without the persons being aware of it. We shall see, by and by, some very possible ways in which that might happen. All that we need concern ourselves with practically, is this—that in 999 cases out of 1000 the disease in the human body is derived from a rabid animal. If it ever be spontaneous, we cannot reckon upon meeting with such a case: indeed, many men pass through life without witnessing the disorder at all.

3. Granting, then, that the disease, in man, is the result of an animal poison, the next question is, from what animals may he receive the infection?

We are sure that the disease, by the inoculation of which hydrophobia may be produced in man, is common in the *dog*, and that it has been communicated to the human animal by the fox also, the wolf, the jackal, and the cat. Mr. Youatt says that the saliva of the badger, the horse, the human being, have undoubtedly produced rabies, and some affirm that it has been propagated even by the hen and the duck. The same author mentions a case in which a groom became affected with hydrophobia from a scratch which he received from a horse that was labouring under the disease. All animals, even fowls, are susceptible of the disease when bitten by the rabid dog. Of course it is an important question to have resolved, whether the saliva of all these is capable of conveying the malady. The case just now mentioned on Mr. Youatt's authority would seem to settle the question as respects the horse: but as horses, cows, turkeys, &c. do not generally bite, we have not many opportunities of supplying a positive answer to the general question: there can be no doubt about the *cat*, the *fox*, the *wolf*, and the *jackal*.

The late Duke of Richmond died, abroad, of hydrophobia, contracted from a tame fox, which was rabid. In the 13th volume of the *Medico-Chirurgical Transactions*, an account is given, by Mr. Hewitt, of several cases of fatal hydrophobia from the bite of a wild and rabid jackal. Many examples are on record of the production of the disease by the bites of mad cats and wolves.

The first case which I have spoken of, as having been seen by myself, would seem to prove, if all the facts were correctly stated at the time, that the saliva of the dog may be sufficient to produce the disease, when it is merely applied to the unbroken skin. It was affirmed by various persons that the teeth of the terrier did not break the cuticle. But we must take care not to draw a hasty general inference from a single case. Mr. Youatt, who has seen more of the disease probably both in man and in other animals than any other person alive, does not think that the saliva of a rabid animal can communicate the disorder through the unbroken cuticle: he believes that there must be some abrasion or breach of surface. He holds, however, that it may be communicated by mere contact with the mucous membrane.

Of its harmlessness on the sound integument, he offers this presumption—that his own hands have many times, with perfect impunity, been covered with the saliva of the mad dog. He mentions some singular instances in which the disease has been transmitted by contact of the saliva with the mucous membranes. "A man endeavoured to untie with his teeth a knot that had been firmly drawn in a cord. Eight weeks afterwards he perished, undeniably rabid. It was then recollected that with this cord a mad dog had been confined. A woman was attacked by a rabid dog, and escaped with the laceration

of her gown. In the act of mending it, she thoughtlessly pressed down the seam with her teeth. She died." If these cases be authentic, they are conclusive of this question; unless indeed the lips of those who perished happened to have been chopped or abraded. But Mr. Youatt's own opinion is that the virus cannot be received on a mucous surface without imminent danger.

The disease is said to have been caused by the *scratch* of a cat. But as we know that cats as well as dogs frequently apply their paws to their mouths, especially when the latter part is uneasy (as it clearly is in mad dogs), this fact, of the production of the disease by a scratch, if thoroughly made out, would not *prove* that the disease can be introduced into the system in any other way than by means of the slaver.

LECTURE XXXIV.

HYDROPHOBIA, CONCLUDED. VARIOUS QUESTIONS CONSIDERED RESPECTING THE DISEASE AS IT APPEARS IN THE HUMAN SUBJECT, AND RESPECTING RABIES IN THE DOG. PATHOLOGY OF THE DISORDER. TREATMENT. PREVENTIVE MEASURES.

AFTER giving you some account of the phenomena of *hydrophobia*, or *rabies canina*, I began to notice, in the last lecture, the chief of the interesting questions which naturally present themselves to the minds of most men, and especially of medical men, in respect to that shocking disorder.

In the first place, there *is* such a disorder. It appears, too, secondly, from statements made upon credible authority, that the same group and succession of symptoms as characterize the disease, when it is produced by the bite of a rabid animal, have been observed to occur in persons who were never known to have been bitten. My own opinion is, that it is more probable that these persons had been exposed to the virus without being aware of it, than that the disease was spontaneously engendered in their bodies. I would make the same remark in regard to an instance which is said to have happened of hydrophobia in a lad who had been bitten five weeks before by a *healthy* dog: the dog remaining well at the time of his seizure and death. Mr. Youatt holds, indeed, that however the disease originated, it never occurs now, not even in the dog, except as a consequence of the application of the specific contagion. It is certain, in the third place, that (besides the dog) the wolf, the fox, the jackal, and the cat, have communicated the disorder to the human animal. Mr. Youatt affirms, that the saliva of the badger, of the horse, and of the human being, has caused rabies; and I mentioned, on his authority, a case in which a groom contracted the disease from a scratch which he received while administering a ball to a rabid horse. But I feel much less certain about these latter animals. Respecting the dog, the fox, the wolf, the jackal, the cat, there can be no question. The result of certain experiments made at the Veterinary School, at Alfort, is opposed to Mr. Youatt's statement. Professor Dupuy made wounds in cows and sheep, and rubbed upon those wounds sponges which had been chewed by rabid animals of the same species; but he never succeeded in communicating the disorder in this way: but when he used a sponge that had been mumbled by a mad dog, then the disease occurred in the sheep and cows.

It is still more interesting to inquire, whether the saliva of a human being, labouring under hydrophobia, is capable of inoculating another human being with the same complaint? Mr. Youatt says yes: that the disease has undoubtedly been so produced. If this be so, the fact will teach us—not to desert or neglect these unhappy patients, still less to murder them by smothering—but to minister to their wants with certain precautions; so as not to suffer their saliva to come in contact with any sore or abraded surface; nor, if it can be avoided, with any mucous surface. On the other hand, all carefulness of that kind will be unnecessary, if the disease cannot be propagated by the human saliva. Certainly many experimenters have tried in vain to inoculate dogs with the spittle of a hydrophobic

man: but there is one authentic experiment on record, which makes it too probable, that the disease, though it may not be communi*cated* often, or easily, is yet communi*cable*. The experiment is said to have been made by MM. Magendie and Breschet, at the Hôtel-Dieu, and to have been witnessed by a great number of medical men and students. Two healthy dogs were inoculated, on the 19th of June, 1813, with the saliva of a patient, named Surlu, who died of hydrophobia the same day, in that hospital. One of these dogs became mad on the 27th of the following month. They caused this dog to bite others, which, in their turn, became rabid also; and in this way they propagated the malady, among dogs, during the whole summer. Now this is a very striking fact, yet it ought not to be considered conclusive: for it is possible that the dog might have gone mad at that time, whether he had been so inoculated or not. It may have been a mere coincidence. We want repetitions of such experiments to *settle* the point: nevertheless we have enough in this one experiment to make us use all necessary caution when engaged in attending upon a hydrophobic patient.

I just touched upon the question, whether the saliva of a rabid dog could produce the disease if it fell upon the *sound skin?* The first of the two cases which I related as having been witnessed by myself, would appear to give an affirmative answer to this question. Mr. Youatt thinks the disease would *not* follow such an application of the virus; but that it cannot be received upon even the unbroken surface of a mucous membrane without the greatest danger. Horses are said to have died mad after eating straw upon which rabid pigs had died. Portal was assured that two dogs, which had licked the mouth of another dog that was rabid, were attacked with rabies seven or eight days afterwards. Mr. Gillman, of Highgate, in a little pamphlet on hydrophobia, quotes an instance from Dr. Perceval, in which a rabid dog licked the face of a sleeping man, near his mouth, and the man died of hydrophobia, although the strictect search failed to discover the smallest scratch or abrasion on any part of his skin.

At the very close of the lecture I observed, that even should it be clearly proved that hydrophobia has ever resulted from the scratch of a rabid animal's *claws*—the claws of a cat, for example—we are not to set it down as a sure thing that the disease can be introduced into the system independently of the saliva of the diseased animal. As we know that dogs and cats are in the habit of putting their paws to their mouths when they feel uneasy there, we may readily understand how the poisonous saliva may be introduced by a mere scratch with the creature's nails. Mr. Youatt believes that the saliva *only* is capable of conveying the disease.

4. Supposing the virus to have been inserted in the part bitten, what becomes of it? Is it immediately taken into the system, and does it, like the poison of small-pox, in some mysterious way, multiply and diffuse itself in the body, until the disease explodes? Or does it remain imprisoned in the wound, or in the cicatrix, for a time? This is an important practical question. For if the poison lurks for some weeks in the place where it was originally deposited, we might successfully remove it at any time between the infliction of the bite and the period of recrudescence. Now the facts that at this period of recrudescence the wound or scar is re-inflamed often, and almost always becomes the seat of some fresh morbid phenomena, pain, swelling, numbness, and the like, spreading towards the trunk—and that, *soon after this*, the peculiar paroxysmal symptoms begin—these facts are strong in favour of the belief that the poison does lie inert in the place of the original hurt, for some time. Dr. Bardsley states that the recrudescent pains seem always to follow the course of the nerves, and do certainly never inflame or irritate the lymphatic glands in the vicinity, though passing in a parallel course towards the trunk. He affirms the entire absence of any fact contrary to this observation in the works of the numerous authors who have written on the subject. I mention the statement, because it certainly is not correct. Mr. Mayo says, "in one case which I witnessed and examined after death, the inner part of the cicatrix was bloodshot; and a gland in the axilla had swelled at the coming on of the hydrophobic symptoms." And I find, among my notes of Mr. Abernethy's lectures, another striking case, still more to the point. "A very intelligent boy had been bitten by a dog in the finger: he was brought into St. Bartholomew's Hospital. Caustic had been liberally used, affecting the sinewy parts, and producing a terrible sore: yet the boy was recovering himself, and the sore was healing. One day, as Mr. Abernethy was going round

the hospital, he saw and spoke to the boy, who said he thought himself getting well, but that he had that day an odd sensation in his fingers, stretching upwards into his hand and arm. Going up the arm, Mr. Abernethy saw two red lines, like inflamed absorbents: they doubtless were so. He affected to make light of the matter, ordered a poultice, and recommended the boy to take some medicine. Early the next morning Mr. Abernethy visited the ward, pretending he had some other patient there, whom he wished particularly to see: and when going out again, he asked the boy, carelessly, how he was. He said that he had lost the pain, but that he was very unwell, and had not slept all night. Mr. Abernethy felt his pulse, told him he was a little feverish, as might be expected, and asked him if he were not thirsty, and would like some toast and water. The boy said he *was* thirsty, and that he *should* like some drink: when, however, the cup was brought, he pushed it from him; he could not drink. In forty-eight hours he was dead.

Facts such as these would lead to the conclusion that, in cases in which excision had not been performed in the first instance, the scar, or the sore, might be cut out with propriety at any time before the period of recrudescence: and if the case happened to be my own, I would have this done even *at* that period, the moment any new sensation manifested itself in the seat of the injury. Mr. Mayo, on the same grounds, advocates the removal of the cicatrix, even although the hydrophobic symptoms may have appeared. I do not mean to say that the facts, now referred to, show with any certainty that the poison remains in the place where it was first deposited until the phenomena of recrudescence take place; but they afford some presumption in favour of that notion: and, in such a disease as hydrophobia, we are bound to act upon the very lowest presumption that affords a chance for our patient's life. The poison may be absorbed into the general system at the period of recrudescence, although no affection of the absorbing vessels or glands should be manifest: through the veins, namely.

Considering the matter philosophically, we might be inclined to suppose that the poison was silently maturing its force in the general system during the period of incubation, just as the poison of small-pox and measles are presumed to do. But looking at it practically, I should recommend, under the circumstances already stated, the excision of the cicatrix.

5. Another important question is this. Is a man who has been bitten by a mad dog, and in whose case no precautions have been taken, a doomed man? will he be sure to have the disease, and therefore to die of it? By no means. But few, upon the whole, of those who are bitten, become affected with hydrophobia.

It is curious that different species of animals appear to be susceptible of hydrophobia in different degrees. Thus, according to Mr. Youatt, two dogs out of three, bitten by one that is rabid, become rabid. The majority of horses inoculated with the virus, perish. Cattle have a better chance: perhaps because in them the skin is looser and less easily penetrated. A full half (he thinks) of those that were seized by a mad dog, would escape. With sheep the bite is still less dangerous. He reckons that not more than one in three would be affected. The tooth, perhaps, has been wiped clean in its passage through the wool. The human being is least of all in danger. John Hunter states that he knew an instance in which, of twenty-one persons bitten, one alone became affected with hydrophobia. Dr. Hamilton estimates the proportion to be one in twenty-five. But I fear these computations are much too low. In 1780, a mad dog, in the neighbourhood of Senlis, took his course within a small circle, and bit fifteen persons before he was killed: three of these died of hydrophobia. The slaver of a rabid wolf would seem to be highly virulent and effective. These beasts fly always, I believe, at a naked part. Hence, probably, the fatality of their bites. The following statement applies exclusively to the wolf. In December 1774, twenty persons were bitten in the neighbourhood of Troyes; nine of them died. Of seventeen persons similarly bitten in 1784, near Brive, ten died rabid. In May 1817, twenty-three persons were bitten, and fourteen perished. Four died out of eleven that were bitten near Dijon: and eighteen of twenty-four bitten near Rochelle. At Barsur-Ornain, nineteen were bitten, of whom twelve died of hydrophobia within two months. Here we have one hundred and fourteen persons bitten by rabid wolves, and among them no less than sixty-seven victims; considerably more than one half. There is no doubt, however, that a majority of persons who are bitten by a mad dog escape the disease. This

may partly be owing to an inherent inaptitude for accepting it. We see some persons who, though often in the way of it, do not contract syphilis; there are others upon whom the contagion of small-pox has no influence. This difference exists, apparently, even among dogs. There was one dog, at Charenton, that did not become rabid after being bitten by a rabid dog; and it was so managed that, at different times, he was bitten by thirty different mad dogs; but he outlived it all. Much will depend also upon the circumstances and manner in which the bite is inflicted; if it be made through clothes, and especially through thick wollen garments, or through leather, the saliva may be wiped clean away from the tooth before it reaches the flesh. In the fifth volume of the *Edinburgh Medical and Surgical Journal*, there is a case described by Mr. Oldknow, of Nottingham, in which a man was bitten in three different places by the same dog; viz. in the scrotum, the thigh, and the left hand; the bite on the hand was the last. Now it seems not improbable that but for this last bite, on a naked part, he might have escaped. At least it was a remarkable circumstance that the phenomena of recrudescence occurred only in the hand and arm. The dog is supposed to have closed his mouth after inflicting the first two bites; and thus to have charged his teeth afresh with the poisonous saliva.

It is this frequent immunity from the disease in persons who have been bitten, that has tended to confer reputation upon so many vaunted methods of prevention. Ignorant persons, and knavish persons, have not failed to take advantage of this. They announce that they are in possession of some secret remedy which will prevent the virus from operating: they persuade the friends of those who die that the remedy was not rightly employed, or not resorted to sufficiently early: and they persuade those who escape that they escape by virtue of the preventive remedy. If the plunder they reap from the foolish and the frightened was all, it would be of less consequence; but unfortunately the hope of security without undergoing a painful operation leads many to neglect the only sure mode of obtaining safety.

Mr. Youatt is of opinion that the power of the virus ceases with the life of the animal. He states, that in many dissections of the dog, the saliva, in spite of all care, must have come in abundant contact with his hands, and they were not always sound. I should strongly recommend you not to act upon this opinion: but to use the same precautions, in dissecting a rabid animal, as you would use if you were persuaded that the disease might be communicated with equal certainty before and after the death of the animal.

There are some considerations respecting this disease, which relate both to the biter and to the bitten; the canine and the human being. And there are some which relate exclusively to the dog, yet concerning which we, as medical philosophers, ought not to be ignorant. I shall advert to a few of these.

One question I have already glanced at; viz. whether the disease may be produced by a healthy, though angry dog or cat. I referred to one instance in which this was supposed to have been the case; and I repeat that I should be more inclined to think, unless we had other examples of the same kind, that the person had been inoculated in some way that he was not aware of. But I have heard Mr. Youatt describe cases in which there had been no symptoms of rabies observed in the dog at the time the injury was inflicted, though soon afterwards the animal became decidedly rabid. It is much to be regretted that the dog is so often destroyed. When a person has been bitten by a dog or cat suspected to be rabid, the beast ought to be secured, and kept under surveillance, and suffered, if it shall so happen, to die of his disease. If he does not die, in other words if he be really not rabid, that will soon appear; and the mind of the patient will then be relieved from a very painful state of suspense and uncertainty, which might otherwise have haunted him for months or years. If the dog dies mad, the injured person will be no worse off than if the animal had been killed in the first instance: nay in one respect he will be better off, inasmuch as certainty of evil is preferable to perpetual and uneasy doubt. "Give a dog a bad name (says the proverb), and hang him:" and it is literally so with the imputation of madness. A poor wretch of a dog is perhaps ill, or weary, or cross, or he may have been worried already by mischievous boys: the cry of mad dog is raised; and then he can expect no mercy. There are gross errors prevalent with regard to the signs of madness in the dog. If a dog be seen in a fit in the street, some person charitably offers a conjecture that perhaps he may be mad; the next person has no doubt about it; and then, woe to

that dog! But Mr. Youatt assures us that the rabid dog never has fits: that the existence of epilepsy is a clear proof that there is no rabies. Again, it is a very common belief that a rabid dog, like a hydrophobic man, will shun water; and if he takes to a river, that is thought to be conclusive evidence that he is not mad. But the truth is, that the disease, in the quadruped, cannot be called *hydrophobia:* there is no dread of water, but an unquenchable thirst; no spasm attending the effort to swallow, but sometimes in dogs an inability to swallow, from paralysis of the muscles about the jaws and throat. They will stand lap, lapping, without getting any of the liquid down. They fly eagerly to the water, and Mr. Youatt states that all other quadrupeds, with perhaps an occasional exception in the horse, drink with ease, and with increased avidity. This erroneous impression is not confined to the vulgar. In the case which I have more than once alluded to, and which is mentioned in Hufeland's Journal, of a lad who died of hydrophobia after having been bitten by a dog that had not been and was not mad, one circumstance stated in evidence of the animal's freedom from rabies is, that he drank without difficulty a large quantity of water.

There is another superstitious notion not at all uncommon, viz. that healthy dogs recognize one that is mad, and fear him, and run away from his presence, in consequence of some mysterious and wonderful instinct warning them of danger. This is quite unfounded. Equally mistaken are the notions that the mad dog exhales a peculiar and offensive smell, and that he may be known by his running with his tail between his legs; except, as Mr. Youatt says, when, weary and exhausted, he is seeking his home.

It will not be out of place to state what *are* the symptoms of rabies as observed in the dog, and as described by Mr. Youatt.

The earliest symptoms of madness in the dog (he says) are sullenness, fidgetiness, continual shifting of posture, a steadfast gaze, expressive of suspicion, an earnest licking of some part, on which a scar may generally be found, if the ear be the affected part, the dog is incessantly and violently scratching it. If it be the foot, he gnaws it till the integuments are destroyed.

Occasional vomiting and a depraved appetite are very early noticeable. The dog will pick up and swallow bits of thread or silk from the carpet, hair, straw, even dung; and frequently he will lap his own urine, and devour his own excrement. Then the animal becomes irritable; flies fiercely at strangers; is impatient of correction; seizes the stick or whip; quarrels with his own companions; eagerly hunts and worries the cats; demolishes his bed; and if chained up makes violent efforts to escape, tearing his kennel to pieces with his teeth. If he be at large he usually attacks only those dogs that come in his way; but if he be naturally ferocious he will diligently and perseveringly seek his enemy. According to Mr. Youatt, the disease is principally propagated by the fighting dog in towns; and by the cur or lurcher in the country: by those dogs, that is, that minister to the vices of the lower classes in town and country respectively. He maintains that if a well-enforced quarantine could be established, and every dog in the kingdom confined separately for seven months, the disease might be extirpated. This opinion is founded of course in the belief that rabies never originates at present, any more than small-pox does, *spontaneously;* but is always propagated by the specific virus. And it is corroborated by the fact that rabies and hydrophobia are unknown in some countries: I fancy that South America is, or was, a stranger to it. It appears to have been imported into Jamaica, after that island had enjoyed an immunity from the disease for at least fifty years previously; and Dr. Heineken states that curs of the most wretched description abound in the island of Madeira; that they are afflicted with almost every disease; tormented by flies, and heat, and thirst, and famine, yet no rabid dog was ever seen there. On the contrary 1666 deaths from hydrophobia in the human subject, are stated to have occurred in Prussia in the space of ten years.

Very early in the disease, as it appears in the dog, the expression of countenance is remarkably changed; the eyes glisten, and there is slight strabismus. Twitchings of the face come on. About the second day a considerable discharge of saliva commences; but this does not continue more than ten or twelve hours, and is succeeded by insatiable thirst: the dog is incessantly drinking, or attempting to drink: he plunges his muzzle into the water. When the flow of saliva has ceased he appears to be annoyed by some viscid matter in the fauces; and in the most eager and extraordinary manner he works with his

paws at the corners of his mouth to get rid of it: and while thus employed he frequently loses his balance and rolls over.

A loss of power over the voluntary muscles is next observed. It begins with the lower jaw, which hangs down, and the mouth is partially open; but by a sudden effort the dog can sometimes close it, though occasionally the paralysis is complete. The tongue is affected in a less degree. The dog is able to use it in the act of lapping; but the mouth is not sufficiently closed to retain the water. Therefore, while he hangs over the fluid, eagerly lapping for several minutes, it is very little or not at all diminished. The paralysis often attacks the loins and extremities also. The animal staggers about, and frequently falls. Previously to this he is in almost incessant action. Mr. Youatt fancies that the dog is subject to what we call spectral illusions. He says he starts up and gazes eagerly at some real or imaginary object: he appears to be tracing the path of something floating around him, or he fixes his eye intently on some spot in the wall, and suddenly plunges at it; then his eyes close, and his head droops.

Frequently, with his head erect, the dog utters a short and very peculiar howl: or if he barks, it is in a hoarse inward sound, altogether dissimilar from his usual tone, and generally terminating with this characteristic howl. Respiration is always affected: often the breathing is very laborious; and the *inspiration* is attended with a very singular grating, choking noise. On the fourth, fifth, or sixth day of the disease, he dies: occasionally in slight convulsions; but oftener without a struggle.

Mr. Youatt gives a detailed account of the appearances met with after death in the carcasses of these rabid dogs. They are not very constant or distinctive. The most curious and uniform consist in the presence of unnatural ingesta in the stomach; straw, hay, hair, horse-dung, and earth. Sometimes the stomach is perfectly distended with these substances; and when it contains none of them, there is a fluid resembling the deepest chocolate mixed with olive; or still darker, like coffee: and when neither the unnatural ingesta nor the dark fluid appear, it will be found, Mr. Youatt says, upon careful inquiry, that the dog has vomited much hair, hay, straw, or the like.

In 1837, a few days after the case of hydrophobia occurred in the Middlesex Hospital, I saw the carcass of a dog, that had died rabid, examined by Mr. Ainslie at his and Mr. Youatt's Infirmary. The most remarkable morbid appearances were in the stomach, which contained some bits of straw and stick, and a considerable quantity of a dark fluid like thin treacle. In various parts of the stomach there were spots, almost black, of a considerable size; apparently produced by dark blood partly extravasated beneath, and partly incorporated with, the mucous membrane.

I believe that Mr. Youatt's opinion, already mentioned, of the cause of rabies in dogs, and in all creatures—viz. that it always results from the introduction of a specific virus into the system—I believe this opinion is not commonly entertained. Most people think that the disease is generated, *de novo*, in the dog, at least, and causes have been assigned for it which certainly are not the true or the sole causes. Thus hydrophobia in the dog has been ascribed to extreme heat of the weather; it is thought by many to be particularly likely to occur in the dog-days: and many cautions are annually put forth, about that period, for muzzling dogs, and so on: very good and proper advice, but it would be as appropriate, if those who have noted the statistics of the disease may be depended upon, at one period of the year as at another. Rabies occurs nearly as often in the spring, in the autumn, and even in winter, as it does in summer. M. Trolliet, who has written an interesting essay on rabies, states that January, which is the coldest, and August, which is the hottest month in the year, are the very months that furnish the fewest examples of the disease. The disorder has often been ascribed to want of water in hot weather, and sometimes to want of food. But MM. Dupuytren, Breschet, and Magendie, have caused both dogs and cats to perish with hunger and thirst, without producing the smallest approach to a state of rabies. At the Veterinary School, at Alfort, three dogs were subjected to some very cruel but decisive experiments. It was during the heat of summer, and they were all chained in the full blaze of the sun. To one salted meat was given; to the second water only; and to the third neither food nor drink. They all died; but none of them became rabid. Nor does the supposition that the disorder has some connection with the period of sexual heat in these animals appear to have any better foundation.

If you are desirous of knowing what my own opinion on this matter is, I must say that I think Mr. Youatt's doctrine by far the most probable one; that rabies never occurs except from inoculation of the specific virus. It has never been proved, and indeed it would scarcely be susceptible of proof, that the disease ever breaks out spontaneously; large tracts of country are totally free from it; and in nineteen cases out of twenty, perhaps, we trace the bite or the fray in which the inoculation has been effected.

If I were asked to define the seat of this terrible disease, I should place it, without hesitation, in that division of the nervous system which comprises the excito-motory apparatus; the true spinal marrow, with its appendages of afferent and efferent nerves. Nay, I should go farther, and say that it is the upper part of this apparatus, of which the functions are primarily and chiefly deranged: that the poison acts mainly upon the nervous arcs which pertain to the throat, and with which the eighth pair of nerves in particular is connected. There is nothing singular in this localization of the influence of a specific poison. The ergot of rye affects principally those arcs which belong to the uterus; cantharides those which govern the muscular fibres of the bladder. It is true that the mental functions are remarkably modified, and that paralysis of the lower extremities occurs, in most instances of the disease. But neither of these phenomena are constant; and they simply illustrate, when they do happen, the facility with which any morbid state of the spinal cord may propagate its influence in either direction. Whether, in hydrophobia, the essential change be centric or eccentric, cannot be determined with any thing like certainty: but it seems to me to be most probable that the sensibility of the afferent nerves of the fauces, of the skin, and of the air-passages, is altered or morbidly exalted: whence, upon the application of the exciting stimulus, the peculiar sighing dyspnœa, and the strangling dysphagia, are produced by a reflected influence through the central axis upon the muscles concerned in these actions. But, as I said before, the pathology of the excito-motory apparatus is as yet in its new birth.

What can I say of the *treatment* in hydrophobia; or in rabies? There is no well-authenticated case on record, that I am aware of, in which a hydrophobic person has recovered. As it has been, so is it still, Ιατρὸς ἰᾶται θανατος. The physician that cures is death. There can be no ground therefore for the recommendation of any especial drug, or form of medicine, or even for any general plan of treatment, after the peculiar symptoms of the disease have once set in.

Of course those powerful remedial agencies that are in common use among medical men, have been fairly tried: copious blood-letting, mercury, opium, arsenic, sugar of lead, oil of turpentine, the cold affusion even: and not only those, but the strong poisons that are sometimes, but not so generally, employed for other diseases; belladonna, stramonium, prussic acid, white hellebore, strychnia, cantharides, the nitrous oxide gas: and no end of less gigantic remedies; such as alkalies, and especially ammonia, carbonate of iron, electricity and galvanism, tobacco-juice, and the guaco (which was introduced into this country a few years ago with high encomiums for its power over the disease), the mineral acids, violent exercise: and if we take into account the substances administered to the brute also, we may increase this list by the alisma plantago, scutellaria, box, and rue, all of which, at one time or another, have been vaunted as successful remedies, veratrum sabadilla, and ticunas poison.

The difficulty of swallowing fluids, and in some cases of swallowing at all, is a serious obstacle to the fair trial of almost every form of internal remedy. It has been proposed to introduce powerful medicines into the rectum, in clysters: but to this also the patients have been found to make great resistance. The injection of medicines into the veins has been tried; Magendie hoped that he had discovered a cure, in first largely bleeding the patient, and then injecting his veins with a corresponding quantity of warm water: but it has always happened with this, and with other promising experiments, that just as the patient seemed to be about to recover, he has died. The nervous irritability has in one case or two been much calmed by the injection of a solution of the acetate of morphia into the veins.

Mr. Mayo has suggested bronchotomy, upon this ground (to use his own words), "that the principal character of the disease, and the rapid exhaustion which attends it, appear to depend in great part upon the fits of spasm and closure of the glottis, brought on, not

merely by the attempt, or the idea of drinking, but by any sudden impression upon the senses. Now it is clear, he adds, that as far as the distressing feelings in the throat consist in a sense of suffocation, they would be put an end to or relieved by the establishment of a free opening in the windpipe." Dr. Marshall Hall would use, in combination with tracheotomy, the hydrocyanic acid. Now I should be sorry to say any thing to damp your reasonable hope of benefit from any experiment; but I am bound to confess to you that I should not expect the smallest advantage from tracheotomy in this disease. The mode of death offers no encouragement to its use. There may be spasm of the glottis, but I doubt it; at any rate the patients do not die of suffocation; the death is not death by apnœa, but by asthenia. We see persons labouring grievously for their breath for hours together, who yet survive, and are presently themselves again; persons, for instance, who are affected with severe spasmodic asthma. I have seen a man sitting up in bed a whole night long, inspiring with such difficulty that, if I had not been aware of his having scores of times been as bad before, I should have thought he could not exist five minutes longer. Now we have nothing of this dyspnœa in hydrophobia: and, as I said already, I am sorry, and diffident too, when I differ from great authorities on practical points, but I see no hope of cure, nor even of sufficient benefit, to counterbalance the inconvenience and hazard of the operation, from the performance of bronchotomy. The principle is that of suffering the parts gradually to recover themselves, and of allowing the patient in the meanwhile to breathe through another channel. The principle is excellent, (as I shall show you by and by,) where there is a permanent obstacle to the admission of air to the lungs through the larnyx; but in hydrophobia there is no such permanent obstacle to surmount. Though your patient, in laryngitis, should be at the point of death, yet open his windpipe, and he breathes again and is safe; but it is not at all uncommon for a hydrophobic patient to lose his spasms, to swallow well, and to breathe easily, yet he does not recover. This amendment is the prelude of death, the last flicker of the expiring lamp. Since I lectured upon this subject last year, Dr. Latham has told me the following circumstance respecting a patient whom he treated for hydrophobia, in the Middlesex Hospital. He went one day to the ward, fully expecting to hear that the patient was dead. But he found him sitting up in his bed quite calm, and free from spasm; and he had just drunk a large jug of porter. "Lawk, sir, (said a nurse who sat by,) what a wonderful cure!" The man himself seemed surprised at the change. But *he had no pulse;* his surface was cold as marble. In half an hour he sunk back, and expired. Furthermore the experiment in question has been tried, and it has been tried by its proposer, Mr. Mayo, upon the dog, without affording, as Mr. Youatt assures us, the slightest relief. In the matter of cure, surgery, I fear, is as impotent as physic.

Not so, however, in the matter of prevention: this is the most important part of the practice. The early and complete excision of the bitten part is the only measure in which we can put any confidence: and even here we are met with a source of fallacy. In the majority of cases, no hydrophobia would ensue, though nothing at all were done to the wound. How can we know, then, that the disease is ever prevented by its excision? No doubt many persons go through the pain of the operation needlessly. But we can never be sure of this. They get at any rate relief from the most harassing suspense, with which they would be probably tortured for months. And if a large number of bitten persons, who had suffered the wound to heal as it would, could be compared with the same number who had had the bitten part cut out, hydrophobia would be found a frequent consequence of the bite in the first class—a very rare consequence of it in the second. Mr. Youatt, who trusts to caustics, and who has himself been bitten seven times, and is yet alive and well, tells us that he has operated, with the caustic, on more than fourteen hundred persons, all bitten by dogs, respecting the nature of whose disease there could be no question; and that he has not lost a case. One man died of fright, but not one of hydrophobia; and he says also that a surgeon of St. George's Hospital told him that ten times that number had undergone the operation of excision there, after being bitten by dogs (all of which might not, however, have been rabid), and that it was not known that any one had been lost. Mr. Youatt, I say, trusts to caustic; and the caustic he uses is the nitrate of silver. But I advise you to trust to nothing but the knife, if the situation of the bite will allow you to employ it effectually. If the injury be so deep or extensive, or so situated,

that you cannot remove the whole surface of the wound, cut away what you can; then wash the wound thoroughly, and for some hours together, by means of a stream of warm water, which may be poured from a tea-kettle; place a cupping-class from time to time over the exposed wound; and finally apply to every point of it a pencil of lunar caustic. If you cannot get the solid caustic in contact with every part, you had better make use of some liquid escharotic; the nitric acid, for example. In my own case—and what I should choose for myself I should advise for another—if I had received a bite from a decidedly rabid animal upon my arm or leg, and the bite was of such a kind that the whole wound could not be excised, my reason would teach me to desire, and I hope I should have fortitude enough to bear, amputation of the limb, above the place of the injury.

But if the wound is of such a size, and in such a part, that it can be excised, what is the proper way of cutting it out? If I were to give you any opinion, as from myself, upon that point, you might think, perhaps, that I was stepping beyond my proper province. I shall, therefore, again retail to you the advice of my old master, Mr. Abernethy. "The cell (he says) into which a penetrating tooth has gone, must be cut out. Let a skewer be shaped, as nearly as may be, into the form of the tooth, and then be placed in the cavity formed by the tooth; and then let the skewer, and the whole cell containing it, be removed together by an elliptical incision. We may examine the removed cell, to see if every portion with which the tooth might have come in contact has been taken away: the cell may even be filled with quicksilver, to see if a globule will escape. The efficient performance of the excision does not depend upon the extent, but upon the accuracy, of the operation." Mr. Abernethy was of opinion that when once the poison had been imbibed into the system, nothing ever had done good, and nothing, probably, ever would. I should be sorry to be so absolutely despairing in respect to a disorder in which dissection after death discloses no reason why the patient *might* not recover. He used to add, that as bleeding had been much extolled, had he hydrophobia he would allow a surgeon to bleed him, even to death. Like Seneca he would be willing to have his veins opened, though his disease might not permit him to indulge at the same time, like Seneca, in the luxury of a warm bath.

I say *early* excision is the only sure preventive; but let me repeat that it will, in all suspicious cases, be advisable, if, for any reason, the operation has been omitted in the first instance, to cut out the wound, or the cicatrix, at any time before the symptoms of recrudescence have appeared. One would do it, though with less hope, as soon as possible *after* they had appeared: but I do not expect to hear of excision being successful then in stopping the disease. Dr. Bright has recorded a case in which the arm was amputated upon the supervention of tingling, and other symptoms, in the hand, in which the patient had been bitten some time before; but the amputation did not save him.

It has been proposed to fill the wound with ink, and then to wash it until every trace of the ink is gone; in this way, it is conceived, the complete ablution of the poison also will be ensured. With a timid or an obstinate patient, who would not submit to the knife or the caustic, some such expedient ought to be diligently tried: but it would be better to try it *after* the excision, or after the application of the escharotic substance. It is impossible to take superfluous pains to obviate so fearful a disease as hydrophobia.

It has been recommended, after the wound has been excised or cauterized, that it should be prevented from healing, and made to discharge for a long time, by means of irritating applications. This may be advisable when thorough excision, or complete cauterization, cannot be effected; but I should think it quite useless as auxiliary to those expedients, and only likely to keep up, or to produce, a hurtful irritability of the system.

I should perhaps have mentioned before, a theory, and a plan of preventive treatment, which made a great figure in all the journals, foreign and domestic, a few years ago. It was pretended by a Russian physician, Dr. Marochetti, that some time between the third and the ninth day after a person had been inoculated with the hydrophobic poison, by the bite of a rabid dog, small pustules appear on or about the frænum of the tongue, containing a small quantity of sanious fluid, of a yellow or greenish colour. The same kind of pustules were declared to exist also under the tongues of the mad dogs themselves. Now Dr. Marochetti pretended farther, that if, from the very time of the bite, you gave the patient large doses of the decoction of broom tops, and looked out for the eruption of these

pustules, which seldom lasted more than twenty-four hours, you might infallibly prevent the disease by opening and emptying the pustules, and then cauterizing them with a red-hot iron; and afterwards causing the patient to gargle his mouth with that same decoction of broom. He held that the poison was deposited there for a short time, and then reabsorbed into the system; and he proposed to prevent such reabsorption. This was a very pretty theory; and took mightily in the medical world. But it has turned out a sort of hoax. I do not mean a wilful hoax on the part of Dr. Marochetti; for I have no doubt that he contrived to hoax himself. These pustules have been looked for again and again; but they have never been discovered in Englishmen affected with hydrophobia; nor in English mad dogs. The truth seems to be that the mucous follicles of the mouth, generally, and those at the base of the tongue, and those beneath the tongue, in particular, are commonly enlarged and exaggerated in the dog, and in the human animal, labouring under the disease; and these enlarged and altered follicles were regarded by the Russian physician as a specific eruption, that furnished the virus and pabulum of the complaint.

As almost every drug that has ever been included in any Pharmacopœia has been administered with the hope of *checking* the *disease*, so a great number of medicines and measures have been praised as preventives. Some people have great faith in sea bathing; and they go to the coast to be ducked and half drowned every day for six weeks: and if they escape hydrophobia they conclude that the immersion in the salt water has saved them. Some of the specifics, as you may suppose, are great secrets; and they who possess them—whether they believe in them or not is another matter—sell them at no cheap rate to those who have been bitten by the dog, and are weak enough to be bitten again by the quack. The composition of several of them has transpired; and they are found to consist either of ingredients the most insignificant and worthless, or of poisons of which the inefficacy had already been ascertained. The celebrated *pulvis antilyssus*, which was introduced by no less a person than Dr. Mead, into the London Pharmacopœia, was a mixture of ash-coloured liverwort and black pepper. The *Ormskirk medicine*, long famous, and scarcely obsolete yet in the north of England, was made up of bole armeniac, alum, chalk, elecampane, and oil of aniseed. The *Tonquin medicine* was composed of cinnabar and musk: and the *Tonjore pills* were a combination of mercury and arsenic. Even now scarce a year elapses but some correspondent of the newspapers, whose philanthrophy is more conspicuous than his judgment or his knowledge, recommends a new and infallible preventive. I confess to you that I have not the slightest faith in any one of them: but as I have a great respect for Mr. Youatt, and as he is not *quite* so sceptical as I am on this point, and as patients or their friends will insist upon the adoption of protective measures sometimes, when the local means of prevention have been omitted or imperfect, I will tell you what he (Mr. Youatt) has done in respect to those prophylactic drugs.

In the first place he never succeeded in curing the disease in the dog with any thing that he ever tried.

In the way of prophylaxis, he experimented with a great number of substances. He thought that the box-wood, which is the basis of some celebrated preventive drinks in Hertfortshire and Kent, had some effect. He tried the alisma plantago, the boasted efficacy of which had been strictly inquired into by the magistracy of Toula, and the receipt purchased by the Russian government at an immense price. But he had no success with it. He then put the belladonna to the test, beginning with two grains, and increasing the dose to a scruple twice every day, and continuing this for six weeks: and he says he is confident that he saved several dogs; but he lost almost as many. They all became debilitated and most rapidly emaciated.

Then, in the year 1820, his attention was directed to the scutellaria lateriflora, which Dr. Spaulding, an American physician, had found highly successful as a preventive of rabies: and upon trial of it, he soon was brought to regard it as really valuable: and (not to tire you with a detail of his proceedings in the interim) he at length combined it with belladonna; "and the result" (I here quote his own language) "has been a medicine which I cannot, dare not, call a specific; for it has failed: but the use of which, in the cases of doubt and fear to which I have alluded, I would most earnestly recommend."

He relates two experiments, which seem to have made a great impression upon his mind. They are as follows:—

"Three pieces of tape were thoroughly moistened with the saliva of a rabid dog, and inserted as rowells in the polls of three dogs. To two the scutellaria and belladonna were given: the third, a fox-hound bitch, was abandoned to her fate. On the 29th day after the inoculation she became rabid." The others, at the time this was written, *i. e.* some months afterwards, were living and well.

He afterwards took the same two dogs, and a third. He moistened two pieces of tape with the saliva of a rabid dog, and inserted them in the polls of one of the old dogs, and of the third dog. Another piece of tape dragged repeatedly through the mouth of the same rabid dog *twenty-four hours after its death*, was inserted in the poll of the second of the old dogs. This dog and the new one were suffered to take their chance. To the other old dog the medicine was given. In the fourth week the new dog died undeniably rabid. The other two survived.

I repeat that I have no faith in these preventives. But *sometimes* some of them must be tried; and I would prefer those which are thus sanctioned by Mr. Youatt's good opinion to any others.

And with respect to the established disease, I think that if I were the unhappy subject of it, I should wish to be put into a hot air bath, and thoroughly sweated, and to take opiates; not so much in the hope of recovering as with a view to the euthanasia. But, with all respect to those gentlemen who advocate that practice, no one, if I could help it, should make a hole in my windpipe.

LECTURE XXXV.

EPILEPSY. ITS SYMPTOMS AND VARIETIES; DURATION AND RECURRENCE OF THE PAROXYSMS; PERIODS OF LIFE AT WHICH THEY COMMENCE; WARNINGS. EFFECTS OF THE PAROXYSMS, IMMEDIATE AND ULTIMATE. PATHOLOGY. ANATOMICAL CHARACTERS. CAUSES.

THE great functions of the brain are sensation, thought, and voluntary motion. The influence of the will is a cerebral influence: it reaches and acts upon the muscles through the interposition of the spinal cord. Motions that are involuntary belong more exclusively to the system of the true spinal marrow. Yet cerebral changes, morbid states of the brain, may excite them.

I have shown you that all these functions are liable, under disease, to be separately affected, and each in various ways and degrees. The number of combinations capable of arising out of disordered conditions of two, or three, or all of these functions, is very great. Yet the symptoms proper to the nervous system do arrange themselves into groups sufficiently definite and constant to allow of our giving them distinctive names, and making them separate objects of inquiry.

At the same time, as might indeed be expected, these several groups have strong resemblances to each other. They are obviously of the same family: "facias non omnibus una, Nec diversa tamen; qualis debet esse sororum:" occasionally the features are so nearly alike, that we find it somewhat puzzling to determine with which of the sisters we are conversing; but usually there is some mark or other by which the individual may be identified.

Of these essentially nervous diseases, there are several in which the most prominent and obvious of the phenomena relate to the muscular system; irregular, and violent, and involuntary contractions occurring in muscles which, in the healthy state of the body, are subject to the control of the will. I have spoken of two very frightful disorders belonging to this head:—of *tetanus*, namely, in which the muscles of voluntary motion present the most striking changes, being affected with tonic spasm; while the sensibility under-

goes no other alteration than what is a consequence of that spasm, pain I mean in the muscles themselves; and the intellectual functions continue undisturbed:—and of *hydrophobia*, in which the natural sensibility suffers much, and the mental functions some derangement; yet still the characteristic features of the malady depend upon the irregular and uncontrollable action of voluntary muscles.

The disease which I am next to consider is scarcely less terrible to witness, when it occurs in its severer forms, than tetanus or hydrophobia; but it is not attended with the same urgent and immediate peril to life. Yet it is, upon the whole, productive of even more distress and misery; and is liable to terminate in worse than death. You will understand that I am alluding to *Epilepsy:* a disease not painful probably in itself; seldom immediately fatal; often recovered from altogether: yet apt, in many cases, to end in fatuity or insanity; and carrying perpetual anxiety and dismay into those families which it has once visited.

The leading symptoms of epilepsy are, a temporary suspension of consciousness, with clonic spasm; recurring at intervals.

It is impossible to frame a perfect *definition* of epilepsy: nay, so various are its forms, so numerous its modifications, that no general *description* even of it can be given. It will be necessary for me therefore here (as it has been before) to describe first the most ordinary type of the disease, as a standard; and then to note the several variations from that standard which are known to occur in practice.

A man, then, in the apparent enjoyment of perfect health, shall suddenly utter a loud cry, and fall instantly to the ground, senseless and convulsed. He struggles violently. His breathing is embarrassed or suspended; his face turgid and livid; he foams at the mouth; a choking sound is heard in his windpipe; he appears to be at the point of death by apnœa: but presently, and by degrees, these alarming phenomena diminish, and at length cease; the patient is left exhausted, heavy, stupid, comatose: but his life is no longer threatened. And in a short time he is once more to all appearance perfectly well. The same train of morbid phenomena recur, however, again and again, at different, and mostly at irregular intervals.—This is a brief description of the most ordinary form of epilepsy.

The suddenness of the attack is remarkable: in an instant, when it is least expected by himself or by those around him, in the middle of a sentence, or of a gesture, the change takes place; and the miserable sufferer is stretched foaming, struggling, and insensible upon the earth. This fearful suddenness is expressed in the name of the disease, ἐπιλημψια, a seizure, an abrupt invasion. The ancients, among whom the complaint was well known, superstitiously ascribed it to the malice of demons, or the anger of their offended deities. If a person was seized with epilepsy in the forum, it was considered an ill-omen, and the meeting was at once dissolved, and all public business suspended for that day: hence the disease was called *morbus comitialus*. *Morbus qui sputatur* was another of its names, because those present were accustomed to spit upon the epileptic man, or into their own bosoms; either to express their abomination, or to avert the evil omen from themselves. In this country its common designation is the *falling sickness:* or, more vaguely, *fits*. The cry which is frequently, though by no means always, uttered, is generally a piercing and terrifying scream. Women have often been thrown into hysterics upon hearing it; it is even said to have caused pregnant females to miscarry: even the lower animals seem to be sometimes startled and alarmed by a note so harsh and unnatural. Dr. Cheyne informs us that, upon one occasion, "a parrot, himself no mean performer in discords, dropped from his perch seemingly frightened to death by the appalling sound." The muscular convulsions are strong, irregular, and often universal. In many of the fits of which I have happened to see the commencement, the first effect of the spasm has been a twisting of the neck, the chin being raised, and brought round by a succession of jerks, towards one shoulder: and one side of the body is usually, I think, more strongly agitated than the other. The features are always greatly distorted. The brows are knit; the eyes sometimes quiver and roll about, sometimes are fixed and staring, sometimes are turned up beneath the lids, so that the cornea cannot be seen, and the white sclerotica alone is visible; the mouth is twisted awry; the tongue, thrust between the teeth, and caught by the violent closure of the jaws, is bitten, often severely; and the foam which issues from the mouth is reddened by blood. The hands are firmly clenched,

and the thumbs bent inwards upon the palms; the arms are thrown about, striking the chest of the patient with great force, or bruising themselves against surrounding objects, or inflicting hard knocks upon the friends and neighbours who have hastened to the patient's assistance. It frequently happens that the urine and excrement are expelled during the violence of the spasm: and seminal emission sometimes takes place. The spasmodic contraction of the muscles is occasionally so powerful as to dislocate the bones to which they are attached: the joints of the jaw, and of the shoulder have been thus put out; and the teeth are sometimes fractured.

When the convulsive paroxysm is over, the patient falls into a deep sleep. You might imagine that he slept from exhaustion, like a man worn out by great fatigue: but there is something more than this; the patient passes into a state of imperfect coma, or rather the insensibility continues after the convulsions have ceased. When he wakes he is often confused and incoherent for a time; by degrees, however, he resumes his ordinary appearance and condition: but he remembers nothing of what passed during the fit.

You may suppose that so much irregular contraction of the muscles of voluntary motion is not likely to occur without some derangement or modification of the functions of the circulation. The breathing is irregular, gasping, or arrested. The heart palpitates violently against the ribs during the paroxysm: the pulse becomes frequent and feeble; and sometimes it ceases to be tangible at the wrist during the height of the fit, and begins to be felt again as the spasms subside. The turgescence of the face indicates obstruction of the venous circulation; the cheeks and lips become purplish and livid, and the veins of the neck and forehead are visibly distended.

This, then, is one form, the most severe and the most common as well as the best marked form, in which an epileptic attack occurs.

But there is a large class of cases, in which the symptoms are much more mild. There is very slight and transient, or even no convulsion at all; no turgescence of the face; no foaming at the mouth; no cry: but a sudden suspension of consciousness, a short period of insensibility, a fixed gaze, a totter perhaps, a look of confusion; but the patient does not fall. This is momentary; consciousness presently returns; the patient resumes the action in which he had been previously engaged, and is not always aware that it has been interrupted. Sometimes, with this temporary abeyance of the mental functions, there is some slight evidence of convulsion or involuntary action; the fingers of one hand, or less commonly of both, are moved irregularly, and without any object; or the eyes roll or are turned upwards; or the muscles of the face are twitched. Sometimes the patient is himself aware of what has been his condition, but shows some cunning in endeavouring to conceal it.

This slighter attack is called by the French *petit mal;* while the severer form is named *grand mal.* The former is spoken of also as *epileptic vertigo,* and distinguished by that appellation from the *epileptic fit.*

Of affections so different in degree, and in some respects so dissimilar in kind, you may be disposed to ask whether they really constitute the same disease. That they are essentially of the same stamp, we have this evidence; that both forms of attack occur in the same individuals. Sometimes a patient will suffer many recurrences of the epileptic vertigo, and at length will become affected with violent epileptic fits. Or the two forms will intermingle, sometimes the milder happening, sometimes the severer. In such cases we cannot doubt that the attacks are the same in nature, though different in form and degree. And when (as sometimes happens) we meet with the slighter disease alone, we cannot refuse to assign to it the character and the name of *epilepsy.*

Between the two extremes, there are many links of gradation. Sometimes the sufferer sinks or slides down quietly, and without noise, is pale, is not convulsed at all, but insensible; much like one in a state of syncope. After recovering he remains sick, languid, and confused, during the remainder of the day.

You will perceive from what I have now said, the difficulty of giving any single description of epilepsy, which will include all its varieties. It is of course still more difficult to offer a strict definition of the disease. Cullen defines it to be "musculorum convulsio, cum sopore." Dr. Copland furnishes a larger and more comprehensive definition. "Sudden loss of sensation and consciousness, with spasmodic contraction of the volun-

tary muscles quickly passing into violent convulsive distortions, attended and followed by sopor, recurring in paroxysms often more or less regular."

But almost every one of these circumstances may, in its turn, be wanting. There may be no convulsion; there may even be very slight and transient interruption of consciousness; there may be no subsequent coma or sopor; there may be no recurrence of the attack.

Yet I trust that you now have obtained some general notion of what is meant by an epileptic seizure. And I go on to inquire into several most important points connected with the paroxysms.

In the first place, they vary considerably in *duration*. Sometimes, as I have already stated, the seizure is slight, and does not occupy more than a moment or two of time. But even the severer attacks are often over in a few minutes. They seldom continue longer than half an hour; and probably the average duration may be safely laid at between five and ten minutes. Attacks that are spread over three, or four, or more hours, generally consist of a succession of paroxysms, with indistinct intervals of comatose exhaustion. In the long-continued fits, or in the protracted succession of fits, the patient often dies.

The periods at which the paroxysms *return* are also extremely variable. Occasionally the patient dies in the first paroxysm; occasionally, though he recovers from it, he never has another. Both of these occurrences are rare. Rather more frequently the fits occur at very long intervals; at the distance I mean of many years. Most commonly of all they revisit the sufferer at irregular periods of a few months or weeks: sometimes they are repeated at intervals of a few days: sometimes every day, or every night: and not very unfrequently they take place many times in the 24 hours. This extreme frequency of repetition belongs principally to the slighter imperfect seizure; the *petit mal*. Sometimes the fits observe a strictly *regular* period of return; but, for the most part, they are quite uncertain and *ir*regular.

The *time of life* at which the fits *commence*, and the circumstances attending their commencement, are deserving of notice They not uncommonly begin in infancy. Those fits of convulsion to which young children are subject during the first dentition, and which sometimes appear to depend upon the irritation of teething, and sometimes upon manifest disorder of stomach and bowels—these fits are not distinguishable in their phenomena from genuine epilepsy; and we must reckon them as instances of epilepsy. It has been remarked by some one, that if you can trace the early history of an adult epileptic, you will almost always find that he or she suffered infantile convulsions. To what extent this is true I do not know, but I recommend it to you, as a point worth attending to in your future opportunities of observation.

The epileptic attack may come on, for the first time, at *any age*. According to Dr. Bright (whose account of the disease, though short, is particularly perspicuous), the most common periods are about the age of seven or eight years, probably about the time of the second dentition; and from fourteen to sixteen, shortly before the age of puberty. And the disease (he says) is very apt to occur for a few years subsequently to this. But sometimes the first fit has taken place between the ages of thirty and forty; in not a few cases, after sixty; and occasionally quite in the decline of life.

Dr. Bright offers a little piece of theory in respect to the periods in which epilepsy is apt to begin. It is a reasonable piece of theory, and serves to tie the alleged facts to one's memory, even if it be not yet proved to be true. Doubtless in many cases the circumstances that determine the first attack are quite accidental. But setting aside these casualties, he says, "there are leading periods in the evolution of the frame, and peculiar circumstances connected with certain periods, which may well be considered as influential in the production of the disease. In infancy, the nervous system is delicate, and easily acted upon by various causes of irritation. Then follows the trying period of teething. In a few years, the second dentition occurs. In a few years later, all the great changes connected with the age of puberty. To this follow the excesses and exposures of manhood; and, after the lapse of years, the vigour of the system fails, and many causes act to derange the nice balance of the constitution; the bowels often become sluggish; changes more or less serious take place in the structure of the arterial and venous system; and

many causes, organic or functional, which had before been unable to exert an influence on the vigorous frame, acquire power from its relative weakness."

The first accession of the disease takes place more commonly before than after puberty. Of 66 epileptic women, in whom the outset of that disease and the first period of menstruation were carefully noted, 38 had epileptic fits before, and 28 not till after that period.

The attacks are very apt to come on during the night; in the commencement of the disease they frequently are *confined* to the night: they are said chiefly to occur at the moment when the patient is sinking into sleep, or awaking from sleep. How far this is true I cannot tell. When the disease is *yielding*, the fits often happen in the night only; so that after they have, for a certain period, taken place in the day-time, or during the day and night indiscriminately, it is reckoned a good prognostic sign if they begin to restrict themselves to the night. Some patients, under these circumstances, suppose that the physician has particular remedies that will make the fits happen in the night rather than in the day; and they ask for these remedies.

Sometimes each paroxysm arrives unannounced and unexpected; sometimes distinct *warnings* of its approach are given. The latter is less frequent than the former. Georget affirms that premonitory symptoms do not occur more than four or five times in 100 cases. I am sure that this is much understated. When warning symptoms do happen, they are sometimes spread over a considerable period, several hours, or a whole day; sometimes they just last long enough to enable the patient to remove from a situation in which a fall would be attended with unusual danger; to dismount from horseback, to lie down in a boat, to get away from the fire-place, from the edge of a precipice, from the vicinity of water, to assume the horizontal position of his own free will and in his own manner, or to give notice to those about him of what is going to befall him. In some cases the warning is too short and sudden even for this. The *kind of notice* that he receives is very variable indeed. Often it consists in some unnatural state of the mind, the feelings, the temper; the patient is fidgety, irritable, low-spirited, timid, sullen; or, on the other hand, he feels unusually strong, and hearty, and cheerful. Sometimes there is a notable change in some one or more of the natural functions, or of the bodily sensations; the patient loses his appetite, or his appetite becomes voracious; a great flow of urine takes place; he smells an ill smell, is aware of a strange taste, hears extraordinary noises, or sees spectral illusions; not mere specks floating before him—muscæ volitantes—but distinct forms of persons and things. This is not very common, but it certainly happens. The late Dr. Gregory, of Edinburgh, was assured by a patient of undoubted veracity, that always when he had a fit of epilepsy approaching, he fancied that he could see a little old woman in a red cloak, who came up to him, and struck him a blow on the head, and then he immediately lost all recollection, and fell down.

Headache, and giddiness, and dimness of vision, are all of them common symptoms among those which have been observed to be precursory of epilepsy. Sometimes there are circumstances which are obvious to the bystander: a flushing of the face, or lividity; difficult articulation; vomiting. Of 21 epileptics treated in the hospital, at Wilna, by Joseph Frank, vomiting announced the paroxysm in 7. Some of the uneasy feelings are apt to come on and continue even for several days previously to the attack; restlessness in particular, disturbed sleep, distressful dreams, a peculiar and sudden coldness of the extremities. An internal *working* is a phrase often used by such persons to express a sensation which is probably indescribable.

But the most curious precursory symptom of all, if we except the spectral illusions, is what is called the epileptic *aura*. This is a sensation which is likened, by different patients, to different things; to a stream of warm or cold air, to the trickling of water, to the creeping of a spider. The sensation proceeds commonly from some distant part of the body, from one of the extremities, from a thumb, or finger, or toe, or from some spot on the trunk, and runs along the skin, towards the head; occasionally it gets no farther than the epigastrium: as soon as it reaches the head, or stops at the epigastrium, or elsewhere, the patient's consciousness forsakes him, and the paroxysm declares itself.

There seems to be some analogy between this epileptic aura, and the well-known sensation, to be spoken of hereafter, of a ball rising from the stomach to the throat, and constituting the globus hystericus; except that in cases of epilepsy the sensation commonly

begins in an extremity, and not in the stomach; and the fit comes on when it reaches the head, and not the throat. Sometimes, I think, these two sensations are blended.

In some instances, spasms of the muscles of the part whence the aura proceeds are observed to take place prior to the more general state of spasm.

This aura is certainly a very curious phenomenon. It has been thought to depend upon some change propagated along the nerve upwards to the brain, and that it is sometimes connected with some injury done to, or some morbid impression made upon, an afferent nerve. I think that this explanation may apply to some cases.

In a patient of my own, who was subject to epilepsy, the warning sensation commenced in one of his thumbs, which presently after began to be twisted inwards; but by tying his handkerchief tightly round his thumb, he could prevent the fit. Dr. Seymour mentions the case of an epileptic boy, who had learned to protect himself against a threatened paroxysm, by biting his tongue.

In other cases the aura probably originates in some change within the head, and is analogous to the numbness or tingling that is often felt in some part of the body or extremities immediately before an attack of palsy or apoplexy. There is no real inconsistency in this twofold explanation; the source of the aura may be centric or eccentric; as I shall explain to you, in due time, the exciting cause of the paroxysm may be.

A knowledge of these warning circumstances is clearly of importance, always as far as respects the comparative security of the patient during the attack; sometimes as affording us the opportunity of staving off the fit altogether. And it is necessary to remark, that they sometimes give, as it were, a *false alarm;* they occur, and yet, although no measures of prevention are taken, no paroxysm follows.

The phenomena that *succeed* the paroxysm are also of great interest and moment.

I have already apprised you that the convulsions terminate in general before the insensibility is over: the patient draws, perhaps, two or three deep sighs, and ceases to struggle. Some few persons are quite themselves again in a few moments; some appear to recover consciousness, and then fall into a deep and prolonged slumber; but many do not regain their consciousness at all upon the cessation of the convulsions, but remain in a state of profound stupor, from which however they can generally be roused for a time. This state of coma (for it is nothing else) has been known to last a week: after the patient emerges from it, he is sometimes merely languid and inert; sometimes he is like a person stunned, or in a state approaching to idiocy, which gradually clears up; sometimes he is furiously delirious for a short time; not unfrequently there is a degree of partial paralysis, which also usually soon goes off, though it occasionally is permanent; the eyes are fixed, or he squints, or the pupils are dilated, or he drags a leg, or he falters in speech. Most commonly he speaks of headache, or discomfort of some kind.

It is very seldom that the patients have any recollection whatever of what has passed during the fit. Many of them are not aware that they have had a fit: and those who do know it, discover the fact by finding themselves wet or dirty; by the injuries they have received during the convulsions; by the soreness of the bitten tongue; by the bruises of their limbs; or by the confused or painful sensations which they subsequently experience, and which they have learned to associate with the conviction that a fit has happened, by having been informed on previous occasions, when they felt the same sensations, that they had suffered a paroxysm of insensibility and convulsions.

Upon the whole, it is seldom that any ill effect can be noticed as having been left behind it by any one single fit; but, alas! this cannot often be said of their repetition.

Doubtless a single paroxysm does often leave the patient in a worse condition than that in which it found him; but this does not become perceptible to an ordinary observer, until after the alteration has been rendered apparent by repeated fits, and repeated small additions to the permanent injury. The friends of the patient perceive that his memory is enfeebled in proportion to the number of the attacks; that his mental power and intelligence decline. His features even assume, by degrees, a peculiar character; and too often he sinks into hopeless fatuity, or confirmed imbecility or insanity. It is this tendency which renders epilepsy so sad and fearful a disease.

Foville affirms, that the intellectual degeneration is more constant, and comes on more early, in persons who are principally afflicted with the epileptic vertigo, the *petit mal*, the

imperfect seizure, than in persons whom the *grand mal*, the violent and decided paroxysm, takes place. Dr. Copland, on the other hand, is of opinion, that "the more severe the fits, the more is that result to be dreaded." This is a point which can only be settled by statistical facts. And as we all have the opportunity of collecting some such facts, and of adding them to the general stock, I mention this, and some other points that are still uncertain or disputed, as worth bearing in mind. More, probably, depends upon the *repetition* of the fits, than upon their precise *nature* or *severity*. Cases do occur in which epileptic persons preserve their faculties to a good old age: but those who are early epileptic do not often attain old age; and *whenever* the disease comes on, if it repeats itself frequently, it is much more often than not followed by impairment of the mind, or by some apoplectic or paralytic affection, which implies and accompanies the mental change. You will sometimes hear the cases of Julius Cæsar, of Mahomet, or of Bonaparte quoted, as examples of high intellectual power, existing and remaining in spite of epilepsy:—and it is allowable, perhaps, to make use of such cases for comforting the friends of epileptic persons; or for giving the advantage of sustained hope to the patient himself. But, in truth, these cases are not worth much. Napoleon is said, I know not upon what authority however, to have suffered something like epilepsy during sexual intercourse; this is not very uncommon in persons subject to that disease. And, with respect to Julius Cæsar, we learn from Suetonius, that it was only in the latter part of his life that he laboured under epilepsy; and that he had two attacks while engaged in business.

Having now described the phenomena of epilepsy; the periods of life at which it is most apt to commence; its varieties; and its tendency and termination: let us next inquire what is known respecting the real seat and nature of this strange and melancholy complaint.

The functions that are affected are clearly the functions of the *brain:* sensation, thought, and motion regulated by the will, are the natural functions of that organ. The temporary abeyance of sensibility, thought, and volition; and violent and irregular action of the muscles, which are thus withdrawn from the empire of the will; constitute a paroxysm of epilepsy. We have, in this malady, another illustration of the fact, that when the controlling influence of the sensorium is suspended, the peculiar functions of the spinal marrow are exercised, not only in a disorderly, but also in an unusually energetic manner. That the brain and the spinal marrow, though physiologically distinct, are yet intimately connected with, and dependent upon, each other, a thousand familiar facts assure us; and there are good reasons for believing that the change, whatever it is, which is the immediate precursor and cause of the epileptic fit, may sometimes originate in the spinal cord, and thence extend to the brain; and sometimes originate in the brain, and communicate itself to the spinal cord. Dr. Marshall Hall's doctrine, that all convulsive diseases are diseases of the spinal marrow, cannot be properly applied to this convulsive disease of epilepsy. It is true that the spinal cord is concerned whenever there is convulsion; but it is concerned in every *voluntary* movement also, through the instrumentality of the brain itself; and it may be, and often is, irregularly influenced by a disordered and unnatural state of the brain. Tetanus may fairly be regarded as a disease of the cord, and its proper appendages; the spasms arise and reach their height, while the powers of thought and sensation are undisturbed, and while the volition remains, although the morbid condition of the cord renders it ineffectual. In epilepsy, these cerebral functions are always implicated. There is *always* a loss of consciousness; and in the epileptic vertigo, the *petit mal*, there is frequently a suspension of consciousness only, *without convulsion at all.* The brain, therefore, we must consider to be essentially concerned in this disorder.

What the precise state of the nervous matter may be, which determines the loss of consciousness and the spasms, we can only conjecture. A derangement in the relation between the arterial and venous circulation within the head; a temporary pressure somehow arising; a determination of blood towards the head; a diminution of the natural quantity of blood sent thither from the heart; all these have been assigned as possible causes of the paroxysms. Plausible reasons might be given in favour of the operation of each of them; but the speculation is more curious than useful. We have not yet pene-

trated the mystery of these remarkable phenomena, and it will be more profitable to turn to another question, which admits of a somewhat more definite answer, viz.:—what is the *morbid anatomy* of epilepsy?

Suppose that a person who has had epileptic fits, but in whom they have not been followed by any durable affection of the intellectual or locomotive functions, dies of some other malady; and you have the opportunity of minutely examining the condition of the nervous system. Often you will find nothing at all which can throw any light upon the occurrence of the epileptic paroxysms; no appreciable alteration whatever in any part either of the brain or of the spinal cord. In other cases you may discover some organic disease within the head: a scrofulous tubercle, a spiculum of bone projecting from the skull. Have we then detected the cause of the disease? All that can be said is, that the piece of bone or the tubercle was probably a *predisposing* cause of that derangement of the nervous substance which determined the paroxysms; the derangement itself, if, indeed, it was of such a nature as to be cognizable by our senses, has gone with the symptoms; the tubercle or bone having in the meantime remained without any sign which could betray its presence.

M. Foville, whose testimony in this matter is entitled to much weight, affirms that in persons who have been subject to epilepsy, uncomplicated as yet with any permanent disorder of the intellect, or of the faculty of voluntary motion, and who have died *in the fit*, constant alterations are observable within the head; viz. a strong injection of the vessels of the encephalon: the membranes, the brain, and the cerebellum, are gorged, he asserts, with livid blood. But he goes on to say that this is to be ascribed to the mode of death; that we see the same appearances in persons who have died by hanging, or any form of apnœa; that they are not peculiar to epilepsy, and do not explain the attack, but only point out the way in which it has been fatal.

If such appearances were uniformly found in the heads of persons who die suffocated, or who die in a fit of epilepsy, this observation of M. Foville's might be a very just one. But I doubt whether such internal congestion of the vessels of the head is met with in either case: for reasons formerly explained to you. In the only two instances in which I have had the opportunity of examining the state of the brain after death by hanging, there certainly was no such congestion.

It is, I fancy, a very common notion, both that such congestion does take place, and that it is the cause of the paroxysm: and it may be worth while shortly to state the reasons which are opposed to the conclusion, that the congestion (granting for the moment that it does happen) is a sufficient explanation of the attack.

In the first place it is not easy to conceive that the congestion should so suddenly arise and subside again, as it must sometimes do, if it be the immediate determining cause of the fit: within the space of a single minute, for example.

And in the second place, the signs of external congestion and plethora, by which signs we measure the amount, if there be any, of the *internal*, are most marked just when the symptoms of the paroxysm begin to subside and disappear. So that we cannot, I think, look upon the congestion as the *cause* of the convulsive symptoms.

Let us go a step farther, and inquire into the state of the encephalon in those persons who, having suffered epilepsy, had, before death arrived, been affected with some permanent impairment of the mental functions, or (what often goes along with such impairment) with some degree or other of muscular paralysis or debility.

The most common alterations met with in the brain in such cases are the following.

Induration of the white matter of the brain, which presents a dull appearance; sometimes, besides the hardening, a general injection of the white matter; and in the majority of cases a marked dilatation of the blood-vessels. In some instances the consistence of the white matter is diminished, it is soft and flabby; but there is the same dilatation of the blood-vessels. These changes pervade the whole of the white matter in every part of the brain. At the same time the gray matter is found irregular on its surface, marbled or of a rosy colour in its substance, and sometimes altered in consistence. And in many cases the membranes are found to be adherent in some parts to the convolutions with which they lie in contact.

Such are the results of the experience of careful observers in respect to the morbid ana-

tomy of epilepsy; of Morgagni, of Foville, and of MM. Bouchet and Casauvielh. The changes last described are such as are produced by chronic inflammation of the brain and its membranes. They are the *consequences* (I imagine) of repeated paroxysms of epilepsy; they are the very same as are frequently met with in cases of insanity complicated with paralysis, and they elucidate, therefore, the connection of these affections: but they certainly teach us little or nothing of that actual condition of the nervous mass upon which the epileptic paroxysms depend. And, in truth, to expect to find in the brain the traces of convulsions that have passed away, would be as unreasonable as to expect to find the traces of former voluntary movements.

Of those organic changes which may be regarded as strong predisposing causes of the paroxysms, my own experience accords with that of Dr. Bright; who states, that they are more frequently such as affect the surface, than the deeper-seated parts of the brain: tumours external to the cerebral matter, alterations in the bones of the skull; or in the membranes that envelope the organ. Various altered states of the spinal marrow have also been recorded.

But besides the morbid appearances that are *sometimes only* visible in the nervous centres themselves, there are others, which it is of great importance to attend to, situated in other parts of the body, and at a distance from those centres: diseased states of the liver; biliary concretions; renal calculi; stones in the bladder; worms in the alimentary canal; diseases of the uterus; and of various other parts. And these morbid conditions have often, no doubt, an intimate connection with the epileptic paroxysms. Accordingly some authors make almost as many varieties of epilepsy as there are organs of the body; they specify the cerebral, the spinal, the cardiac, hepatic, gastric, intestinal, nephritic, genital, uterine, and so on. It will be sufficient, however, to consider two species only; that, namely, in which the disease originates in the nervous centres themselves, and especially in the brain; and that in which it originates in some other part. Most persons who have written on epilepsy make this distinction, although they employ different terms to express it. Cerebral and occasional; primary and secondary; idiopathic and sympathetic; centric and eccentric. The two last terms are the best. But let us clearly understand them. The disease may, in one sense, be considered eccentric, even when it is situated in the brain; eccentric, *i. e.* in respect to the true spinal marrow. But I apply the epithet centric to epilepsy when its cause lies in either of the two great nervous *centres;* the brain, or the cranio-spinal axis. The distinction itself we shall find to be an important one, both as regards the prognosis and the treatment. But I must first say a few words respecting the causes, and the diagnosis of epilepsy.

There is no doubt that a tendency to epileptic disease is frequently *hereditary*. It may be derived from parent to child; or it may skip over a generation or two, and appear in the grandchild or great-grandchild; or it may be traceable only in the collateral branches of the ancestors. This is just what takes place in other hereditary maladies. You may often notice also that other forms of nervous disorder prevail in the same families.

MM. Bouchet and Casauvielh found that among 110 instances of epilepsy, 31 were hereditary. Of 321 persons afflicted with epileptic insanity, and seen by Esquirol, 105 were descended from insane or epileptic parents.

Again, a tendency to epilepsy is very often found to go along with an *unnatural form of the head,* which is pinched up like a sugarloaf; or misshapen and unsymmetrical, one-half being unlike the other; or oddly configurated in some way or other. Epilepsy is no uncommon attendant of chronic hydrocephalus.

And thirdly, the *scrofulous diathesis* is a strong predisposing cause of epilepsy. Dr. Cheyne even holds that epilepsy is as certain a manifestation of the strumous disposition, as tubercular consumption, or psoas abscess. Now of the two predisposing circumstances last mentioned, it may be observed, that they commonly merge in that which preceded them: the strumous diathesis, and a particular conformation of the head, are both very likely to descend from parents to their progeny.

Whether the sex has any influence in determining a predisposition to epilepsy, is a question that remains to be settled. Foville thinks it is most common in females; Dr. Elliotson in males. *I* have certainly seen more epileptic boys and men, than girls and women. But the casual experience of a single observer is not enough to determine the point. We

want numerical statements on a large scale. At the close of the year 1813 there were 162 male epileptics in the Bicêtre; 289 female cases in the Salpétrière. Jos. Frank observed that, of 75 patients, 40 were females.

LECTURE XXXVI.

EPILEPSY, CONTINUED. RECAPITULATION. EXCITING CAUSES. SIMULATED EPILEPSY. DIAGNOSIS. PROGNOSIS. TREATMENT DURING THE FIT; DURING THE INTERVALS; DURING THE WARNINGS.

ON Tuesday last I began to speak of epilepsy: but as some of the gentlemen belonging to the class were prevented by the weather, or by other circumstances, from being present at our first reassembling after the holidays, I will very rapidly retrace the substance of the last lecture.

An epileptic seizure may be very severe; or very slight. The very severe attacks are characterized by a sudden cry, an immediate loss of consciousness, general and violent convulsions, and subsequent coma or heavy sleep. The very slight attacks consist in a momentary abeyance of the mental faculties, sometimes with and sometimes without slight and partial convulsion. These extreme forms of epilepsy we judge to differ only in degree, inasmuch as they both attack the same persons at different times; or the one form conducts to the other. Between these the gradations are innumerable. We call the extremes the *epileptic fit*, and the *epileptic vertigo;* the French call them the *grand mal* and the *petit mal.*

These *fits* may last from a few seconds to half an hour. Paroxysms apparently longer than this commonly consist of a succession of fits. The average duration is from five to ten minutes.

The fits recur at variable intervals; which are sometimes periodic, mostly irregular. There may be many in a single day; there may be only one for many years. They are commonly more severe in proportion as they are less frequent.

The epileptic seizures sometimes begin in early infancy: another period at which they often commence is about the age of seven or eight: another about fourteen or sixteen, or for some few years after that age. They more often begin before puberty than after it. Sometimes the first fit takes place in the middle period of life; sometimes even in declining age. They often occur in the night, especially in the outset and the decline of the disease; usually when the patient is between asleep and awake; *i. e.* at the commencement or the termination of his slumber.

In the majority, perhaps, of cases, the fit is unexpected, and preceded by no warning. But in other instances there is some alteration perceptible by the patient himself, or by his friends, giving notice of its approach; some change in the temper, feelings, appearance; some disturbance of the senses; ocular spectra, or what is called the epileptic aura, a creeping sensation arising in some part of the surface, generally of the extremities, and gliding towards the head. Some of these warnings precede the paroxysm by a day or two, or a few hours; some by two or three seconds only. Sometimes the blow is threatened by their appearance, but it does not fall.

The fit is almost always, in its severer forms, attended and followed by coma; sometimes after the coma by temporary confusion of mind; deafness; slight paralysis; delirium; inarticulate speech. There is seldom any marked permanent alteration effected by a single fit.

A repetition of the fits leads, in a large majority of instances, first, to a defect of the memory, and of the general intelligence; and at length to a peculiar expression of countenance, to decided imbecility of mind, to complete fatuity; and with this there is often associated some paralysis or muscular debility.

The convulsions take place, necessarily, through the medium of the spinal cord and nerves—just as voluntary movements do; but the suspension of sensation, thought, and volition (which suspension is seldom absent, while the irregular muscular action often is wanting) shows that the *brain* is essentially involved in the disease.

Accidental organic lesions are sometimes (and sometimes only) found in the encephalon, or in the spinal cord, of persons who have suffered epilepsy uncomplicated with any permanent mental or paralytic affection: tubercle, for example, or bony growths from the interior of the skull; but as these are *constant*, and the paroxysms are *occasional*, and as in the intervals they give no signal of their presence, we can only regard them as being probably predisposing causes of the seizures.

When the epilepsy *has* been complicated with permanent alienation of mind, or some degree of paralysis, evidence of chronic inflammation of the brain and its membranes is generally discovered. This has been the consequence of the repetition of the paroxysms. This explains the frequent connection of fatuity and palsy with epilepsy of long standing.

The diseased condition which excites the paroxysms may be situated in the nervous centres themselves, or in some other part of the body. In the one case we call the disorder centric, or idiopathic; in the other, eccentric, or sympathetic. We cannot always be sure with which species of the disease, the centric or eccentric, we have to deal: but the distinction, when it can be made, is of considerable importance, in respect to the prognosis, and in respect to the management of the case.

The predisposition to this fearful complaint is often hereditary: malformation, or defect of symmetry in the two sides of the head, is a frequent predisposing cause; so, pre-eminently, is the scrofulous diathesis; and these two, viz. the scrofulous diathesis, and a peculiar conformation of the head, are both liable and likely to be propagated from parents to children. But the predisposition is found to be hereditary even when the shape and structure of the body is, to all appearance, quite perfect and natural; and when no outward indication of the strumous diathesis is perceptible.

At the very close of the lecture I informed you that it is an unsettled question—and it is not a question of very great importance—whether the disease is more common in females or in males: whether the *sex* has any thing to do with the predisposition.

There are certain vices, which are justly considered as influential in aggravating, and even in creating, a disposition to epilepsy: debauchery of all kinds; the habitual indulgence in intoxicating liquors; and, above all, the most powerful predisposing cause of any, not congenital, is masturbation—a vice which it is painful and difficult even to allude to in this manner, and still more difficult to make the subject of inquiry with a patient. But there is too much reason to be certain that *many* cases of epilepsy owe their origin to this wretched and degrading habit: and more than one or two patients have voluntarily confessed to me their conviction that they had thus brought upon themselves the epileptic paroxysms for which they sought relief.

Exciting causes.—Among the *exciting* causes of epilepsy, fright is conspicuous. And any *strong mental emotion* is apt to produce the fit, in a person who is already subject to the disease. This fact alone would be enough, I conceive, to forbid our ascribing the paroxysms exclusively to an affection of the spinal cord. Bodily pain; manifest and great disturbance of almost any of the principal functions of the body; may act also as exciting causes. Sometimes the cause is obvious, sometimes it is quite inscrutable. If the attack occurs every night, Dr. Bright thinks it may be attributed to the "congestion" of sleep: if it takes place at monthly intervals in women, we may "often trace it to nervous irritation in sympathy with the uterus; and when long periods have intervened we may usually trace each distant paroxysm to the repetition of some excess, or to a neglected state of the bowels." In these latter cases, the epilepsy is of the sympathetic, or eccentric kind; the irritation being seated in some part at a distance from the nervous masses, in the stomach, or intestines, or uterus. Now I would suggest the expediency of observing what muscles or sets of muscles are *first affected* by the spasm in such cases, and in what part the warning aura (if there be any) arises: because by accurately noting these particulars, we may be led to a knowledge of the part or organ in which the irritation operates: and if we know the *seat* of the irritation, we shall be more likely to know its *nature*, and its *cure*.

Among the exciting causes of epileptic fits are also enumerated—and I believe, from

what I have myself noticed, with great justice—the repulsion of eruptions, and especially of eruptions about the head, when proper artificial evacuations are not employed at the same time; the cessation of habitual discharges; and, on the other hand, profuse and unusual discharges. Hæmorrhage certainly will often bring on convulsions and a state of insensibility exactly like certain forms of epilepsy. Persons who are bled till they actually faint, are often at the same time convulsed. And animals that are killed by loss of blood, are always affected with convulsions before they expire.

There is yet another very singular occasional cause of epilepsy that deserves to be mentioned; viz. the sight of a person in a fit of that disease. This has been noticed over and over again. Not only will a patient who has already suffered such attacks often fall into one upon seeing another so affected; but people will even sometimes do so who have never before shown any symptom of epilepsy. In this way the disease will now and then run through a boarding-school; or through a ward in a hospital. There is a very good example of this recorded in the 11th volume of the Medical Gazette, by Dr. Hardy, of Bath. A strong healthy young man was hired to take care of an older patient, who suffered frequent and exceedingly violent paroxysms of epilepsy. He remained with the patient night and day; and at the end of seven weeks became himself epileptic in a very high degree. An acquaintance of his, of equally robust make, but some years older, occasionally visited the parties. In a fortnight from his first visit he also was seized with similarly violent attacks. Dr. Hardy quotes the following short case also from Baglivi:—"Vidimus, anno 1690, in Dalmatiâ juvenem gravissimis correptam convulsionibus, propterea quod inspexerat solummodo alium juvenum dum epilepsiâ humi contorquebatur."

Dr. Cullen, who, as well as many others, had noticed the same thing, starts the question whether this mode of propagation of the disease is imputable to dread and horror; or to the mere force of imitation, which is often so strong, in health as well as in disease: and he decides in favour of the force of imitation: and in fact there are many other sights equally horrifying with that of a person in convulsions; yet there is *no spectacle* of horror so efficacious in producing a fit of epilepsy in others, as that of a person suffering under epilepsy.

This principle of imitation holds good in many of the spasmodic diseases; and in some of them, especially in hysteria, its influence is more remarkably seen than in epilepsy; and I shall therefore have to recur to it again. There is one very curious fact, however, which relates to epilepsy in particular. You are aware that this disease is often feigned, by impostors. Now I believe it is ascertained beyond the possibility of doubt that fits and actions which were at first, in these pretenders, strictly voluntary, have at length become involuntary and uncontrollable, and have passed into paroxysms of real epilepsy. The rogue is caught in his own trap.

And the mention of these impostors leads me to consider the *diagnosis* of epilepsy. First, how are we to distinguish the feigned disease from the true? Secondly, are there any other real diseases which may be mistaken for epilepsy, or for which epilepsy may be mistaken?

Simulated epilepsy.—In the number of *feigned* diseases epilepsy is one of the most common. Soldiers and sailors pretend to have epileptic fits, in the hope of obtaining their discharge from the service. Cases of simulated epilepsy occur also continually in our streets among mendicants, and impostors who think to excite the compassion and pecuniary charity of the credulous; and are even sometimes actuated I believe by a desire to obtain admission into hospitals, where they live tolerably well, and quite idly. It is easy enough, they think, to throw their legs and arms about, and to grin; and many of them get up a capital show of foaming at the mouth, by placing a bit of soap between the gums and cheek. The means of detecting these vagabonds are of some importance to us all; and it is more particularly necessary that they should be well known to those who are likely to join the medical department of our fleets or armies.

It is of course desirable, in questionable cases, to witness a fit. But pretenders are not very willing to perform when they know that a medical man is looking on. You may sometimes convict them, in the absence of the fits, by cross-examination. A cheat will seldom be consistent in the account which he gives of his fits; as to whether they are regular or irregular; and as to the times and places in which he has suffered them. An

impostor chooses such situations for his exhibition as are most suitable to his own purposes; a crowded street, or a well-frequented public walk. True epileptics almost always select retired places to take exercise in; especially if they have any warning or expectation of the approach of a paroxysm. You will find also that the impostor is not attacked at his own home; but always fixes upon some spot in which he is not only sure to be seen by others, but in which he is not likely to sustain any injury by tumbling down. True epileptics often get themselves seriously hurt by their falls; feigned ones generally come off without much bodily damage. However, if the fits are alleged to be frequent, and if also they are regular, you may soon expect one, and must make a point of being present; and then you will seldom fail to remove or verify your suspicions. In the first place the muscular power of epileptics is far beyond what is natural. It will sometimes take four or five stout men to hold a weak emaciated lad, in a fit of epilepsy. Of course no impostor can command more than his natural strength. In the second place a real epileptic fit, if it lasts long, is seldom violent; whereas impostors, for obvious reasons, make their fits both long and violent. You may often get much information from the state of the eyes, which in true epilepsy are generally partly open, with the eyeball visibly rolling and distorted. In feigned epilepsy the actor almost always prefers to shut his eyes completely. Sometimes, if he be closely watched, and no suspicion is expressed, he will be seen to open his eyes occasionally, to ascertain the effect of his exhibition upon the bystanders. In real epilepsy, too, the pupils are often considerably dilated, and do not contract when stimulated by light. This is a very sure criterion; for no impostor can prevent his iris from acting on exposure to vivid light. The pulse, in true epilepsy, is not only frequent but often irregular also; a circumstance which never can be imitated. The skin of an epileptic, during the fit, is commonly cold, but that of an exhibitor is hot, and covered with sweat, obviously the consequence of his violent and voluntary exertions. In this respect, also, it is scarcely possible for him to deceive us. Again, an impostor will not bite his tongue, as epileptics often do: nor very willingly void (like them) his excrements and urine during the fit: indeed it would not be very easy for him to do so, and at the same time to carry on the necessary pretence of convulsions. Besides, epileptics, during a fit, are quite insensible to external impressions; and hence the vulgar modes of detection, though harsh and not to be recommended, are often effectual ones; such as dropping melted sealing-wax upon the patient; putting some gin in his eye: burning him with a hot poker; or (what I believe is more fashionable among beadles and police constables, when they have to administer to such patients) the pressing your thumb-nail violently under that of the supposed impostor. This causes exquisite pain, yet inflicts no lasting or serious injury; and I believe that few pretenders stand out against this expedient. It is astonishing, however, how much torture some of them will bear before they can be brought to confess their imposition. If we speak of having recourse to some of these painful tests in the hearing of the pretender, we shall find that the fit will soon come to an end. Dr. Cheyne mentions an instance in which one table was placed upon another, and a soldier, who was supposed to be shamming, was laid upon the upper one, while his paroxysm was on him; and the fear of falling from such a height soon stopped the convulsions. Mr. Hutchinson relates the case of a sailor who was suspected to be a cheat, in whom the convulsions were instantly removed by blowing some fine Scotch snuff up his nostrils through a quill. This brought on *another* kind of fit, viz. a fit of *sneezing*, which lasted nearly half an hour: and there was no return of the epilepsy as long as Mr. Hutchinson remained in that ship. He tried the same expedient in cases of *real* epilepsy, but never could produce any similar effects, although the patients were not snuff-takers. There was a beggar in Paris, who often fell into epileptic fits in the streets: one day some compassionate spectators, fearing that he might injure himself in his struggles, got a truss of straw and placed him upon it; but when he was in the height of his paroxysm, and performing remarkably well, they set fire to the straw; and he presently took to his heels.

There is another ingenious plan, very likely, I should think, to detect an impostor, and yet not calculated, like the one last mentioned, to injure a real sufferer; which is, to propose, gravely, in his hearing, to pour *boiling* water upon his legs, and then to proceed actually to pour *cold* water upon them.

Diagnosis.—Of the *real* diseases which are apt to be confounded with epilepsy, *hysteria* is the chief. The question whether a given case be one of epilepsy or of hysteria, very often arises. By a careful attention to several circumstances, the discrimination is generally to be made. In the first place the total suspension of consciousness, which is so constant an accompaniment of the epileptic paroxysm, does not take place in the hysterical: in epilepsy there is no globus hystericus, no alternations of laughter and tears: the solitary cry which ushers in the epileptic attack so frequently, and which is so characteristic, is not heard in hysteria; not that hysterical girls do not scream, for they often do; but then it is repeatedly and continuously. The heavy comatose sleep that succeeds epilepsy is not common in hysteria. Hysterical patients contrive also to avoid hurting themselves by their contortions: they do not bite their tongues, nor foam at the mouth. Dr. M. Hall tells us that, in epilepsy, there is a forcible closure of the larynx, and expiratory efforts which suffuse the countenance, and probably congest the brain, with venous blood. In hysteria, the respiration, on the contrary, is rapid and sobbing.

Observe that I have been speaking, all along, of what has been sometimes called *habitual* epilepsy. It is not every attack of convulsions with insensibility which ought to be so named. Such attacks are apt to follow sudden injuries done to the brain; stunning blows on the head, fractures of the skull, the eruption of the blood in sanguineous apoplexy, and even overwhelming emotions of the mind. With these casual occurrences of epileptiform convulsion I do not here meddle.

Prognosis.—Epilepsy is one of those complaints concerning the *probable issue* of which the patient, and still more the patient's friends, are sure to make repeated and anxious inquiries. It is seldom that we can pronounce with any confidence a favourable prognosis; but there are some cases in which the prospect is much worse than in others.

If we have any reason to believe that the disease is *centric*, and connected with any organic derangement of the nervous centres themselves, the prognosis must be bad. *Cæteris paribus* it is rendered worse by the coexistence of any sign of scrofulous disease, or of the well-known bodily characteristics of the scrofulous diathesis: it is rendered worse, also, when the disease has occurred in the parents, or among the more immediate ancestors, of the patient; whenever, in short, there is reason to think the disposition to it is inherited. The prognosis is bad when the complaint occurs in persons who have slanting foreheads and misshapen skulls; and when the epileptic physiognomy has become established. The prognosis is always the more unfavourable the longer the disorder has lasted; the oftener the fits have been repeated; and the more habitual they have become. And when the memory is permanently enfeebled, or fatuity has come on, or the disease is complicated with any form or degree of paralysis, the case is hopeless; so far, at least, as a perfect cure is concerned.

On the other hand the prognosis is better when the disease is *eccentric; i. e.* when there is any obvious exciting cause of the paroxysms, manifest in structural or functional disorder of some part of the body other than the nervous matter. And when this eccentric cause is removable—a stone in the bladder for instance, worms in the intestines—then the prognosis still farther improves. On this account the prognosis is better in children than in older persons, for the exciting cause is often clearly eccentric, and likely to be transitory; the irritation of teething for example: and besides this, it is stated by many practical writers that even repeated and habitual attacks of epilepsy in children often go off as the patients grow older; and especially at the age of puberty. The experience of Heberden, however, was against this. He says that he had known several persons *become* epileptic at that time; but that he had never met with one who had *then* got rid of the disease. He had known a few who had recovered before, and some after the age of puberty. Dr. Elliotson mentions a case in which a girl had epilepsy prior to the first period of menstruation: then the fits stopped; and she remained free from them until in advanced life the catamenia ceased to recur; and then the epilepsy returned. In all those cases in which we can assign some evident cause for the fit—such as the use of improper food, uterine irritation, mental emotion, and so on—the prognosis is somewhat better than usual. "The eccentric epilepsy (says Hall) is to be viewed as *curable*, however *difficult* of cure." And however unfavourable the prognosis may be, there is nothing that can excuse any apathy or neglect of it on the part of the practitioner. Though few cases of

habitual epilepsy admit of a *cure* under any treatment, yet there are few which may not be *relieved* by treatment, so far as regards the *frequency* or the *violence* of the fits, or *both*.

Treatment.—The treatment of epilepsy resolves itself into the measures to be adopted during the fit; and the measures to be adopted during the intervals between the fits.

During the fit.—In the paroxysm itself we have to provide against the risk of injury from the struggles and contortions of the patient; and if possible to mitigate the violence, and to shorten the duration of the fit. The patient should be placed in the centre of a large bed; his neckcloth, and any ligatures about his person, should be loosened; his head should be somewhat elevated. When the risk of his hurting himslf cannot be avoided in any other way, his limbs should be restrained by the bystanders, or secured in a waistcoat. Some persons have advised that a piece of cork or soft wood should be placed between his teeth, to prevent him from biting his tongue, or breaking his teeth. But it is not easy to manage this expedient cleverly. If the head be visibly congested, and hot, cold wet cloths may be applied to it with propriety; and if, at the same time, the extremities be cold, means of restoring warmth to them should be adopted.

I do not know whether art can abbreviate the paroxysm. Some years ago the late Barry O'Meara wrote a letter to some of the newspapers, saying that he fancied he had seen a popular remedy useful in such cases; that, namely, of cramming salt into the patient's mouth: he thought he had succeeded in bringing the patient about by that expedient. Of the epileptic patients that come into hospitals, the physician, not being always on the spot, does not see all, or even many of the paroxysms; but after reading that letter, I desired the nurses to treat all my patients who might be seized with epilepsy in the wards upon that plan: and upon comparing the length of the paroxysms when the salt was used, with their ordinary duration as reported by the friends of the patient, or as previously observed in the hospital during some of the earlier fits, it certainly did seem to curtail the convulsions. Probably it is more calculated to relieve an hysterical than an epileptic fit. In the epileptic fits of children much benefit often results from immersing them in warm water: particularly if there be any coldness of the extremities.

It is very much the fashion to bleed persons who are seen in a fit, of whatever kind; and to bleed them largely. I have already given you my opinion of the indiscriminate use of this decided measure in apoplectic attacks. If it be clear, from the phenomena, or from the known history of the patient, that the case is one of epilepsy, bleeding, *during the fit*, will seldom be necessary or proper; unless, indeed, the evidence of cerebral plethora is very strongly marked: and even then I would advise you not to do more than take a moderate quantity of blood, by cupping, from the neck or temples. The convulsions and the sopor may be expected soon to pass off; as soon, probably, and as completely, without as with any abstraction of blood. Whereas the difference of the alternative is not trifling, in respect to the condition in which the patient will be left when the fit is over. The injurious effect of excessive blood-letting upon the system at large, is manifest, sometimes, for months afterwards.

During the intervals between the attacks we seek to prevent their recurrence; and this end is to be attained, when it is attainable at all, by getting rid of the predisposition to the disease, on the one hand, and by protecting the patient against its exciting causes on the other. Now there are certain kinds and causes of predisposition which we *cannot* get rid of; such are the tendency that is *inherited;* the strumous diathesis: malformation of the head; the presence of some organic lesion in the brain or spinal cord. Vicious and dissolute habits are also difficult, but not impossible, to eradicate. It will be our duty, when such are discovered, to set strongly before the unhappy patient the dreadful end to which he is hastening; the certain loss of reason to which, when once the disease has shown itself, the continuance of his baleful indulgences will drive him; and to urge upon him the necessity for a short and sudden turn on his part, if he would expect any aid from medicine. Where no physical cause of the proclivity exists, or can be detected, it is of much importance to ascertain whether there be any deviation from the standard condition of health; towards general plethora in the one direction, or towards emptiness and asthenia in the other. The first of these unnatural states may be redressed by regimen and exercise, by abstinence from stimulating food and drink, and by a slender diet also; and, if need be, by direct depletion. The second, which, perhaps, is the most common of the two, and

which often leads (as I have explained before) to *local* plethora, may often be removed or lessened by a tonic treatment. The object in both cases is to give stability and firmness to the nervous system; to diminish that mobility, or readiness to be impressed, which is so strong a characteristic of the class of patients affected with epilepsy, although it may not be very apparent in some few individuals among them. It is upon this principle that *mineral* tonics sometimes do good in epilepsy, and not by any *specific* virtue which they possess in restraining the fits.

It is owing, perhaps, to a neglect of these two somewhat opposite conditions of general plethora and general debility, or to the difficulty that sometimes is met with in distinguishing these conditions, that such a variety of opinions have been expressed concerning the proper treatment of habitual epilepsy. Plethora is to be reduced without causing hurtful debility: tone is to be given without inducing dangerous fulness. It requires some nicety to carry the balance even; to attain the hoped-for good, and at the same time to avoid the evil that is apt to wait upon it. In very many cases the requisite extent and measure of the tonic plan on the one hand, or of the lowering system on the other, can only be learned by careful trials. But sometimes the indications of treatment are more plain. When the patient is young and strong, and full of blood, and not of a particularly movable temperament; when he has a hard pulse, or any degree of feverishness; when the disorder has supervened upon the suspension of some customary discharge, so that there is an obvious cause of plethora; and when the disease is in its early stage, and the recurrence of the fits has not yet been established by habit; in any or all of these circumstances it will often be proper to abstract blood from the patient, and it will always be right to purge him actively, and to insist upon an abstinent regimen. When former paroxysms have been preceded by signs of fulness of the vessels of the head, by headache, for instance, throbbing of the temporal arteries, distension of the superficial veins, a flushed or loaded countenance, you may sometimes, by a timely use of the lancet or the cupping-glass, avert an attack that was apparently impending.

On the other hand, if the patient is pale and weak; or unduly susceptible; or if his malady has been fastened upon him through many repetitions of the fit; you will generally find that any form of active depletion is injurious, and learn to place your best hope in measures which are calculated to invigorate the frame.

One of the most useful of the particular remedies employed for strengthening the body, is the cold shower bath. This tends, more perhaps than any other single measure, to give permanent firmness and stability to the system. The best test, in all cases, of the tonic and bracing effect of this remedy is the occurrence of a pleasant and general glow after each application of it. It is the only safe mode in which the cold bath can be used by an epileptic person.

You will find, in books, a great many tonic medicines recommended for this disease, which medicines you will have opportunity and ample time for trying. Of the mineral tonics, the salts of silver, zinc, copper, and iron, have been chiefly praised. The nitrate of silver used to be highly thought of; but there is one very serious objection to it which must never be forgotten: viz. that it is apt to produce a permanent discoloration of the skin, a frightful lead colour. There is a footman in a house near Cavendish Square who has been thus blackened: and there is a gentleman of property resident at Brighton in the same predicament: his face looks as if it had been thoroughly and carefully pencilled over with plumbago. A barrister and friend of my own had a narrow escape from a similar misfortune: in fact his skin has acquired a just perceptible tinge of gray. Now if the remedy were sure to cure the disease, I am not certain that every one would accept of a cure on such terms. It would be proper, even on that supposition, to tell the patient that though he (or, *a fortiori*, she) would get rid of the epilepsy, there was a likelihood that this unamiable complexion might ensue. But the truth is that in giving this nitrate of silver we run a great risk of obtaining its disfiguring effect, for the sake of a very small chance of curing the epilepsy. I have been assured, by one of his friends, that the Brighton gentleman has carried a dark outside for a quarter of a century at least; and that he is as subject to epileptic fits now as ever he was. If the lunar caustic is to do good, it must be given for some time together, and the probability is that it will not do good even then; and if it be given for some time together, there is great danger of its

changing the colour of the skin. For these reasons I never give it myself, and therefore I cannot recommend it to you. If you wish to try it, or if you have a patient that insists on trying it, as some will, you may begin with half a grain in a pill three times a day; and the dose has sometimes been carried as high as fifteen grains. And it is worth observing that in the larger doses this drug proves purgative: it is possible that its good effect, when it has any, may be attributable to its operation in that way.

There is no danger of spoiling the beauty of your patient by administering the oxide or the sulphate of zinc; or the cuprum ammoniatum. The liquor arsenicalis has been thought useful; but it requires to be exhibited with great caution. Of all the metallic remedies I should prefer some preparation of iron. I think I have seen much good done by the vinum ferri; not by any specific agency, however, but by its giving what is called tone to the nervous system, and rendering it less prone to be affected by the slighter exciting causes of the disease. I cannot pretend to weigh the merits of the long list of substances which have been lauded as efficacious in keeping off and curing the disease; and which, when they have been useful at all, have operated, I conclude, in diminishing the disposition to epilepsy by corroborating the nervous system. The most renowned of them are valerian, assafœtida, wormwood, the misletoe of the oak, the cardamine pratensis, rue, the sedum acre, indigo; narcotic vegetable preparations, stramonium, belladonna, hemlock, lettuce; animal substances, musk, castor, ox-gall; and the number might be many times multiplied: and this long array of drugs, all of which have been known, or supposed, to accomplish a cure, affords, in truth, one of the strongest evidences of the intractability of the disease under any plan of treatment. There is a shrewd remark of Esquirol's, which I believe to be quite true, however difficult it may be to account for the fact, which is, that epileptics are apt to improve for a time under every new plan of treatment.

Whatever drug you may see reason to prefer (and the patients will have drugs, and you must be prepared to ring the changes upon them), there are certain other points in the management of the disease which are of considerable importance. The patient who is subject to epilepsy should live by rule, and be temperate in all things. His diet should be simple, nutritious, but not stimulating; he should renounce all strong liquor, and become, in the new-fangled and vulgar phrase, a tee-totaller. He should rise early, and take regular exercise in the open air; keeping his head cool, and his extremities warm. He should avoid all mental excitement, and the fatiguing pursuit of what is called pleasure; all probable sources of sudden anger, surprise, alarm, or deep emotion of any kind; all striving and contention of the intellect. The student, of whatever age and sort, in whom epilepsy has declared itself, should shut his books, the man of business abandon or abridge his professional toil: at least they must be instructed to abstain habitually, in their respective callings, from such application as would task and strain their powers, whether mental or bodily; and endeavours should be made to engage their thoughts and to interest their minds in less engrossing objects of attention. No minute rules can be laid down on these points, but, keeping the general indication in view, it will seldom be difficult to follow it up in practice.

When the fits appear to have been brought on by a species of moral contagion, or by imitation of the same disease seen in others, care should be taken to exclude as much as possible those objects or trains of thought which produce the mental emotion or the morbid propensity. In these cases, and, indeed, I may say in almost all cases, it is more rational to expect benefit from such measures as tend to calm the mind and to fortify the nerves, than from this or that substance thrown at random into the stomach.

There is sometimes cause for suspecting that epileptic fits depend upon a syphilitic affection of the bones of the skull; I am much mistaken if I have not seen such cases. When that suspicion arises, it will be proper to give mercury a full and fair trial. Such a plan has been followed by success. I should always premise, however, in such cases, the iodide of potassium; the efficacy of which in getting rid of syphilitic nodes is no longer doubtful. I am accustomed to recommend a gentle and long-continued course of mercury whenever organic disease of the brain is suspected; the influence of that remedy being carefully watched. It will be right and proper also to try the effect of counter-irritation; of blisters, a seton in the neck, or the tartar emetic ointment. But I must

confess to you that, often as this expedient is employed, I have seldom witnessed any such result from it as would encourage me to expect benefit from repeating it in another case. There is one form of counter-irritation which I have never seen put to the test, but which has of late been very strongly recommended by a very able and observing physician, Dr. Pritchard; and which I have heard very good accounts of from a gentleman who had seen it extensively employed in Bristol; I mean the making a long issue in the head itself, dividing the integuments down to the bone by means of a scalpel in the direction of the sagittal suture, and keeping the incision open and discharging for some time, by means of issue peas. The formation of the issue is said to be not so painful as one might suppose.

Dr. Quain, in his edition of *Martinet's Pathology*, relates the following case:—"Some years ago I saw a boy who was epileptic from infancy, and who, in one of his usual fits, fell over a cliff by the sea-side, and received a very severe lacerated wound of the scalp, which healed slowly, and with a copious suppuration. While the discharge continued he was free from any epileptic attack; but as soon as the wound healed, the fits returned as before."

Twice I have seen similar good effects from the insertion of a seton in the neck. Twenty times that measure has disappointed my hopes.

When the disease is ascertained or believed to be of the eccentric kind, we must search diligently to find the seat of the distant irritation, in some disturbance of function; and apply our remedies accordingly. The irritation may be found, as I have already intimated, in almost any organ of the body. Painful or irregular dentition is perhaps one of the commonest of the eccentric sources of epilepsy. Sometimes the attacks are attended with symptoms of disease in the liver; slight yellowness of the skin, uneasiness and tenderness of the right hypochondrium, and lowness of spirits. In such a case we must rectify that state of the liver, by such means as I shall have to specify hereafter. If the disorder depends on a stone in the bladder, the cure must be committed to the surgeon. I have a patient at present under my occasional inspection, who from time to time has slight fits of epilepsy; on most occasions he passes about the same time a small calculus by the urethra. I make no doubt that in his case the exciting cause of the epilepsy lies in the kidney.

You will find that most persons, in respect to such diseases as that which we are now considering, have some favourite or usual mode of treatment; and if I were called upon to name any single drug, from which, in ordinary cases of epilepsy, I should most hope for relief, I should say it was the oil of turpentine. And I find that other persons have come to the same conclusion. Dr. Latham the elder was, I believe, the first person who made known its efficacy in this disorder. Foville states that he has seen excellent effects from it. It is highly spoken of by Dr. Percival, in the Dublin Hospital Reports. It is not to be given in large doses, but in smaller ones that are frequently repeated; from half a drachm to a drachm every six hours. You are aware that it sometimes produces strangury, and therefore the patient must be forewarned of this, or carefully watched. Occasionally turpentine has done good in virtue of its anthelmintic properties. I know that a physician of my acquaintance cured a case of epilepsy in this way, somewhat to his own surprise. Without having in his mind any notion of worms, he thought it well to purge his patient, who had laboured under epilepsy for some time, with the oleum terebinthinæ. The patient, who is the brother of a person holding at present a high office in this country, was residing two or three miles out of town. In the middle of the night the Doctor was summoned to him in a great hurry; the messenger said he was supposed to be dying. He was only intoxicated, however, by the free dose of turpentine he had swallowed: the next morning he voided into the close-stool a large tape-worm; and he has never had epilepsy since. The late Lord Hardwick, the father of the present Earl, had epilepsy, and he too got rid of his epilepsy and of a worm at the same time. I believe that the cure was effected by turpentine in his case also; but I am not certain of that. Such cases are remarkably interesting: they show that irritation of the stomach or intestines may be sufficient to cause the fits; they illustrate extremely well the eccentric form of the disease; and they deserve to be always borne in mind when we are asked to prescribe for an epileptic patient. A cure from so dreaded a complaint, by such simple

means—the cause of his malady, and the certainty of his having got rid of that cause, being both so obvious and intelligible to the patient, may be enough, sometimes, to make a practitioner's fortune. But I think you will sometimes find the oil of turpentine very useful, even though it expels no worm, and when there is no worm to expel. If the bowels should be costive, the oil of turpentine and castor oil, in equal proportions, go exceedingly well together.

During the warnings.—When the patient has a distinct warning of an approaching paroxysm, can any thing be done to ward it off? Why, in some cases, by interrupting the precursory symptoms, it certainly may be prevented. A pupil of the class informs me that a brother of his, 12 or 13 years old, has been subject to epileptic fits for two years. They occur in the night, especially if he is waked, even though the awakening cause has no tendency to startle him. He often is dull and drowsy the evening before, and if he is roused from this lethargic state by conversation, or amusements, the attack expected that night sometimes does not happen. Another student knows a young girl, in whom the occurrence of very high spirits is always precursory of the paroxysm: when this extreme vivacity is moderated by those about her, the threatened fit is sometimes averted. I mentioned before an instance in which the aura, proceeding from one of the thumbs, was frequently checked by tying a ligature tightly round the thumb. Other examples of exactly the same kind are on record. Mr. Wardrop cured a case, beginning with aura in one finger, by amputating a joint of the finger. Dr. M. Hall states that the immediate accession of the paroxysm may sometimes be prevented by dashing cold water on the face, or by exciting the nostrils by snuff. In this manner the disposition to closure of the larynx and to expiratory efforts is exchanged for sudden acts of inspiration. Another patient of my own, an old College friend indeed, who is afflicted with epilepsy, feels convinced that he sometimes prevents a fit by applying smelling salts to his nose; and he always carries a bottle about with him for that purpose: but, unfortunately, the warning (which consists chiefly in giddiness) is generally so short, that he has not time to have recourse to his preventive before he falls down. It is a question whether the fit may not be obviated by a strong mental effort in some cases. I make no doubt that it may, especially in the imitative form of the disease, which originates in and depends upon mental and moral causes.

It is scarcely necessary that I should do more than advert generally to those precautions which every one who is subject to epilepsy ought to observe, and which it is the business of his medical adviser to enforce both upon the patient himself, and upon his friends. His bed should be large; or if not large it should be enclosed with some netting or other defence against his falling out of it. If he sleeps in a room by himself, care should be taken that, in the winter, a proper temperature is kept up, for if in his attacks he gets out of bed on the floor he may be seriously injured by the cold. He should not, however, be left alone if it can be helped. Guards should be placed over every grate near which the patient may come; he should avoid ascending and descending stairs as much as he can; he should not ride on horseback, nor on the top of a coach, nor even in a gig; nor go about, especially in solitary places, without an attendant. A patient of Dr. Cheyne's, a young man of 20, was drowned in his own garden by falling into a little runnel of water which was not four inches deep. Neither, on the other hand, will it be proper or safe for him to frequent crowded or hot rooms; or the streets of a populous town, in which the multiplicity and distraction of objects are apt to produce, even in a healthy person who is not accustomed to them, a degree of vertigo and confusion. Dr. Cheyne advises that when the patient's circumstances will admit of his having a constant attendant with him, the latter should be provided with some diffusible stimulus; a potion, for example, composed of camphor mixture and æther, by swallowing which the impending paroxysm may sometimes be repelled.

LECTURE XXXVII.

CHOREA. SYMPTOMS; PATHOLOGY; COMPLICATIONS; CAUSES; TREATMENT. CHRONIC CHOREA. OTHER NERVOUS DISORDERS TO WHICH THE SAME NAME HAS BEEN APPLIED.

Chorea.—Another disease of a spasmodic kind, and essentially belonging to the nervous system, is *Chorea—St. Vitus' dance.* This is far less serious than the complaints which we have recently been considering; but it is a very unpleasant disorder to suffer, and it has several points of analogy with the other nervous and spasmodic ailments. Its prominent system is an irregular and involuntary clonic contraction of some of the voluntary muscles, which, however, are not wholly or constantly withdrawn from the government of the will. In tetanus we had rigid spasm, while the mind was clear and free; volition was unaffected, but the muscles which should have obeyed the effort of the will were seized upon by some stronger overruling power. In epilepsy, with convulsive spasm, there was suspension of the mental functions; a temporary interruption of consciousness, and therefore of volition. But in chorea we have a different state from either of these: there is no loss of consciousness; no defect of volition; the ordinary movements of the body can be performed in some degree, or sometimes, under the direction of the will; but it would seem as if some other power wantonly interfered to excite them when they are not needed, to render them unsteady and imperfect, to arrest the natural action, and give a new direction to the limbs, and to cause the patient to gesticulate and grimace like a Merry-Andrew. Moreover, these apparently absurd movements do not occur in paroxysms, but continue throughout the day, sometimes for weeks together; but they generally cease during sleep: for the most part, but not always, the agitated limbs are still, while the senses are shut up in slumber. The complaint is not attended with *fever.*

Symptoms.—This disorder was first distinctly described by Sydenham, whose account of it is very graphic and excellent, and has been copied by most subsequent writers. Without reference, however, to the portrait which he has left us, I will sketch the disease as it has occurred under my own observation. It usually begins with slight twitches of a few muscles in the face, or one of the extremities; and by degrees the spasmodic action becomes more decided and more general. All the voluntary muscles are liable to be affected by it. Those of the face seldom escape. The features are twisted into all sorts of ridiculous forms; you might suppose that the patient was what is called pulling a face, or making mouths at you; but there is neither mirth nor mockery in the contortion; it is a convulsion: it is succeeded by a vacant look, and then it begins afresh. The disease occurs much oftener in young girls than in any other persons. If you ask the patient to put out her tongue, she makes sundry attempts to do so before she can accomplish it; and then the tongue is suddenly thrust out, and as suddenly withdrawn, and the jaws snap together as if she were resolved that you should have as short a glimpse of it as possible. She writhes and contorts her shoulders; she cannot keep her hand or arm half a minute in the same position; when, at meals, she desires to carry her hand to her mouth, it is arrested midway, and suddenly pulled back again, or pushed out in some other direction; and it is only after many deviations and fruitless efforts that she succeeds. The lower extremities are equally affected: when the patient intends to sit or stand still, her feet scrape and shuffle on the floor, or one is thrown over the other; and if she endeavours to walk, her progress is most uncertain; she halts, and drags her leg rather than lifts it up, and advances in a jumping manner, and by fits and starts. In short, the voluntary muscles are moved in that capricious and fantastic way in which we might fancy they would be

moved if some invisible, mischievous being, some Puck or Robin Goodfellow, were behind the patient, and prompted the discordant gestures. With all this the articulation is impeded; there is the same perverse interference with some of the muscles concerned in the utterance of the voice. By a strong figure of speech, the disorder has been called "insanity of the muscles."

Such is a picture of the main symptoms of this strange malady, as they have presented themselves to me; and such, I venture to say, you will often see in your future practice. You will find, moreover, that the irregular jactitations are usually more marked and general on one side of the body than on the other; and sometimes they are confined to the muscles of one side. Here, therefore, we have a trait of resemblance to epilepsy and to hemiplegia. If you take hold of the only limb which happens to be thus agitated, and keep it still by main force, some other limb or part will take on the convulsive action. The persons who are subject to chorea are always inordinately sensitive, and what is popularly called "nervous." They are easily moved by new ideas and sudden feelings, and pass readily, and upon slight occasion, from one mood of mind to another. The mind is affected, as Dr. Cullen remarks, in the same way, and often shows the same varied, desultory, and causeless emotions, as in hysteria. You see the indication of this nervousness in the fact that the fidgety catching of the muscles increases when the patient is spoken to, especially by a stranger—by the physician, for example. The nurses of the hospital constantly tell me that such and such a patient, who has chorea, is much more composed at other times than she is during my visit, when she is surrounded by students, and made the object of their attention. In most cases the jactitations are partly and in some degree under the influence of the will: sometimes the patient seems to give way to them, indulges in or exaggerates them; at other times she can, by making an effort, control them. Many of the patients, especially such as are old and intelligent enough to understand the directions given them, and to make the trial fairly, can suspend for some seconds the convulsive movements, by taking a deep inspiration, and resting upon it, without expiring, for a little while. Like other spasmodic diseases occurring in movable constitutions, chorea is liable to be propagated also by a species of contagion, or rather of involuntary imitation. These diseases constantly approximate and touch each other in some of their characters.

Chorea, in this its standard form, is essentially a disease of youth. Sydenham, and Cullen who closely copies him, state that for the most part it attacks boys and girls who have not reached the period of puberty; between the tenth and fourteenth years of their age. These limits are, however, too scanty. It is very common between the eighth and sixteenth years; it sometimes comes on as early as five or six: and now and then it begins in adult life, or in old age. I have already intimated that it is much more frequent in girls than in boys. Dr. Heberden says the proportion is 3 to 1. Dr. Elliotson, out of 30 patients, had 22 females and 8 males. Of 84 cases reported by Dr. Reeves, of Norwich, 57 were females, and 27 males. Of 72 occurring in Dr. Manson's practice at Nottingham, 53 were females, 19 males. Of 18 cases in the Hampshire County Hospital, 12 were girls, and the rest boys. Now taking all these numbers together, we have 204 cases, of which there were 144 females and 60 males; the proportion is as 12 : 5, or a little more than 2 : 1; and leaving out Dr. Reeves' list—which differs considerably from the others in containing a larger number of males—we have 120 cases, of which 87 were females, and 33 males: this ratio is as 29 : 11, or nearly, but not quite, 3 : 1. I have also observed that the disease occurs much more frequently in children having dark hair and eyes, than in those of a light complexion; and I think I have seen the same remark in some author, but I forget where.

When the disease is strongly marked, or lasts long, there is usually some imbecility of mind manifested; a slight degree of fatuity, and a foolish expression of the features. But this goes off with the other symptoms. The child generally recovers, but the disease is apt to recur, and that more than once. In this respect we may trace a distant resemblance to epilepsy: if we regard each attack as a long and mild paroxysm, then these paroxysms are liable to repetition. No doubt the duration of the disorder is often abbreviated by proper treatment: there are *cures* in this disease as well as *recoveries*. It is a very rare thing for chorea to prove fatal; and the few fatal cases that have occurred have thrown no

light on its pathology. Dr. Elliotson saw a strong girl affected with it die of apoplexy; but perhaps she would have died of apoplexy whether she had had chorea or no. Chorea offers no protection against the invasion of other diseases. My colleague at the Middlesex Hospital, Dr. Hawkins, had a fatal case. He found great vascularity of the uterus, earthy concretions in the pancreas, omentum, and mesentery, and tubercles in the lungs. But these appearances had no connection probably with the chorea. In an instance that proved fatal under Dr. Bright's observation, there was considerable disease in the uterus and its appendages. I am afraid that we shall seek in vain in the dead body to discern the *nature* of chorea. When we find organic disease accompanying it, we must look upon such organic disease, if it have any connection with the chorea at all, as being a predisposing cause; as producing or increasing that irritability and mobility of the nervous system which fits it for submitting to the exciting causes of various nervous diseases.

Pathology.—There is a speculation of some of the French writers respecting the seat and nature of chorea so ingenious, that I cannot refrain from mentioning it.

It is affirmed by certain modern physiologists, as you may perhaps know, that one of the functions, the principal office indeed, of the *cerebellum*, is to preside over and regulate the faculty of locomotion; to keep the muscles in due subordination, as it were, to the will. No voluntary movement, almost, can be executed without the combined and consenting action of many muscles: it is the business of the cerebellum, they say, to maintain this consent and community of purpose; to prevent any mutiny of individual muscles, and to make them unanimously co-operate in producing a given movement. How far this doctrine is true I do not intend to inquire; but, supposing it well founded, then they very ingeniously assign the cerebellum as the seat of that change, whatever it is, which gives rise to the phænomena of chorea. And it is most certain that the irregular movements by which chorea is characterized can neither be considered as the effects of imperfect paralysis, as some have stated, nor of convulsion, in the proper sense of that word, as others have asserted; but rather as consequences of the want of due harmony and agreement between the various muscles, which should combine to produce the desired state either of rest or of motion. There is a defect of the requisite association in the action of the different muscles; and it is in this sense that chorea has been denominated *insanity* of the muscles. There is a certain portion of the brain which ministers to the intellectual functions; there are certain altered states of that portion, which lead to mental aberration; the persons so affected form false judgments, cannot associate their ideas aright. So also there is a certain portion of the encephalon which presides over the locomotive functions; and there are altered states of *that* portion, which lead to a loss of the due association of the muscular contractions. That portion is the cerebellum. Such is their theory: and it is a very plausible and pleasant, but withal an unsatisfying theory. The disorder really belongs, I apprehend, to the excito-motory division of the nervous system. From some unhealthy or unnatural state, either of the cord or of the incident nerves that convey impressions to it, its reflex function is called into irregular play, and voluntary muscles contract independently of volition. Sometimes at the same instant the patient *wills* certain definite movements through the instrumentality of the very same muscles. The consequence is, that the same muscles, receiving at the same time contradictory orders from these two sources, obey neither mandate completely, but give rise, by their discordant action, to the grotesque and seemingly antic gestures which these patients exhibit.

But to leave these seductive theories, and to return to duller matters of fact. Chorea is a complaint that is seldom attended with any bodily pain. I have in several instances, however, known it to be accompanied by pain of the head, and in some of them, with pain on that side *only* of the head which was opposite to the agitated limbs. I mention this as being of some practical importance: for I have found the disease to become sensibly less severe, and very soon to cease, upon drawing blood by leeches, or cupping, from the painful side of the head. In a greater number of cases, however, no such pain is experienced. Sometimes you will find that in all respects, excepting the nervousness, and the irregular movements, the patient is in the enjoyment of perfect health. But neither is this very common: generally there is something manifestly wrong in the state of the stomach and bowels, either before or during the complaint; a capricious appetite, costiveness, a tumid abdomen, offensive breath, a foul tongue.

Complications.—Chorea is sometimes complicated with other disorders, and above all, with hysteria: and no wonder, since they both occur chiefly in persons of the same sex, of the same temperament, and at nearly the same period of life. It is said also to happen in conjunction with acute rheumatism and rheumatic pericarditis; and with certain affections of the skin. Its coincidence with cutaneous complaints I suspect to be accidental; and I should not say, judging from my own experience alone, that the disease was often associated with acute rheumatism. Dr. Copland and Dr. Bright have both, however, noted that connection, and therefore I cannot doubt that it does sometimes exist. I certainly have seen jactitations like those of chorea in a few instances of rheumatic carditis. Very lately a boy, affected with chorea, became my patient in the hospital. We soon detected a strong bellows-sound of his heart; and tracing his history back a little, we found that he had suffered acute articular rheumatism. In a recent volume of the *Medico-Chirurgical Transactions*, there is a paper by Dr. Bright, detailing "cases of spasmodic disease accompanying affections of the pericardium." Now we do not perceive any obvious or direct connection between the cardiac disorder and the nervous disorder. There are just two conjectures which occur to me upon the subject. Rheumatism (as we shall see by and by) is especially a disease of fibrous structures, and it usually affects various fibrous parts at the same time. It is not improbable, therefore, that, in the cases in question, some morbid condition of the membranes of the spinal canal may have arisen, simultaneously with the inflammation of the pericardium; or the cardiac disease may perhaps operate, by some ill-understood influence, upon afferent nerves of the cord, as an eccentric cause of the irregular movements.

Causes.—Probably any thing which makes a strong impression on the nervous system may act as an exciting cause of chorea. Strong mental emotion, or a sudden mental shock, is very likely to bring it on in those of a movable constitution who are predisposed to it. Of its ascertained or alleged exciting causes, *fright* is beyond all comparison the commonest. It has been known to follow a blow or fall on the head; but even in these cases the alarm may have had more to do with the disorder than the blow itself. It sometimes seems to depend upon irritation of the stomach or bowels, by improper diet, by accumulated fæces, or by worms; and it is found to be connected, in not a few cases, with difficult and painful menstruation. It frequently begins about the period of the second dentition: the late Dr. Gregory, of Edinburgh, was in the habit of relating instances of that kind. In one case, the old teeth were remaining while the new ones were appearing by their sides. The old teeth were drawn, and the removal of the chorea was complete. This Dr. M. Hall would justly call eccentric chorea. But even in such cases the state of the gums cannot be regarded as the sole cause of the chorea: there must be the predisposition, as well as the accidental exciting cause; for the complaint is apt to recur under the agency of some new irritation, and may then be removed by other means.

Chorea, such as I have been describing it, may last from a week or two to some months. In those eighty-four cases which I have already mentioned as having been reported by Dr. Reeves, the shortest period of medical treatment was two weeks; the longest eight months; and the common average seven weeks. This appears to me a long average. The disorder often terminates—at any rate much more often than epilepsy does—at the period of puberty; especially upon the first coming on of the menstrual discharge in the female.

I had occasion, in the last lecture, to remark, that when a vast number of different drugs are recommended as specifics in any given disease, we may sometimes infer from that very circumstance that the disease is difficult of cure, and generally intractable under all plans of management. But there is another class of diseases which a variety of drugs are supposed capable of curing,—those, namely, which tend to terminate in health. I believe that many cases of chorea—most cases—would at length get well without any aid from physic: I believe also that many of the boasted specifics have been quite innocent of any share in the recovery of the patients to whom they were administered; at the same time I am quite certain that treatment has a great influence over the disease.

Treatment.—It was Sydenham's practice first to bleed and purge his patients, and then to administer bitters, aromatics, and antispasmodics, with the object of strengthening the nerves. After his time the blood-letting and purgatives fell into disuse, until the publication of Dr. Hamilton's well known work brought the latter deservedly into favour again.

A main part, however, of the treatment, consists in the employment of such measures

as are calculated to give stability to the movable nervous system. I can confidently recommend you to abstract blood locally in those cases in which there is a fixed pain in the head; but with this exception, blood-letting is neither useful nor even, in my opinion, justifiable.

I shall not attempt to distract your attention by discussing the various remedies that have been vaunted against chorea; but shall take the liberty of referring you to books (to Dr. Copland's *Dictionary*, for example) for farther information on that subject, and content myself with telling you what modes of treatment I have been in the habit of employing, with very satisfactory results. I think, then, that, setting aside the complication with headache just mentioned, you will be able to deal successfully with most of the cases of chorea which you may have to treat, if you have at your command purgative medicines, the shower-bath, preparations of iron and of arsenic, and oil of turpentine.

It will be right, in all cases, to begin by clearing out the bowels by calomel and jalap, or some active aperient; and you should persist in the regulated use of purgative medicines, if they continue to bring away much fæcal matter. We are to be guided less by the amount of the doses than by the effects they produce; at any rate one full evacuation of the bowels should take place every day. But though purgatives are good auxiliaries, we cannot trust to them alone for the cure of the complaint.

One of the most effectual of the tonic remedies is the cold shower bath. If the patient be of a feeble constitution, the water may at first be used tepid; by degrees it should be used cold. This remedy should be employed every morning, or every other morning, early, as soon as the patient gets out of bed. Of the best indications of the propriety of its continuance I spoke in the last lecture only; I need not tire you, therefore, by repeating the observations I then made.

With this external tonic it will be right to combine some internal one; and for the most part the best for the purpose is some preparation of iron. The carbonate of iron is an exceedingly good form, and it may be given in the way recommended by Dr. Elliotson, one of whose *pets* it is,—namely, mixed with twice its weight of treacle, so as to form an electuary. You may begin with it in half-drachm doses, and presently increase the quantity to a drachm, or a drachm and a half, or two drachms. Much larger quantities than this have been given, and that for a long time together; but I am not in the habit of *so* pushing this drug. Patients do not like to swallow from half an ounce to an ounce of the powder and twice as much treacle three or four times a day; and some of them cannot get so much down. And I mentioned on a former occasion that the iron is apt to accumulate in the large intestines, and to be expelled at last, often with difficulty and pain, in large, hard, red masses, like what is called, I fancy, slag, or the dross of iron ore from a furnace. When one or at most two drachms given three or four times a day make no impression on the disease, you had better (in my humble judgment) change the form of the medicine. Give a grain or two of the sulphate of iron for a dose, or frequent draughts of Griffith's mixture (mistura ferri composita), or twenty or thirty minims of the tinctura ferri muriatis. Dr. Bright says he has found the sulphate of zinc answer when the carbonate of iron had failed, and the iron succeed when the zinc had done no good. One most severe case, about which I was consulted, and which had resisted other remedies, got well under the use of the sulphate of zinc; the dose of which was gradually increased to ten grains, given three times a day. Whenever the medicine was pushed beyond this point it became emetic. Certainly the disease is often very obedient to arsenic; but, for plain reasons, it is better to effect a cure, when we can, by less hazardous substances. The severest case I ever had to treat occurred, not long since, in one of my hospital patients. I tried the carbonate of iron in vain. The shower bath so terrified and agitated the girl that I could not persist with it. I then gave her arsenic, under which she improved at first, but it ultimately was very injurious; her bowels were greatly irritated by it, she became paralytic in her lower extremities, and sunk into a typhoid state; and I really was afraid that I should lose her. But she recovered from this condition, which I could not but ascribe to the arsenic; and as soon as I dared venture, I began to give her the muriated tincture of iron, twenty drops at a time, every six hours. Under this treatment she steadily and rapidly improved, and was soon quite well.

The oil of turpentine also is certainly a valuable medicine in this disease; whether

there be worms at the bottom of it or not. When the bowels are torpid, and the girl is of that age that menstruation may be conjectured to be at hand, its arrival seems sometimes to be accelerated, and great relief to be produced, by the turpentine. The best way of exhibiting it in such cases is in combination with an equal quantity of castor oil; two drachms or half an ounce of the *mixture* may be given every morning, or every other morning, according to its effect upon the bowels: and when they are very sluggish, or the stools are unnatural, it will often be serviceable to give a couple of grains of calomel also, twice or thrice a week, at bed-time.

It is scarcely necessary for me to say that in all cases due attention must be paid to the diet. This ought to be plain and simple, but at the same time nourishing, or even generous. And exercise, short of that which produces fatigue, in the open air, in fine and dry weather, will greatly conduce to the patient's recovery.

Chronic chorea.—There is an affection (it scarcely deserves to be spoken of as a disease) which is sometimes called chorea, of a chronic nature, and resembling the disorder I have just been speaking of inasmuch as it commonly is met with in nervous persons, and consists in the irregular, unmeaning, and involuntary contraction of certain muscles, especially of the limbs, neck, or face: but differing from it in this—that the same muscles are always affected, and in the same way; that it lasts long, almost always for life, and implies no accompanying derangement of the general health. In its slighter form the irregular movements are rather *awkward tricks* than spasms: a repeated shake of the head, or knitting of the eyebrows, or corrugation of the integuments of the nose, or shrugging of the shoulders—which the person seems scarcely conscious of. At other times, however, the motions are more extensive; a limb starts out, or the head is turned awry; and the individual who performs these evolutions is quite aware that he does so, and vexed and annoyed at the ridiculous figure he makes, but he cannot help performing them; or if he can prevent it, the necessary effort is worse than the disease. One young man who was subject to this infirmity told a friend that he could stop the movement by a strong exertion of the will; but that *that exertion* was extremely painful, and was followed by languor and much discomfort. In some instances I make no doubt that the continuance of the affection is the result of a long-established *habit*. It occurs more frequently in men than in women. I had for a long time, as an out-patient at the hospital, a girl about seventeen years old, in all other respects the picture of health, but who was annoyed by an involnutary shake of the head, which took place two or three times in a minute. She received no benefit from medicine. A lad in my own service was affected in a similar manner. He seemed to be giving me, and my friends, from time to time, a familiar nod; and I was obliged to part with him. Others are subject to twitchings of the face. I am acquainted with one gentleman who is perpetually wrinkling his nose; and he has assured me that he was subject, when young, to an involuntary shake of the head, like the two persons just mentioned; but a blister having been once applied to his throat for some disorder in his air-passages, the shaking of the head was thereby rendered painful and difficult, and the movement there ceased: but (as he expressed it) it broke out in his nose, where it triumphs to this day. The *chronic chorea*, as it has been called, I merely mention to prevent your confounding together two affections which, though they have received the same name, and are in some respects analogous, yet differ in still more points, and those points of more importance. I believe that medicine has no power over any of these tricks. They are distressing and unsightly; but in no way dangerous.

The word chorea, which you know signifies a *dance*—and the trivial term of St. Vitus's *dance*—are not very appropriate to *either* of the modifications of the nervous affection which I have been noticing. In fact that term was originally applied, and much more suitably, to another set of symptoms of a most singular kind, concerning the real occurrence of which we might well be sceptical, if we had not authentic narratives of many instances of such disorder from different persons of credit, as well in this country as in others. What has happened, many times before, may happen again; and you ought not to be in ignorance of the histories to which I allude. They relate to an affection characterized by movements that cannot be called spasmodic, but were rather owing to an irresistible propensity to muscular action, increased sometimes to a sort of mania by the force

of imitation, or by the sound of music. It is the *volition* that, in these cases, is morbid and perverse. You might fancy the patient to be possessed and coerced by an evil spirit, like the δαιμονιζομενοι of the Gospel history.

Some of the subjects of these extraordinary affections, impelled by a strange unintelligible necessity, execute measured and regular movements with surprising energy, rapidity, and perseverance. When music is performed in their hearing, the movements become an actual dance; and where crowds are collected together, the dancing mania is apt to spread from person to person by a sort of imitative infection; realizing the fable of Orpheus, and giving origin (it may be presumed) to those romantic legends, met with in the literature of most ages and countries, of universal, involuntary and unceasing saltation, at the sound of a magic pipe. To these feats the term *chorea* is apposite enough. Indeed I have seen it somewhere suggested that the phrase *chorea Sancti Viti* is but a vulgar corruption of *chorea Sancti inviti;* and took its rise in the misfortune of some holy person who chanced to be afflicted with one of these unwilling but invincible impulses to caper. The common explanation makes this holy person to have been a certain German Saint Weit, to whom a chapel is said (I know not with how much truth) to be dedicated at Ulm, in Suabia.

Sometimes, instead of dancing on their feet, these patients drum and beat with their hands, either upon their own knees, or upon the objects near them. This variety has received the bombastic title of "malleation." Sometimes they circumvolve with great rapidity; or they turn their heads repeatedly from side to side with great velocity: this is "rotation." When they are irresistibly impelled to move in a given direction, the term "propulsion" is employed. The very invention of these names attests the reality of the disorder.

You will find one of these singular cases related by Mr. Kinder Wood, in the seventh volume of the *Medico-Chirurgical Transactions.*

The patient was a young married woman. After having suffered severe pain in one side of her face, she began to be troubled with involuntary movements. They commenced in the eyelids, which were opened and shut with excessive rapidity. Then the muscles of the extremities became affected. The palms of the hands were beat rapidly upon the thighs, and the feet upon the floor. The motions soon extended to the trunk and pelvis. The patient was suddenly half raised from her chair, and instantly reseated. This was repeated as quickly as one action could possibly succeed another. Sometimes she had a propensity to leap upwards, and strike the ceiling with the palm of her hand; or to touch little spots or holes in the furniture of the room. Or she would dance on one leg, holding the other in her hand. These attacks were accompanied by headache, sickness, and vomiting. At last she took to making steps about the room, regulated by an air, or by a series of strokes on the furniture as she passed by: her lips moving as if words were articulated, but no sound escaping them. A person thinking he recognized the tune which she beat on the furniture, began to sing it; and she danced directly up to him, and continued dancing till he was out of breath. A drum and a fife were now procured, and the same air played upon them. She immediately danced up to the drum, and as close to it as possible, till she missed the step, when the motions instantly ceased; and this was found always to be the case. The motions stopped also when the measure was changed; or increased in rapidity beyond her power to keep pace with it. A continued roll on the drum had also the effect of putting an end to her movements. And this being discovered their approacch was watched; and by always rolling the drum as soon as they threatened to begin, the chain of association which seemed to constitute the disease was at length broken. The bowels were in an unnatural state during the complaint; and the menstrual discharge appeared on the evening of the day on which it ceased. One might conceive that the conduct here described was an indication of folly or of insanity; but Mr. Wood declares that the patient's spirits were good, and her perception and judgment accurate and just: that during the absence of the paroxysms she went about her household affairs as usual; and that she had a correct knowledge of her situation, and of the advantage she derived from the drum, with an anxious desire to continue its use. She stated "that there always was a tune dwelling upon her mind, which at times becoming more pressing, irresistibly compelled her to commence the involuntary motions."

In a lady, whom Dr. Abercrombie saw, the following symptoms, among others, occurred:

—After she had been ill with various nervous affections for two years, she began to suffer convulsive action of the muscles of the back, and involuntary twitches of the legs and arms, producing a variety of movements of the whole body very difficult to describe. These were much increased by touching her, especially on any part of her back. This is a symptom quite in conformity with with Dr. Hall's doctrine of eccentric irritation. At one time there was difficulty of deglutition, so that attempts to swallow produced spasms resembling those of tetanus. At other times, after lying a considerable time quiet, she would in an instant throw her whole body into a kind of convulsive spring, by which she was jerked entirely out of bed: and in the same manner, while sitting or lying on the floor, she would throw herself into bed, or would leap, as a fish might do, on the top of a wardrobe fully five feet high. These are feats that surpass the powers of a person in health: and I say we should hesitate to believe them if they were not related by a physician of such sober judgment and unquestionable veracity as Dr. Abercrombie. He tells us that during the whole of these symptoms her mind continued entire; and the only account she could give of her extravagance was, a secret impulse which she could not resist.

But after a time motions still more wonderful commenced, affecting the muscles of the upper part of the back and neck, and producing a constant semi-rotatory motion of the head. This sometimes continued without interruption night and day for several weeks together; and if the head or neck were touched, the motion was increased to a most extraordinary degree of rapidity. These paroxysms were relieved by nothing but cupping on the temples to the amount of ten or twelve ounces; when the affection ceased in an instant with a general convulsive start of the whole body. She was then immediately well, got up, and was able to walk about in good health for several weeks; when the same symptoms returned, and required a repetition of the same treatment. All this went on, at intervals, for four years; the menstruation during that time being irregular and scanty, and the bowels torpid. She was pale and bloodless from the frequent bleedings, but not reduced in flesh. At last, in the spring of 1829, she had a severe paroxysm of the rotatory motion of the head; and it was then determined to allow the attack to take its course, and to direct the treatment entirely to the menstruation. Sulphate of iron, and Barbadoes aloes, were prescribed. She went on for three weeks, the convulsive motion of the head continuing without intermission night and day. At length, in the middle of the night, the paroxysm ceased in an instant, with the same kind of convulsive start of the whole body with which it used to cease after cupping. At the same instant menstruation took place in a more full and healthy manner than it had done for many years. From that time she remained well; at least up to the period when Dr. Abercrombie wrote the account.

This alternating rotatory motion of the head is by no means an uncommon feature of these singular cases. It occurred in a patient of Dr. Conolly's; in whom the menstruation was irregular, and about to cease altogether. It came on in paroxysms which were repeated many times a day, and was attended with inordinate loquacity. The head was turned from side to side about eight times in a second, and each paroxysm lasted three or four minutes. The patient got well after being cupped and leeched, and thoroughly purged. I have seen precisely the same thing in a hospital patient. Dr. Crawford met with an instance of involuntary rotation of the head, without pain, but attended with intolerance of light. And there is a striking example of it described in the twenty-third volume of the *Edinburgh Medical and Surgical Journal*, by Mr. Hunter, of Glasgow, who speaks of it under the name of "rotatio or chorea." The motions are said to have been furious and alarming: they were executed with such immense rapidity, that it was difficult even for the eye to follow them. She appeared, Mr. Hunter says, absolutely to be looking backwards and forwards, and in every direction, at the same moment. This woman had sometimes fifty paroxysms of this kind in a day, which greatly exhausted her: but she was perfectly rational in the intervals. A modification of the same kind of affection took place in a most extraordinary case recorded by Dr. Watt, of Glasgow, in the fifth volume of the *Medico-Chirurgical Transactions*. His patient was a girl ten years old. First she had headache, accompanied by vomiting, and increased by the slightest deviation of the body from the erect posture, either backwards or forwards, or to one side. These symptoms lasted about a month; and during that time she lost the power of speech and of walking.

At the end of that period she was seized with a propensity to turn round on her feet, like a top, with great velocity, always in one direction; and was pleased when those about her assisted in increasing the rapidity of her movements. After continuing nearly a month, these motions ceased, the headache returned, and she became unable to move her neck, or support her head. Soon after she was seized with a new kind of motion; she would lay herself across the bed, and turning round like a roller, move rapidly from one end of it to the other. At first the fits of this kind lasted two hours; but they gradually increased to six or seven hours every day. On being carried into the garden she rolled rapidly from one end of a walk to the other: and even when laid in the shallow part of a river, though on the point of being drowned, she began to turn round as usual. The rotations were about sixty in a minute. She made little or no use of her arms in revolving. In about another month or six weeks an entirely new set of movements began. She lay upon her back, and, by drawing her head and heels together, bent herself like a bow, and then allowing her head and heels to separate, her buttocks fell with considerable force upon the bed. She repeated these movements ten or twelve times in a minute, first for six hours daily, and at length for fourteen. After another space of about five weeks had elapsed, the most singular freak of all ensued; she was seized with a propensity to stand upon her head with her feet perpendicularly upwards. As soon as the feet were elevated in this manner, all muscular exertion seemed to be withdrawn, and the body fell down as if dead; her knees first striking the bed. This was no sooner done than she instantly mounted up as before; and continued to do so from twelve to fifteen times in a minute, for fifteen hours a day. After a variety of fruitless treatment, a spontaneous diarrhœa came on, and she recovered.

The spinning motions observed during a part of this case have been observed in other instances.

In Magendie's *Journal de Physiologie*, the two following singular forms of disease are referred to. A man, after some other symptoms of cerebral affection, was seized with an irresistible propensity to move forwards, stopping only when exhausted. He would sally forth into the streets, and continue walking straight forward until he dropped down, and was obliged to be brought home in some conveyance. This man at length died, and several tubercles were found in the anterior hemispheres of the brain. Dr. Laurent, of Versailles, exhibited to the Academy of Medicine a young girl, labouring under the exactly opposite necessity. In the attacks of a nervous disease she was irresistibly propelled backwards, and with some rapidity: being unable to avoid obstacles or hollows, she received many falls and bruises in her course.

I say that histories such as I have been giving you some samples of, and those mostly in an abridged form, would sound very like romances, if they were met with in the older authors alone, or if they were not attested by unimpeachable authority. They resemble chorea in this respect, that they are examples of muscular actions performed by persons in possession of consciousness, and performed in spite of themselves. But in most other respects they differ from what we now-a-days mean when we speak of chorea. Perhaps they may rank among *hysterical* vagaries. It is remarkable that the majority of them occur in young women, in whom the menstrual function is suspended or irregularly performed. Some persons may consider them as varieties of insanity. The patients certainly did not *feign* to be ill, for the feats of strength and agility which many of them performed were much beyond their natural power of endurance. The truth seems to be, that there are innumerable modifications of the nervous functions, and that some of them are more common and more capable of being arranged into groups than others; but that they all offer points of resemblance, like (as I observed before) the different members of a large family, of which the individuals have the same general cast of features, and yet preserve each his particular identity.

I advert to these odd forms of disease with the view of directing your attention to such of them as may come in your way. We are yet terribly in the dark about morbid affections of the nerves, both organic and functional. Hereafter a medical Newton will arise, and reduce all these apparently complicated phenomena to some simple law. At present all that we can do is to collect and, as far as we may, to arrange facts, in the hope that at length some better light will be shed upon the subject. And it must be observed that

some of the modern researches into physiology do throw a little glimmering of illumination into these dark corners of pathology.

In certain of M. Magendie's experiments on animals the following curious facts were ascertained:—When a vertical section of the cerebellum of a rabbit was made, leaving one-fourth of the whole adhering to the crus of the right side, and three-fourths to the left, the animal rolled over and over incessantly, turning itself towards the injured side. The same phenomenon occurred upon the division of the crus cerebelli. The animal lived for eight days, and continued during the whole of that time to revolve upon its long axis, unless stopped by coming in contact with some obstacle. How like is this to the symptoms exhibited at one period in the girl whose case is related by Dr. Watts! Nor is Dr. Watts' case a singular one: M. Serres has described another much like it. A shoemaker, 68 years old, of intemperate habits, after one of his debauches exhibited a kind of drunkenness which surprised his friends. Instead of seeing objects turn round him, as a drunken person is apt to do, he thought he was himself turning, and soon began to revolve; and this lasted till he died: and when his head was examined, extensive mischief was found in one of the peduncles of the cerebellum.

Again, M. Magendie noticed that when the upper part of the cerebrum is gently removed in birds and mammalia, they become blind; but no affection of the locomotive powers is produced. No farther result is occasioned by the removal of a portion of the gray matter of the corpus striatum: but when the striated part is cut away, the animal immediately darts forward with rapidity, and continues to advance as if impelled by some irresistible force, until stopped by an obstacle; and even then it retains the attitude of one advancing. The experiment was tried with the same result upon various species of animals—dogs, cats, hedgehogs, rabbits, Guinea pigs, and squirrels. It seems that there are horses that cannot *back:* although they make good progress enough in a straight-forward direction. Now Magendie says that he has opened the heads of such horses; and has always found, in the lateral ventricles of their brain, a collection of water, which must have compressed and even disorganized the corpora striata. It has further been found, by the same experimenter and by others, that certain injuries of the cerebellum cause animals to move backwards contrary to their will; if the tail of the animal so mutilated be pinched, he still persists in his retrograde course. Injuries of the medulla oblongata had the same effect: pigeons into which he forced a pin through that part, constantly receded for more than a month, and even *flew* backwards. A section of the medulla oblongata, where it approaches the anterior pyramid, gives rise to a movement in a circle, like that of a horse in a mill: the animal, in its walk or its flight, bearing round continually to the injured side. Surely, we have, in these facts, supplied by experiments on living animals, and by observation of the phenomena of disease, in the living human body, some of the *materials* for a more exact knowledge, both of the physiology and of the pathology of the nervous system, than we have yet reached. M. Magendie supposes that different portions of the encephalon are endowed with energies which tend to cause motion in various directions; that in the healthy state these balance each other, and that a preponderating impulse can be given to any one of these forces by the will; but that when the equilibrium is destroyed by disease, the will is not sufficient to counteract the tendencies which are then brought into play. Mr. Mayo offers a different explanation of the phenomena. He supposes that the injuries inflicted on the nervous matter produce a sensation analogous to vertigo; and that the animal conceives itself either to be hurried forward, and makes an exertion to repel the imaginary force; or to be moving backward, or turning round in one direction, and endeavours to correct this by moving the corresponding muscles. Whatever may be the true explanation, the facts themselves are abundantly curious and interesting, and I recommend them to your attention.

Some of the affections that I have been describing, fall, perhaps, under the category of those to which the appellation of the *leaping ague* has been given in some parts of Scotland. There is a class also of convulsive spasmodic affections which resemble epilepsy on the one hand, and chorea on the other, or rather form a link of alliance between the two, and which are especially remarkable for this, that they are capable of being propagated by that kind of imitative contagion of which I have several times spoken. This point might be well illustrated by the history of various sects of religious enthusiasts.

One or two of those enthusiasts have apparently at first worked themselves up into a state approaching to epilepsy, accompanied even by insensibility sometimes; and then this state has been communicated by sympathy to the more susceptible of their auditors. I must not however go into any farther details on this subject; and perhaps I have prosecuted it too far already: those among you who are inclined to pursue it farther may find some curious accounts of an epidemic which occurred in Lanarkshire, under the head of the "Conversions of Cambuslang," in Sir John Sinclair's *Statistical Account of Scotland;* and in one of the early volumes of the *Edinburgh Medical and Surgical Journal,* Dr. Robertson has described, in an inaugural dissertation *De Chorœâ Sancti Viti*, a similar epidemic, which occurred in the states of Tennessee and Kentucky, in the western districts of America. This also is referred to in the same volume of the journal. Among other things Dr. Robertson says, that while extravagant sounds, and actions, and gesticulations, were in the first instance *wilful,* the actors "at length, to their own astonishment, and the diversion of many of the spectators, continued to act from *necessity* the curious character which they had commenced from *choice.*" I will only remark farther of such forms of nervous disease, that as they spring often from moral causes, so they admit, in a great degree, of moral remedies. The pranks played by the Scotch enthusiasts were brought to an end by threatening to duck every one who should thereafter be attacked; and, I believe, a few of them were horse-ponded, by way of example. With respect to the solitary instances of perverted locomotion, our business must be to correct whatever is wrong in the state of the bowels; in women, to amend the disordered uterine functions; to strengthen and confirm the system generally: and, in addition to the measures proper to affect these objects, I suspect that the cold sousing would in many cases be found of most material service.

LECTURE XXXVIII.

PARALYSIS AGITANS. MERCURIAL TREMOR. HYSTERIA: TWO FORMS OF HYSTERIC PAROXYSM; DIAGNOSIS FROM EPILEPSY; CLASS OF PERSONS MOST LIABLE TO HYSTERIA; DISEASES APT TO BE SIMULATED BY HYSTERIA; TREATMENT; PREVENTION.

Paralysis agitans.—In the last lecture I spoke of chorea, and of some singular forms of disorder that have sometimes been included under the same appellation; and I shall begin the present with a few observations concerning a disease very closely allied to some of those which we were then considering, and yet distinct enough to deserve and require a separate notice. I refer to what has been called the *shaking palsy—paralysis agitans.* Allusions to this form of disease are to be found in many of the older systematic writers on physic; but it never was much attended to in this country until Mr. Parkinson published an essay upon it in the year 1817; and a very interesting little pamphlet it is. He defines the disease thus:—"Involuntary tremulous motion, with lessened muscular power, in parts not in action, and even when supported: with a propensity to bend the trunk forwards, and to pass from a walking to a running pace: the senses and intellects being uninjured." The latter symptoms constitute the *scelotyrbe festinans* of Sauvages; and the former symptoms of the definition are not always attended by the latter. In old persons you may often observe incessant and involuntary nodding and shaking of the head, without any tendency to run forwards. There is an old woman whom I see regularly sitting in the aisle at church every Sunday: she walks to her seat slowly and steadily enough, and sufficiently upright; but her head never ceases to nod and wag, and tremble in various directions. It may be that she is in the less advanced stage of the malady; but I have remarked her for three or four or more years, and I see no change.

Mr. Parkinson's notice was first called to the disease during his professional attendance upon a person affected by it. From observation of that case, and of several others that

he subsequently met with, his account of the disorder was drawn up. He states that its first approach is insidious, and its progress often so slow and imperceptible that the patient cannot recollect precisely when it began. A sense of weakness, and a disposition to trembling, fastens on some particular part; sometimes it is the head, but more commonly it is one of the hands or arms. These symptoms gradually become more decided; and at length the morbid influence is felt in some other part. At a still more advanced period the patient is found to be less strict than usual in preserving an upright posture, even when standing or sitting, but especially when walking. By degrees he finds a difficulty in making the hand obey the dictates of the will when he is engaged in any delicate manipulation—in writing, for example; and he is obliged to walk with circumspection and care: his legs are not raised to that height, nor with that promptitude, which the will directs; so that much attention is necessary to prevent frequent falls. Then, as the malady proceeds, the propensity to lean forwards becomes more strong—the patient is forced to step on the toes and forepart of the feet, while the upper part of the body is thrown so far forward as to render it difficult to avoid falling on the face: in some cases he is irresistibly impelled to take much quicker and shorter steps than common, and thereby to adopt unwillingly a running pace. When once this state has been pointed out, I make no doubt that some of you may recognise it, in old persons, whom you may have seen walking about. But the disorder does not stop here; the unhappy patient becomes unable to feed himself; or to walk at all without an attendant, who walks backwards before him, and prevents his falling forwards by the pressure of his hands against the forepart of the patient's shoulders: his powers of speech and deglutition fail; and the saliva dribbles from his mouth: he can no longer retain his urine or fæces; and at length death closes the miserable scene.

Mr. Parkinson conjectures that this complaint results from some chronic change of the upper part of the spinal cord, or of the medulla oblongata: but dissections are wanting to support or to refute that conjecture. Some of the patients, whose cases he has given, had been intemperate livers; hard drinkers: others had not been guilty of such excesses: several had suffered a good deal from rheumatism, which he thought might have laid the foundation of their lamentable disease. But a more exact pathology of the shaking palsy is still wanted. Dr. M. Hall observes that the symptoms have, in several particulars, a marked resemblance to the effects observed by M. Serres (and related in his *Anatomie du Cerveau*) of disease of the tuber annulare, or of the tubercula quadrigemina.

Nor have we any ascertained means of curing this disease; or rather, this state of decay. Dr. Elliotson indeed says that he succeeded in one instance (of which, however, the particulars are not given), with the carbonate of iron: but that he had tried the same medicine in vain in several other cases. We must administer to symptoms, and endeavour to set those functions right which may be obviously wrong: to regulate the bowels, to procure sleep, to nourish and uphold the patient without unduly stimulating him: and this is all that I can tell you of the shaking palsy.

Mercurial tremor.—Another analogous disorder, deserving a moment's notice, is that peculiar kind of trembling which is apt to occur in persons who are much exposed to the poisonous fumes of mercury: *mercurial tremor* it is called, and popularly, *the trembles*. It consists in a sort of convulsive agitation of the voluntary muscles, which is most violent whenever efforts are made to move the limbs by the help of those muscles; whenever, in fact, volition is brought to bear upon them. It differs therefore from the shaking palsy, inasmuch as the tremor ceases when the muscles are supported, or are not called into action. It is also more susceptible of relief by medicine. The last person in whom I have witnessed this curious affection has been twice my patient in the Middlesex Hospital, and has twice got well there. John Chattin, 33 years old, was first admitted in August 1837. He was led into the room, walking with uncertain steps, his limbs trembling and dancing as though they had been hung upon wires. While sitting on a chair he was comparatively quiet; you would not have supposed that he ailed any thing; but as soon as he attempted to rise, and to walk, his legs began to shake violently with a rapid, incessant, and irregular motion. He could neither hold them steady, nor direct them with precision. Indeed without support he must have fallen down. His arms

were agitated with similar involuntary movements. His tongue was tremulous, and he spoke in a hurried, abrupt, interrupted, staccato manner, not natural to him. He had no fever. His pulse was 66, and soft; his skin natural; his bowels costive. He complained of slight nausea. At the end of six weeks he went out well, or with very slight remaining weakness of his knees, and a little occasional tremor upon unusual exertion. In June 1839, he again presented himself, in a similar state of agitation and helplessness.

This man was a water-gilder; and had been employed in that business for eighteen years. Till somewhat more than a twelvemonth prior to his first appearance at the hospital, he had been free from disease. Then he began to tremble a little; but for a fortnight before his admission the shaking had become so much worse that he could not go up stairs, nor even walk upon uneven ground. The trembling, when once brought on by efforts to move, did not cease until he sat down, or got one of his fellow-workmen to grasp his limbs tightly.

This singular disorder is produced by the agency of mercury as a poison upon the body; and especially by the absorption of that metal when raised into vapour by heat, and inhaled in breathing. It is accordingly very common among water-gilders. Water-gilding is the gilding of metals, and of silver in particular, by means of fire. It is called *water*-gilding, I believe, to distinguish it from other kinds of gilding, called gilding in *oil*. The silver to be gilded is covered with an amalgam of gold and mercury, and then is placed over a charcoal fire, by which the mercury is raised in fumes, and driven off, and the gold alone is left adhering. To these fumes the workmen are necessarily exposed; and numbers of them become affected with this tremor, which is not a common result of mercury applied to the system in other ways. The same complaint is frequent among the workmen in the quicksilver mines of Friuli and Almadin, where the crude ore is purified by the aid of heat. Dr. Bateman relates, in the 8th volume of the *Medico-Chirurgical Transactions*, some cases like that which I have been describing. But the best account of the disorder that I have seen is given by Merat, in an appendix to his book on the *Colique Métallique*.

The malady comes on sometimes suddenly, more often by degrees. The patient is less sure of his arms than usual; they become tremulous, and at last shake, and, if the patient continues to pursue his employment, the force of the trembling goes on increasing, till at length it is so general and violent that he can continue no longer. His power of locomotion is impaired; his mastication, his speech, all his manual operations, are interfered with; he becomes unable to convey food to his mouth, and is obliged to be attended to and fed, like an infant; and by and by, if he does not quit the poisonous atmosphere, graver symptoms supervene—wakefulness, delirium, loss of consciousness.

As the tremor increases, the digestive organs become disordered; the appetite falls off, nausea is felt, the tongue becomes furred, and gas collects in the intestines. The patients acquire a remarkable brown hue; and their teeth turn black. The pulse is generally full and slow.

The time required for the production of these effects varies much in different cases; from two years to five-and-twenty. Something depends, no doubt, upon the quantity and intensity of the fumes. Chattin told us that the workmen became ill whenever they had *a large job* on hand. In both his severe attacks (and very often besides, both in him and in his companions) the mercury produced salivation. This was unfrequent in the patients observed by Merat. The duration of the complaint is considerable: it may last two or three months, or longer; and sometimes it is not completely recovered from at all. Yet it is not a fatal disorder.

Although the visible affection is of the *muscles*, the mischievous operation of the poison is really upon the *nerves*, weakening their natural influence. When the will is directed upon the muscles, they contract unsteadily, and with frequent remissions; their action is not sustained; and it is a general observation by all who have written upon the disease, that it is aggravated by all kinds of mental emotion, by alarm, anger, surprise. My patient's shaking was, at first, augmented by the shock of the shower-bath; and always became excessive in thundery weather. So, on the other hand, it has been noticed that whatever tends to stimulate and steady the nervous power, does temporary good: a glass of wine, for example. Chattin informed us that, while the malady was coming on, he

could not get up stairs to his work without first swallowing half a quartern of gin: and that he was obliged to drink porter two or three times a day.

The treatment consists in withdrawing the patient from the injurious atmosphere, and in administering tonics. Conium has been recommended by Mr. McWhinnie. Quina has been found useful. But I have most faith in preparations of iron. My patient Chattin mended decidedly and rapidly when he began to take steel. It was not the mere avoidance of the cause of the complaint that produced the improvement, for he had been away from his work for a fortnight before he applied for admission.

To prevent this effect of mercury, the workmen should be instructed to avoid, as much as possible, inhaling the poisonous fumes, to ventilate the room freely, and to pay great attention to cleanliness. I believe that the furnaces may be so built that the metallic vapour cannot reach the operator. If he cannot avoid being involved in it, perhaps some sort of respirator might afford protection.

Hysteria.—I proceed to the subject of *hysteria:* a subject highly interesting and important, as well as obscure and difficult. I scarcely know how to arrange what I have to say, so as to present the disorder to your notice in the most intelligible manner. Hysteria has characters peculiar to itself: but it also is apt to assume the form, and mimic the symptoms, of various other diseases of a much graver nature. If we are not capable of distinguishing the true malady from that which is its double, we shall be constantly committing most serious mistakes in the prognosis, to our own damage and discredit; and in the treatment, to the injury of our patient. I shall first attempt to describe to you the phenomena which are peculiar to hysteria; and then to point out the class of persons who are most subject to it; and afterwards I shall briefly advert to the imitative freaks which we are almost daily witnessing in hysterical constitutions, and to some other points connected with this extraordinary complaint.

I need not tell you that the hysterical *paroxysm* is almost exclusively confined to women. It occurs under a great variety of forms, but they may all be reduced, for convenience of description, to two. The first of these has a general resemblance to an epileptic fit. The trunk and limbs of the patient are agitated with strong convulsive movements; she struggles violently, like a person contending, rises into a sitting posture, and then throws herself back again, forcibly retracts and extends her legs while her body is twisted from side to side; and so powerful are these muscular contortions that it often is all that three or four strong persons can do to restrain a slight girl, and prevent her from injuring herself or others. The head is generally thrown backwards, and the throat projects; the face is flushed; the eyelids are closed and tremulous; the nostrils distended; the jaws often firmly shut; but there is no *distortion* of the countenance: the cheeks are at rest, unless when, as often happens, the patient is uttering screams, or exclamations. If the hands are left at liberty, she will often strike her breast repeatedly and quickly, or carry her fingers to her throat, as if to remove some oppression there; or she will sometimes tear her hair, or rend her clothes, or attempt to bite those about her. With all this her breathing is deep and laborious; and the heart palpitates. After a short time this violent agitation is calmed: but the patient lies panting and trembling, and starting at the slightest noise or the gentlest touch; or sometimes she remains motionless during the remissions, with a fixed eye; till at once the convulsive movements are renewed: and this alternation of spasm and quiet will go on for a space of time that varies considerably in different cases: and the whole attack frequently terminates in an explosion of tears, and sobs, and convulsive laughter.

There is a *variety* of this form of hysterical paroxysm, in which the patient suddenly falls down insensible, and without convulsions: with slow and interrupted breathing, a turgid neck and flushed cheeks; and she recovers from that condition, depressed in spirits, fatigued, and crying.

You will observe that the symptoms I have been enumerating belong to the nervous system; and indicate great derangement in the functions of animal life. In the other of the two forms to which all the various modifications of the attack may be reduced, the principal marks of disturbance are referable to some of the viscera. The patient experiences a sense of uneasiness in some part of the abdomen, frequently towards the left flank;

a ball appears to roll about, and to rise first to the situation of the stomach, and then to the throat, where the patient feels a choking sensation; the action of swallowing is frequently repeated; the abdomen becomes distended with wind, loud rumblings and sudden eructations take place; there is much palpitation of the heart, the patient is sad and sorrowful, and prone to shed tears.

After the paroxysms, these patients commonly void a large quantity of limpid, pale urine, looking almost like water; and this is sometimes expelled during the fit.

Such is a brief, and, I am aware, incomplete account of the hysterical paroxysm: it sets forth, however, the outline of the two principal varieties of the attack; and you are to observe that the last, the quieter form, is often the prelude to the convulsive; but it not seldom also occurs alone, and then is as indicative of hysteria, as the *petit mal*, to which it is somewhat analogous, is of epilepsy.

Diagnosis from epilepsy.—And before I go any farther, let us again inquire into the circumstances which distinguish the *paroxysms* of those two diseases, epilepsy and hysteria. I have shortly adverted to them before; but we shall be better able to appreciate them now that the main features of each diseased state has been under our consideration. It is of great importance to be able to render the diagnosis certain and accurate. It is a dreadful announcement to have to make to a father or a mother that their child is epileptic: whereas hysteria, though it is sufficiently distressing, is attended, in 999 cases out of 1000, with no ultimate peril either to mind or body. In some instances the diagnosis is perfectly easy; in others it is dubious and full of anxiety. Whenever you fail to satisfy yourself completely as to the nature of a given case, you will do well, in legal phrase, to give your patient the benefit of your doubt, and acquit her of epilepsy; or pronounce her guilty of the minor offence of hysteria.

The points of resemblance, and the points of distinction, belonging to the hysterical and epileptic paroxysm respectively, have been very clearly summed up by Foville.

There are two principal forms of each disorder. In each, one of these forms is convulsive, and the other is not. The non-convulsive form of epilepsy relates exclusively to the sensorium: it is characterized by vertigo, and a suspension (however brief and transitory) of the mental powers. The non-convulsive form of hysteria has little apparent connection with the animal functions: its palpable phenomena consist in derangement of the organic functions of the thorax and abdomen. It is the ganglionic portion of the nervous system that seems chiefly disturbed.

In the epileptic *fit* there is an entire loss of consciousness. The patient, on emerging from the paroxysm, recollects nothing of what has been going on during its continuance. It is not so in the hysterical fit: the loss of consciousness is very seldom complete; and it never occurs at the outset of the attack. The patient often is able to repeat (though she may not always choose to confess it) what has been said by the bystanders during the period when she appeared insensible. This is a point of distinction well worth remembering, for more reasons than one. It not only helps the diagnosis when the fact comes out; but it suggests certain cautions to ourselves. We must take care not to say any thing by the bed-side of an hysterical patient which we do not wish her to hear; and we may take advantage of her apparent unconsciousness, and pretend to believe in it, and speak of certain modes of treatment which she will not much approve of, but the very mention of which may tend to bring her out of the fit.

In the epileptic paroxysm the face is usually livid; and foam which is frothy with air, or red with blood, escapes from the patient's mouth. These are symptoms which we do not see in the fits of hysteria. The convulsive movements even offer some characteristic shades of distinction. In epilepsy they are often more marked on one side of the body than on the other; and the same movements are rapidly repeated: there is a strangling rattle in the breathing: while in hysteria the forcible flexion and extension of the limbs, and the contortions of the trunk, are more sudden, irregular, and as it were, capricious; the respiration is deep, sighing, mixed with cries, and sobs, and often with laughter. But, perhaps, the convulsive motions differ most in the face: the epileptic expression is usually frightful: the eyelids half open, the eyeballs rolling, the teeth grinding, and the gums exposed by the retraction of the lips, the tongue protruded and bleeding; the complexion leaden: while in hysteria the cheeks are red, but at rest; the eyelids are closed and trem-

bling; if you raise the upper one, you will see the eye fixed, perhaps; but it is bright, and very different from that of the epileptic, which, if it be not rolling, is dull, projecting, and the pupil usually dilated.

Foville states that when, besides a sudden loss of consciousness with convulsive movements, there are also lividity of the face, and an escape of frothy saliva from between the lips, and the convulsions are more pronounced on the one side of the body than the other, the disease is epilepsy, and not hysteria; and I think he is right.

The hysterical seizure may be over in a quarter of an hour, or in less time than that; or it may last many hours, or even several days.

The hysterical seizure is almost peculiar to women: and it seldom occurs in them except during that period of their lives in which the menstrual function of the uterus is or ought to be in activity. In this country it is most apt to occur between the ages of fifteen and forty; and in the vast majority of patients who do suffer it, you will find some marked derangement of that particular function. These facts alone afford a strong corroboration of the ancient theory, which ascribed the whole of the phenomena to uterine disorder; and *named* the disease accordingly. You will hear or read of disputes as to whether the womb, with its appendages, or the nervous system, is the seat of hysteria. But such disputes are merely verbal, I conceive: no doubt the convulsive movements, and the mental affection, and the unnatural sensations, depend upon some altered condition of the brain and nerves; but it does not follow that the disease originates in that altered condition. We know that the uterus or ovaries cannot of themselves determine the muscles to contract; but if they be in an unhealthy state they may act upon the muscles through the *medium* of the nervous system: and such I take to be the fact. *How* they do so we no more know, than we know how the little finger is bent when we resolve to bend it.

But, say some, we every day meet with diseased conditions of the uterus and ovaries—amenorrhœa, dysmenorrhœa, menorrhagia, even disorganization, without any of these nervous symptoms: true; and we cannot always fathom the mystery of this. But one thing is certain, that there exists in some persons a much greater readiness to take on the disease, upon the application of the exciting cause, than in others. This predisposition I have had occasion to advert to again and again, since I began to speak of the *spasmodic diseases* of the nervous system. Such diseases occur in certain individuals only; and in these individuals there pre-exists a peculiar condition of the nervous system, "for which," says Dr. Alison, "we have no more precise or definite expression than *nervous irritability*, or *mobility;* a condition which is more common in women and children than in men, and more common in all persons when in a state of weakness, than when in the full enjoyment of muscular strength; in women, particularly, more common about the menstrual periods, and immediately after delivery, than at other times; more common likewise in those in whom the monthly discharge is habitually *excessive*, or *altered*, as in *leucorrhœa*, or suddenly *suppressed*, or more gradually obstructed in the different forms of *amenorrhœa*, than in others. In this condition of mobility, both sensations and emotions are intensely felt; and their agency on the body is stronger and more lasting than usual; continued voluntary efforts of mind, and steady or sustained exertions of the voluntary muscles are difficult, or impossible; the muscular motions are usually rapid and irregular, and the 'animus nec sponte, varius et mutabilis.'" In persons of this movable temperament, spasmodic complaints are easily excited: and the tendency to their recurrence is increased by each repetition of them.

Now the persons who suffer hysteria are of this class. They are commonly young women, in whom the process of menstruation is in some way or other disordered; and who either are naturally of a feeble constitution, or have been debilitated by disease, or by their habits of life. They often are pale; have cold hands and feet; are subject to chilblains; eat but little, and do not fancy meat, which they sometimes absolutely dislike and refuse; or their taste is depraved and capricious; they will devour wax candles, wafers, chalk, sealing wax, slate pencil, and such trash. And, what is very curious and characteristic, although they often abstain almost entirely from animal food for weeks or months together, and take very little nourishment of any kind, they do not in general emaciate. You might expect that, under such a mode of life, they would waste away: but they continue round, and plump, and smooth. Some of them are even ruddy.

And belonging to women of this peculiar constitution there is one other very remarkable character, which it behoves us to make ourselves thoroughly acquainted with. Almost any part of the nervous system, in these persons, is liable, under the influence of slight causes, and even without any obvious cause, to fall into a disordered state of action and suffering more or less resembling that which inflammation or organic disease might excite in the same part.

This is a most important fact: because if we erroneously ascribe symptoms which really result from inflammation to mere nervous or hysterical disorder, we may suffer the patient to perish for want of active measures that would have saved her: and, on the other hand, if we apply to these nervous, imitative, hysteric complaints, the treatment proper for inflammation, we shall generally, indeed, relieve our patient for the time; but we shall leave her more prone to the nervous affection than before, and permanently damaged by our mischievous activity.

Disorders simulated by hysteria.—I say that almost every kind of serious disease may be mimicked by what we must call hysteria. And your skill will sometimes be severely tasked to determine the true import of the symptoms, and the real nature of the case.

One of the diseases which is most often copied by hysteria, is *inflammation of the peritoneum*. You will find a patient complaining of acute pain of the abdomen, aggravated by the slightest pressure; and she shall have, perhaps, a hot skin, a quick pulse, and a furred tongue. When you meet with such symptoms in a young female, in whom there is any derangement or irregularity of the uterine functions, you will do well, before you bleed her to syncope and cover her abdomen with leeches, to ask yourselves whether all this suffering may not be simply nervous. Search into her previous history as much as you can: if you find that she has had similar attacks before; if she has been known to suffer hysterical fits; and if the tenderness is excessive, and, as it were, superficial, felt upon the slightest touch as much as when firmer pressure is made, you may generally spare the blood-letting, purge the patient well, and cause an assafœtida enema to be thrown into the rectum; and in a few hours you will find that the peritonitis has vanished.

Among the pains that infest females of the hysteric constitution, and which are apt to be erroneously ascribed to inflammation, *stitches and pains in the hypochondria* are probably the most common. They are oftener complained of in the left hypochondrium than in the right. These things are much more generally understood now than they used to be even a few years ago. I cannot tell you how many persons I have seen who had been diligently treated with leeches, and blisters, and blue pill, for supposed chronic inflammation of the liver or spleen, or still more actively depleted for presumed pleurisy or pericarditis, when no such inflammation existed, and when the treatment, by reducing the strength, tended to rivet that mobility of system which was the chief predisposing cause of the pains.

You would scarcely suppose that *palsy*—perfect hemiplegia or paraplegia—could be simulated by hysteria: yet this certainly is the case; and I have seen instances of it even among hospital patients. They are difficult and perplexing cases. The sudden occurrence of the paralysis, without any of the other symptoms which commonly mark the real disease, its sudden disappearance, and above all, the supervention of an hysterical paroxysm, will often disclose the true nature of the affection. Hysterical affections referred to the throat are very common. *Aphonia*, for example: the voice being lost on a sudden, and returning as suddenly. Mock *laryngitis*. I remember being asked by Sir Charles Bell some years ago to see a young woman in the Middlesex Hospital under his care. She had recently arrived, and was breathing with the stridulous noise peculiar to inflammation of the larynx. She had twice before, in the country, had tracheotomy performed for similar attacks: and there were the scars of the operations on the neck: but both Sir Charles and myself were satisfied, upon considering all the circumstances of the case, that the difficult inspirations were spasmodic and hysterical: and she recovered under the remedies which do good in hysteria. Inability to swallow, *dysphagia*, is another of the hysterical vagaries relating to the parts about the throat. Dr. Bright has a very instructive case of that kind. A patient was sent to Guy's Hospital for stricture of the œsophagus. It was stated that the difficulty of deglutition had existed for several weeks, and was increasing. The surgeon under whose care she was admitted was instantly struck by certain circum-

stances which did not seem to consist very well with the notion that there was organic disease. Her appearance belied it, and her age. But he thought it right to examine the œsophagus by means of a probang; and no sooner was the instrument introduced, than the patient went into an hysterical fit, which was followed immediately by hysteria in several females in the ward. The complaint turned out to be nothing but an hysteric constriction, and was soon completely removed. This incongruity and want of harmony among the symptoms often affords a clue to the real character of the complaint. Dr. Conolly states that he was summoned to a lady, whose uterine functions were disordered, and who was affected with what she called *asthma*, a peculiar *difficulty of breathing*. He observed this great inconsistency; the respirations were forty in the minute; the pulse only twenty. And he says that he has met with the same interchange as it were of frequency between the number of pulsations of the heart, and the number of the breathings, in other hysterical females; "the pulse being sometimes as slow as the ordinary respiration, and the respirations sometimes advanced to the ordinary rapidity of the pulse."

Among the hysteric affections of the air-passages, there is a peculiar kind of *cough* which you ought to be acquainted with. It is loud, harsh, dry, more like a bark than a cough. Sometimes it is incessant, sometimes it occurs in paroxysms which, I verily believe, are more annoying to hear than to suffer. Hysterical affections of the diaphragm again are by no means rare. I had a very obstinate case of that sort in one of my hospital patients. She would sit in her bed all day long, uttering every eight or ten seconds a loud and most discordant *hiccup*. And I remember an out-patient, who presented a picture of perfect health, and who came week after week, to be cured of what I could consider nothing but an hysterical *eructation;* it was continual and distressing, and prevented her from obtaining any employment as a servant. Hysterical *vomiting* is also frequent, simulating cancer of the stomach. Nay, hysterical *hematemesis*. A romantic girl was for some *months* under my care in the hospital with that complaint. She vomited such quantities of dark blood, (which did not coagulate, however,) as I would not have believed if I had not seen them. Day after day there were potfuls of this stuff; yet she did not lose her flesh, and she menstruated regularly; and what was very curious, the vomiting was always suspended during the menstrual period, and recurred again as soon as the natural discharge ceased. I said she was romantic; but I should rather have said that she had that peculiar mental constitution which belongs to hysterical females. She used to write me long letters of thanks for my attention, though I was heartily tired of her; and these were couched in all the fine language of the Minerva press. At last I sent her away; just as bad as when she came into the hospital. This was five or six years ago; and last year she called at my house with a present of some game, and told me she had got married to a hair-dresser, and was quite recovered.

There is a kind of sanguineous *expectoration* belonging to females of this class, and very likely to mislead the unwary. I meet with two or three instances of it every year. The patient excreates daily, or at irregular intervals, a thinnish fluid something like saliva, more or less tinged and streaked with brown or florid blood. A young hand investigates diligently the source of the bleeding, and puzzles himself to determine whether the case be one of *hematemesis* or of *hemoptysis*. Nine times out of ten it is neither the one nor the other. The blood comes from the mouth or fauces.

Hysterical affections of the *joints* are very common. A young girl became my patient in the hospital for some trifling ailment, and after a short time she began to complain of great pain in her knee and hip; she could not stand upon the limb, nor bear to have it moved or touched. I got one of my colleagues to see her: he was so satisfied of the nature of the case—so convinced that it was a genuine example of inflammation and ulceration of the hip-joint—that he gave a little lecture to the pupils who stood round the bed upon the characteristic position in which the patient lay; and he took her into one of the surgical wards to be under his own care. Some time afterwards I had occasion to go into that ward, and there I found my former patient with her heel drawn tight up against her buttock. It turned out that she had had no serious disease of the hip at all; both it, and the rigid contraction, gave way under measures which could have done no good to an ulcerated joint. I think the first clue to the real nature of her malady was the occurrence of a fit of hysteria. Sir B. Brodie says that among the higher classes of society, at least

four-fifths of the female patients who are commonly supposed to labour under diseases of the joints, labour under hysteria, and nothing else.

Another prank belonging to hysteria, and one which it is very necessary that you should be on your guard against, is that of mimicking disease of the bones of the spine. The patient complains of pain and tenderness in her back, and of weakness probably in her lower extremities: and it is now become notorious that scores of young women have been unnecessarily confined for months or years to a horizontal position, and have had their backs seamed with issues, for supposed disease of the bones of the vertebræ, who had really nothing the matter with them but hysteria, and who would probably have soon ceased to complain, if, instead of being restricted to that unnatural imprisonment and posture, they had taken a daily gallop on horseback.

It is curious enough how the mind is apt to become affected in some of these cases. After the patient has been lying supine for some weeks, she *is* unable to stand or walk, simply because she *thinks* she is unable. The instant she makes a fair effort to use her limbs again, she can and does use them. Her condition is at once reversed. *Potest quia posse videtur.* Mr. Corfe, the present apothecary to the Middlesex Hospital, has no little trouble with patients of this kind; but he generally succeeds in *making* them walk, and in convincing them, as well as himself, that they may do so with impunity. Sometimes, though the authority of the Doctor may not be efficacious in this respect, some stronger influence prevails. A lady told me not very long ago that an acquaintance of hers, a member of a family of distinction, had been lying I know not how long on her back; that position having been prescribed to her by some medical man for a presumed disease of the spine. She lost all power of using her legs; but she got quite fat, as, indeed, well she might, for her appetite was remarkably sharp, and she lived chiefly upon chickens; and the number of chickens she devoured was incredible. She lived at some little distance from town, and at last Sir Benjamin Brodie was sent for to her. Now Sir Benjamin, to use a vulger phrase, is *up* to these cases; and he wished to see her *try* to walk: but she declared that the attempt to do so would kill her. He was resolute, however, and had her got out of bed; and in a few days time she was walking about quite well, and very grateful to him for his judicious conduct. A medical man of less name, or of less determination, would probably have failed. Dr. Bright has a good example of a somewhat similar kind; showing the power of another form of influence. He was asked to see a young lady who had been confined to her bed for nine months. If she attempted to move she was thrown into a paroxysm of agitation, and of excruciating agony, affecting more particularly her abdomen. She had almost lost the use of the lower extremities; and she and her friends seemed to have given up all hope of her restoration. But she presented no appearance of important disease; her countenance bore no marks of visceral mischief; nor was it possible to discover any proof of organic change. Dr. Bright set the case down in his own mind as one of hysteria. She seemed to have derived relief from some stimulating injection, and from certain pills. As her friends were in moderate circumstances, Dr. Bright talked seriously to the mother, and recommended that simple water should be employed for the injection, and that bread pills should be substituted for those she had been taking. The mother soon perceived that these means produced the same tranquillizing effects on her daughter which had hitherto been ascribed to the medicine. "My visits," he says, "became less frequent; I was absent a fortnight; on my renewing my visit, no change had taken place. I attempted to get her shifted gently from the bed to the sofa, but it was impossible; the paroxysm almost overcame her. Once (after having attended altogether about nine months) I called after an absence of nearly a month; her sister met me at the street door with a smiling face to tell me that our patient was quite well: and on inquiry, she related how, three mornings before, under a *deep religious impression*, she had completely recovered all her powers; and I found her sitting up, working and amusing herself as if she were completely convalescent from some ordinary illness."

These are the cases which suit the purposes of miracle-mongers. A few years ago all the journals belonging to a certain party in the religious world were full of an instance of miraculous cure. The patient was a young woman; her legs had been paralytic, or contracted, I forget which; some enthusiastic preacher had influence enough with her to make her *believe* that if on a certain day she prayed for recovery with a strong faith, her

prayer would be successful, she would recover at once; and she did so. No one can doubt that it was just such a case as those I have now been mentioning. *Many* of these pseudo-diseases terminate suddenly under some strong moral emotion. A fall—a fire in the house—any overwhelming terror, will sometimes put an end to them. And where the joints have been the parts affected, several patients have declared that they felt a sensation as if something had snapped or given way in the part, immediately before the sudden recovery took place.

Some of the shapes assumed by this pathological Proteus are hideous and disgusting. Paralysis of the muscular fibres of the bladder, or spasm of the sphincter, sometimes really occurs, sometimes is only aped, in hysteria. It is a common trick with these patients to pretend that they labour under *retention of urine;* and that, although the bladder is full, they cannot make water. The daily introduction of the catheter by a dresser or apprentice appears to gratify their morbid and prurient feelings. Sometimes, no doubt, the difficulty is real; but it is oftener feigned or exaggerated. I have again and again known it disappear upon the patient's being left, without pity, to her own resources. But girls have been known to drink their urine, in order to conceal the fact of their having been obliged and able to void it. The state of mind evinced by many of these hysterical young persons is such as to entitle them to our deepest commiseration. The deceptive appearances displayed in the bodily functions and feelings find their counterpart in the mental. The patients are deceitful, perverse, and obstinate; practising, or attempting to practise, the most aimless and unnatural impositions. They will produce fragments of common gravel, and assert that these were voided with the urine: or they will secrete cinders and stones in the vagina, and pretend to be suffering under some calculous disease. A young woman contrived, in one of our hospitals, to make the surgeons believe that she had *stone in the bladder;* and she actually submitted to be placed upon the operating table, and to be tied up in the posture for lithotomy, before a theatre-full of students; and then the imposture was detected. Sometimes they simulate *suppression of urine*, and after swallowing wh they have passed, vomit it up again, to induce the belief that the secretion has taken place through a new and unnatural channel.

It is impossible, I say, not to pity the unhappy victims of this wretched disorder, when their morbid propensities drive them to such acts as these. I mention them because you must expect to meet with such cases; and because, while you take care not to express your suspicions prematurely, or on light evidence, you should be on your guard against the mortification of being deceived, by the false signals held out, into active and ill-directed measures of treatment.

There is another very common hysterical *pain* which I ought to have mentioned, viz. a pain occupying some one point in the head; the patient speaks of it as a sensation like that which would be caused by driving a nail into the part; and the affection has therefore been called the *clavus hystericus*. It is often situated just above one eyebrow; and it sometimes comes on every day, at the same hour. Now in these cases it imitates very closely the hemicrania, which constitutes no uncommon form of an intermittent, and is called, accordingly, the *brow ague*. The distinction between the two—whether the affection, I mean, be hysterical or aguish—is not of any great consequence: but in many of the former examples of hysterical pain mimicking organic or inflammatory disease, the diagnosis is obviously of the greatest moment.

How, then, is it to be made? You may generally, I believe, be led to a right judgment if you look to the several points that I have incidentally touched already. You may guess that the affection is hysterical if the patient be a young unmarried woman; if there be any disorder or irregularity in the uterine functions; if you can trace any history of previous hysterical disease; and especially if she is subject to *fits* of hysteria. The suspicious symptoms may often be traced back, and found to spread themselves over a considerable previous period of time; yet there is no such wasting or commensurate deterioration of the general health and strength as might be expected in organic disease. When the complaint simulated is some acute local inflammation, and there is pain increased upon pressing the part, you will find that the pain is aggravated by the gentlest touch; it is more felt if you brush your hand over the surface, or slightly pinch the integuments, than when firm pressure is made: and you will find also that this exquisite tenderness is not

limited to the part complained of. Suppose it is the abdomen, the patient will shrink and exclaim if you suddenly put your finger on her neck, or her arm. The suspicion that the disorder is nervous or hysterical will also be corroborated if the symptoms which resemble the symptoms of inflammation arise and subside rapidly, without obvious cause for such fluctuation; and if various organs appear to be attacked in succession. Between the several symptoms that mark real disease there is always (as we learn by experience) a certain congruity and relation; but in the simulative displays of hysteria the symptoms are apt to be irregular, inconsistent, contradictory. When, after the most careful investigation of the case, you still doubt, it will be right either to pause, or to treat it upon the most *un*favourable supposition. The consequences of suffering active inflammation to go on unchecked would be far worse than the temporary and slight and remediable injury to the system which might result from once applying the remedies of inflammation to a case of mere hysteria. There is another hazard also which you must be aware of, and seek to avoid; that of overlooking real disease when it is mixed with, and masked by, hysterical symptoms. It is not easy to lay down positive rules of action for all these supposable cases; but I trust that I have said enough to convince you of the importance of making the diagnosis of hysteric complaints a careful object of your future study.

I have hitherto spoken of hysteria as if it were exclusively a malady of females. Etymologically, to apply that term to the diseases of males would be absurd. But that peculiar modification of the nervous system which is observed in hysteric girls does certainly present itself, though rarely, in young men. I have seen two or three instances of what I could give no other name to than hysteria, in males. One of them was in the person of a young surgeon who had been house-surgeon to the Middlesex Hospital. I believe he applied to not less than a dozen medical men for advice; and in that batch I happened to have my turn. He had some of the symptoms that are ascribed to hypochondriasis; *i. e.* he was exceedingly attentive to his own sensations, and fancied he had a number of diseases which had no existence but in his own imagination: he showed great unsteadiness and infirmity of purpose; was what is called "very nervous;" and had occasional bursts of choking, and tears, and laughter, exactly resembling those which we so often witness in the other sex. Many cases of hysteria in the male have been recorded by different writers. The same movable state of the nervous system, and the same symptoms referable to that system, may exist in both sexes. In females, in nine cases out of ten, or in a much larger proportion, the exciting cause of the hysteria is connected with the sexual functions; and that is all that can be meant when it is asserted that for the female the complaint is not badly named, but has an intimate dependence upon the uterine sympathies. At the same time it is quite true that the "uterus is not the only organ of which the irritation may so affect the nervous system as to produce hysteria."

As in epilepsy, so also in hysteria, the treatment to be adopted regards, first, the paroxysm itself; secondly, the condition of the patient during the absence of the paroxysm.

One object, during the paroxysm, is to prevent the patient from injuring herself, by her hands, or by her teeth, or in her convulsive movements. Her dress should be loosened; but it may be necessary to confine her hands and arms. The next thing to be aimed at is the putting an end to the fit. Various measures are found more or less useful for that purpose. The patient should be surrounded, as far as that is possible, with cool fresh air. If she is able to swallow, you may sometimes shorten the attack by administering a couple of ounces of the mistura assafœtidæ; or half a drachm of æther, with fifteen or twenty minims of laudanum, in camphor julep; or a draught containing half a drachm of the tincture of valerian. When the patent cannot or will not swallow, she may sometimes be brought about by stimulating volatile substances offered to the nostrils. Signal good may also be effected by fœtid or stimulant enemata: the enema assafœtidæ, for example, made by mixing two drachms of assafœtidæ with half a pint of water, by means of the yolk of an egg; or the turpentine injection, made in the same manner, and containing half an ounce of turpentine: or the same quantity of ice-cold water thrown into the rectum, or applied to the pudenda, will often bring the fit to a speedy termination. Indeed I believe there is more virtue in cold water, in hysterical diseases, than in any other single remedy. In the paroxysm it may be freely and repeatedly sprinkled, or dashed with some force, upon the face and chest. Active purges are beneficial and requisite in almost all these cases. There

is commonly a costive, sometimes an obstinate, and always an unnatural, state of the bowels.

In those long paroxysms—if they may be so called—in which some other disease is simulated by hysteria, the cold affusion is a most valuable resource: especially in those forms of the disorder in which a limb is permanently bent, or incapable of motion. In several instances, in which such contraction had existed for a long time, it has yielded, in the Middlesex Hospital, to a few minutes' application of the cold douche. Mr. Corfe, as I stated before, takes much pains with these cases; he pours cold water from a tea-kettle, or any other convenient vessel, in a small stream, from a moderate height, upon the contracted limb: it has been bent up for weeks perhaps; no power that you are able to exert can extend it, and any *very forcible* attempts to straighten it give the patient extreme pain. After the stream of water has been kept up for a short time, the patient complains of it very much; but Mr. Corfe is inflexible—more so than the culprit limb—he goes on: presently the limb begins to tremble, the tight state of the muscles is evidently on the point of yielding, and in no long time they are entirely relaxed and manageable, and the member becomes as lithe and movable as ever. It often happens that the state of contraction recurs; but a repetition of the douche has always the same good effect, and by degrees the habit is broken, and the patient set free. It requires some determination to put this expedient in practice. The patient looks upon you as a monster of cruelty; and, in private, the friends will not always allow such "rough" treatment, as they consider it. Sir Charles Clarke, who necessarily sees a great number of these cases—they are more common in the upper than the lower classes of society—is a great advocate of this ducking system. A paper of his upon the subject was read before the College of Physicians a few years ago. He recommends a "sudden and lavish" application of water to the face; or the immersion of the whole body. He describes the class of patients, in whom the hysterical affection which is curable by that method occurs, as being generally females of a pasty complexion, fat, pale, and weak; or such as evince the ordinary signs of debility, a feeble pulse, cold extremities, and purpleness of parts distant from the centre of circulation. The age of the patients varied from ten to thirty years; in many of them menstruation was imperfect, or absent. A medical practitioner whom I met lately at a patient's house, told me he had just come from another patient, upon whom he had seen a surprising cure performed. A young lady, for many days, had been affected with trismus. She was unable to open her jaws, and therefore could neither speak nor eat. At last Sir C. Clarke was called in to see her. He presently comprehended the nature of her ailment, had her placed with her head hanging over a tub by the side of the bed; and proceeded to pour pitchers of water on her face. Before he had emptied the second the patient began to scream and complain, and to give very audible indications that she could open her mouth. I say although these patients get relief by the treatment, they do not like it; and if they are convinced that it will be put in force, they will generally contrive not to require it. Of all the spasmodic affections, hysteria is that which is most readily propagable by what may be called moral contagion. If, in a large ward, one girl goes off in a fit, half a dozen others perhaps, all who happen to possess the hysteric diathesis, will experience a strong inclination to follow her example. But this chorus, as it were, of hysteria, is much more common in some wards than in others. A stern nurse, or a general order that the cold affusion shall at once be employed in every instance of an hysterical fit, will keep the complaint wonderfully in check: and on the other hand, great sympathy with such patients has a striking effect in encouraging the paroxysms. These facts show that the symptoms are, to a certain degree, under the patient's control: the fits are not wholly wilful; neither are they wholly unconquerable.

I have but little to say respecting the medical management of such patients in the intervals between the paroxysms. The objects to be aimed at are, to restore the nervous system to the requisite degree of stability: and to correct the disordered functions of the uterine system. Now much the same plan of treatment is applicable to both these objects; and I have spoken of the remedies that are found most beneficial for giving tone and firmness to the system, when upon the subject of epilepsy, and other nervous spasmodic ailments. The following points must be kept in view. The regulation of the bowels, which are mostly sluggish, by aloetic aperients; the exhibition of some form or other of steel;

the steady employment of the shower bath; regulated exercise, both on foot and on horseback; the avoidance of hot rooms and of late hours, both in respect to going to bed, and to rising from it; the avoidance also of strong moral emotions, of novel reading, and of all the other thousand modes of dissipation, mental and bodily, which always accompany, and abate the blessings of, a high state of civilization. Marriage often proves a cure: sometimes it does not.

The disposition to hysterical disorder may be more easily prevented than cured; but upon this point medical men are not consulted. Parents do not foresee the misery they are often laying up for their daughters by the unnatural mode of life to which they are subjected for the sake of filling them with fashionable accomplishments. I cannot close this subject, and this lecture, better than by quoting Sir Benjamin Brodie's remarks on the same point, as I find them in a little work very recently published by him, and containing many highly valuable observations and instructions in respect to *local hysterical affections.*

"You can render (he says) no more essential service to the more affluent classes of society, than by availing yourselves of every opportunity of explaining to those among them who are parents, how much the ordinary system of education tends to engender the disposition to these diseases among their female children. If you would go further, so as to make them understand in what their error consists, what they ought to do, and what they ought to leave undone, you need only point out the difference between the plans usually pursued in the bringing up of the two sexes. The boys are sent at an early age to school, where a large portion of their time is passed in taking exercise in the open air; while their sisters are confined to heated rooms, taking little exercise out of doors, and often none at all, except in a carriage. Then, for the most part, the latter spend much more of their time in actual study than the former. The mind is over educated at the expense of the physical structure: and after all, with little advantage to the mind itself: for who can doubt that the principal object of this part of education ought to be, not so much to fill the mind with knowledge, as to train it to a right exercise of its intellectual and moral faculties; or, that other things being the same, this is more easily accomplished in those whose animal functions are preserved in a healthy state, than it is in others."

LECTURE XXXIX.

CATALEPSY. EXTACY. NEURALGIA: TIC DOULOUREUX; SCIATICA; HEMICRANIA.

THERE are yet some strange forms of nervous disorder which require to be *mentioned;* but upon which I do not intend to *dwell. Catalepsy* is one of these; and what is called *extacy* another. These affections are very *rare* as well as very *wonderful:* so wonderful and rare, that weak and superstitious persons have referred them to the interposition of supernatural agents in human affairs; and stronger minded persons, who happen never to have witnessed such diseases, deny their occurrence as fabulous, or laugh at them as the tricks and cheatings of imposture. They certainly do happen, however; and they happen mostly in the same class of persons in whom hysterical and nervous complaints of all kinds are most common. They often appear to be produced by similar causes with these: they resemble hysteria in being seldom attended with any danger to life; their pathology is, if possible, still more obscure than that of hysteria: and if I were to speak of the treatment which would seem to be most suitable for their cure or prevention, I should merely have to repeat what I said, upon the treatment and prevention of hysteria, in yesterday's lecture. I shall content myself, therefore, with a short description of these two affections, that you may be aware of their characteristic phenomena, and not be taken by surprise in case either of them should occur to you in your practice.

Catalepsy.—A fit of catalepsy implies a sudden suspension of thought, and sensibility

and voluntary motion; the patient remaining, during the paroxysm, in the position in which she (for it is almost always a female) happened to be at the instant of the attack, or in the position in which she may be placed during its continuance; and all this without any notable affection of the functions of organac life.

This is certainly a very curious state, and one different from any that we have yet contemplated. We have had the muscles rigidly contracted with tonic spasm, while the powers of the mind, and the sensibility of the body, were unimpaired. We have had the same muscles shaken with clonic convulsions; both with and without accompanying disorder of the intellectual functions. But here we have a new phenomenon: the mental faculties are in abeyance, and the sensibility is abolished, and so also is the function of voluntary motion; but the limbs are not tied down by spasm; nor agitated by successive contraction and relaxation of their muscles; not yet left, like portions of dead matter, passively obedient to the laws of gravity: they assume any posture, however absurd, however (to all appearance) inconvenient and fatiguing, and that posture they retain, until some new force from without is applied to them, or until the paroxysm is at an end. The patient so affected, with open staring eyes often, and outstretched limbs, looks like a waxen figure; or an inanimate statue; or a frozen corpse. Indeed, Hoffman seems to have formed the strange conclusion that, as catalepsy, so far as he knew, occurred most frequently in winter, it must depend on congelation of the nervous fluid.

These singular attacks occur in paroxysms; and they have been known to alternate with well-marked hysteria; and to take place in connection with insanity. I have never seen an instance of perfect catalepsy; which I now regret, as I once had an opportunity of doing so of which I did not avail myself. Dr. Gooch has described a case of it, as he witnessed the disease in a patient who suffered puerperal mania. She had long been subject to the common forms of hysteria: this is illustrative of what I have often mentioned, viz. the consanguinity of these nervous disorders. It had become necessary to confine this patient in a straight waistcoat; she was attended by Dr. Gooch and Dr. Sutherland. I will quote Dr. Gooch's account of the cataleptic state; for it is authentic and modern. He says, "a few days after our first visit we were summoned to observe a remarkable change in her symptoms: the attendants said she was dying, or in a trance. She was lying in bed, motionless, and apparently senseless. It had been said that the pupils were dilated, and motionless, and some apprehensions of effusion on the brain had been entertained; but on coming to examine them closely, it was found that they really contracted when the light fell upon them; her eyes were open, but no rising of the chest, no movement of the nostrils, no appearance of respiration, could be seen; the only signs of life were her warmth and pulse; the latter was, as we had hitherto observed it, weak, and about 120.

The trunk of the body was now lifted, so as to form rather an obtuse angle with the limbs (a most uncomfortable posture), and there left with nothing to support it; there she continued sitting while we were asking questions and conversing; so that many minutes must have passed. One arm was now raised, then the other; and where they were left, there they remained. It was now a curious sight to see her, sitting up in bed, her eyes open, staring lifelessly, her arms outstretched, yet without any visible sign of animation. She was very thin and pallid, and looked like a corpse that had been propped up, and had stiffened in this attitude. We now took her out of bed, placed her upright, and endeavoured to rouse her by calling loudly in her ears; but in vain. She stood up, but as inanimate as a statue. The slightest push put her off her balance. No exertion was made to regain it. She would have fallen if I had not caught her.

She went into this state three several times. The first time it lasted fourteen hours, the second time twelve hours, and the third time nine hours; with waking intervals of two days after the first fit, and one day after the second. After this the disease resumed the ordinary form of melancholia; and three months from the time of her delivery she was well enough to resume her domestic duties."

There is a minor form of this affection described, in which the patient is incapable of moving or speaking, but is conscious of all that goes on around him at the time. I saw a lady last year, who was subject to these attacks of imperfect catalepsy; which have been whimsically, but very expressively, called also attacks of *day-mare*. From her time of life, her habits, and some other points in the history of the disease, I concluded that in her case

these seizures of temporary loss of muscular power without loss of consciousness were dependent upon a diseased state of the blood-vessels of the brain. She afterwards consulted Dr. Chambers; and he told me that he had formed the same opinion of the nature and cause of the symptoms.

Extacy.—In what is called *extacy*, the state is different. The patient is lost to all external impressions; but wrapt and absorbed in some object of the imagination. The muscles are sometimes relaxed; sometimes rigid as in slight tetanus: but the loss of voluntary power over them is not complete or universal, for these patients often speak in a very earnest manner, or sing: they are, as the term extasis imports, out of the body at the time, wholly engrossed in some high contemplation. This state is not uncommon as forming a part of religious insanity: and sometimes it runs into ordinary hysteria. Nervous and susceptible persons are apt to be thrown into these trances under the influence of animal magnetism: and grave authors assure us that the intelligence which then deserts the brain concentrates itself in the epigastrium; or at the tips of the fingers: that people in that state read letters which are placed upon their stomach; answer, oracularly, enigmatical questions; describe exactly their own diseases; and even foretell future events. Credat Judæus Apella, Non ego. I take for granted that they who were in the habit of speaking, a few years since, in some of our places of worship, in what they called *unknown tongues*, were either gross impostors, who deserved to be publicly whipped, or persons labouring under this disease, and wanting physic. Dr. Copland mentions a curious fact in connection with this subject. He says that many of the Italian Improvisatori are in possession of their peculiar faculty only while they are in a state of extatic trance; and that few of them enjoy good health, or consider their gift as otherwise than something morbid.

I repeat that I can add nothing respecting the pathology or the management of these diseases, to what I have already said in reference to the whole class to which they belong.

Neuralgia.—Leaving these nervous disorders, in which the function of voluntary motion is so curiously modified; and in which there sometimes is no alteration of the intellectual faculties, and sometimes very great disturbance, or the complete suspension of them; I would beg to turn your attention to another class of complaints, in which the nervous system is still the part principally interested, but in which the deviation from the natural state is manifested chiefly in the function of sensation; the powers of thought, and of voluntary motion, being scarcely affected, or not affected at all. Complaints, I mean, in which the sensibility is perverted, and augmented: cases of nervous *pain*. We have considered before that modification of sensation which consists in numbness, or anæsthesia, *i. e.* in the diminution of the natural sensibility, or its total privation. We have noticed also incidentally many perversions of sensation; such as giddiness, nausea, faintness, and the like: and in the same incidental way the morbid *exaltation* of the sensibility which is called *pain*, has come before us, as a symptom of various other diseases; of inflammation, and of hysteria. But there are other diseases which consist of pain, and of nothing else, that we can perceive: they are often attended by no inflammation, no detectible change of structure in the painful part, no fever. These affections are included under the general term, *neuralgia*. Now pain is one of the things which we are oftenest consulted about; and these neuralgic pains are often excessively severe and troublesome; and it cannot but be of importance to understand what has been ascertained of their nature, and causes, and capability of cure.

That pain is owing to some morbid condition, or to some irritation of a nerve, we may sometimes know, by finding that it occupies exactly the course, and follows the distribution of the nerve. But when, as often happens, the pain is confined to a certain spot, we then conclude it is neuralgic, if and because we can find no other explanation of its existence.

What increases the difficulty of making out the cause and origin of these nervous pains, is that they may be produced by some source of irritation situated at a distance from the part in which the pain is felt. It may be placed in the brain itself, or in the spinal cord; or in the trunk of the nerve that supplies the affected part; or in one of the branches of the same trunk, which branch is distributed to another part. If you strike the inside of the elbow in a certain way, so that the blow lights upon the ulnar nerve, a

peculiar tingling sensation is felt in the little finger: that is, *not* in the part struck, but in the sentient extremity of the same nerve; and the same thing happens continually in disease. There is an excellent paper on this subject, by Sir Benjamin Brodie, published in one of the earlier volumes of the Medical Gazette, in which he has collected numerous and striking illustrations of the production of nervous pain by irritation situated in a distant part. Thus, to take a case in point; a man was admitted into St. George's Hospital on account of severe pain on the inner side of his knee. The joint was carefully examined, but no mark of disease could be detected in it. On tracing the limb upwards, however, an aneurism of the femoral artery, as big as an orange, was discovered in the thigh. This the patient thought nothing of; his only concern was the pain in his knee. Sir E. Home performed the usual operation for aneurism: and the moment the ligature was drawn firmly round the artery in the upper part of the thigh the tumour ceased to pulsate, and the pain in the knee ceased also. This man died four or five days after the operation: and on inspection of the limb after his death, the aneurism was found reduced to one-half its former size; and some branches of the anterior crural nerve, which passed over it, and which must have been kept on the stretch previously to the operation, were seen to terminate in the part to which the pain had been referred on the inside of the knee. There is just such another case related by Dr. Denmark, in the *Medico-Chirurgical Transactions*. A sailor was wounded by a musket-ball in the arm. The wound healed; but the patient remained affected with agonizing pain, beginning in the extremities of the thumb and fingers, except the little finger, and extending up the forearm. His sufferings were so great that he willingly submitted to have the limb amputated; and the operation gave him complete and immediate relief. When the amputated limb was dissected, a small portion of lead, which seemed to have been detached from the ball when it struck against the bone, was found imbedded in the fibres of the median nerve.

These examples teach us, when we receive complaints of pain in any part, and can discover no cause of pain in the part itself, to look for some possible source of irritation in the trunk of the nerve, from which the part in question is supplied with nervous fibrils.

But the source of irritation may be farther back than this: it may depend upon a diseased state of the spinal marrow, or of the brain. Of this we have had so many examples before us already, that I need not seek for any new illustrations of it.

Sometimes, again, irritation applied in the course, or at the extremity, of *one* branch of a nerve, will give rise to pain in the extremity of *another* branch of the same nerve. The sensation appears to be reflected, as it were, along the branch which is not, directly, the subject of the irritation. Thus filaments of the phrenic nerve penetrate the diaphragm and communicate with the ganglia that lie around the cæliac artery; other filaments are distributed to some of the muscles about the shoulder; and in this way has been explained the well-known fact, that disease or irritation of the liver is very apt to be accompanied with pain in the shoulder.

Thus also we have pain in the glans penis, from the irritation of the bladder, produced by a stone there: pain of the thigh and testicle, from irritation of the kidney: pain in the left arm, from disease of the heart: pain in the feet, from stricture and irritation of the urethra.

There are many pains also, plainly enough connected with irritation of distant parts, although no other nervous connection can be traced between the part, except that which is afforded by the brain, or spinal cord. In such cases we must suppose that the morbid impression travels to the brain or cord, and then the sensation is referred to the part affected through another nervous channel of communication. Dr. Wollaston was accustomed to relate the following story of himself. He had eaten some ice-cream after dinner one day; and his stomach did not seem to be capable of digesting it. Some time afterwards, when he had left the dinner table for the drawing room, he found himself rendered lame by a violent pain in one ankle. Suddenly he became sick, the ice-cream was vomited, and instantaneous relief of the pain followed its ejection from the stomach. "A gentleman (says Sir Benjamin Brodie) awoke in the middle of the night, labouring under a severe pain in one foot. At the same time certain other sensations, to which he was not unaccustomed, indicated the existence of an unusual quantity of acid in the stomach. To

relieve the latter he swallowed a large dose of alkaline medicine. Immediately on the acid in the stomach having been thus neutralized, the pain in the foot left him."

The lesson that we learn from all these facts is this: that when we can find no explanation of a pain in the very spot in which it is felt, we should look for some condition that may explain it in the trunk of the nerve supplying that part; or in the parts supplied by other branches of the same nerve; or (if we are still unsuccessful) we look for other indications of disease in the brain or spinal marrow: and if these be wanting, we should extend our search, and inquire whether there be any intelligible disorder or cause of irritation elsewhere, which operating through the medium of the brain or spinal cord, may have occasioned the sympathetic pain of which our patient complains.

I say we should institute this search, because, if it be successful, it may teach us, on the one hand, that the cause of the pain is fixed and irremediable; or, on the other, it may enable us by some simple and obvious expedient to cure the pain. But sometimes we shall be quite disappointed in all this seeking. We shall find nothing, either in the living patient, or in the dead body, which throws the smallest light upon the cause of the neuralgia.

Now with respect to those neuralgic pains, for which we can discover no adequate cause, either in any diseased structure, or in any diseased action of the blood-vessels, there are certain general facts observable which I will mention before I specify any particular forms of neuralgia. They occur in all parts of the body; but they are more frequent about the head than in any other part; and next of all, probably, in the abdomen. In the head, or face, the branches of the fifth pair of nerves are very frequently the seat of neuralgia; and to such pain in that situation the name *tic douloureux* is generally given. The painful affection called *hemicrania* is another example of neuralgia of the head. Certain forms of *angina pectoris*, and of *gastrodynia*, seem to belong to the same class of disorders: and *sciatica*—which depends on different causes in different cases—is often rather a neuralgic than a rheumatic pain. I have stated that the pains sometimes follow the track of certain nerves; but this is not, I think, very common. Inflammation of the nerve, or its investments, generally causes pain having that property: but the truly nervous pains are much better characterized by the suddenness with which they come on, and the suddenness with which they sometimes go off also; by their intermittence in many cases, and the regularity of the period at which they often, though not always, return; by the total absence (in most cases) of heat and swelling, and often of tenderness, too, when they are external, and of febrile symptoms when they are internal, even although their intensity be extreme; by their apparent dependence, in numerous instances, upon sudden changes of the weather; by their occurring chiefly in persons of a nervous temperament, in whom the health is otherwise disordered; and by their frequently abating under tonic remedies, or what are called specifics, rather than under antiphlogistic treatment. (*Alison's Outlines.*) There is another circumstance, characteristic of these pains, which has been mentioned by Sir Benjamin Brodie, and I do not know that the same thing has been noticed by other writers. These pains are often suspended by sleep. "A person suffering from tic douloureux in the face may for a time be prevented from falling asleep, but, if once asleep, his sleep is likely to be sound and uninterrupted for many hours." He says that though there may be exceptions to this rule, they are comparatively rare. Now this, you will observe, is quite analogous to what takes place in certain spasmodic affections of the muscles also. The jactitations of chorea are almost always suspended during sleep. It is the same with the spasmodic wry neck, in which the involuntary contraction of some muscle, commonly the sterno-cleido-mastoideus, drags the chin round, and the head awry. Persons affected with that sort of deformity when awake, have their necks flexible enough, I believe, while they are sleeping.

I mentioned just now one character of these neuralgic pains, viz. the total absence in most cases, when they occupy the surface, of heat, redness, swelling, or tenderness; and I said *in most cases*, because there are unquestionably exceptions to this. After these pains have been long-continued and intense, they may give rise even to a moderate degree of inflammation of the part; which will become tender to the touch, manifestly vascular, and even swollen a little. "In a gentleman." mentioned by Sir B. Brodie, "who suffered for a great length of time what was regarded as a most severe tic douloureux in the face, at

first the parts to which the pain was referred retained their natural appearance, but ultimately they became swollen, from an effusion of serum into the cellular texture, and so exquisitely tender that they could not bear the slightest touch. In a patient who had laboured for some time under pain in the testicle, depending on a calculus passing down the ureter into the bladder, the testicle became tender and considerably swelled."

The attacks of neuralgia may recur at intervals of a few seconds only; or they may take place daily, or every other day; or they may be separated by much longer intervals, regular or irregular. Sometimes there is continual pain, but it is wonderfully exalted and aggravated by fits. It is sometimes described as being sharp, and sudden, and like an electric shock in its momentary duration; sometimes it is attended by a feeling of constriction and cramp, although no muscular contraction accompanies it. I suppose that is one reason why such pain is so often spoken of, even by medical men, and almost always by the vulgar, as *spasm*. Whenever a patient tells me he has spasms here or there, I am obliged to request that he will explain himself farther. I want to know how he construes spasm; and nine times out of ten I find that he intends a sudden and sharpish, and generally a transitory attack of *pain:* whereas spasm really signifies, and ought to be restricted to, involuntary muscular contractions. When a medical man prints a case in which he states that spasms occurred in such or such a part, it is impossible to tell what he means, unless that term is explained by the context. Pray avoid this inexactness.

Tic douloureux.—The most common of these neuralgic pains, as I have said, is that which has been called, κατ' ἐξοχήν, *tic douloureux*, and which is situated in the facial branches of the fifth pair of nerves, which, as you know, are nerves of sensation: and it is usually restricted to one of the three branches that emerge severally to supply the parts in their neighbourhood. Sometimes two, sometimes all of them are implicated. The middle one of these branches, the infra-orbitary, is, I believe, the most commonly affected in the severer forms of the complaint. The torture occasioned by this dreadful malady is sometimes excessive. The sufferers speak of it as anguish that is scarcely endurable; and you see, in their quivering features and restless limbs, that the acute bodily pang is, indeed, hard to bear.

When the uppermost branch of the tri-facial nerve is the seat of the complaint, the pain generally shoots from the spot where the nerve issues through the superciliary hole; and it involves the parts adjacent, upon which the fibrils of the nerve are distributed; the forehead, the brow, the upper lid, sometimes the eyeball itself. The eye is usually closed during the paroxysm, and the skin of the forehead on that side corrugated. The neighbouring arteries throb, and a copious gush of tears takes place. In some instances the eye becomes blood-shotten at each attack; and when the attacks are frequently repeated, this injection of the conjunctiva may become permanent.

When the pain depends upon a morbid condition, or morbid action, of the middle branch of the nerve, it is sometimes quite sudden in its accession, and sometimes comes on rather more gradually; being preceded by a tickling or pricking sensation of the cheek, and by twitches of the lower eyelid. These symptoms are shortly followed by pain at the infra-orbitary foramen, spreading in severe flashes (so to speak) over the cheek, affecting the lower eyelid, ala nasi, and upper lip, and often terminating abruptly at the mesial line of the face. Sometimes it extends to the teeth, the antrum, the hard and soft palate, and even to the base of the tongue, and induces spasmodic contractions of the muscles of the face.

When the pain is referable to the inferior maxillary branch of the fifth pair of nerves, it darts from the mental foramen, radiating to the lips, the alveolary processes, the teeth, the chin, and to the side of the tongue. It often stops exactly at the symphysis of the chin. Frequently it extends in the other direction to the whole cheek, and to the ear. During the paroxysm the features are liable to be distorted by spasmodic action of the muscles of the face, amounting sometimes to tetanic rigidity, and holding the jaw fixed and immovable.

The paroxysms of suffering in this frightful disease are apt to be brought on by apparently trivial causes; by a slight touch, by a current of air blowing upon the face, by a sudden jar or shake of the bed in which the patient is lying, by a knock at the door, or even by directing the patient's attention to his malady by speaking of it, and asking him

questions about it. This was remarkably manifest in a patient who came into the hospital under my care for another complaint; but who had for some time been subject to tic douloureux. The necessary movements of the face in speaking, or eating, are often sufficient to provoke or renew the paroxysm. At the same time firm pressure made upon the painful part frequently gives relief, and causes a sense of numbness to take the place of the previous agony.

This fearful disorder occurs most commonly in persons who exhibit, in other respects, the signs of an unsound, or disordered, or debilitated system. It is more apt to fasten upon those who are pale, and asthenic, and upon individuals whose powers have been broken by advancing years. It is not unfrequently attended with some obvious disorder of the digestive organs, and ceases or is mitigated when that disorder is corrected. Sometimes it is clearly connected with a disposition to rheumatic affections; coming on in persons who suffer rheumatism in other parts, and even alternating with rheumatism in other textures. It is observed to be common among fishermen, and the inhabitants of marshy districts; and in some of these sufferers it may be attributable to their habitual exposure to cold and moisture; and this nerve, lying superficially, and being unprotected by any artificial covering, is more likely, perhaps, for that reason, to be affected by vicissitudes of temperature; but in many of these cases the disease seems to be produced by the *malaria* which is prevalent in those situations. The paroxysms are then not only intermittent, but periodical, and they will frequently yield to the remedies which have been ascertained to be specific against ague and its various modifications. Sometimes the facial neuralgia is evidently dependent upon some general state of the system; for it will cease in the face, and fix itself in some other place; and in this way it may come to occupy several distant parts of the body in succession. There are other cases again in which the disease has a local origin, and results from some diseased bone, or exostosis, in the neighbourhood of the painful spot. The late Dr. Pemberton afforded a well-known example of this. He was seized with tic douloureux in the very zenith of his reputation, and when he was in the fullest practice of his profession in this town. It completely ruined him: compelled him to give up business. He ultimately died of apoplexy. When his head was examined after death, the os frontis was found to be unusually thick, and on the falciform process of the dura mater, at a little distance from the crista galli, a small osseous substance was discovered, nearly half an inch long, and almost as broad. Sir Henry Halford has recorded several other instances in which the disease was connected with some morbid condition of the bones of the head or face.

Now tic douloureux is one of those complaints for the cure of which there exists a number of specific remedies. But what I have been stating of this disease will suffice to convince you that, as it depends upon different causes in different persons, it is absurd to expect that any single drug—or even one plan of treatment—will always remove it. Our first care, in every example of it that comes before us, must be to investigate all the particulars of the case. We must not be satisfied with learning that the complaint is tic douloureux, and then go on prescribing one after another the reputed specifics for tic douloureux. It may happen that the origin of the disease is plain, and the remedy obvious. We must endeavour to make out whatever is amiss in the system at large, or in the state of particular functions. Very rarely, I believe, tic douloureux is dependent upon a condition of general plethora. Mr. John Scott gives the case of a gentleman who suffered severely from it for some time; at length he had an attack of apoplexy, and for this last disorder he was largely bled, and the bleeding seemed to cure the neuralgia. Much more frequently we find evidence of a feeble or a shattered state of the system; debility and paleness: and then we may expect to do good by the treatment so strongly recommended by Mr. Hutchinson, viz. by giving the carbonate of iron. This remedy has been put largely to the test, since Mr. Hutchinson wrote in commendation of it, by Dr. Elliotson, and subsequently by others. Dr. Elliotson states it as the result of his experience that, "in all cases of neuralgia, whether exquisite or not, unaccompanied by inflammation, or evident existing cause, iron is the best remedy." I have already explained the manner of administering the carbonate of iron, the doses in which it may be given, and the limits within which I should be inclined to restrict the doses. Sir Benjamin Brodie thinks it probable that the carbonate of iron proves beneficial by its mechanical operation on the internal surface of

the intestines: but I should rather ascribe its good effect to the well-known property of preparations of iron, to give firmness to the nervous system; apparently by increasing the quantity of red blood that circulates in it. However, it is of the utmost consequence that the state of the digestive organs should be attended to. Mr. Abernethy used to relate, in his lectures, many instances of tic which he had been successful in curing by measures which were solely directed to the improvement of the stomach and bowels. He had a notion, that in patients who suffer under this disorder, there were always two functions wrong; those of the nervous system on the one hand, those of the digestive system on the other. And I am sure you will commonly find indications of a faulty state of both these systems. "The two," he used to say, "were the common parents of a numerous progeny of very dissimilar local diseases. In tic douloureux, you must seek to put the digestive organs right, or to soothe the nervous system, according as the one or the other may seem to be the principal and primary cause of the disease. Take away one of the parents, and there will be no more propagation."

In these cases, the unhealthy state of the digestive apparatus may be marked by obvious signs; a furred tongue, loss of appetite, costive bowels: or it may reveal itself by no other symptoms than the pain. It may depend upon the mere presence of acid in the stomach. Dr. Rigby tells us that having suffered in his own person an intense attack of tic douloureux, which opium did not assuage, he swallowed, at the suggestion of a friend, some carbonate of soda dissolved in water. The effect was almost immediate: carbonic acid was eructated, and the pain quickly abated. More often the cause of offence appears to lie in some part of the intestines; and purgatives do good. Sir C. Bell—drawing a bow at a venture, achieved the cure of a patient, upon whom much previous treatment had been expended in vain, by some pills composed of cathartic extract, croton oil, and galbanum. He mixes one, or two, drops of the oleum tiglii, with a drachm of the compound extract of colocynth; and gives five grains of this mass, with ten grains of the compound galbanum pill, at bed-time. I mention the exact proportions and dose, because other cases have been since reported, both by Sir Charles and others, in which the same prescription was followed by the same success.

When the disease occurs in a rheumatic individual, and especially when, as is sometimes the case, it alternates with rheumatism of other tissues, the remedies which have been found useful in rheumatism deserve a fair trial: guaiacum; colchicum; calomel and opium.

When all has been done that can be done towards restoring or improving the general health, we may turn our thoughts to local remedies. It is plain that these must be inefficient when the local pain results from constitutional causes that are unredressed, or perhaps incurable. Yet even then topical measures may soothe the pain for a while.

One of these topical expedients, which promised well when first thought of, is the division of the trunk of the painful nerve, so as to cut off the nervous communication, through that main channel at least, between the painful part and the brain. This was originally proposed by Dr. Haighton, and was at first attended with some little success; but in a great number of instances it has signally failed, as indeed might have been expected. In Dr. Pemberton's case the several branches of the fifth pair were cut by Sir Astley Cooper: but in vain. When there is any reason to think that the disease has a constitutional origin, or a local *distant* origin, the division, or even the excision, of a part of the nerve must be perfectly useless: it would be as reasonable (as Mr. Abernethy has observed) to expect to cure gout by cutting the nerve that goes to the great toe: or to perform castration with the view of remedying that pain in the testicle which is apt to be produced by the passage of a calculus through the ureter. Nevertheless there are cases, in which the division of the nerve, or some other surgical operation, is required. If you can make out that there is any tumour pressing upon or adherent to some part of the nerve—or if some foreign body, as a splinter, or a shot, should be ascertained to be in contact with the surface of the nerve, or to be entangled in its substance—the tumour or the foreign body may be removed by the knife, with the strong expectation that a cure will be thus affected And if this cannot be done, or if the nerve itself be altered in structure, either from disease or injury, (I am referring now to neuralgia in general, and not merely to that in which the facial branches of the fifth pair of nerves are implicated,) under those circumstances it

will become a very proper subject of deliberation whether the nerve should be divided, or the limb amputated.

In the Medical and Physical Journal there is a case described by Mr. Jeffries, of a violent facial neuralgia, cured by the removal of a small fragment of china, which had been lodging there for fourteen years. And Mr. Descot mentions an instance in which a very severe affection of ten years' standing was removed by the extraction of a carious tooth. I saw, not many days ago, a young woman whose finger had been amputated for very acute neuralgic pain which she had suffered in it; and the amputation had been successful in liberating her from that pain.

Sometimes we may hope to afford relief to a suffering patient by means which tend to remove or lessen the *exciting* cause of the paroxysms. Of this I may mention one remarkable example, which fell in part under my own observation; although I had nothing to do with the treatment. I was asked, a few years ago, by a friend, to go with him to call upon a relation of his, who laboured, he said, under tic douloureux: he did not wish me to see her professionally, but was desirous that I should witness what he considered an extraordinary complaint. I saw a young girl, about twelve or thirteen years old, very pale and delicate, lying on a sofa; and I learned from her and from her mother that she was subject to the most excruciating agony in one side of her face and neck. The pain came on whenever she swallowed any thing: the action of deglutition proved invariably the exciting cause of the torment. She was at that time under the care of a practitioner who had desired that she might eat mutton-chops three or four times a day. Of course this was a sentence full of misery to her; but so desirous was she to get rid of her disease, that she resolved steadily to follow the directions that were enjoined. This plan was to be tried for at least a month; after that time, if she were no better, her mother had resolved to consult another practitioner who had been much recommended to her. I should say that she had already consulted a great number of medical men; for the malady had existed nearly two years. At the end of the month she was worse than at the beginning; and the new practitioner, Mr. Pennington, was called in. He acted, like a man of sense and sagacity, upon the fact that the act of swallowing always gave rise to the pain; and he advised that she should not attempt to swallow for twenty-four hours. That period passed without any return of the pain; but it immediately recurred upon her eating a morsel of bread. The result of this experiment, however, encouraged him to hope that the morbid habit might be broken through by a sufficiently long abstinence from swallowing. And as she had been subjected to a great variety of fruitless treatment, he gave her no medicine, but advised that she should refrain altogether from taking food or drink by the mouth. Nourishing injections composed of beef tea, with an egg beat up in it, or of milk, were thrown up into the rectum two or three times a day. This plan was persisted in for a longer time than I should have supposed she could have endured it. No nutriment whatever was taken by the mouth for five weeks and three days, and no paroxysm of pain occurred. At the end of that period the pulse sank suddenly, from between 70 and 80, to 35 beats in a minute; and thereupon Mr. Pennington thought he had carried his experiment far enough; and deemed it advisable to administer by the mouth a dessert-spoonful of beef-tea twice a day. This was continued for four days without producing any return of the pain. A small piece of fish was then allowed, and afterwards some chicken; and proceeding thus cautiously, in the course of a month she was able to eat and drink any thing, without the slightest inconvenience.

I should state, however, that some time afterwards the neuralgia returned in another situation, and affected the left knee: and this was remedied by a different mode of treatment. She is since dead.

When other means fail, or in conjunction with other means, local applications to the affected part may be tried. Belladonna will sometimes materially palliate the pain; so will opium: but within the last few years a new anodyne has been brought into use; and it really seems to have been of essential service in several instances of this most painful disorder: I allude to *aconitine:* the active principle of the monkshood. The property belonging to this plant, of benumbing sensation, has long been known. Sir Benjamin Brodie found many years ago, that after chewing its leaves, a remarkable numbness of the lips was left, which lasted some hours. We may understand therefore the beneficial operation

of the aconitine upon a part of which the sensibility is unduly exalted. It is only very recently that pure aconitine has been procured; and consequently it has not yet been very extensively employed, and the less so on account of its very high price: but what experience we have of it, as a benumber of pain, is highly encouraging. It has been of singular benefit to a surgeon who formerly lived in Charter-house Square, and whose case is well known, I believe, to the profession. Mr. Spry had suffered greatly, for eight years, under very acute neuralgia affecting the parts supplied by the lowermost or mental branch of the fifth pair of nerves. After exhausting almost every expedient that ever has been recommended for tic douloureux, except that of dividing the nerve, he was induced to make trial of the aconitine. It was mixed with cerate, in the proportion of one grain to one drachm, and a small portion of this was smeared over the track of the painful nerve once or twice a day for six days. By that time he had entirely lost the pain. He states, I understand, that the application of the ointment produced a sense of numbness, which continued for twelve or eighteen hours. Dr. Hue, who first told me of Mr. Spry's case, told me at the same time that he knew of two others in which the same application had been equaliy successful. This encouraged me to try it upon my patient, whom I mentioned before, and who happened at that time to be in the hospital. I bought five grains of the aconitine at Morson's, in Southampton Row, where I knew it would be genuine, for ten shillings. One-third of a drachm of ointment, containing one-third of a grain of the alkaloid, was smeared two or three times a day over my patient's face, and the attacks presently diminished in intensity, and in a few days ceased altogether. He soon after left the hospital, so that I cannot tell whether the cure was permanent. I presume it was so, as he did not return. The particulars of Mr. Spry's case have been published by Mr. Skey in the 19th volume of the Medical Gazette. It is now six years since the aconitine was applied, and the pain (as Mr. Skey has recently informed me) has never recurred. It used to be excited by gentle friction of the hand, or by a current of cold air, but Mr. Spry "can now face any wind or temperature with impunity." In the same paper Mr. Skey relates another instance of the utility of this substance in facial neuralgia. It occurred in one of his patients at St. Bartholomew's Hospital.

This is a remedy therefore which is not to be neglected. Even if it only allayed the pain for a time it would be highly valuable. But, judging from the instances now referred to, we may hope that, in some forms of tic douloureux, the aconitine may be found equal to their cure. It seems probable that the recurrence of the pain is sometimes kept up by the influence of habit; and will cease if the habit can be broken for a while. You must take care, however, to obtain a genuine article: the manufacture of aconitine is difficult, and therefore the cost is considerable. Mr. Skey, in the Bartholomew case, failed with some aconitine that had been imported into this country, but succeeded at once when he employed the same quantity as it is prepared by Mr. Morson.

A few years ago Mr. John Scott published a little book on the diseases we are now considering, with the professed object of introducing to general notice a species of local treatment which he had found successful in several long standing and previously obstinate cases. It is well to be aware of these things, though probably the aconitine ointment will beat Mr. Scott's. Mr. Spry used Mr. Scott's ointment, but without benefit. It consists of the iodide of mercury, mixed with lard, in the proportion of two scruples to the ounce: and it is rubbed into, or placed in contact with, the affected surface, until some degree of irritation is produced.

There is a kind of face-*ache*, which cannot properly be included as a species of neuralgia, for it does not occur in short stabbing paroxysms, nor is the pain acute enough to entitle it to the name of tic douloureux; but which is very common, very distressing, and under ordinary treatment sometimes very intractable. It is called often a rheumatic pain; it occupies the lower part of the face, the jaw principally, and the patient cannot say exactly whereabouts it is most intense. It is often thought to proceed from toothache, and bad or suspected teeth are extracted, but with no good effect. Now I allude to this for the sake of saying that some years ago I was told by an experienced old apothecary, that this face-ache might be almost always and speedily cured by the muriate of ammonia;—a medicine that we seldom give internally here, although it is so much used in Germany.

And I have again and again availed myself of this hint, and been much thanked by my patients for the good I did them with this muriate of ammonia. It does not *always* succeed; but it *often* does. It should be given in half-drachm doses, dissolved in water, or in almost any vehicle, three or four times a day. If the pain does not yield after four doses, you may cease to expect any benefit from it. In two or three instances of a similar kind that I have recently had to treat, I have found the iodide of potassium, in doses of five or six grains, work a speedy and permanent cure. This induces me to suppose that the pain in some of these cases is periosteal; judging from the ascertained efficacy of the iodide in other periosteal affections attended with pain.

Sciatica.—Tic douloureux is the principal form of severe neuralgia which you may expect to meet with, in regard to acuteness of suffering and difficulty of cure. Two other forms, more common, and luckily more manageable, are generally spoken of under the same head: *sciatica*, namely, and *hemicrania*. I have very little to say, in this place, of either of these. Sciatica, or pain radiating from the sciatic notch, and following the course of the sciatic nerve, is sometimes an inflammatory complaint, and yields to the remedies of inflammation—bleeding and blistering: sometimes it is plainly a part of rheumatism; and then may often be relieved by calomel and opium, or by colchicum; sometimes, again, it results from irritation within the pelvis, affecting the nerve before it emerges externally; this irritation may be connected with a disordered state of the kidney, and I suspect that it is in such cases that the oil of turpentine is of so much use: lastly, it is sometimes a purely nervous and neuralgic pain; and then the treatment applicable to facial neuralgia will, *mutatis mutandis*, be applicable to it. I had some time ago a butler under my care at the hospital, whom I am afraid I did not manage well. He suffered severe sciatica, and I had him cupped and blistered, and gave him a variety of medicines, for some time, to little purpose: at last he got what I ought, I suppose, to have given him at first, viz. the carbonate of iron, and was presently well.

Hemicrania.—Hemicrania is simply headache, confined to one side, and occupying generally the brow and forehead, but sometimes affecting very exactly one moiety of the head. It is the *migraine* of the French, the *megrim* of our vernacular language; each of these terms being obviously traceable to the same Greek root. It is often attended with sickness; and in many instances it is periodical, coming on every day at a certain hour, lasting a certain time, and then subsiding. Like the other forms of neuralgia, hemicrania may be produced by various causes, which are, however, almost all of them such as tend to debilitate the system: it sometimes occurs in connection with hysteria; sometimes it plagues women who have suckled their infants too long; sometimes it acknowledges the same cause as ague; and sometimes also it occurs independently of all other disease, and when no obvious exciting cause can be traced.

Whatever may be its origin, it is usually a very manageable complaint. When it is associated with evident anæmia, steel and the shower bath may be expected to cure it. When its visits are strictly periodical, it will yield to quinine. Arsenic is considered by many to have a specific power over the complaint; and I believe that four or six drops of the liquor arsenicalis, given three or four times a day, with due attention to the state of the bowels, will be almost sure to remove hemicrania in nine cases out of ten in which it occurs. But steel or bark, being milder and safer drugs, are, *cæteris paribus*, to be preferred.

I say this complaint often acknowledges the same *cause* as *ague;* namely, the miasm of marshes, or malaria: and as that cause, mysterious as it is in some respects, exerts apparently its primary or chief influence upon the nervous system, and as ague has no definite seat in the human body, if it be not in the nervous system, I shall not find a more convenient place in these lectures for the consideration of ague than here, at the close of the remarks which I had to make respecting the diseases of the brain and nerves. In the next lecture, then, I shall begin to speak of Intermittent Fever.

LECTURE XL.

INTERMITTENT FEVER. PHENOMENA OF AN AGUE FIT. SPECIES AND VARIETIES OF INTERMITTENTS. PREDISPOSING CAUSES. EXCITING CAUSES. MALARIA; KNOWN ONLY BY ITS EFFECTS; PLACES WHICH IT CHIEFLY INFESTS; CONDITIONS OF ITS PRODUCTION; ITS EFFECTS UPON THE HUMAN BODY; INFLUENCE OF SOIL UPON ITS EVOLVEMENT.

I AM now to enter upon the consideration of that disorder of which the trivial English name is *ague*, and which is called by nosologists *intermittent fever*. This is one of the diseases which is known to us only in its group of symptoms. Before we can inquire successfully into its history, it is necessary that we have the group of symptoms which characterize the complaint set fairly before us. I must first, therefore, describe the *phenomena* of ague.

You will observe that ague resembles several other maladies that essentially belong to the nervous system, in being *paroxysmal*. A certain series of symptoms occurs, and then the patient reverts to a state of health: but this alternation commonly happens (or would happen if the disease were left to itself) a great many times. You may therefore look upon this succession of attacks as so many repetitions of a short distemper; or you may regard the whole period during which the attacks continue to recur at short intervals, as being occupied with one single disease.

An ague fit is composed of three distinct stages; and they are named, from the phenomena that respectively characterize them, the *cold*, the *hot*, and the *sweating* stages.

A person who is on the brink of a paroxysm of ague, experiences a sensation of debility and distress about his epigastrium; becomes weak, languid, and listless, and unable to make any bodily or mental exertion. He begins to sigh, and yawn, and stretch himself; and he soon feels himself chilly, particularly in the back along the course of the spine; the blood deserts the superficial capillaries; he grows pale, his features shrink, and his skin is rendered dry and rough, and drawn up into little prominences, such as may at any time be produced by exposure to external cold, and presenting an appearance somewhat like the skin of a plucked goose: hence it is called goose's skin, and in Latin *cutis anserina*. Presently the slight and fleeting sensation of cold, first felt creeping along the back, becomes more decided and more general; the patient *feels* very cold, and he *acts* and *looks* just as a man does who is exposed to and subdued by intense cold; he trembles and shivers all over; his teeth chatter, and sometimes so violently that such as were loose have been shaken out; his knees knock together; his hair bristles slightly, from the constricted state of the integuments of the scalp; his face, lips, ears, and nails turn blue; rings which before fitted closely to his fingers become loose; his respiration is quick and anxious; his pulse frequent sometimes, but feeble, and he often complains of pains in his head, back, and loins: all the secretions are usually diminished; he may make water often, but generally he voids but little, and it is pale and aqueous; his bowels are confined, and his tongue is dry and white.

After this state of general distress has lasted for a certain time, it is succeeded by another of quite an opposite kind. The cold shivering begins to alternate with flushes of heat, which usually commence about the face and neck. By degrees the coldness ceases entirely; the skin recovers its natural colour and smoothness; the collapsed features and shrunken extremities resume their ordinary condition and bulk. But the reaction does not stop here; it goes beyond the healthy line. The face becomes red and turgid; the entire skin hot and pungent, and dry; the temples throb; a new kind of headache is

induced; the pulse becomes full and strong, as well as rapid; the breathing is again deep, but oppressed; the urine is still scanty, but it is now high coloured; the patient is exceedingly uncomfortable and restless. At length another change comes over him: the skin, which, from being pale and rough, had become hot and level, but harsh, now recovers its natural softness; a moisture appears on the forehead and face; presently a copious and universal sweat breaks forth, with great relief to the feelings of the patient; the thirst ceases; the tongue becomes moist; the urine plentiful but turbid; the pulse regains its natural force and frequency; the pains depart; and by and by the sweating also ceases, and the patient is again as well, or nearly as well, as ever.

This is surely a very remarkable sequence of phenomena: and they would appear still more remarkable if they were less familiar to us. The earlier symptoms are all indicative of debility, and of a depressed state of the nervous functions. There is the same sensation of exhaustion, and incapacity of exertion, which are produced by fatigue. The sighing, yawning, and stretching, are all indications of debility. The paleness of the surface, and constriction of the skin and collapse of the features, are all owing to the retirement of the blood from the superficial capillaries. The skin shrinks, but the parts containing the bulbs of the hairs cannot contract so much as the other parts, and therefore the surface becomes rough, and the hairs bristle up, or become erected in some degree. *Horripilatio* is the learned term for this state of the surface. The coldness of the skin is another consequence of the emptiness of its blood-vessels, and the tremors, which are always indicative of debility, seem to depend upon the coldness: the chattering of the jaws has been said to be so violent as to fracture the patient's teeth. This you can believe or not as you please, but certainly the whole bed is often strongly shaken by the shiverings of the patient. The necessary accumulation of the blood in the larger and internal vessels offers a reasonable explanation of the distressed and anxious breathing.

In their attempts to render a "ratio symptomatum," authors have sometimes spoken of the hot stage as though it were a necessary consequence of the cold. But if the latter, the cold fit, be in any sense or degree the cause of the hot fit, it can only be so partially; there must be some other cause, for these reasons: the cold stage may occur and never be followed by the hot; or the hot stage may come on without any previous cold stage; and when they do both happen, they are not by any means proportioned to each other. When we thus see that a supposed cause is not always followed by the effect, or that the effect is produced without the agency of the supposed cause, and also that the supposed cause and effect are not proportioned to each other, we cannot but conclude that the supposed cause is at most but a partial and accessory cause. We can more easily conceive how the hot fit may conduce to bring on the sweating stage: the stronger action of the heart and the more forcible propulsion of the blood will fill the superficial vessels, and in this way the natural secretions may be restored. We see exactly the same thing happen when the force of the circulation is increased by exercise: the extreme vessels seem to relax, and sweat ensues.

There are many curious facts to be observed in respect to the paroxysm of an intermittent, such as it has been now, in general terms, described. In the first place the paroxysm *returns*. Cullen makes this a part of his definition; and quibbling objections to his statement have been made, which are scarcely deserving of mention. Thus it is said that this circumstance should not have been introduced into the definition, because it is not *necessarily* or *universally* true; that the patient *may die* in the *very first* paroxysm; or that he may be cured by the proper remedies of ague, *before* a *second* paroxysm has time to show itself. But all this is trifling. The paroxysms, if the disease be left to itself, will recur for a certain length of time; and, unlike the paroxysms in many of the spasmodic diseases which we have lately been speaking of, they recur at regular periods, and often with singular exactness. This is a circumstance which we should waste our time in attempting to account for. Dr. Cullen has tried to explain it on the principle of some diurnal *habit* of the body; but the truth is, that no *satisfactory* explanation of it has ever been given, and we must be content, for the present at least, to receive it as an ultimate fact; and doubtless a very strange and interesting fact.

In distinguishing some equally curious varieties of these successions and alternations of disorder and health, certain terms have, by common consent, been adopted by pathologists,

which terms it is necessary that I should explain. The period which elapses between the *termination* of one paroxysm and the commencement of the next is called an *intermission;* while the period that intervenes between the *beginning* of one paroxysm and the beginning of the next, is called an *interval.* As the paroxysms are liable to vary in length, the intermissions may be very unequal, even when the intervals are the same. When the intermissions are perfect and complete, the patient resuming the appearance and sensations of health, the disorder is an *intermittent* fever. When the intermissions are imperfect, the patient remaining ill and feverish and uncomfortable in a less degree than during the paroxysm, then the complaint is said to be a *remittent* fever.

But, confining ourselves for the present to intermittents, it is another curious property of this complaint that, although the intervals are commonly constant in each case, and quite regular, they differ in duration in different cases. Upon this circumstance is founded a division of agues into species. When the paroxysm occurs at the same hour *every day*, the patient is said to have *quotidian* ague. When it comes on at the same hour *every other day*, appearing and remaining absent day by day alternately, he is said to labour under *tertian* ague. The paroxysm, strictly speaking, repeats itself every *second* day; and if the species I first mentioned be fitly termed quotidian, that in which the fits occur on alternate days ought to be styled *secundan*. But nosologists have chosen to reckon the day on which the preceding fit happens as the first; and then the day on which the succeeding fit will happen, in the species now under consideration, is the third. In the same way, when a paroxysm absents itself for two whole days, and then recurs, the complaint is called a *quartan* ague. These are the three principal species or types of intermittent fever. Of course it follows, from what I have been stating, that in the quotidian type the interval is 24 hours; in the tertian, 48; and in the quartan, 72.

Each of these types has some other characters peculiar to itself. Thus, the paroxysms of the quotidian ague begin in the morning; those of the tertian, at noon; those of the quartan, in the afternoon. These are the *rules*. You are not to expect to find them always or rigidly observed; for the most part you will find that they *are* observed. It is probable that quotidian paroxysms, occurring at noon or at night, have sometimes been ascribed to ague, when they were merely symptoms of some local disease or inflammation; or perhaps accessions of *hectic* fever. It is observed also of the paroxysms, that when the disease is about to yield, they often occur later day after day, before they take their final departure. This is called *postponing;* and when they occur earlier than their stated hour, the paroxysms are said to *anticipate*. Now a postponing quotidian may be deferred till noon. But when the disease is pursuing its regular undisturbed course, the rule is such as I have mentioned.

The three principal types differ from each other, not only in their respective intervals, and in the periods of the day at which the paroxysms severally commence, but also in the *duration* of the paroxysms; and in the proportions which the stages of these paroxysms bear to each other. The average duration of the paroxysm in the quotidian is ten or twelve hours; and of course the average duration of the intermission is nearly as much. The tertian paroxysm commonly begins at noon, and is finished the same evening; its average duration may be estimated at six or eight hours. And that of the quartan does not exceed four or six hours.

You will observe also that while the quartan has the longest interval and the shortest paroxysm, it has the longest cold stage; while the quotidian has the shortest interval and the shortest cold stage, but the longest paroxysm. To express these facts in mathematical language, the length of the paroxysm varies inversely as the length of the cold stage; inversely also as the length of the interval.

Of these three principal types or species the tertian is by much the most common; but the quotidian and quartan are neither of them unfrequent where ague is rife.

I should tell you that there are other types also spoken of, as quintans and sextans; but they are scarcely worth our attention. It is probable that when they are observed (and that is very rarely) they are merely irregular quartans, postponing perhaps for a day or two. They never prevail epidemically. Galen describes one of these; so does Van Swieten. Boerhaave talks of a septiman, and even octavans are mentioned; or if you want still more of the marvellous, Pliny, the naturalist, informs us that a certain Improvisatori

was in the habit of having a paroxysm once a year, and that exactly on his birth-day; yet he died at a good old age.

There are, however, some curious modifications of the three principal types; or rather of two of them, the tertian and the quartan. For instance, a paroxysm may occur daily, and yet the ague not be of the quotidian type, but of the tertian. The paroxysm of one day will differ from the paroxysm of the next, but exactly resemble that of the third day; while the paroxysm of the second will be like that of the fourth; and so on alternately. And these differences will be decidedly marked; the paroxysms of two consecutive days will come on at different hours, and will differ in duration and severity. This form of ague is called the *double* tertian. One case of this kind, very distinctly characterized, was some time ago under my care in the hospital.

There is another form of double tertian. Two fits will occur on the same day—Monday for example, one in the morning, the other in the evening; on Tuesday there shall be no fit; on Wednesday again two; on Thursday none; and so on. The Latin nomenclature is more precise than the English in denoting these variations. The form I have last mentioned, in which two dissimilar paroxysms occur every other day, is called tertiana *duplicata;* while the other form, in which there is a fit every day, but those on the alternate days resemble each other, is called tertiana *duplex*.

In the same way you may have a double quartan. In that case, a paroxysm occurs on two days in succession, and leaves the third day free; then it returns on the fourth day as it did on the first, and on the fifth as it did on the second, and leaves the sixth day free like the third, and so on. This is the quartana duplex. But two fits may happen on one day: say on Monday; none on Tuesday or Wednesday; and two again on Thursday. This is the quartana duplicata. Nay, the paroxysm of quartan ague may recur every day, and so far resemble a quotidian; but the fit of the first day will differ from those of the second or third, and resemble that of the fourth; the fit of the second day will be dissimilar from that of the first or third, and like that of the fifth; and the fit of the third will be unlike that of either of the two preceding days, and find its counterpart in that of the sixth. This is a triple quartan; and where three paroxysms occur on the first day, which we will again suppose to be Monday, and none on Tuesday or Wednesday, but three again on Thursday, corresponding respectively to the first three, we have the quartana triplicata. And there are other complications still, with which I need not trouble you. In Dr. Cleghorn's book on the diseases of Minorca, you may find a very good and authentic account, evidently drawn from nature, of the irregular types and varieties of ague. They are well worthy of the attention of any among you who may be likely to practise abroad.

Some physicians have used the words *double* tertian, and so on, in the literal sense, and have supposed that two or more distinct agues coexisted. This savours a good deal of the error that I formerly warned you against, of looking upon diseases as separate entities, and not merely as modes of being and of acting different from those which are proper to the state of health. I refrain from speculating on so doubtful and obscure a subject, and limit myself to the exposition of well-ascertained facts.

Besides these varieties in *type*, some other deviations from the normal and regular paroxysms require to be noticed.

Sometimes the paroxysm is *imperfect;* it is shorn of one or more of its stages: the heat and sweating occur without any previous rigors; or the patient shakes, but has no subsequent heat; or the sweating stage is the only one of the three that manifests itself. These imperfections are often noticeable when the complaint is about to take its departure; but they may also occur at other periods of the disease. Sometimes there is no distinct stage at all; but the patient experiences frequent and irregular chills, is languid and uneasy, and depressed. This state is commonly known among the inhabitants of our fenny and aguish districts as the *dumb* ague, or the *dead* ague; the patient is said not to *shake out*.

Again, there is often observed a tendency to a change of type in the course of the same disease affecting the same person. The quotidian will be transformed into a tertian; a tertian into a quartan; or, on the other hand, a quartan into either of these. I have already noticed the fact that the paroxysms will also alter their time of invasion, sometimes

coming later and later in the day, at each recurrence, sometimes earlier and earlier. When the paroxysm thus *postpones*, the disease is growing milder; when it *anticipates* its usual period of attack, the disease is increasing in severity. The postponement or anticipation, therefore, of the fit, has a close relation to the prognosis.

There are yet other cases, in which from first to last no determinate type or order of succession is observed by the paroxysms; and these cases authors speak of as *erratic* forms of ague.

There are also many modifications or complications observable in the symptoms which constitute the fits. Occasionally each paroxysm is attended by violent delirium; this is most common, I believe, in the hot stage. This symptom has been known to be almost constant throughout an epidemic. Sometimes the patient is convulsed in the paroxysms; or syncope comes on; or tetanic symptoms; or petechiæ take place on the skin, and disappear with the paroxysm. These deviations from the common and regular kind and order of the symptoms may sometimes depend upon the constitutional predispositions of the person affected; but there is another way also in which they may be explained. I shall presently have a good deal to say upon the one grand—I may say *sole*—exciting cause of intermittents. Now exposure to that cause, a residence in aguish districts, will sometimes impart a periodic character to *other diseases:* and I apprehend that this explanation will apply to many of the instances which have been observed of hysterical, tetanic, or other paroxysmal complaints, occurring at perfectly regular intervals.

The duration of ague—of the whole disease, and not merely of a separate paroxysm—it is not easy to estimate. If persons who laboured under it were always removed at once from the influence of the exciting cause, and were always suffered to remain without treatment calculated to check the malady, we might then find materials for determining its average natural duration. But we have not these data. In point of fact, agues sometimes consist of a very few paroxysms only, half a dozen, or four, or three, or even of one fit; and on the other hand, they may be protracted over a space of several weeks, or months; nay of many years.

An ague may attack a person at any time; but they are much more common in spring, and in autumn, than in the other seasons of the year: so that you will hear and read a good deal of *vernal* intermittents, and of *autumnal* intermittents. Now the autumnal agues are, *cæteris paribus*, the more severe and dangerous. The quotidian is most common in the spring; the quartan in the autumn; and the tertian is frequently met with both as a vernal and as an autumnal ague. You will bear in mind that in all this I am stating the prevailing *rules;* which are liable to numerous exceptions.

Ague is one of those disorders of which (like common inflammation) all persons, at all periods of their existence, seem to be susceptible, when submitted to the influence of the specific exciting cause. Individuals of all ages, from sucking infants to persons of four score, are liable to it, but they are not *equally subject* to it. It is less likely (*cæteris paribus*) to affect the very young, and the aged, than those of middle life. However, the very old are by no means exempt from the operation of the cause of ague: and with respect to the very young, some extremely curious statements have been made. It is said that persons have had ague before they were born. We know that the period of intra-uterine life is obnoxious to many forms of disease; for we trace the consequences of such disease, in visible changes of structure, immediately after birth. Pulmonary tubercles constitute one malady, to which the fœtus in utero is liable: hydrocephalus is another: acute inflammation of the peritoneum a third. And there can be no doubt that various specific poisons influence, occasionally, the included being, even although they may have no sensible effect upon the parent. The fœtus may thus contract small-pox, which sometimes proves fatal to it, sometimes not. The daughter of my bed-maker at Cambridge had a child ill of hooping-cough in the house with her while she was in the last months of pregnancy; and the infant in the womb must have caught the disease, for I was assured that he hooped the very day he came into the world. The sins of the parent are thus visited often upon the child, when, before its first breath is drawn, its frame is contaminated by the virus of syphilis. And in like manner unborn infants are capable of being affected by the poison that produces ague. One case in proof of this is recorded by Dr. Russell, in his *History of Aleppo*. The woman had tertian ague, which attacked her, of

course, every other day: but on the alternate days, when she was well and free, she felt the child shake; so that they both had tertian ague, only the paroxysms happened on alternate days. Bark was prescribed for her; and it cured the little one first, and afterwards it cured the mother.

One probable reason why ague more commonly affects persons about the middle period of life, than those near its extremes, is, that the former are much more likely to be exposed to the primary exciting cause. And the same reason may be given, I presume, for another fact; viz. that the complaint is much more frequently seen in men than in women.

Among the circumstances which predispose to ague, debility has a powerful influence. It is important to be aware of this, as it concerns the prophylaxis, and the management of the patient after the disease has been subdued. Soldiers have been exposed to the exciting cause, without becoming affected by it, while strong and in good health; and have fallen ill of intermittent fever upon being weakened by exertion and fatigue. When I have told you that debility, any how produced, constitutes a predisposition to intermittent fever, I need scarcely add that all the multiform causes of debility may also be regarded as predisposing causes of this same disease; as they are of so many others.

But the strongest predisposing cause of all is an actual occurrence of the disease itself. The effect of former intermittents upon the system is such that the complaint may be reproduced by agencies which under any other circumstances would be quite inoperative in exciting ague. I have stated already my persuasion that, strictly speaking, there is but one exciting cause of intermittent fever; but in making that statement I refer to its *first production*. The disease leaves the body in a condition in which other injurious influences may, of themselves, be sufficient to renew it. It brings into play a new order of exciting, or rather of re-exciting causes. If a person were never exposed to the malaria, he could never, as I believe, have ague: but, having once had ague, he may many times have it again, although he should never again be subjected to the direct influence of the malaria. The late Dr. James Gregory, of Edinburgh, had a brother-in-law who illustrated well in his own person the effects of predisposing circumstances in respect to ague. This gentleman was a strong active man, and commanded a battalion in the West Indies; and he escaped for a long time, while others were falling down around him in remittent fever. At last he was wounded by a musket-ball which passed through his shoulder. He insisted, much against the will of the surgeon of the regiment, on resuming his duties before his strength was completely re-established; and the consequence was that he was immediately attacked by a remittent fever of such violence, that his life for some time was despaired of. But this was not all. The remittent disease assumed by degrees a distinctly intermittent form, and became a tertian; and at last he got well, and strong, and came over to this country. But for a long while, though to all appearance his health was established, ague fits would from time to time occur; and they came precisely at the day and hour on which they would have happened if the tertian had continued with its original type; and slight causes were sufficient to reproduce them. He had marked, in an almanac, the days of the expected accession; and on those days it recurred, for some time, whenever the *east wind blew*. This very circumstance, the east wind, is a *common* re-exciting cause in such cases; exposure to cold in any way is another.

The exciting cause of intermittent and remittent fevers—the primary exciting cause I mean, that without which ague would never occur at all—deserves a somewhat particular consideration. I need scarcely say that it consists in certain invisible effluvia or emanations from the surface of the earth, which were formerly called marsh miasmata, but to which it has, of late years, become fashionable to apply the term *malaria*. In some respects, the latter designation is the more convenient of the two.

The malaria is a specific poison, producing specific effects upon the human body. In its medical sense, it is not simply bad air, or impure air, although the word is loosely employed by many to express any mixed kind of contamination of the atmosphere. Thus we hear of the malaria of London: but ague, even when it occurs in London, is very seldom, indeed, now-a-days, of London growth. The impure air incident to large and populous cities is prejudicial enough to health, as I formerly took occasion to show you: but it does not *generate* fever: neither continued fever, nor intermittent.

The emanations which cause ague have been called marsh miasmata, because they are

notoriously common in marshy places. But they are not peculiar to marshy places. For this reason, and for brevity's sake, I prefer using the single word *malaria*. In this country, thank God, we witness its milder evils only, and those not very often: but it is the bane and scourge of large portions of the world. Whether you practise here or abroad, it is very fit that you should know the qualities, habitats, and habits, of this wide-spread poison. The mildest form of fever to which it gives birth is the intermittent fever, or ague: but in climates and places where it exists in greater abundance and intensity, the fever becomes remittent, or even assumes the continued form. This has led to strange errors, and proved a fertile source of difference and controversy amongst medical men; not a few of whom confound the severe continued fevers which spring from the malaria, and which are never contagious; with the severe continued fevers usually called typhous, which are unquestionably communicable from person to person.

The effluvia which thus form the sole exciting cause of intermittent and remittent fevers proceed from the surface of the earth, and are, probably, gaseous or aëriform: at any rate they are involved in the atmosphere. But they are imperceptible by any of our senses. Of their physical or chemical qualities we really know nothing. We are made aware of their existence only by their noxious effects; and the inference that they exist was not made till within the last century and a half. Time out of mind, indeed, it had been matter of common observation that the inhabitants of wet and marshy situations were especially subject to these definite and unequivocal forms of disease. But the Italian physician, Lancisi, was the first, so far as I know, to put forth distinct ideas concerning malaria, in his book, published about 1695, *De noxiis paludum effluviis*. This is the great original work upon the subject.

To the production of this deleterious agent, a certain degree of temperature seems necessary. It does not appear to exist within the arctic circle: nor does it manifest itself during the colder seasons of more temperate climates. It is very seldom traceable beyond the 56th degree of north latitude; and it is supposed to require for its development a continuous temperature higher than 60° of Fahrenheit's thermometer. The nearer we approach the equator, the more abundant, virulent, and pernicious does the poison become, wherever it is evolved at all. In this climate it gives rise to intermittents, and principally to tertians. As we go south, in Spain, and along the shores of the Mediterranean, the remittent becomes the predominant form, and (what is very instructive) remittents there contracted often improve into intermittents upon the removal of the patient to a colder climate. Under the tropical heats, in the West Indies for example, the fevers very frequently assume the continued form.

And another condition of the development of the poison soon becomes apparent. It requires a certain degree of moisture. Of all these regions, malaria, showing itself always by its effects alone, infests certain parts only; which parts are, most generally, remarkable for their humid and swampy character. Thus, in this island, intermittents are produced chiefly, I may say almost exclusively, along the eastern coast; in parts of Kent, Essex, Cambridgeshire, Norfolk, Lincolnshire, and the East Riding of Yorkshire: and in each of these counties there are marshes, or fens, or low grounds and lands that are occasionally overflowed. Many of these spots have, within the last fifty years, been drained, and brought under cultivation; and agues are consequently much more rare in England than they formerly were. In Sydenham's time they were very frequent, and very fatal indeed, in this metropolis. James the first, and Oliver Cromwell, both died of ague contracted in London. At present (as I said before) we seldom meet with them. Except in the year 1827, I have never, since I have been in practice, known ague to be at all common here. This comparative freedom from malaria is mainly owing, no doubt, to the improved character of the draining and sewerage.

Agues, or aguish fevers, are endemic along every part of the low and level coast of Holland. In Italy, the Pontine marshes, near Rome, have possessed for ages an infamous celebrity of the same kind. The whole of the district called the Maremma, which stretches for about thirty leagues along the shores of the Mediterranean, and which is in some places ten or twelve leagues broad, is rendered dangerous, and almost uninhabitable, by the vast quantity of malaria annually evolved from its soil. In America large districts are, for the same reason, prolific of disease: and the late Bishop Heber, in his *Narrative of a*

Journey through the Upper Provinces of India, gives the following striking picture of the influence of the malaria in that part of the world. It seems to be alike pestiferous to man and beast.

"I asked Mr. Boulderson if it were true that the monkeys forsook these woods during the unwholesome months. He answered that not the monkeys only, but every thing which has the breath of life, instinctively deserts them from the beginning of April to October. The tigers go up to the hills; the antelopes and wild hogs make incursions into the cultivated plain; and those persons, such as dâk-bearers, or military officers, who are obliged to traverse the forest during the intervening months, agree that not so much as a bird can be heard or seen in the frightful solitude. Yet, during the time of the heaviest rains, while the water falls in torrents, and the cloudy sky tends to prevent evaporation from the ground, the forest may be passed with tolerable safety. *It is the extreme heat, and immediately after the rains have ceased*, in May, the latter end of August, and the early part of September, *that it is most deadly*. In October the animals return. By the latter end of that month the wood-cutters, and the cow-men, again venture, though cautiously. From the middle of November to March troops pass and repass, and with common precaution no risk is usually apprehended."

Persons who live in England might perhaps be disposed to think lightly of the malaria, had not such fearful evidence of its appalling power been brought home to the experience of our countrymen, in the early part of the present century, by the result of the unfortunate expedition to Walcheren. Sir Gilbert Blane has given an account of the ravages it there committed among our troops. You may see his paper, to which I shall presently again refer, in the third volume of the *Medico-Chirurgical Transactions*.

Not only a certain degree of heat, and a certain quantity of moisture, but the presence of all the four elements of the ancients, would appear to be requisite for the production of this poison. Air of course there must be; and earth also is essential. If heat and moisture were alone adequate, we should find the fever prevailing among sailors when out at sea: but it is not so, whatever may be the temperature under which they cruise. It is when they approach the coast, or land upon it, that they are attacked. The water of marshes has been examined under the microscope, and analyzed again and again, with a view to the discovery of the nature of this pestilential agent; but in vain. A more likely way to detect the noxious material would seem to be by examining the *air* of malarious districts: and this has been done carefully and repeatedly by expert chemists; and with the same want of success. The poisonous principle eludes the test of the most delicate chemical agents.

Where there is much heat, and much moisture, there we usually find also much and rank vegetation, and much vegetable dissolution and decay. The belief was as natural, therefore, as it has been general, that the putrefaction of vegetable matters was somehow or other requisite to the formation of the poison that exists so commonly in swampy situations. This belief has descended, almost unquestioned, from the time of Lancisi; and it obtains almost universal acceptance, I fancy, among physicians of the present day. Yet very strong facts have been adduced to show that the decomposition of vegetable substances is only an accidental, though a frequent, *accompaniment* of the miasm; and not by any means an essential condition of its evolution.

In the first place, the decomposition of vegetable matter goes on abundantly without the production of malaria. The rotting cabbage-leaves of Covent Garden, and those which taint the air of the streets from the neglected dust holes of London during the hot weather of summer, give rise to no ague. The same may be said of the putrefying and offensive sea-weed, which is deposited in large quantities upon some very healthy parts of our sea coast. But the converse facts are the most remarkable and conclusive. I have stated that *marshes* are not necessary to produce malaria: but Dr. William Ferguson —a physician who has had, and who has well used, very sufficient opportunities of investigating the question—shows that *vegetation* is not necessary: that the peculiar poison may abound where there is no decaying vegetable matter, and no vegetable matter to decay. As the prevailing belief is, in my opinion, an erroneous one, and as it is really of great importance that correct views of this subject should be taken, and disseminated by medical men, I will mention a few of the most striking of the facts detailed by Dr.

Ferguson. They are contained in a very interesting paper "*On the Nature and History of the Marsh Poison*," published in the *Edinburgh Philosophical Transactions*.

In August 1794, after a very hot and dry summer, our army in Holland encamped at Rosendaal and Oosterhout. The soil, in both places, was a level plain of sand, with perfectly dry surface, where no vegetation existed, or *could* exist, but stunted heath plants. It was universally percolated to within a few inches of the surface, with water which, so far from being putrid, was perfectly potable. Here fevers of the intermittent and remittent type appeared among the troops in great abundance. It is interesting to observe that the soil in Walcheren is precisely similar. Sir Gilbert Blane describes it as consisting "of a fine white sand, known in the eastern counties of England by the name of silt, and about a third part of clay." It was after a hot and dry summer also that the British army suffered in that island from the endemic fever, to a degree which Dr. Ferguson speaks of as "being almost unprecedented in the annals of warfare."

In the year 1809, several regiments of our army in Spain took up an encampment in a hilly ravine which had lately been a water-course. Pools of water still remained here and there among the rocks, so pure that the soldiers were anxious to bivouack near them for the sake of using the water. Several of the men were seized with violent remitting fever before they could move from the bivouack the next morning. "Till then (says Dr. Ferguson) it had always been believed amongst us that vegetable putrefaction (the humid decay of vegetables) was essential to the production of pestiferous miasmata; but in the instance of the half-dried ravine before us, from the stony bed of which (as soil never could lie for the torrents) the very existence even of vegetation was impossible, it proved as pestiferous as the bed of a fen."

After the battle of Talavera, the army retreated along the course of the Guadiana river, into the plains of Estremadura. The country was so arid and dry for want of rain, that the Guadiana itself, and all the smaller streams, had in fact *ceased to be streams*, and were no more than lines of detached pools in the courses that had formerly been rivers. The troops there "suffered from remittent fevers of such destructive malignity, that the enemy, and all Europe, believed that the British host was extirpated."

Cividad Rodrigo is situated on a rocky bank of the river Agueda, a remarkably clear stream: but the approach to it on the side of Portugal is through a bare open hollow country, that has been likened to the dried up bed of an extensive lake; and upon more than one occasion, when this lowland, after having been flooded in the rainy season, had become as dry as a brick-ground, with the vegetation utterly burned up, there arose fevers to our troops, which, for malignity of type, could only be matched by those before mentioned on the Guadiana.

Many more facts to the same purpose are related in Dr. Ferguson's paper, which is in every way well worth your perusal. He tells us "that in the most unhealthy parts of Spain, we may in vain, towards the close of the summer, look for lakes, marshes, ditches, pools, or even vegetation. Spain, generally speaking, is then, though as prolific of endemic fever as Walcheren, beyond all doubt one of the driest countries of Europe; and it is not till it has again been made one of the wettest, by the periodical rains, with its vegetation and aquatic weeds restored, that it can be called healthy, or even habitable with any degree of safety."

Our time will not allow of my extracting any farther evidence on this point: one circumstance of contrast, however, I am unwilling to omit.

The river Tagus is, at Lisbon, about two miles broad; and it separates a healthy from a very unhealthy region. On the one side is a bare hilly country; the foundation of the soil, and of the beds of the streams, being rock, with free open water-courses among the hills. This is the healthy side. But the Alentejo land, on the other side, though as dry superficially, being perfectly flat and sandy, is most pestiferous. Moreover, in and near Lisbon there are numerous gardens, where they keep water, during the three months' absolute drought of the summer season, in stone reservoirs. These reservoirs, containing water in the most concentrated state of foulness and putridity, are placed close to the houses and sleeping rooms: the inhabitants literally live and breathe in their atmosphere. "Yet no one ever heard or dreamt of fever being generated amongst them from such a source; though the most ignorant native is well aware that were he only to cross the river, and

sleep on the sandy shores of the Alentejo, where a *particle* of water at that season had not been seen for *months*, and where water, being absorbed into the sand as soon as it fell, was *never* known to be *putrid*, he would run the greatest risk of being seized with remittent fever."

Now these facts, and facts like these, seem to prove that the malaria, and the product of vegetable decomposition, are two distinct things. They are often in company with each other, but they have no necessary connection. Whoever, in a malarious country, waits for the evidence of putrefaction, will wait, says Dr. Ferguson, too long. For producing malaria it appears to be requisite that there should be a surface capable of absorbing moisture, and that this surface should be flooded and soaked with water, and then dried: and the higher the temperature, and the quicker the drying process, the more plentiful and the more virulent (more virulent probably because more plentiful) is the poison that is evolved.

The putrefaction of *animal* matter is sometimes spoken of as an element in the formation of the malarious poison. But the evidence I just set before you refutes this supposition as completely as it excludes the alleged necessity of vegetable decay. I hope to prove to you, in a future part of the course, that neither animal nor vegetable decomposition is sufficient to generate fever of any kind.

Dr. Ferguson's facts are generally in accordance with the observations which others have made upon the same subject; and his views will be found to account for some phenomena which the ordinary theory of vegetable putrefaction did not cleverly explain.

There is good reason for believing that in all cases the poisonous emanations proceed from parts of the surface that have been flooded and then dried, rather than from parts that are still wet, or putrid. And this elucidates a circumstance very often noticed, viz. that neighbouring places—especially high and low lands lying near each other—change their character in respect to salubrity upon the occurrence of rains. The low grounds, which had previously been very dangerous, become healthy when they are flooded over; and the higher lands, which are made wet, and which rapidly dry again, produce the malaria abundantly. For the same reason, the edges or borders of swamps, which of course expand or contract according to the wetness or dryness of the season, are more unsafe than their centres. The drying and half-dried margins of the purest streams may be prolific of the evil, when, from the want of confining banks, those margins have been flooded by the rising of the waters.

There is no observation more general than that, in malarious places, agues and remittent fevers abound more in hot and dry years than in those which are cold and moist. And this influence of temperature it is which mainly determines the differences observable in regard to these fevers at *different elevations*, and in *different seasons* of the year. In the higher grounds of the West Indies *agues* occur, as in this country: as you descend, and the mean atmospheric temperature increases, *remittents* are met with: and in the lowest and hottest parts the fever becomes *continued*. The following instructive facts are stated by Dr. Ferguson. In 1816, the British garrison of English harbour, in Antigua, was disposed in three separate barracks, on fortified hills surrounding the dock-yard. One of the barracks was on an eminence named Monk's Hill, six hundred yards above the level of the marshes. The other two were situate on an eminence called the ridge, one at the height of five hundred, and the other at the height of three hundred feet. So pestiferous were the marshes among which the dock-yard was placed, that it often happened to a well-seasoned soldier, coming down from Monk's Hill, and mounting the night-guard in perfect health, to be seized with furious delirium while standing sentry, and to expire within less than thirty hours after being carried up to his barracks, with a yellow skin, and having had black vomiting. Those in the barracks on Monk's Hill, *who did not come down*, the superior officers, the women, children, and drummers, had no fever of any kind. Seventeen artillery men, in the barrack at the height of three hundred feet, did not come down to the night guards. (We shall see hereafter that malarious places are always most dangerous at *night*.) Every one of these men was attacked with remittent fever, of which one of them died. At the barrack on the top of the ridge, at the height of five hundred feet, there scarcely occurred any fever worthy of notice. Thus, *in the same place*, the malaria, in the level plain, caused continued fever, resembling, and

I believe identical with yellow fever; at the elevation of three hundred feet it gave rise to remittent fever: and at the height of five hundred or six hundred feet its influence was scarcely felt at all. In the neighbourhood of the Pontine marshes you see the villages perched curiously on the intervening hills; the Italians having been taught by experience that these elevated spots afford comparative security against the effects of the miasmata.

Wherever the malaria prevails, it produces its peculiar consequences chiefly in certain seasons: and it is in the autumn especially that agues and aguish fevers occur; that is to say, after the heats of summer: and the hotter and drier the preceding summer, the more frequent and fatal are the autumnal fevers. The Pontine marshes lie to the southward of Rome; and Horace, you know, says or sings,

> Frustra *per autumnos* nocentem
> Corporibus metuemus austrum.

The effects of these morbific effluvia upon the human body vary much under different circumstances. Where they are most concentrated and deadly, their operation may be almost immediate. Witness their speedy influence upon the soldiers who descended at night from Monk's Hill. So also sailors, who have gone on shore for a single night only, have been attacked by the fever before they could return to the ship. On the other hand, when the emanations are less copious, or less intense, there is sometimes a long and uncertain period of incubation. The disease remains latent, or the poison lies dormant, for a considerable space of time. Many of the soldiers who were exposed to the malaria at Walcheren did not experience its bad effects until after they had returned, and even resided several months in England. In the same way, labourers, especially the itinerant Irish, will go down in the autumn for harvest work into Lincolnshire, and bring back the seeds of the disorder within them, and yet may not be attacked with ague for weeks or months; upon the occurrence of an east wind, perhaps, or unusual exposure to cold and wet. We trace in all this some analogy with the animal contagions; but the period of incubation is more irregular and accidental, and it is probable that in many instances the ague would not happen at all, but for the concurrent operation of some other malign influence.

Another fact worthy of notice in respect to the agency of the malaria upon the human frame, is that it affects strangers much more readily and decidedly than the natives of the place. In other words, habit mitigates the injurious effects of the poison. Persons become *seasoned* to it. At Walcheren, though almost every adult among the lower classes had laboured, in the course of his life, under the endemic intermittent, yet they were infinitely less subject to it than strangers: and they will not believe that their beloved birthplace is unhealthy. Sir Gilbert Blane says that persons of education, and even medical men, denied indignantly that their country was less healthy than any other; and attributed the sickness that raged among our troops to some trivial circumstance of diet or habits, and not to any insalubrity of the air. This is a curious moral feature; but a very general one. In the pestilential plains of Estremadura the superstitious natives, unable or unwilling to account for disease of a type so uncommon among the soldiers, from any unwholesomeness of the air, declared that they had all been poisoned by eating mushrooms.

It was found also, at Walcheren, that the strangers who survived the first attacks became thereafter much less liable to the endemic fevers. The French General, Monnet, who had held the command at Flushing for seven years, had acquired a knowledge of this fact, and endeavoured to turn it to practical account. He recommended that troops should not be frequently changed; for when it was the custom to send battalions from Bergen op Zoom every fourth night, in succession, to work on the lines of Flushing, these men never failed, on their return, to be taken ill in great numbers. General Monnet therefore advised, however displeasing it might be to the officers, that a stationary garrison should be retained at Walcheren, in order that the men might be habituated or seasoned to the air (*acclimatés*), and he adduced the instance of a French regiment which suffered in the second year of its being stationed there only one half the sickness and mortality which it suffered during the first year; and hardly suffered at all in the third year.

But although the natives and residents in malarious places are not so liable as new-

comers to the violent and distinct forms of fever, they are chronically affected by the insalubrity of the atmosphere. They are spoken of by travellers as being puny, yellow, and sickly; feeble in body and spiritless in mind; as having yellow faces, swelled bellies, and wasted limbs; as being subject to dropsies and fluxes; phlegmatic, melancholy, and short-lived.

One remarkable exception is mentioned by Dr. Ferguson. From some peculiarity of idiosyncrasy (which he conjectures may be somehow connected with the texture of his skin) the negro appears to be proof against endemic fevers. "To him marsh miasmata are in fact no poison; and hence his incalculable value as a soldier, for field service, in the West Indies. The warm, moist, low, and leeward situations where these pernicious exhalations are generated and concentrated, prove to *him* congenial. He delights in them, for there he enjoys life and health, as much as his feelings are abhorrent to the currents of wind that sweep the mountain tops, where alone the whites find security against endemic fevers."

No very certain or extensive observations have yet been made in respect to the *kind of soil* from which the miasmata are most apt to be extricated. That which is loose, penetrable, porous, and sandy, appears highly favourable to their formation. So are soils which, containing much clay, are very rententive of moisture. One curious fact, however, bearing upon this question, seems to have been made out; viz. that what is termed peat-bog, or peat-moss, is not productive of malaria. Many parts of Scotland and of Ireland, that are occupied by large tracts of marsh in which the peat-moss abounds, are completely free from these fevers. Dr. Bisset affirms that the exhalations from black peat-moss do not occasion intermittents, "at least in high moors under a clear sharp air." Now in the climate of Virginia, this counteracting influence of a sharp air can scarcely be looked for: yet it is a remarkable fact, that though the provinces of North America, especially North and South Carolina and Virginia, are full of ague, that disease is never seen among the inhabitants near the country of the *Dismal Swamp*, a moist tract of 150,000 acres on the frontiers of Virginia and North Carolina. Weld, the traveller, informs us, that this immense tract is covered with trees, and abounds with water, which appears the moment the shallowest trench is dug. This water is brown, like brandy, but quite clear, and not unpalatable. The colour is ascribed by the inhabitants to the roots of juniper; and the water is said to be diuretic. (*Craigie.*)

LECTURE XLI.

AGUE, CONTINUED. SPECULATIONS RESPECTING ITS PERIODICITY. HABITS AND PROPERTIES OF THE MALARIA: MOST NOXIOUS AT NIGHT; LIES NEAR THE GROUND; IS CARRIED ALONG BY WINDS; CANNOT PASS ACROSS WATER; ATTACHES ITSELF TO TREES; IS DIMINISHED BY THE INCREASE OF CULTIVATION AND OF POPULATION. ULTIMATE EFFECTS OF THE POISON ON THE BODY. AGUE FORMERLY THOUGHT SALUTARY. PROGNOSIS. PROPRIETY OF STOPPING THE DISEASE.

You will remember the progress we made, at our last meeting, in the subject of intermittent fever. I described the ordinary phenomena of a paroxysm of ague; and afterwards mentioned certain unusual symptoms with which it is sometimes complicated. The three principal types of ague were also delineated; the quotidian, the tertian, and the quartan: as well as their respective characters, and intervals, and varieties, and changes of type. I spoke too of the predisposing causes of intermittent fever, which may all be briefly included under the head of circumstances that tend to debilitate the body: the strongest predisposing cause of all being a former attack of the disease. And I began to consider the great exciting cause of agues and aguish fevers; the malaria. I first directed your attention to the circumstances under which the malaria appears to be evolved. Since the

time of Lancisi it had been very generally supposed that the humid putrefaction of vegetable substances was necessary to the production of this peculiar and wide spread poison; and that heat accelerated the putrefactive process. That was Dr. Bancroft's opinion. That also is (I believe) the opinion held, and stated in lectures, by many pathologists at the present time. I showed you, from facts which rest upon Dr. Ferguson's authority, that this opinion is founded in mistake: that the products of vegetable decay and decomposition may and do often coexist with malaria, but are distinct and separable from it, and by no means essential to its formation. There is reason to believe that the flooding of a porous earthy surface with water, and the subsequent drying of that surface under a certain degree of heat, constitute the sole or main conditions of the generation of the poison. We found that the effects of the malaria are modified by the temperature of the place: that in low and hot situations it may give rise to an affection not distinguishable in its symptoms from yellow fever; and that in proportion as the locality is higher and cooler, the fever tends to assume first the remittent, and then the intermittent type: that the period of incubation—the period which intervenes between exposure to the malaria and the invasion of the fever—is extremely variable in duration: that the poisonous effluvia affect strangers more certainly and more severely than natives of the place; that persons may become in some sort seasoned to the malarious districts: but that, with the exception of the negroes in the West Indies, the inhabitants of places much infested with the peculiar miasmata, are feeble, and sickly, and short-lived.

Periodicity of the fit.—There was one point which I briefly adverted to, and dismissed perhaps too unceremoniously: I mean the very curious fact of the *regular periodic recurrence* of the paroxysms of intermittent fever. I ought, I think, to have informed you of the views which pathologists have entertained respecting the explanation of that singular circumstance; although it must be confessed that the solution of the phenomenon is still to be sought for. A great number of persons have tried their hands, however, upon this question. Many of the earlier attempts at explanation are either quite hypothetical, or totally insufficient and illogical. Willis ascribed the intermisson to a periodic development of *fermentable matter* in the *blood*. But if any such development took place (of which we have no evidence) we should be no nearer the mark: the question would still recur, why the development of this matter should happen *periodically:* and the same remarks apply to various other so-called explanations brought forward by different writers of considerable reputation. Reil referred the intermittence of fevers to some *general law of the universe;* by which he meant, I believe, some vague generalization of such facts as the alternation of light and darkness, the periodic recurrence of the seasons, the ebbing and flowing of the tides, the succession of appetite and satiety, of the states of sleeping and waking, and so on: but this evidently is no explanation at all. M. Bailly offers a very singular conjecture upon the subject: he attributes the periodic phenomena to the modifications necessarily induced in the human system, and particularly in the function of circulation, by the alternate change of position from the upright to the recumbent, and from the recumbent to the upright. every twenty-four hours; and he adduces in corroboration of this notion the alleged fact that animals, which undergo no such oscillation of posture, are not subject to intermittent fevers: but this is said not to be a fact. Rodet and Charpentier affirm that horses are liable to such complaints. Dr. Macculloch refers to the case of a dog which laboured under a regular tertian ague for some years; the cold paroxysms taking place always at three o'clock in the afternoon. Even if this were not so, M. Bailly's theory fails to account for the occurrence of *continued* fevers. If his views were correct, then we might avoid having ague by refraining from these changes of position from the vertical to the horizontal during sleep, and back again upon awaking. Recently M. Roche has put forth the opinion that the attacks of ague are periodic, because the *causes* of them are periodic. And if this could be made out, the conjecture would carry with it some show of reason. He observes that the spring and the autumn are the seasons in which intermittent fevers chiefly break out, especially the autumn: and that during those periods there is a very sensible difference in the temperature and humidity of the atmosphere by day and by night, and even within the space of three or four hours; and that a consequent alternation of action and reaction is thus produced in the human body, and soon becomes an established habit. Throughout a part of the twenty-four hours, the operation of the mias-

mata is slight, or not manifest at all: while during another part of that period it is in full energy, and at about the same time daily. The emanations (which he conceives to proceed from putrefying vegetable matter) are most abundantly disengaged during the hottest part of the day: these watery effluvia are dissolved by the warm air to a certain amount; but after sunset, they are again deposited, and deposited the more copiously in proportion to the coldness of the atmosphere at that time: and coming in contact with the surface of the body, with the mucous membrane of the air passages, and perhaps also with that of the digestive organs, and being absorbed by those surfaces, it occasions the phenomena which constitute an ague fit. The influence of the miasmata being intermittent, we need not wonder, he says, that their effects should be intermittent too: and then he goes on to ascribe the repetition of the paroxysms, after the cause has ceased to be applied, to that tendency observable in the animal system to reproduce certain actions, simply because they have been produced before; in one word to the effect of *habit*. At length the habit wears out, which accounts for the spontaneous recovery of those who are removed from the malarious district.

It seems to be a very serious objection to M. Roche's theory, that the disease does not show itself, sometimes, for weeks or months after the patient has been exposed to the miasmata. His theory fails altogether also to account for the different *types* of intermittent fever. The differences of type are indeed *opposed* to the theory.

After all it is most probable that Cullen had discovered a *part* though not the whole of the truth respecting the periodicity of intermittent fevers, when he ascribed it to some law of the animal economy whereby it is subjected, in many respects, to a diurnal revolution. "Whether this depends," he says, "upon the original conformation of the body, or upon certain powers constantly applied to it, and inducing a *habit*, I cannot positively determine; but the returns of sleep and watching, of appetites and excretions, and the changes which regularly occur in the state of the pulse, show sufficiently that in the human body a diurnal revolution takes place." But he also is much perplexed with the differences of type; and all that he can say on that point amounts to this—that as the three principal types observe, severally, the same time of day for their accession, and as quartans and tertians are apt to become quotidians, these to pass into the state of remittents, and these last to become continued; and that as even in the continued form daily exacerbations and remissions are generally to be observed—all this marks the power of diurnal revolution.

A most interesting experiment, as it appears to me, performed by M. Brachet upon himself, shows in a strong light the influence of acquired habit in continuing certain unnatural states of the system when once they have been originated: the experiment connects itself also with the peculiar phenomena of intermittent fever. Towards the end of the month of October, in the year 1822, M. Brachet took a cold bath, at midnight, for seven nights in succession, in the river Saone. On the first occasion he remained a quarter of an hour in the river; on the second half an hour; till at length he was able to stay in the water a full hour at a time. After each bath he betook himself to a warm bed, and in a short time became affected with considerable heat, followed by copious perspiration, in the midst of which he fell asleep. At the end of the seven days M. Brachet ceased to repeat this experiment: but what was his surprise at finding on the following nights, between twelve and one o'clock, that all the phenomena of a true ague fit appeared in due order and succession! As, however, this artificial paroxysm was not very severe, and as he felt quite well during the day, M. Brachet determined not to interfere with it; but to observe the result. It recurred six times with great regularity. On the seventh night after he had omitted the baths, he was summoned, towards midnight, to a woman in labour: the ride to her house heated him, and on his arrival he kept up the heat by placing himself before a large fire, and from that time the febrile phenomena ceased to recur.

The facts and theories which I have thus brought roughly together, in respect to the periodicity of agues, are not without interest, but they show that we have yet much to learn on this subject. Granting that habit may have its share in continuing the regular recurrences, we want some explanation of the return of the second and third fit, after certain determinate intervals, to give a *beginning* to the habit. In respect to the quotidian, Dr. Cullen's *diurnal revolution* might come to the rescue. But this principle evidently will not apply to the tertian type. I know of no two-day, or bidual habit. And the objec-

tion holds still more strongly in regard to quartans. Indeed in quotidians themselves there is much difficulty in applying the explanation, for though by anticipating, or postponing, they *may* come on at different hours of the day, yet their usual and natural paroxysms occur, not in the evening, but in the morning, when, on the principle of diurnal habit, there should be the *least* tendency to exacerbation of febrile action.*

I pointed out, in yesterday's lecture, the favourite *habitats*, if I may so speak, of the malarious poison. I have still a few observations to make respecting its ascertained habits and properties. Some of the laws to which it is subject are of great practical importance, and ought to be popularly known: much more ought every medical man to be familiar with them.

The malaria most noxious at night.—In the first place, all malarious places are (as I have already hinted) much more dangerous *at night* than in the day time. Whether the poison be then more copiously evolved, or whether it be merely condensed and concentrated by the diminished temperature, or whether the body is at that time more susceptible of its influence, it certainly is most active and pernicious during the hours of darkness. To *sleep* at night in the open air in such places is almost to ensure an attack of the fever. Lancisi was quite aware of this, and devotes a chapter to the question. "Cur juxta paludes noctu præsertim indormientes magis quam vigilantes lædantur?" It has repeatedly been observed among the crews of ships, when off a malarious coast, that the sailors could go on shore in the day to cut wood, or for other purposes, with impunity; while the men who remained on shore through the night, guarding the water casks, were many or all of them seized with the fever. Take one instance as a sample of many. It is recorded by Dr. Lind. In 1766 the Phœnix ship of war was returning from the coast of Guinea. The officers and ship's company were perfectly healthy till they touched at the island of St. Thomas. Here nearly all of them went on shore. Sixteen of the number remained for several *nights* on the island. Every one of them contracted the disorder, and thirteen of the sixteen died. The rest of the crew, consisting of 280 men, went in parties of twenty or thirty on shore in the day, and rambled about the island, hunting, shooting, and so on: but they returned to the ship at night; and not one of those who so returned suffered the slightest indisposition. Exactly similar events occurred the following year, with the same ship, at the same place, where "she lost eight men out of ten, who had imprudently remained *all night on shore;*" while the rest of the ship's company, "who, after spending the greatest part of the day on shore, always returned to their vessel before night, continued in perfect health." Many more examples of the same kind are stated or referred to by Dr. Bancroft in his book *on the Yellow Fever:* a book which is rich in information respecting the malaria.

The reapers in the "Campo Morto"—a well-named part of the Maremma which I yesterday mentioned—are permitted to sleep for two hours about noon. They do so at that time without danger: but when the dews of evening have fallen down upon the earth, which serves them for their bed, it is then that the poison puts forth its most deadly power. Upon this principle, Lancisi admonishes those who in summer travel through the Pontine marshes, not to do so *at night*, as many had been accustomed to do, in order to avoid the greater heat of the day: and similar advice is still given at Rome to all strangers. Though the passage requires but six or eight hours, there are numerous instances of travellers who, in consequence of their having crossed these fens during the night, have been attacked with violent and mortal fevers.

The practical lesson to be derived from a knowledge of this fact is too obvious to dwell upon. In malarious countries the open air at night must be avoided. "Early to bed," is always a good and wholesome rule; but the other half of the proverb, "early to rise," becomes, in such countries, an unsafe precept. At least it is hazardous to *leave the house* early.

It tends downwards.—Secondly, the malaria *loves the ground.* Whether this results from its specific gravity; or from its adhering to the moisture suspended in the lower strata of the atmosphere; or from some peculiar attraction for the earth's surface, I cannot tell you.

* I would beg to refer the reader to Dr. Holland's interesting chapter (in his *Medical Notes and Reflections*, published since these lectures were delivered) "On Morbid Actions of Intermittent kind."—T. W.

There is reason to suppose that the poison combines somehow, or becomes entangled, with fog: and fogs usually brood and settle, at night especially, upon the surface. This may be one reason why *lying down* to sleep in the open air at night is so very perilous. The lower rooms of the same house may contain the noxious effluvia, while the upper are free. "In all malarious seasons and countries," says Dr. Ferguson, "the inhabitants of *ground floors* are uniformly affected in a greater proportion than those of the upper stories. According to official returns during the last sickly season at Barbadoes, the proportion of those taken ill with fever in the lower apartments of the barracks exceeded that of the upper by one-third, throughout the whole course of the epidemic. At the same time it was observed that the deep ditches of the forts, even though they contained no water—and still more the deep ravines of rivers and water-courses—abounded with the malarious poison." Dr. Hunter, in his work on the diseases of the army in Jamaica, says, "the barracks of Spanish Town consist of two floors, the first upon the ground, the second on the first. The difference in the health of the men on the two floors was so striking as to engage the attention of the Assembly of the island; and upon investigation it appeared that *three* were taken ill on the ground floor, for *one* on the other. The ground floor was not therefore used as a barrack afterwards." Mr. Ralph, in a table printed as an appendix to a paper of Dr. Ferguson's in the eighth volume of the *Medico-Chirurgical Transactions*, states the result of an inquiry into the comparative healthfulness of the upper and lower apartments of barracks in Barbadoes, to have been that the individuals residing in the lower apartments were attacked in the proportion of two to one of those living in the upper: and with certain apparent exceptions, which I shall notice presently, experience is uniformly in favour of the proposition that the poison is most prevalent and destructive near the surface of the earth, and does not rise high into the atmosphere.

To specify the sanatory precautions dictated by an acquaintance with this property of the malaria, must be quite superfluous.

Is carried along by currents of air.—Thirdly, the malaria is *movable by the wind*. It is capable, therefore, of being carried *from* the spot where it was generated; and *to* other places which might else be free from it and healthy. In this respect it is analogous to a heavy fog or vapour: and, in some cases, it is accompanied by a palpable mist; to which, perhaps, it may cling. The following passage relative to this subject occurs in Bishop Heber's Journal. "From Cheeta Talao our road lay through a deep and close forest, in the lower parts of which, even in the present season, the same thick milky vapour was hovering as that which I saw in the Terrai, and which is called *essence of owl*." This Terrai is the region which I mentioned in the last lecture as being so pestiferous, that it is deserted, during certain parts of the year, by every living creature.

This conveyance of the poison, like a cloud or fog, from one part of the surface of the ground to another, it is very important to attend to in all places; and especially in tropical climates, where the wind blows for a long time together from the same quarter. We are thus enabled to account for the apparent exceptions to the last-mentioned property of the malaria, viz. its preference of low to elevated situations. You will readily understand how the miasmata may roll up, and hang accumulated upon, the side of a hill towards which a current of air sets steadily from or across a neighbouring marsh. Nay, the poison may be thus blown *over* a hill, and deposited on the other side of it. In this way, I presume, are to be explained the following curious facts, related in Dr. Ferguson's paper.

"The beautiful port of Prince Rupert's, in the island of Dominica, is a peninsula which comprehends two hills of a remarkable form, joined to the main land by a flat and very marshy square isthmus *to windward*, of about three-quarters of a mile in extent. The two hills jut right out on the same line into the sea, by which they are on three sides encompassed. The inner hill, of a slender pyramidal form, rises from a narrow base nearly perpendicular, above and across the marsh from sea to sea, so as completely to shut it out from the port. The outer hill is a round-backed bluff promontory, which breaks off abruptly, in the manner of a precipice above the sea. Between the hills runs a very narrow clean valley, where all the establishments of the garrison were originally placed; the whole space within the peninsula being the driest, the cleanest, and the healthiest surface conceivable. It was speedily found that the barracks in the valley were very unhealthy; and to remedy this fault, advantage was taken of a recess or platform near the top of the

inner hill, to construct a barrack which was completely concealed by the crest of the hill from the view of the marsh on the outside, and at least three hundred feet above it: but it proved to be pestiferous beyond belief. In fact no white man could possibly live there, and it was obliged to be abandoned. At the time this was going on, it was discovered that a quarter which had been built on the outer hill, on nearly the same line of elevation, and exactly five hundred yards farther removed from the swamp, was perfectly healthy; not a single case of fever having occurred in it from the time it was built."

There is a striking anecdote given by Lancisi, showing, on a small scale, the effect of the wind in carrying the malaria with it. Thirty ladies and gentlemen had sailed to the mouth of the Tiber on an excursion of pleasure. Suddenly the breeze shifted to the south, and began to blow over a marshy tract of land situated to windward of them. Twenty-nine of the thirty were immediately after attacked with tertian ague. So also Humboldt informs us that the town of Cariaco is afflicted with intermittents by the north-west wind conveying across it the miasmatic emanations of the Laguna of Campona.

And as the wind will thus transport the malaria to a distance, and thereby render a spot unhealthy which naturally would not be so: it also is often of service in clearing the poison from other places, and preventing its concentration.

A knowledge of these facts ought to be valuable in determining the choice of encampments, and of sites for dwelling-houses in aguish districts. Settlers in hot climates, especially where trade-winds prevail, would do well to avoid founding towns on the lee side of any swampy or suspicious ground. The outlets of rivers are commonly selected, for the convenience of commerce: and there is often a right and a wrong bank. I believe that most of the principal towns in the West Indies are built, for the advantage of the outward bound vessels, upon the western, or lee side of the islands.

It cannot pass across water.—Fourthly, it is a singular, but well-ascertained fact, that the miasmata lose their noxious properties *by passing over even a small surface of water.* Probably they are absorbed by it. And this is another proof of their tendency downwards. Many instances have already been referred to, where some of the crew of a ship have landed on a malarious coast, and have all been attacked by the fever; while the rest of the sailors, who remained on board, continued all healthy and well, though the ship was close to the shore. You could not have a better or more striking example of this than what took place at Walcheren. "Not only the crews of the ships in the road of Flushing were entirely free from the endemic; but also the guard-ships which were stationed in the narrow channel between this island (Walcheren) and Beveland. The width of this channel is about six thousand feet, yet, though some of the ships lay much nearer to one shore than to the other, there was no instance of any of the men or officers being taken ill with the same disorder as that with which the troops on shore were affected." This Sir Gilbert Blane has told us; and it is curious that Sir John Pringle made the very same remark in the very same place in 1747. He is speaking of the diseases of the campaign in Dutch Brabant; and especially of four battalions which had remained for some time in Zealand: and he says, "But Commodore Mitchel's squadron, which lay all this while at anchor in the channel between South Beveland and the island of Walcheren, in both which places the distemper raged, was neither afflicted with the fever nor the flux, but amidst all that sickness enjoyed perfect health; a proof that the moist and putrid air of the marshes was dissipated, or corrected, before it could reach them."

It is probable that this peculiarity has led to an erroneous and contracted estimate of the space through which the poisonous effluvia can be carried, upon land, by the wind. Although the distance to which they are capable of being so conveyed, without losing their morbific power, has never been precisely defined, there can be no doubt that it is considerable. In Italy, according to Dr. Macculloch, the poisonous exhalations of the lake Agnano have been ascertained to reach as far as the convent of Camaldoli, situated on a high hill three miles distant.

Attaches itself to trees.—Fifthly, another remarkable property of the marsh poison, is its attraction towards, and its adherence to, the foliage of lofty umbrageous trees: so that it is very dangerous, in malarious places, to go under large thick trees, and still more dangerous to sleep under them. But this property, thus a source of danger to those who are ignorant of it, affords, when known and rightly made use of, a mode of protection and

remedy against the influence of the miasmata. In the territory of Guiana, where large trees abound, the settlers live fearlessly, and unhurt, close to the most pestiferous marshes, and to leeward of them, provided that a screen or belt of trees be interposed. New Amsterdam, in Berbice, lies on the lee side of an immense swampy forest, in the direct tract of a strong trade wind that blows night and day, and pollutes even the sleeping apartments of the town with the stench of the marshes; yet it brings no fevers. The inhabitants are well aware that it would be almost certain death for an European to sleep, or even to remain after nightfall, within the verge of the forest. To cut down the trees would not only be a perilous operation in itself; but would let in pestilence to the town.

This property also of the malaria, as well as the use to which it may be turned, was known to Lancisi. He describes the vast increase of agues and remittent fevers in Rome during the summer of 1695, after a great overflowing of the Tiber, by which the lower part of the city, and the fields adjacent, had been inundated in the preceding winter. The bad effects of this flood were felt throughout the whole of Rome, with the exception of one particular quarter, which was protected by a belt of trees around it. Lancisi even addressed a remonstrance to the Pope against a project which was entertained of felling some wood near the Pontine marshes, between them and the city. He endeavours to show that woods and groves were first made sacred on account of their conservative influence in this way, to prevent their ever being cut down.

It would appear, from the facts I have just been detailing, that dwellings unfortunately built in the vicinity of marshes, might sometimes be rendered safe and salubrious by encircling them at a little distance by a hedge of trees—or (perhaps) even by drawing round them a broad moat of water. Such experiments deserve at least a fair trial.

It is diminished by cultivation.—Sixthly, the production, and consequently the effects, of the malaria are prevented, or lessened, by *cultivation*. It is to this that the diminution of agues in this country is mainly attributable. The fenny lands have been drained; and many of them brought under the plough. Dr. Craigie states that East Lothian, in Scotland, was at one time so productive of malaria, that it was quite an expected thing that the reapers in harvest should be attacked with ague: but that now, in consequence of the perfect tillage, and the numerous tracts of wood with which the country is covered, that disorder is quite unknown there. Conversely, in regions which have been suffered to fall out of cultivation, intermittent and remittent fevers multiply. The more thoroughly any country is cultivated, the more fully, in general, is it *peopled* also: and in many places the prevalence of these fevers has been observed to diminish and increase with the increase and diminution of the population. *Cæteris paribus* agues are much less common in large towns than in country villages. This has been oddly enough accounted for by saying that populous cities are so full that there is no room for the malaria. A much more rational and probable explanation is that which ascribes the freedom of crowded towns, and thickly inhabited districts, to the number of fires burned in them.

Many instances might be adduced to show that the more any place, naturally productive of malaria, is depopulated, the more evident does the power of the poison become. The Italians date the introduction of the malaria into the Maremma, from the great plague in the sixteenth century, since which period the inhabitants of that district have never been sufficiently numerous to counteract the bad air which increases as population and agriculture diminish.

Bishop Heber, in the narrative I quoted before, bears testimony to facts of the same kind with those I have now been stating. He says, "At the foot of the lowest hills, a long black level line extends, so black and level, that it might seem to have been drawn with ink and a ruler. This is the forest, from which we are still removed several coss, though the country already begins to partake of its insalubrity. It is remarkable that this insalubrity is said to have greatly increased in the last fifteen years. Before that time, Ruderpoor, where now the soldiers and servants of the Police Thanna die off so fast that they can scarcely keep up the establishment, was a large and wealthy place, inhabited all the year through, without danger or disease. The unfavourable change is imputed by the natives themselves to depopulation. The depopulation of these countries arose from the invasion of Meer Khân, in 1805. He then laid waste all these Pergunnahs, and the population, once so checked, has never recovered itself."

Ultimate effects of the poison.—When persons having the intermittent fever are unable to leave the unhealthy situation in which they have been exposed to the influence of the malaria—and especially when they are placed under unfavourable circumstances in respect to food, and clothing, and shelter—the disease is apt to become exceedingly serious, leading to disorder of the sensorium, and great disturbance of the abdominal viscera, even in the intermissions; sickness, diarrhœa, dysentery, diseases of the liver. In Zealand, the biliary functions suffer so much during the complaint, that it is commonly known among the inhabitants of that country under the name of the *gall fever*. The frequent unnatural concentration of the blood in the internal parts may afford a reasonable explanation of these phenomena. When death takes place, morbid appearances present themselves such as might be expected: hepatic alterations; inflammation and ulceration of the mucous membrane of the alimentary canal: but the most characteristic morbid condition produced by repeated attacks of intermittent fever consists in enlargement of the spleen; with or without induration of its substance. That viscus is sometimes enormously increased in bulk, so as to be *felt*, and even its outline *seen* through the integuments of the abdomen. It has been known to weigh nearly eleven pounds. So common is this state of the spleen, that it is familiar to the observation of the vulgar, who have even given it a name: it is called among the inhabitants of the fenny parts of this country, the *ague cake*. I believe that whenever the abdominal circulation is much embarrassed, and the abdominal veins gorged, as they must be during the cold stage of an intermittent, the spleen in particular becomes distended with blood. Constantly we see this happen when the passage of the blood through the portal vessels is impeded by disease of the liver. Now this distension may, perhaps, not thoroughly subside at once. If the paroxysms of ague be frequently repeated, we may understand how the spleen may become fuller of blood on each successive occasion. It may be that a portion of the blood coagulates; or that inflammation of a slow kind is set up in the stretched covering of that organ. At all events, this is a very common sequel of ague: and it can scarcely be doubted that the repeated congestions of the internal vessels and viscera are the determining causes of the ague cake. Independently, however, of the paroxysms of ague, there is abundant evidence to show the injurious influence of the malarious districts upon the general health. In this country such effects are not much seen; but in places where the malaria is more constantly and abundantly present, the race of the inhabitants deteriorates. Their stature is small; their complexion sallow and yellowish; they are prematurely old and wrinkled; even the children early acquire an aged aspect; and the spirits and intellects of those who dwell in these unhealthy spots are low and feeble, and partake of the degeneration of their bodily qualities. It is a strange thing, therefore, that a notion should ever have prevailed, of the *salutiferous* effects of an attack of ague. But such a notion may be traced from very nearly our own times up to the earliest records of physic. "The late Dr. James Sims, who was a physician of some note in this town, felt convinced, at the commencement of the illness that terminated his life, that he should recover if he should catch an ague: and he went down into one of the marshy districts expressly for that purpose; but returned to London without having succeeded, complaining that the country had been spoiled by draining, and that there were no agues to catch. The superstitious Louis the XI. entertained a similar opinion, and prayed to the Lady of Selles that she would confer upon him a quartan ague. *Our* monarch, James the First, had more sensible notions on that score. There is an old English proverb which says, 'An ague in the spring, is physic for a king;' and when this was repeated to him by his courtiers, he being then ill of that disease, he answered that the adage might be applicable to a young man, but that it would not do for an old one like him. In fact, as I mentioned before, he died of his ague. The same doctrine has, however, been handed down to us by the father of physic himself. Hippocrates says, in the 57th Aphorism of his 4th Section, υπο σπασμου, η τεταγου ενοχλουμενω, πυρετος επιγενομενος λυει το νουσημα. And Celsus, in his capital digest of the medical knowledge of his time, preserves the same opinion, with some apparent astonishment that it should be true. 'Denique ipsa febris, quod maxime mirum videri potest, sæpe præsidio est.' I recollect hearing Dr. Graham, the professor of botany in Edinburgh, relate the following anecdote in one of his clinical lectures:—His brother was intimate with the professor of natural history at Cremona; and this gentleman was

resolved to put the truth of the aphorism that I have quoted from Hippocrates to the test. Accordingly he sent a patient afflicted with *epilepsy*, to pass a night or two in a marshy place, where the malaria was known to be so abundant, and so powerful, that few escaped ague who were there once exposed to its influence; and the twofold design succeeded admirably. The patient got an ague, and lost his epilepsy. The worthy professor contented himself with moderating and keeping in check the new complaint, thus intentionally produced, for a period of six months, when he administered its *coup de grace* in a few doses of Peruvian bark; and the epilepsy never returned. If I had believed that this could have been any thing more than a mere coincidence, I should have told you of it before, when I was speaking of the treatment of epilepsy. I should rather imagine that the notion thus prevalent for so long a time, that ague had a salutary tendency, and that it was wrong to stop it too soon, originated in the difficulty which physicians found in stopping it, before its cause was so well understood, and the specific for it was discovered. They found it obstinate under the feeble and inert methods then employed, and therefore they endeavoured to persuade their patients, and perhaps themselves also, that the disease had better proceed a certain length.

I have very little to say, in addition to what you must have inferred from what I have already said, as to the *prognosis* in intermittent fevers. In cold countries, such as ours, it is almost always favourable. Of course it will be modified by the previous condition of the patient: if he was beforehand the subject of serious organic visceral disease; or if he be very old or infirm; the supervention of ague may destroy him. But to persons of tolerable health and strength, prior to the setting in of the ague, we may confidently promise a *cure*. In warm countries intermittent fevers are much more dangerous: they are sometimes very rapidly fatal. They are often accompanied by most severe affections of the head, stupor, delirium, convulsions; and of the alimentary canal, diarrhœa, sickness, and not unfrequently the black vomit. They are prone, also, in those climates to run into the remittent or continued form; and this tendency is shown by long protracted paroxysms, or by the anticipation or doubling of the paroxysms. In all countries quartans are cured with more difficulty than either tertians or quotidians. And quartans are most common in the autumn: and accordingly autumnal intermittents are more pernicious and intractable than the vernal. This fact has passed into a proverb in Italy; which proverb has been thus translated into Latin, "Febris autumnalis—vel est *longa*, vel *lethalis*." The longer intermittents have lasted, the more difficult also are they to cure; and certainly there is much more danger of *visceral disease* in those that are of long standing.

It is probable that agues, such as we see in this country, would generally terminate under favourable circumstances, in spontaneous recovery, provided that the patient could be put beyond the further operation of the malaria, protected from exposure to wet and cold, and suitably nourished. But possessing as we do a specific cure for ague, if there be such a thing as a specific, there would be no sense in our allowing the experiment of a spontaneous recovery to be made: or rather we should be inexcusable, knowing as we do that the complaint is the more obstinate the longer it has lasted, and that it tends to the establishment of organic visceral disease, we should be inexcusable if we did not stop it as quickly as we can. The disease is always distressing to the patient, and always debilitating. It may be dangerous, even in these climates, to weak or old persons: and it is dangerous to all persons in hot climates. "If the first fit (says the wise and observant Heberden) has been marked so clearly as to leave no doubt of its being a genuine intermittent, the remedy should be immediately given in such a manner as to prevent, if possible, a second." There needs very little preparation of the patient before administering the specific substance which is to cure him; and which every one here knows before he hears me say so, is the celebrated Peruvian bark, or its active principle as presented by the salts of quina. The old practice was to wait a few returns of the fits, either till some hypothetical ferment had taken place, or until supposed morbid matter had been expelled by vomiting, or purging. There is, however, one very simple and short preparative which I am in the habit of using, and which I learned at Cambridge. You are aware that Cambridge is situated on the very edge of the fenny country which extends along that part of the east coast of the island. Numerous patients afflicted with ague come in from the surrounding villages; and Dr. Haviland found that many of these had taken quina before they applied for assistance as out-patients at the hospital; but with

very poor success. Now these cases readily gave way—the patient remaining in all other circumstances as before—after the operation of a calomel purge. I have adopted this practice, therefore, upon his recommendation; but it does not delay the specific treatment. I generally prescribe three grains of calomel with six or eight grains of rhubarb at bed-time, and commence with the quina the next morning. Very lately, in perusing the late Dr. Baillie's posthumous volume, I met with the following passage:—"I have known a good many cases in which bark alone would not cure an ague. In all these cases, as far as I now recollect, when a grain of calomel was given every night for eight or ten nights, bark cured the ague in the course of a few days. This practice I learnt from my friend Dr. David Pitcairn."

I believe that calomel given once in a purgative dose is enough.

But first of all, what is to be done for the patient while he is *in* the fit? I confess to you that I seldom give myself much concern on that head. In ague, as we see it in this country, nature generally prompts to the patient what to do: to cover himself up in bed, and apply warmth to his feet, and to take some hot drink, during the rigors; to adopt a cooler regimen during the hot stage; to wipe his skin dry, if the sweating should be very profuse or protracted. But in hot countries, and in severer forms of intermittent, the patient really requires some help; and therefore I must consider shortly in the next lecture the management of the paroxysm; and I am the more bound to do so, because certain measures which I do not think necessary or judicious, at any rate for the complaint as we see it here, have lately been strongly recommended during the ague fit.

LECTURE XLII.

TREATMENT OF INTERMITTENT FEVER: DURING THE PAROXYSM; DURING THE INTERMISSION. PROPHYLAXIS.

I WAS about, when we last separated, to consider the treatment of ague: first, during the paroxysm; secondly, during the intermissions.

Treatment during the fit.—In this climate we need not, I say, encumber a patient in an ague-fit with too much help. But in hot countries, where the disorder is apt to run into the remittent, or even the continued form, and where, during its violent and rapid course, internal organs are liable to sustain serious damage, the best and indeed almost the only time for the effectual interference of the physician is in the first assault or paroxysm of the disease.

The objects of treatment during the paroxysm are, to alleviate the uneasy sensations of the patient; to abridge, if possible, their duration, by shortening the fit; and to avert the danger which, under certain circumstances, may arise from intense internal congestion long continued, or from the severity of particular symptoms.

Now in the cold stage of ague, *diluent drinks* have been recommended, and *cordials*, and *external warmth*, and *opium*, and *emetics*, and *blood-letting*. One would suppose that if some of these expedients were useful, others could scarcely be so too. The *diluent drinks* are very proper: and I should allow the patient to use his own discretion in the choice of them. It was customary, formerly, to prescribe medicated drinks of this kind; and one pleasant, but neglected ptisan still lingers in our Pharmacopœia, the decoctum hordei compositum. Now-a-days we are contented with the simple barley water, toast and water, weak tea, gruel, and the like. These diluents should be taken warm, and for persons who are very feeble or exhausted, they may be made gently *cordial;* weak negus, for example, or white wine whey, may be given.

External warmth, being what nature and common sense would suggest, is certainly advisable and beneficial in the cold fit; even the warm bath, if it can be procured. In some places it is the custom to await an expected fit *in* the warm bath. When this cannot so

conveniently be obtained, the pediluvium may be employed; or the patient may be put into a warmed bed, and have bags of hot salt or bran applied to his epigastrium; and a hot bottle, or a hot brick, wrapped up in flannel, to his feet. Or, what perhaps is best of all, he may have a hot air bath *applied to him*, as he lies in bed. This may be very easily done, by means of a semicylinder or cradle of wicker work, closed at one extremity by a board. This is laid over the patient, and then covered with blankets. Through a hole in the centre of the board one end of a curved iron tube is passed; the other end, expanded into a bell, looks downwards; and a spirit lamp being placed beneath it, the air between the wicker work and the sick person is soon made very hot. This apparatus was constructed, and introduced into the Middlesex Hospital many years ago, by Dr. Gower; and we often find it of the greatest utility. External warmth applied in some one of these ways, affords singular comfort oftentimes, and contributes to shorten the cold stage. And the same may be said of friction, with stimulating liniments, along the course of the spine. Lind found that, in children, rubbing the spine with an embrocation composed of equal parts of soap liniment, and laudanum, at the approach of the cold stage, often prevented the paroxysm.

Opium has often been exhibited in the cold stage, with the view of cutting short the fit; and not without some success. The strongest evidence of its usefulness in that stage of the paroxysm is furnished by Dr. Trotter, in his *Medicina Nautica*. Agues being very frequent among the crew of the Vengeance, he resolved to try the full effect of opium in preventing the fit. At its first approach, a dose of laudanum (never less than thirty drops) was given; if this did not bring on some warmth within ten or fifteen minutes, from twelve to twenty drops more were administered. In most cases, "in a few minutes an exhilaration of spirits was perceived: the pulse from being weak, quick, and sometimes irregular, became less frequent, full, and equal; an agreeable warmth was diffused over the whole frame, and every unpleasant feeling vanished, sometimes in a quarter of a hour. The patients were themselves surprised at the sudden change in their sensations." Dr. Trotter speaks of these as being the completest cures that ever came under his observation. If, at the next period, the paroxysm threatened to recur, the opiate was repeated always with the same success. "Few instances were met with where any indisposition indicated a third attack at the expected period of accession." Notwithstanding this testimony, it appears that opium is still better adapted to another stage of the paroxysm.

Emetics were formerly much prescribed in the cold stage, at its earliest approach. Cullen recommends them; and they may sometimes be useful, in spite of Chomel's assertion that they are always hurtful. That they have gone so much out of fashion, is, however, a proof that they cannot be depended upon for cutting short the paroxysm. Vomiting is itself no small distress to many persons; and for my own part, I should not think of giving an emetic unless some indications of a loaded and oppressed state of the stomach were present; such as nausea, an ill taste in the mouth, a coated tongue, and foul breath. A scruple of ipecacuanha will even then be sufficient. The object is to empty the stomach effectually, but mildly. I would not give antimony. Irritability of the stomach, in the severer of these fevers, is too apt to rise spontaneously. Sir Gilbert Blane tells us that the greatest impediment to the cure of the severer intermittents at Walcheren, in their early stages, proceeded from the extreme irritability of stomach, which made it difficult to administer the requisite medicines. In hotter climates nausea and vomiting are still more common and more urgent; and we have to guard against the risk of inducing or aggravating these symptoms. "Emetics (says Dr. Mackintosh, in his *Practice of Physic*) have been often extolled, but I believe every experienced tropical physician will agree with me in cautioning young practitioners against their indiscriminate employment. Irritability of the stomach is one of the most frequent and troublesome symptoms; and once excited, it is always difficult, and in many cases impossible, to restrain it. I have seen emetics exhibited and the vomiting has continued till death, in spite of every remedy.

Lately, the practice of *blood-letting* in the cold stage has been revived (for it is not a new practice), and strongly recommended by the physician whose name I have just mentioned; and whose opinion carries with it the more weight from its having been founded on much personal experience in the treatment of these fevers. Dr. Mackintosh affirms that bleeding, performed in the cold stage, will often stop at once the paroxysm, and with

it the disease: that even when its curative effects are less decisive, it will generally stop the cold stage, and shorten the paroxysm, and mitigate its severity, and afford speedy and great ease to the distressful sensations of the patient; and that any subsequent paroxysms that may occur will be mild and few. One bleeding, he says, is commonly sufficient; sometimes two are required; seldom more than two. The blood is to be suffered to flow till the patient feels relief: which usually consist in liberation from pain of the head and loins; freedom of respiration; the departure of the painful sensation of cold; and the cessation of the tremors and of the debility. Most of the patients fall asleep after the operation. These effects have been produced by the abstraction of an ounce and a half of blood; they have sometimes (but rarely) required for their production twenty ounces.

Now this is the piece of practice to which I adverted at the close of yesterday's lecture, as being, in my humble opinion, inexpedient, and not to be recommended; at least in the agues of this country. I have seen a good many cases first and last, and certainly I have never seen one in which I could have thought such an heroic remedy necessary, in the cold stage; if indeed it be, in that stage, a remedy at all. But I do not desire to oppose my experience alone, or my judgment, to that of Dr. Mackintosh. His method has been tried, since he first made it public, by various practitioners in this country. Drs. Townsend and Law, of Dublin, found it fail in the majority of cases. In Dr. Stokes's hands, the most usual effect of blood-letting in the cold stage was, to check the shivering; and, next to this, to mitigate its severity, without abridging its duration. In most instances, no modification was produced of the hot and of the sweating stages. In Dr. Kelly's experience, the general effect was, to shorten the cold stage, and to render the hot one milder; but in some cases it seemed to aggravate the symptoms. Mr. Gill found that, although the blood-letting might cut short the cold stage, it appeared to lengthen the period of febrile disturbance.

Confining myself, then, to intermittents as they show themselves in this climate, I cannot advise you to adopt the practice introduced by Dr. Mackintosh—of bleeding in the cold stage. I object to it because it appears to me quite unnecessary; because it is not such as the nature of the symptoms would suggest; because it tends to produce subsequent debility, which we should not needlessly inflict; and because the experience of other sober-minded men, who have given the method a fair trial, does not bear out the statements made by Dr. Mackintosh in respect to its usefulness.

At the same time, after a careful perusal of nearly a hundred cases adduced by Dr. Mackintosh to illustrate the efficacy of this measure, I think it highly probable that blood-letting may constitute the most important part of the treatment, in the very outset of the severer malarious fevers of hot climates; attended as they are with a degree of internal congestion and disturbance which is dangerous to the integrity of vital organs.

If, in this country, bleeding be requisite at all, it is in the *hot* stage. But it is not requisite at all, except when there appears to be danger of some internal inflammation. The best remedy of the hot stage is undoubtedly opium. Dr. Lind, who wrote after large experience, says that he never saw a person die in the cold fit, but had known several carried off in the hot one, with strong convulsions and delirium. He happened to notice the beneficial effect of an opiate given while the patient was very hot and feverish. He determined therefore to make further trial of opium in the paroxysm. "Having at that time (says he) twenty-five patients labouring under intermitting fevers, I prescribed an opiate for each of them, to be taken *immediately after* the hot fit, provided the patient had then any inquietude, headache, or any such symptom usually subsequent to the fever. The consequence was, that nineteen in twenty-two received immediate relief; the other three had no occasion to take it."

"Encouraged by this surprising success, I next day ordered the opium to be given *during* the hot fit. In eleven patients out of twelve to whom it was thus administered, it removed the headache, abated the fever, and produced a profuse sweat; which was soon followed by a perfect intermission. Since that time I have prescribed an opiate to upwards of three hundred patients labouring under this disease; and I observed that if taken during the intermission, it had not the least effect, either in preventing or mitigating the succeeding fit; when given in the cold fit, it once or twice seemed to remove it; but when

given half an hour after the commencement of the hot fit, it generally gave immediate relief."

Dr. Lind goes on to state that he found the influence of opium more uniform and constant in intermittent fever than in any other disease; and more quick and sensible than that of any other medicine.

Very little need be said in regard to the sweating stage. Up to a certain point the perspiration is to be promoted and encouraged. When the uneasy feelings of the patient have abated, it should be restrained; not suddenly, but with caution. Now the sweating may be promoted by diluents; by keeping the patient in bed, and covered with moderately warm clothes; by sippings of hot gruel, or of hot chicken broth. On the other hand, when the sweating has continued long enough, it may be stopped by drying the patient carefully with towels, changing his linen, and getting him up, out of bed.

It is well to bear all this in mind; but I repeat once more that in agues, such as you are likely to meet with in this country, it is unnecessary, and therefore objectionable, to be *over-busy* during the paroxysm. Wherever the disorder assumes a distinctly intermitting form, the most important part of the practice is that to be employed during the intermissions. Now there are certain general remedies advised for adoption in this period; and there are certain specific remedies. The general remedies are bleeding, emetics, and purgatives. They need not detain us a moment. Blood-letting may be used if there be any apparent tendency to local inflammation, or any marks of severe topical congestion; especially in young and robust subjects. Barring such circumstances, there can be no occasion to bleed your patient in the intermissions.

An emetic given a short time before the expected paroxysm has been known to prevent its accession; and even has sometimes cured the disease. But we can stop the paroxysms by gentler and better means; so that I should not prescribe an emetic unless I saw symptoms of a foul and loaded stomach.

Purgatives should always be given at the outset. They clear the stomach and intestines of hurtful accumulations, which are apt to impede the beneficial operation of the quina, or of other drugs given to check the disorder. I mentioned in the last lecture my own custom in this matter; viz. to give a couple or three grains of calomel with eight or ten of rhubarb at bed-time; and the specific remedies may be commenced with the next day.

Of these specific remedies, bark and arsenic are by far the most certain and important; but a multitude of others have been highly praised for possessing similar virtues. I shall by and by say a word or two about *some* of these, because bark is dear, and arsenic is scarcely a safe drug to be entrusted to the hands of unprofessional persons; and it is often expedient, in country places, where agues are rife, to provide the poor with remedies which they may have at hand; and which should both be reasonably cheap, and perfectly safe.

I shall not detain you with any account of the difficulties and objections which were thrown in the way of the Peruvian bark, upon its introduction into the materia medica about the middle of the seventeenth century. Its use met with the most violent opposition, even from physicians of the highest authority. It was resisted by Stahl and Hoffman; and Boerhaave was never quite reconciled to it. Sydenham, by his example and recommendation, greatly promoted its adoption in this country. All this history is sufficiently curious and interesting, but I have no time for it: and you will doubtless hear it from one of my colleagues. I will merely say that in the Peruvian bark we have one of the very few *specifics* that we can boast of possessing; and that, unlike most other highly vaunted substances, so far from falling off from the accounts first given of its virtues, it has acquired in the lapse of time an increase and stability of reputation.

Neither shall I enter at all into the consideration of the qualities of the several species of cinchona; nor of the several principles that may be educed from them; nor of the modes in which the quina even may be best procured. This would not belong legitimately to my province. I must suppose that the professors of chemistry and of materia medica have furnished you with the sulphate of quina, which is the only preparation of the bark I intend particularly to notice: and *my* business is to tell you what I know in respect to its employment as a *remedy for ague*.

I may observe, however, that this is a remedy to which we could never have been led by any process of reasoning. It is a matter of pure empiricism. We know nothing of the seat or the essential nature of the disease; we are equally in the dark as to the *modus operandi* of the quina in curing it; yet our knowledge of ague, upon the whole, estimated in reference to its precision and practical bearing, is more satisfactory than of many other complaints, with the seat and nature of which we are much better acquainted. The group of symptoms is so distinct, that we have no trouble or doubt as to the diagnosis; and experience has taught us a remedy which is all but infallible.

The discovery of quina and its salts formed a great era in the history of the materia medica. As far as my own experience goes, the sulphate of quina has quite superseded the necessity for exhibiting any other form of cinchona for the cure of ague. Before quina was unshrouded by the chemist, the bark in substance was the only form in which the remedy could be confidently relied upon; and I am old enough to be aware of the infinite superiority of the salt, over the actual bark. To obtain the desired effect, it was often necessary to give it in such quantities as almost justified Mr. Abernethy's sarcastic way of speaking of it and of physicians. He said the doctors talked of throwing in the bark, as if it were to be pitched into the stomach with a shovel. The sulphate of quina lies in a much smaller compass, and a more commodious form; and it does not cause that insupportable nausea which the woody mass of the powdered bark was so apt to occasion.

I am in the habit of giving two, and sometimes three, grains of the sulphate of quina every four or six hours during the intermission, to those patients whom I have occasion to treat for ague. This plan has succeeded so well, that I have never been tempted to try any other. I may, indeed, say that I have never known it fail to stop an ague; and that in a short time: so that very few paroxysms have occurred after the patient has begun to take the medicine. You may give it in the infusion of roses, which contains a convenient quantity of sulphuric acid to ensure the solution of the sulphate of quina. It changes the colour of the infusion, however, and renders it pinker and opaque. Whether the draught be more or less elegant on that account, I will not take upon me to say; I know that the virtue of the quina is not interfered with by the change. In private practice, I commonly prescribe as many drops of dilute sulphuric acid as there are grains of the quina, with a drachm of the tincture of orange peel, and a drachm of the syrup of the same; completing the draught with water. This I find my patients commonly approve of, except in its bitterness, which, in solution, nothing can disguise. Or the salt *may* be administered in the shape of a pill: it is best, however, and surest in solution.

A question has been raised, whether this remedy should be given in repeated doses during the intermissions, or whether one very large dose should be given a short time before the paroxysm is expected. Dr. Home made some experiments on that point in the clinical wards of the Edinburgh Infirmary, some time ago; and he thought that the result was in favour of the plan of giving the bark regularly at short intervals. I have told you the amount of my own experience, which, however, is not very great; nor have I had any severe cases to deal with. I think it not improbable that my patients would have been cured quite as soon if I had given the remedy in half the strength. Dr. Barker, of Dublin, has found small doses equally effectual with large ones; and this is very likely to be the case with *specific* remedies. It would appear, however, that in some quartans it is better to give large doses before the return of the paroxysm. Dr. Elliotson gives large doses just *after* the paroxysm; and then smaller doses during the remainder of the intermission, at regular periods. A great majority of those who suffer ague are poor persons. Of course the first object is to make the cure as *speedy* as possible; the next to make it as *cheap* as possible. So that it is not a matter of indifference, or mere speculative curiosity, to ascertain with how small doses of quina you may cure an ague. I repeat that it has not happened to me to be disappointed, when I have given the medicine in small doses, as already described; which amount to about twelve grains in twenty-four hours; but, then, I suppose my cases have been well behaved and submissive. Dr. Elliotson states that he is continually obliged to give twenty or thirty grains in the twenty-four hours, before he can cure the complaint; sometimes in obstinate quartans, forty-five grains; and he mentions one case in which a scruple of the sulphate of quina, with ten minims of

the liquor arsenicalis, were given every eight hours in vain, but succeeded perfectly when given every six hours.

It appears also, upon the testimony of careful observers, that in warm climates larger doses *are* required; and that it takes a *larger quantity*, upon the whole, to repel the complaint. In the aguish tracts of Italy, in the Maremma, small doses are said to be inadequate; and the physicians there are in the habit of giving twelve, twenty-four, or even thirty grains at a time: and in one recorded instance, the dose, in seven days, was got up to 108 grains, before the ague was arrested. The medium dose, in many parts of America, seems to be eight grains.

It sometimes happens that the irritability of the stomach is so great as to make it difficult to introduce a sufficient quantity of the remedy into the system. This difficulty was very much felt at Walcheren: it is in a great measure removed since the discovery of quina. But even the quina sometimes sits ill on the stomach; and it is often very difficult to get children to swallow any preparation of bark, on account of its bitter taste. It is an important thing to know, therefore, that it has been found scarcely less effectual, in curing the disease, when thrown up into the rectum. The menstruum in which it is dissolved should not exceed two or three ounces, lest the bowel should reject it. Its expulsion may sometimes be prevented by adding a few drops of laudanum to the enema.

It is said that bark in substance will sometimes cure the disease when quina fails. I have never witnessed this: but in obstinate cases I would give the quina in *decoction* of bark.

You must not be satisfied with merely stopping the paroxysms. Patients will often be too ready to give up their medicine, as soon as the paroxysm has once missed. But the disease is very apt to recur; and it will always be right and prudent to go on with the quina for ten days or a fortnight after the patient *seems* cured, gradually diminishing, after the first week, the amount and the frequency of the doses.

There have been some curious facts observed in regard to the relapses that are apt to take place after the bark or the quina has been omitted. Clark, of Dominica, states that if no more of the remedy be taken, in the West India ague, than is barely sufficient to stop a fit, and then the bark is suspended, a relapse may take place on the eighth day, in the case of a quotidian; on the fourteenth or fifteenth in the case of a tertian or double tertian; and on the twenty-first or twenty-second in the case of a quartan: thus making (you see) in each type, seven periodical revolutions from the time the fit was suppressed to the next attack; and the fit was found to return on the proper day, at the same hour at which it would have returned if its course had not been interrupted by the administration of the remedy. All this is very curious, and inexplicable; but it points clearly to the propriety of continuing the remedy for some time after the disease appears to have vanished.

Arsenic is another substance which has great and unquestionable power over ague. It carries with it these marked advantages: it is efficacious; it is cheap; and it is tasteless. It is well adapted by these qualities for the poor, and for children, and for patients of every age and rank in whom there is much irritability of stomach present; but then it has also the serious disadvantage of being an active poison. One over-dose may be fatal: and even its long-continued use in minute doses leads sometimes to evident and lasting disorder of the health. Arsenic, therefore, is an unsafe remedy to be trusted in the hands of the ignorant. It should never be administered except under the immediate supervision of a medical eye; and even then it requires to be given with much caution. Its bad effects may be very certainly prevented, however, by care and attention; and it becomes a valuable instrument of cure, and should be adopted without scruple, in cases where its operation can be watched, and where the quina does not agree with the stomach, or fails to stop the disease. I often prescribe arsenic for other complaints; but, as I said before, I do not recollect ever having been foiled in removing ague by the sulphate of quina. Some persons are of opinion that relapses are less frequent after the cure by arsenic than after the cure by bark. It would require a large induction of particular facts to make that point clearly out.

When substances, which even in small quantities are active poisons, are used as remedial agents, it is convenient to have some definite form in which they may be administered at all times, and in all places. The liquor potassæ arsenitis of the London Pharmacopœia supplies such a form. This is the form in which it was recommended to the public by

Dr. Fowler: and it is therefore sometimes called *Fowler's solution*. It was founded upon an analysis of the *tasteless ague drop*, which had been in considerable repute in some parts of England. The pharmacopœial preparation is an arsenite of potass in solution. There are eighty grains of arsenic in the new or imperial pint, and therefore four grains in an ounce of the solution. Ten minims two or three times a day is a full dose for an adult; and you had better commence with not more than five minims. Ten minims contain one-twelfth of a grain. Twice that quantity has been administered at once; but this ought never to be done except when the system has been gradually inured to the arsenic, and thereby enabled to bear such a dose. It is a good precaution not to give this corrosive substance on an empty stomach.

The poisonous or hurtful effects of the mineral that we have to look out for are loss of appetite, nausea, and sometimes vomiting; griping pain of the stomach and bowels, and diarrhœa; and if the medicine be continued, *fainting* is often added. Other symptoms, less constant perhaps, and less important, are painful and hot tumefaction and stiffness of the face and eyelids, or even a tingling eruption something like nettle rash. These effects may, I believe, be controlled by adding a few drops of laudanum to each dose; but I would rather advise you to suspend the use of the arsenic; or to leave it off altogether. When this is done, the unpleasant symptoms will readily yield to mild laxatives, followed by opiates.

When the paroxysms continue to recur in spite of the bark, it has been recommended (and I think the plan a good one) to try to stop them by arsenic; and then, the periodic recurrence having been broken, to employ sulphate of quina to prevent a relapse.

These, then, quina and arsenic, are the two sheet anchors to which we trust in the cure of ague. A host of other remedies, I say, have had their praises sung. I do not intend to enumerate them. But there are a few which I think it right to mention, for reasons already assigned. There is strong evidence of the efficacy of some of them; they are cheap, and easily accessible, and above all, safe; and therefore, in aguish districts, they may with much propriety and benefit be recommended to the poorer classes, or distributed by Lady Bountifuls.

One of these is willow-bark; in substance, or in decoction. If this does cure agues, as it is affirmed to do, it would seem as if Providence had placed the antidote alongside of the poison; for these trees, as you know, abound and flourish in marshy places. The bark of the willow furnishes an alkaloid substance called *salicine*, in which the febrifuge property is believed to reside. Holly leaves, and *ilicine* derived from them, stand in much the same repute in France, as willow bark here.

Another curious remedy, said to be very successful, is the web of the black spider, which inhabits barns, stables, and cellars. This substance has been tried on a tolerably large scale, and the testimony to its influence in curing ague is very strong. Dr. Craigie has given this account of it. In the year 1760, a number of prisoners from the vanquished squadron of Thurot having been landed in the Isle of Man, Dr. Gillespie, who was practising there, found that many of the agues which came to prevail both among these prisoners and the inhabitants of the island, obstinately resisted bark, and such other remedies as he had recourse to. He was informed, by an old French physician belonging to the squadron, of the alleged efficacy of cobweb, in certain forms of the disease. He therefore made trial of cobweb, and found it to answer admirably. He was successful with it in more than sixty cases of different types, in the Isle of Man, and he had farther experience of its utility subsequently in Ayrshire.

After this, the same remedy was tested in the West Indies, by Dr. Jackson, to whom Dr. Gillespie had recommended it. Dr. Jackson's observations were made in the hospital of the army depôt, in the West Indies, in 1801. Several cases of ague, on which bark, arsenic, or mercury, singly or alternately, had made either a very temporary impression or none at all, were selected for experiment. And in four of these cases, two pills, containing each five grains of cobweb, were given at intervals of two hours, commencing six hours before the anticipated time of the return of the paroxysm. The fit did not return. On subsequent trials it was found not only to arrest the course of agues, but to remove various symptoms, such as pain, delirium, vomiting, griping, in ague and in continued fever, when these symptoms were unconnected with inflammation.

Charcoal is another substance which has been found effectual for the cure of intermittent fevers. You may find an account of it in the 10th volume of the *Edinburgh Medical and Surgical Journal*. It would seem to be especially useful in those cases in which there is a marked disturbance of the digestive organs; nausea, flatulence, hiccup, diarrhœa, or dysentery. It is said generally to cure the complaint by the time two drachms of it have been taken. It may be given in doses of ten or twenty grains, in arrow root; or with a few grains of rhubarb. If the power of this substance should be confirmed by future observations, a cheap remedy would thus be open to the poor. A clergyman of my acquaintance assures me that he seldom fails to cure agues among his parishioners by administering to them the snuff of candles, which he takes care to have collected. He does not inform them of what this black powder consists. I presume that its virtue may proceed from the charcoal it contains; unless it is derived from the confidence his flock is accustomed to place in his specific. The very same remedy, the snuff of a candle, is mentioned by Lind.

Piperine, the crystalline salt of pepper, has obtained a considerable reputation of late years, as a remedy for intermittent fever. It was largely tried by an Italian physician, Meli; and Dr. Gordini has repeated Meli's experiments at the hospital at Leghorn; and the following are the general conclusions at which these physicians have arrived:—1. Piperine, in doses of six or eight grains, cures intermittents. 2. It is more efficient in powder than in pills. 3. It succeeds in certain cases in which the sulphate of quina fails. And 4. It is more effectual in preventing relapses. I have seen letters from some practitioners in this country, bearing testimony to the power of the piperine. That pepper will cure ague, has long been the vulgar belief; and a very popular remedy for the disease is a teaspoonful of pepper in a glass of gin.

I presume that the efficacy of chamomile flowers in the removal of intermittent fever is to be attributed to the piperine which they have been ascertained to contain. These flowers had been long in use for the treatment of ague, before the Peruvian bark was discovered; and they are said to have accomplished a cure, since that time, after the bark had failed; but this was before the quina had been educed from it. Heberden advises us to have recourse to chamomile flowers, if the bark should disappoint us. I am always willing to embrace an opportunity of referring to his commentaries, for the exact observations they contain, but above all for the beautiful Latinity of which the whole book is an example. I recommend it strongly to you, next to Celsus, the best model you can study for good medical Latin. In reference to the point before us he says, "Cortex, quanquam rite sumtus, interdum parum efficax est: quo in casu suspicio erit ventriculum sordibus onustum vim remedii impedire. Itaque vomere oportet; quo facto, febris raro non cedit. Quod si redire perseveret, confugiendum est ad flores chamæmeli, quorum contritorum scrupulus dandus est loco drachmæ cinchonæ, et ad idem præscriptum repetendus. Hos flores, sic sumtos, semel atque iterum profecisse expertus sum."

Several mineral substitutes for the bark, or for arsenic, have been tried and found useful. Preparations of iron and of zinc. From five to ten grains of the sulphate of zinc have been given several times a day; or three grains of the oxyde of zinc every three hours. Sir G. Blane says that both in the West Indies and in London, intermittents have been cured by the use of this oxide, when they had previously resisted the bark. Sir James Mac Grigor speaks of it also in terms of praise; from what he saw of its effects in the agues of the Peninsula during the war.

Some of the remedies of this mysterious disorder operate upon the mind, or rather upon the nervous system through the mind. Hence it becomes probable that the *drugs* which have such power over the disease, act also on the nervous system through the body. And hence also we derive a confirmation of the opinion, that the disease itself is essentially a disease of the same nervous system. Ague has often been cured by the agency of strong mental emotion, such as sudden and great joy, anger, terror, or eager expectation. Thus we read that Quintus Fabius Maximus was cured of an old quartan on the day of a great battle. Strong impressions upon the imagination, producing feelings of disgust and horror, have had the same effect: such as those caused by drinking blood; swallowing a spider gently bruised, and wrapped up in a raisin, or spread upon bread and butter; keeping a spider suspended from the patient's neck in a nutshell, till it dies; and the like. The

undoubted success, in many cases, of charms, must be referred to the principle of *faith*. The patient recovers, because he firmly believes in your power to cure him. Dr. Gregory used to relate the case of a patient in the clinical wards in Edinburgh, who, with sundry ceremonies, swallowed some word, written on a slip of paper: the result was, that he had not another paroxysm. And I perfectly recollect having a great awe, when I was quite a child, of my maternal grandmother, because she was reputed to have the power of curing agues by means of some charm. I believe all that she did was to assure the poor people who came to be relieved from their ague, that they should have no more of it *after such a day;* and their implicit reliance upon this prophecy brought about its fulfilment. There seems to be this general principle observable in respect to agues, and to all other diseases which occur in paroxysms, viz. that after they have continued for some time, their farther continuance depends more upon the effect of *habit* than any thing else; and this habit may be broken by strong impressions made upon the nervous system; and the cure of one paroxysm is thus often the cure of the disease. We have seen examples of the existence of this morbid habit in hysteria, and in some cases of epilepsy. *Cæteris paribus*, that physician will be the most successful in these disorders, who is best able to acquire the confidence of his patient, and to gain a powerful influence over his mind.

There is no disease in which the prophylaxis is of more importance; but this you will have gathered from the facts which were stated in the two preceding lectures. The disposition to relapse is strongest soon after the disease has been removed; but it generally continues long, perhaps even for life. The late Dr. Macmichael caught an ague many years before his death, by sleeping on a rock somewhere in Greece; and he was ever after subject to occasional attacks of periodic headache, and other aguish symptoms, for which he was obliged to have recourse to bark or arsenic. Of course one essential point in the prophylaxis is the withdrawal of the patient from the influence of the exciting cause; taking him away from the malarious locality. But this cannot always be done; and when it cannot, we must impress upon him those cautions which arise out of the facts ascertained in regard to the operation of the malaria upon the human body. Persons who have been exposed to the exciting cause, or who have once had the fever, should, in whatever place they may happen to be, avoid over-fatigue and exhaustion of all kinds; sudden exposure to cold or heat; and the neglect of changing wet clothes, wet shoes and stockings for instance. In a malarious district, persons should bear in mind the facts, that the miasmata are much more virulent in the night-time than in the day; and close to the surface of the earth, than in a higher part of the atmosphere. They should refrain, therefore, from going out late in the evening, or early in the morning: and they should rather select the attic than any other floor for their bed-chamber. They who are obliged to go out in the morning in countries where agues are rife, should take care not to go out fasting; a good hot breakfast should be first taken, or at any rate some moderate stimulus; a crust of bread and a glass of wine, or a small quantity of ardent spirit, will fortify the system against the pestilential miasma. Measures of this kind have been found extremely beneficial in the navy: the giving, for instance, the men a warm breakfast before going out in the morning on malarious shores in boats, whatever the hour of starting might be. Generous diet, and a fair allowance of fermented liquor, are proper also for all persons in aguish countries. The late Dr. James Gregory used to mention in his lectures an anecdote in point, told him by his father. The elder Dr. Gregory studied at Leyden, under Boerhaave; and there were twenty-four other English students there at the same time: that is, they were called English, on account of their common language, but they were in fact composed of English, Irish, Scotch, West Indians, and Americans. The celebrated John Wilkes and Charles Townsend were among the number. These twenty-five students lived a good deal together: in truth they were cut, as the phrase is, by the Dutch, for some raffish behaviour on their parts. However, of the twenty-five, only one was a water-drinker. The other twenty-four drank each a bottle of claret daily: and the water-drinker, and he alone, fell ill of ague there.

Persons who have recently become residents in aguish districts, or who even happen to be travelling through them, would do well to take moderate doses of quina by way of safeguard. And in regulating the bowels, which, of course, is of much importance, warm

stomachic laxatives should be made use of, rather than cold aperients, such as the neutral salts.

There is just one more expedient which I would suggest as not unlikely to afford complete protection to those who are of necessity exposed to the malaria; and it is, that they should wear an *orinasal respirator*. It is possible that as a breeze is filtered of the poison which was mingled with it, by passing through a dense mass of foliage, so, on a smaller scale, the air inspired in breathing may be strained and purified, and rendered harmless, in its transit through the sieve-like structure of Mr. Jeffreys' ingenious instrument. The principle of the suggestion is not new; but this mode of applying it has not, so far as I know, hitherto been tried. It is said that by surrounding the head with a gauze veil, or conopeum, the action of malaria is prevented; and that thus it is possible even to sleep in the most pernicious parts of Italy without hazard of fever. Dr. Macculloch says that in Malta, and elsewhere, this belief is universal: and hence the popular practice of covering the mouth and nose with a handkerchief in the morning on going out, or in other suspicious circumstances: a practice (he observes) the efficacy of which is attested, as far as popular belief can attest any thing.

LECTURE XLIII.

EPISTAXIS. BRONCHOCELE: CRETINISM: THEIR PHENOMENA AND PROBABLE CAUSES. MEDICAL AND SURGICAL TREATMENT OF BRONCHOCELE.

I YESTERDAY finished what I had to say respecting intermittent fever: its symptoms; its cause; and its cure. The subtle poison which produces it is thickly distributed over the fairest portions of the habitable globe: blighting human health, and shortening human life, more often, and to a far greater numerical amount, than any other single cause whatever. Known only by its noxious effects, holding out no signal of its presence, this unseen and treacherous enemy of our race has yet been tracked to its haunts and lurking-places, and detected in some of its habits. It was necessary, therefore, that I should enter somewhat fully into the history of the malaria, and show how it may sometimes be shunned, sometimes be averted; how also, in this climate at least, the effects it has already produced upon the human body may be successfully combated. But I shall not pursue, in further detail, the ravages committed by this invisible agent, and the remedies they require, in hotter and less favoured regions than our own. Of these, personally, I know nothing; and I must refer you, for information on such diseases, to authors who have seen and treated them: particularly to Drs. Lind, Jackson, Bancroft, Johnson, and Sir William Burnett.

It is customary, with writers and lecturers, to pass from the consideration of ague to that of continued fever. A paroxysm of ague has been regarded as exhibiting a paradigm or sample of fever in general. But this has always appeared to me rather an ingenious refinement, than a useful matter of fact. Practically, I see nothing to be gained by the association. Intermittent fever, it is true, does not often run, in hot climates, into the remittent, and the remittent into the continued form. But these are very different disorders from the continued fever with which, in these climates, and in this country, we have to do. Intermittent fever, and continued fever, as *we* see them, differ in their phenomena, in their cause, and in their treatment. They are alike, insomuch as they both are called fever, and both are attended, in some part or other of their course, with *pyrexia;* but, in essential symptoms, I have known many a compound fracture more like continued fever than any ague that we are likely to witness. I shall take up the subject of continued fever, then, in connection with the eruptive febrile diseases, with which it has many strong links of analogy; and I resume the consideration of the disorders that come within the province of the physician, according to their anatomical seat.

Epistaxis.—After what was stated of hæmorrhage in general, in an earlier part of the course, I hardly know whether *epistaxis* needs or deserves any formal notice. There are, however, some points relating to this simple, and commonly harmless hæmorrhage, which it may be worth while very briefly to touch upon. Sometimes a remedy; sometimes a warning; sometimes really in itself a disease. The readiness with which the mucous lining of the nasal passages pours forth blood is familiar to the experience of every school-boy, who "often wipes a bloody nose." A slight blow, brisk exercise, a strong bodily effort, a fit of sneezing, or the summer heat, is sufficient, in many boys, to make the nose bleed; and this facility of hæmorrhage furnishes, often, an index of some unnatural state of the circulation; and especially of undue fulness of the vessels of the head. But the import of this symptom is not always the same. Epistaxis may indeed be taken as affording an epitome of the various forms of hæmorrhage by exhalation. In childhood and early youth it is idiopathic, dependent upon active congestion, and probably arterial. It is nature's favourite mode of blood-letting at that period of life. In old age it is symptomatic, the result of passive or mechanical congestion, and probably venous. In some adult persons it happens periodically, and is habitual: and its *suspension*, rather than its *occurrence*, becomes a token of disease or of danger. In young women it is not seldom vicarious of suspended menstruation: in men it is apt to take the place of hæmorrhöis. Lastly, it may proceed from disease in the nares themselves; or form a part of a more general hæmorrhagic disorder.

It is unnecessary to go at length into the phenomena of epistaxis. The main phenomenon becomes obvious at once both to the patient and to those around him: and the accessory and incidental circumstances are easily discoverable when the attention is aroused to them by the sight of the blood. Usually the blood flows *guttatim;* in a succession of drops: but these may follow each other so fast as to constitute a little stream. Sometimes a few drops only fall; sometimes several pints are lost. A moderate hæmorrhage of this kind is generally succeeded by a sense of relief and refreshment. A large efflux of blood may cause pallor, faintness, debility, exhaustion, even death.

Active idiopathic epistaxis, as it occurs in children, is almost always salutary, and may be left to work its own cure. When it runs into excess, or is too often repeated, it may be checked by applying cold water to the forehead and bridge of the nose. The sudden contact of some cold substance with a distant part of the surface of the body will often have the effect of restraining the hæmorrhage: apparently by producing a general and sympathetic constriction of the superficial blood-vessels. This is doubtless a reflex phenomenon. The nursery remedy is to slip a cold key down the child's neck, between its back and its clothes. The aspersion of cold water is still better. Besides these external appliances, cooling laxatives should be given: and if the bleeding proves obstinate, some astringent internal remedy may be thought proper. I have, myself, hitherto found none so efficacious as the acetate of lead. But I have been recently informed by Dr. Latham that his experience has led him to trust much to *mercury* in the management of epistaxis; and that the same indications have governed him, in adapting its use to this form of hæmorrhage, as serve to guide him in cases of inflammation. Thus, when the hæmorrhage has been profuse and frequent, and moderate depletion by blood-letting, purgatives, &c. has not arrested it, he has brought the constitution rapidly under the influence of mercury; and as soon as the mouth was sore, the hæmorrhage has ceased, not a drop more of blood has been lost. Again, when the epistaxis has been, not in large quantity, but habitual or frequently recurring, without any excess of vascular action, or any other apparent ailment in the constitution at large, Dr. Latham has often cured his patient by a moderate salivation, gradually induced, and continued for a few weeks.

In conversing with Dr. Southey on the same subject, I find that he also has been taught by experience to rely upon mercury as almost a specific remedy for obstinate hæmorrhage, occurring under similar conditions, from whatever organ of the body it may proceed.

This plan of treatment it is therefore my purpose to prove, as future opportunity may permit.

When epistaxis *begins* to show itself in advanced life, it is a symptom which cannot safely be neglected: for it indicates that the veins of the head are loaded. It implies a morbid condition that requires to be redressed. You will look for disease of the heart—

or for threatenings of apoplexy—and take your measures accordingly. The blood-vessels which ramify upon and beneath the pituitary membrane, communicate by indirect inosculation with the veins and sinuses of the skull, as well as with the jugular veins. You see, therefore, how it is that hæmorrhage from this membrane may perform the office of a safety-valve, and protect the important organ within the cranium from impending mischief.

On the other hand, when epistaxis, which is known to have been habitual, fails to recur at or about the usual periods, you will look, with a jealous care, into your patient's state, and watch for and obviate any tendency to plethora capitis.

When epistaxis forms a part of more general hæmorrhagic disease—as when it occurs among other symptoms of purpura—its treatment merges in that of the whole malady.

In any case, if the flow of blood be excessive, and cannot be restrained by the ordinary remedies, but is exhausting the patient's strength, it becomes an absolute disease: and it will be requisite to staunch the blood by manual expedients.

These consist in stopping the bleeding orifices mechanically; which is most effectually to be done by plugging the cavity. A dossil of lint must be carefully inserted into the bleeding nostril. Its mechanical effect, which is pressure, may be chemically aided by first wetting the lint with a saturated solution of alum. The mode of introducing these plugs it is the business of the surgeon—and not mine—to teach. The operation is not a very comfortable one either to bear or to perform.

Bronchocele.—Before we trace this mucous membrane downwards, through the mouth, to the inside of the throat, let me turn your attention to a singular disorder which may be deemed external, for it is scarcely more than skin deep: I mean that enlarged state of the thyreöid gland to which the name of *bronchocele* has been given. This word is not merely derived from the Greek, but was used by the Greek writers in the same sense in which we now employ it. In Switzerland, where it is very common, and in France, the complaint is called *goitre;* a corruption, it is believed, of the Latin "guttur," the throat. It is known in England as the *Derbyshire neck;* from its frequent occurrence in that county.

The term bronchocele has been sometimes applied indiscriminately to all protuberances or swellings in front of the throat; or, at any rate, to all enlargements of the thyreöid gland; whereas it should be restricted to *hypertrophy* of that part: an exaggeration of its natural structure, with augmentation of its volume. The texture of the gland becomes coarser; its blood-vessels grow larger and more numerous; its cells are magnified, and filled with a thick, viscid secretion. It usually presents a soft, smooth, elastic tumour, which is neither painful, nor tender, nor discoloured. The lobes of the gland become more obvious. Sometimes the whole tumour is irregularly lobulated: sometimes the exact form and relative proportions of the gland are preserved, each lobe and portion being equally increased in size. Occasionally there is a soft uniform or irregular swelling, without much distinction of parts. Alibert states that the right lobe is more frequently enlarged than the left. Mr. Rickwood found it so in every instance of bronchocele that came under his notice in the neighbourhood of Horsham.

Unless the tumour be very large, it follows all the motions of the larynx: and this is a point of considerable importance whenever the diagnosis is at all doubtful. It is just possible that an enlarged lymphatic gland, or an encysted tumour in the neighbourhood of the larynx, or even a collection of pus thereabouts, might, in some degree, embarrass the diagnosis. But swellings of this accidental kind may be ascertained, in general, if the head and neck be placed in different successive positions, to be unconnected with the larynx; and they do not follow its up and down movements when the act of deglutition is performed.

It is of importance to know, also, that the gland itself is subject to different kinds of enlargement. It may swell from inflammation, chronic or acute: and then it will be hard, and tender, and painful. But it does not seem very prone to inflame; and probably Dr. Copland is right in his opinion that inflammation occurs spontaneously in this organ in scrofulous persons only. Baillie and Alibert speak of it as being occasionally the seat of cancer; but that must be very rare. Sometimes cartilaginous or ossific deposits take place in the gland. It is necessary, I say, to be aware of these circumstances, and to distinguish

one kind of thyreöid tumour from another: for some of the morbid changes just referred to are clearly beyond the power of any *medicine* to remove; and if all forms of enlargement incidental to this part are lumped together under one common name of bronchocele, we shall be liable to arrive at false conclusions concerning the power of remedies over that disease.

Bronchocele is not, *in itself*, a painful disorder; nor does it taint the system, or affect the constitution in any way. It has no character of malignancy about it. It is always, however, a deformity; and, by its mechanical effects, that is, by its weight when large, and by the pressure it exercises on contiguous parts, it may occasion great distress, and suffering, and even death itself. The size, and the effects, of the tumour both vary much in different cases; but its *injurious* effects are not always, though they are generally, in proportion to its bulk. Sometimes there is no more than a slight fulness of the throat, which some persons, I believe, think rather graceful than otherwise. Now and then, the swelling, after its first commencement, developes itself with great rapidity; but its ordinary progress is slow. It often continues for months, or years, without reaching any extreme or very troublesome magnitude. Sometimes it remains stationary for a considerable time, and then *suddenly* increases, without any apparent cause. The worst effects of bronchocele are its interference with the circulation, and with respiration. By its pressure it may obstruct the free descent of the blood through the veins of the neck, and give rise to headache, giddiness, noise in the ears, confusion of thought, and a turgid condition of the head and face. Or, by pressing upon the windpipe, it may cause hoarseness, wheezing, and dyspnœa. It may even impede deglutition. But these effects, I say, do not depend altogether on the actual size of the tumour. A very large goitre may produce no other inconvenience than what results from its weight and its unseemly appearance. It may surround all the front and sides of the neck like a thick collar, and rise as high as the ears; or it may hang down, in a pendulous lump, and be supported upon the chest. Nay, the tumour is said to descend, in some rare instances, so low as to be in contact with the abdomen: and Alibert mentions one case in which the swelling was of a tapering cylindrical shape, and reached to the middle of the thigh. On the other hand, a small tumour, not bigger than one's fist, especially if it happens to occupy the central portion, or what is called the isthmus, of the gland, may so press inwards upon the trachea as materially to hinder the breathing, and even to threaten suffocation. A pupil now attending the hospital has informed me of a case, which he himself saw, of death produced by the encroachment of a bronchocele; not so much, however, from suffocation as from starvation: for the swelling encircled the trachea, and came at last to press so much upon the woman's œsophagus, that she could not get food into her stomach. I suppose that the reason of these differences may be sometimes found in the manner in which the tumour grows, and in its relative situations. When it is bound down by the muscles of the neck, it presses, as it continues to enlarge, upon the parts behind it. When it is not so confined, the skin readily yields, and the entire growth of the tumour takes place anteriorly.

This disease is much more common in women than in men. Indeed, we seldom see it, in this country, except in females. Yet I happen to have an example of it now (December, 1837) in a male among my patients in the hospital. Dr. Andrew Crawford states that forty-nine cases were admitted into the Hampshire County Hospital, in ten years, and forty-eight of these were in women. Of seventy patients treated in the Chichester Infirmary in nine years, two only were males, and they were boys of a feeble and feminine habit, and backward for their years. Among one hundred and sixteen patients of Dr. Manson, fifteen were men. Taking an average from these three lists, we have one male for twelve females. It is well to bear in mind that our fashion of dress renders a small bronchocele much more noticeable, much less easily concealed, in women than in men. In the former the swelling has been known to come on, or at any rate to increase rapidly, during their confinement in child-bed: and it is frequently observed to undergo a temporary enlargement at the menstrual period. Dr. Copland has seldom met with an instance in the female, unconnected with some kind of irregularity in the catamenial discharge, or disorder of the uterine functions; and he never saw a case in which the disease made its appearance before the period of commencing puberty. In Switzerland, however, and in some parts of India, where the complaint is much more prevalent than here, the proportion of males

affected is greater; and it begins, often, prior to the age of puberty, in both sexes. It seldom shows itself earlier than the age of eight or ten. Dr. Elliotson states, indeed, that he himself, when in Switzerland, saw goitre in a little boy only four years old; and the natives told him that it rarely made its appearance before the age of six. But children have been *born* goitrous. M. Godelle, physician to the hospital at Soissons, had a preparation of the body of an infant, which only lived a few hours, and which came into the world with a goitre; the mother being affected with the same diease. A case is mentioned in the *London Medical Repository* of a child born in Derbyshire with bronchocele of considerable size. The disease, therefore, undoubtedly may be *congenital:* and one of the facts I have just mentioned points to the question of its being *hereditary*. It is said to be so; and there is much probability in favour of that opinion. Children born of goitrous parents often have goitre. But that, you will say, may depend upon their being in the same place, and exposed to the same causes, which produced bronchocele in the mother or father. Dr. Crawford states, however, that he knew a woman, with goitre, whose grandmother, father, paternal aunt, and cousins, also had it; although they did *not* all live in the same place, and no other person in their respective neighbourhoods was affected by the disease.

However, if the disease be, as it probably is, sometimes hereditary, in the sense in which I formerly explained that term, there can be no doubt that it is often *acquired*.

In the first place, bronchocele is *endemic*—prevalent in certain localities, and scarcely occurring elsewhere. And persons who, being previously well, go to live in those localities, often become affected with the complaint: and persons who migrate *from* those localities, having the complaint upon them, sometimes get rid of it by there change of residence. The physical circumstances of the places thus selected by the disease have been studied with the natural hope of discovering what the cause may be of an effect so singular. Some morbific quality of the *air* was long suspected. The habits of the unknown cause of bronchocele appeared at first sight to be very much like those of the malaria. Goitre abounds in the hollows of many mountainous districts; among the Alps, for example, and in the Pyrenees. This was notorious to the ancients. Juvenal asks—

Quis tumidum guttur miratur in Alpibus

And it is in the deep, close, and humid valleys of Switzerland, which lie at the feet of, and between, high mountains, that bronchocele is most common. Several writers, who have personally investigated this subject in places where goitre is rife, concur in the belief that it depends upon insalubrity of the air, arising from the peculiarities of the situation. They affirm that it is most frequent in low, damp, confined situations, where the stagnant atmosphere is seldom stirred by wholesome breezes; and where the sun, in summer, has great power. Dr. James Johnson remarks, "We find in the Valais (one of the Swiss Cantons) and in the lower gorges or ravines that open on its sides, both cretinism and bronchocele in the most intense degrees. As we ascend the neighbouring mountains, cretinism disappears, and goitre only is observed. And when we get to a certain altitude, both maladies vanish." Dr. Reeve, again, states that "all the cretins he saw in adjoining houses in the little village called La Batia, situated in a narrow corner of the valley, the houses being built up under the ledges of the rocks, and all of them very filthy, very close, very hot and miserable habitations. In villages situated higher up the mountains, no cretins are to be seen."

Cretinism.—The *cretinism* mentioned in these quotations is a strange and melancholy disease: a sort of idiotcy, accompanied by (and doubtless dependent upon) deformity and imperfection of the bodily organs. The mental affection exists in all degrees, from mere obtuseness of thought and purpose, to the complete obliteration of intelligence. Many of the cretins are capable of articulate speech; some are blind, some deaf, and others labour under all these privations. They are mostly dwarfish in stature, with large heads, wide vacant features, and goggle eyes, short crooked limbs, flabby muscles, and tumid bellies. The worst of them are insensible to the decencies of nature, and obey, without shame or restraint, every animal impulse. In no other class of mortals is the impress of humanity so pitiably defaced.

More recent and extensive observation of the localities infested by goitre have rendered

it improbable that the disease derives its origin from any deleterious properties of the air. Certainly it is not owing to any thing that is common to *all* mountainous countries. Some parts of Switzerland are free from it. So are the Highlands of Scotland. It is met with also in flat situations—as in Norfolk. I have seen several cases of it in Cambridgeshire, which is a *very* flat country. In one village in particular, about five miles from Cambridge, it is extremely common. There are some striking facts collected by the celebrated and philosophic Humboldt, which go to show that the prevalence of bronchocele does not depend on any particular configuration of the surface of the earth, nor on any peculiar condition of the atmosphere. He tells us that in South America bronchocele is met with, both in the upper and in the lower course of the Magdalen river; and in the flat high country of Bogota, 6000 feet above the level of the river. The first of these regions is a thick forest; while the second and third present a soil destitute of vegetation. The first and third are exceedingly damp: the second peculiarly dry. In the first the air is stagnant; in the second and third the winds are impetuous. In the first two the thermometer keeps up all the year at 22 or 23 degrees of the centigrade scale: in the third it ranges between 4 degrees and 17.

The researches of Mr. M'Clelland, in India, lead to the same conclusion. He found goitre extremely frequent in one portion of the district which he surveyed, while the other portion was almost exempt from the complaint, "although an equality of moral as well as physical circumstances appeared to affect the whole. The external alpine characters of the province are the same in every part, the inhabitants all belong to the same tribes of Hindoos, and are subject to fewer irregularities in their mode of life than any other people in the world."

The different localities of the villages, in the portion where goitre was *not* prevalent, he describes as being as diverse as can well be imagined. "Some are erected on narrow ridges, others in deep valleys, surrounded by abrupt and lofty mountains; others on rugged declivities between lofty peaks on one side, and dark ravines on the other, into some of which the sun can scarcely penetrate. The different altitudes of these villages vary from 2000 to 6000 feet."

Facts of this kind have turned the attention of scientific inquirers towards the only other obvious source to which the disorder could, with probability, be attributed: viz. the quality of the *water* used for drinking. Wherever goitre prevails, the popular belief assigns it to the water, as a cause: and the more accurately the search is prosecuted, the more strength and likelihood does this supposition acquire. Its very universality is a presumption in its favour. The disease was formerly ascribed to the use of *snow* water: a notion which originated, I imagine, in its frequent occurrence in alpine regions. But the people in almost *all* the valleys of Switzerland drink the water that comes from the Glaciers; while bronchocele is known in *some* of the valleys only. It prevails also in certain spots where pump water is used, and *there* the people accuse the *pump* water of producing it. Besides, goitre occurs in other countries, where the snow never lies, as in Derbyshire; and even in Sumatra, where there is *no* snow. Dr. Bally, a native of a goitrous district in Switzerland, believes that bronchocele is caused by certain *waters*, which issue from the hollows of rocks, trickle along crevices of the mountains, or rise from the bowels of the earth. And in support of that opinion he refers to some fountains in his own neighbourhood, the drinking of the waters of which will produce, or augment goitrous swellings, in eight or ten days. Such of the inhabitants as avoid these waters are free, he says, from goitre or cretinism. In Captain Franklin's narrative of his expedition to the shores of the Polar sea, there is the following statement, made by his fellow traveller, Dr. Richardson:—"Bronchocele or goitre is a common disorder at Edmonton. I examined several of the inhabitants afflicted with it, and endeavoured to obtain every information on the subject from the most authentic sources. The following facts may be depended upon:—The disorder attacks those only who drink from the *water* of the (Saskatchanan) *river*. It is, indeed, in its worst state, confined almost entirely to the half-breed women and children who reside constantly at the fort, and make use of river water, drawn, in winter, through a hole made in the ice. The men, from being often from home on their journeys through the plain, where their drink is *melted snow*, are less affected: and if any of them exhibit during the winter some incipient

symptoms of the complaint, the annual summer voyage to the sea-coast generally effects a cure. The natives, who confine themselves to *snow water* in the winter, and drink of the small rivulets which flow through the plains in the summer, are exempt from attacks of this disease. A residence of a single year at Edmonton is sufficient to render a family bronchocelous. Many of the goitres acquire great size. Burnt sponge has been tried, and found to remove the disease; but an exposure to the same cause immediately reproduces it. A great proportion of the children of the women who have goitres are born idiots, with large heads, and the other distinguishing marks of cretins. I could not learn whether it was necessary that both parents should have goitres to produce cretin children." We are able even to go a step farther, and to announce a probable conjecture as to the specific quality of the suspected water. Bronchocele is very prevalent in Nottingham and its neighbourhood; and the vulgar there ascribe it (so Dr. Manson informs us) to the *hardness* of the water. You know that the rough practical distinction between soft and hard water is that the former *dissolves* soap, while the latter *decomposes* it. The hardness is generally occasioned by the presence either of *sulphate of lime* or of *carbonate of lime*. In the one case the remedy is to mix the carbonate of an alkali with the water; in the other you simply boil it. Now the well water in and about Nottingham is more or less hard, and unfit for the purpose of washing. Dr. Coindet, of Geneva, declares that the use of hard or pump water in the lower streets of that town brings on the goitre very speedily. At Cluses, on the Arve, numerous cretins and goitrous persons are seen in the streets: lofty cliffs of limestone tower over the town, and through its caverns copious streams of water find a passage. The soil in the neighbourhood of Edmonton was found by Dr. Richardson to be calcareous, and to contain numerous fragments of magnesian limestone. In a *Treatise on English Bronchocele*, very recently published, Dr. Inglis states his belief that the presence of magnesian limestone always implies the coexistence of the disease. "Take (he says) that ridge of magnesian limestone running from north to south through the centre of Yorkshire, and margining the shires of Derby and Nottingham. All along that line we have goitre to a very great extent; whereas, on our diverging to either side, the disease is found to diminish."

These scattered indications that the hurtful quality of the water is somehow derived from its contact with limestone rocks, receive a powerful corroboration from the result of Mr. M'Clelland's minute and valuable inquiries, which were carried on in the province of Kemaon, south of the Himalayan mountains. I have not been able to obtain his book; what I am about to tell you I take from a full and instructive notice of it in the fifteenth number of the *British and Foreign Medical Review*. Mr. M'Clelland finding goitre very abundant (as I mentioned before) in one great section of a district, and almost entirely absent from another section, set himself to find out in what other particulars these sections were distinguished from each other. And he ascertained that they agreed perfectly in external aspect, altitude, and climatology, but differed remarkably in their geognostic relations: "and this distinction was even traced down to the very villages in which the disease is found, with such perfect nicety, as to enable one almost to predict *à priori*, on examining the rocks of a neighbourhood, whether the inhabitants are affected with goitre or not."

It would be impossible for me to give you even an abstract of Mr. M'Clelland's numerous observations; but I select one or two striking instances in favour of his opinion that the endemic prevalence of goitre is connected with the use of water impregnated with calcareous salts.

One extremity of the long village Deota, which occupies half a mile of the foot of Durge mountain, is inhabited by Brahmins; the other by Rajpoots and Domes. Of the first caste there are about twenty persons, all of whom are free from goitre. There are forty of the second, and two-thirds are affected, more or less. Of the third caste, forty-six in number, nearly the whole are goitrous. "To what cause can we ascribe the immunity of one caste of the inhabitants of this village, and the almost universal affection of the other two castes? They are all alike well fed, and have little toil; their land producing the requisites of life almost without labour. Difference of caste does not here imply a difference of pecuniary circumstances, and consequently of the comforts of life. In these respects the three castes in this village are on perfect equality. Nor will here-

ditary predisposition acquired by intermarriages be sufficient to explain the interesting fact: for the affected parties are confined to the Rajpoots and Domes, who cannot intermarry, while the Brahmins and Rajpoots may. The village is raised about 100 feet above the level of the valley; and the mountain at the foot of which it is situated, rises with a gentle slope, and is not, in this vicinity, at all rugged. It is chiefly composed of transition limestone, and the village is erected on a conglomerated rock, composed of calcareous tuff, inclosing fragments of other rocks. There is a spring in the valley, about 100 yards from the village, bearing on its first appearance the character of a mineral spring. The water bursts forth with strong ebullition, in the quantity of at least forty gallons in a minute, and agglutinates the sand and gravel by which it is surrounded, by the deposition of calcareous tuff. The temperature and quality of the water is the same at all seasons. The former inhabitants of this village, aware perhaps of the noxious effects of the spring, had an aqueduct formed, by which water is conveyed into the Brahmin portion of the village from a distant source. The aqueduct having been suffered to get out of repair, the quantity of water it transmits is reserved exclusively for the Brahmins; except during the rainy season, when, the water being plentiful, the Rajpoots also use that of the aqueduct; but the Domes have no alternative at any season but to use the water from the spring."

The valley of Baribice is elevated 4000 feet above the sea. Its eastern extremity is composed of *clayslate*, and in five villages, containing 152 inhabitants, there is not one goitre. The other extremity of the valley is partly composed of limestone; and of 192 inhabitants, distributed in six villages, 70 are affected with goitre: but Ducygong, one of these villages, supplied with water from clayslate, has not a single case of the disease; while Ager, only half a mile distant, and containing 50 inhabitants, has no less than 40; and of that number 20 are cretins. They use the water that issues from an old copper mine in limestone, and which contains carbonate of lime, and of soda, but no sulphate.

Mr. M'Clelland affirms that in the course of his personal inquiries, which extended over 1000 square miles, and which were prosecuted without regard to any theory, no instance occurred in which goitre prevailed to any extent where the villages were not situated on or close to limestone rocks.

Cretinism has a close, but an ill-understood, connection with goitre. Wherever cretinism is endemic, bronchocele never fails to be abundant. But bronchocele may prevail in a place where there are no cretins. With but few exceptions, cretins are goitrous; whereas many of those who have bronchocele are not affected with cretinism. The two disorders either spring from the same cause, requiring for their joint production that this cause should be in active operation; or, if they have separate causes, these frequently co-exist and act in combination. It is said, I know not with what accuracy, that when both parents are goitrous for two generations in succession, the offspring, being in the third generation, are sure to be cretins. Certainly cretinism is most common where bronchocele is most common, and especially in mountainous places. It occurs in the Pyrenees as well as in the Alps, in the mountains of Syria, in the hilly parts of China, and in the Himalayan regions. Yet cretinism is confined within much more limited bounds than goitre. Saussure, Foderé, and Dr. Reeve, agree entirely as to circumstances under which cretinism appears to be most commonly engendered in Switzerland. They say that the disease is usually met with in valleys which are nearly surrounded by high and steep rocks, where there is but little circulation of air, and where the inhabitants are exposed to the direct rays of the sun, and to the reflection of them from the rocks: and also to effluvia from marshes. It is in the filthy habitations built in these close, hot, and humid situations, that cretinism abounds most. The children that are taken away from the low valleys, and carried up, when young, into the high grounds, escape the disease; or even get the better of it if removed soon enough. And the amendment is said to be perceptible even in a very few days. These facts have led many persons to conclude that cretinism, if not bronchocele, depends on some condition of the *air*. It appears to me probable that the exciting cause of both is the same, and that the local circumstances just now mentioned operate as predisposing causes only. Cretinism, as well as goitre, was observed by Ramond, in the "open, well-watered, and well-ventilated valleys of the Pyrenees."

There are some difficulties opposed to the implicit reception of the opinions formed by

Mr. M'Clelland and by others, respecting the origin of these diseases. And the facts upon which those opinions are grounded are not without apparent exceptions. Moreover, the actual substance which exercises or confers the noxious power has yet to be ascertained. This etiological problem, so full of interest, is not solved. One step more, and probably one step only, remains to be taken. We look to the medical geologist for its complete solution; and I trust that, now, we have not long to look. The deleterious agent has been traced, with tolerable certainty, to water: and thence to some element of the soil washed by that water. And if what at present is probable only, shall hereafter be proved —namely, that the hidden cause of goitre and of cretinism lurks in some chemical quality of man's natural beverage—it can scarcely be doubted that chemistry will be found ready to supply a simple and effectual corrective of the evil. This hope it is which makes it so important that medical men should be accurately possessed of the present state and bearings of the question; and prepared to take advantage of every opportunity that may arise for its practical determination. For surely it would be a noble achievement of our art, and a signal blessing provided for hundreds of human beings yet unborn, thus to prevent the deformity, the discomfort, and the sometimes danger, of bronchocele; and to forbid, in its very source and fountain, the more hideous and loathsome disfigurement, of mind as well as of body, that distinguishes the wretched cretin.

I ought, perhaps, to tell you, that other causes, many of them very vague and unsatisfactory, have, at different times been assigned. Thus Valentin supposes the disease to be more common in women than in men, simply because women more frequently have the neck uncovered. It has been affirmed that young women who have taken the veil in catholic countries have lost their goitres in consequence of the change then made in their costume: and a medical man in Guatimala asserts that the same infirmity has sensibly diminished among the men in that part of the world, since cravats became fashionable there. These views of the matter accord with some whimsical notions entertained by the late Sir Anthony Carlisle. He held that the chief purpose of the thyreöid gland was to protect the delicate organ of the voice from the injurious influence of cold; and he consequently regarded the tumour of bronchocele as being rather of a sanative than of a morbid nature. He looked upon it as an additional great coat thrown over the important instrument of speech, in circumstances of extraordinary need. His theory agrees with some of the facts on which other theories, not perhaps more plausible, have been founded. Thus although snow water may not be, indeed I may say certainly is not, the cause of bronchocele from any peculiar quality that belongs to it, yet the disorder is confessedly frequent in many places where snow water is habitually drunk; *i. e.* as Sir Anthony would have reasoned, where *very cold* water is frequently drunk; the swelling being a provision of nature to obviate the hurtful effects which might else be produced on the larnyx by these cold potations. The summer change for the better, described by Dr. Richardson, consists also with the same theory; which would suggest the covering an incipient bronchocele continually with warm clothing, such as flannel, to check its growth, to obviate its necessity, and so gradually to occasion its subsidence. And this plan is mentioned, I see, by Bouillaud, among the curative indications.

I mention these theories, not because I have any faith in them myself, but as being curious specimens of the manner in which the human mind strives to account for obscure phenomena; and as showing how readily facts may be culled and pressed into the service of very slender and infirm hypotheses.

Treatment of Bronchocele.—One very important point in the treatment is obvious; the removal of the patient, when that can be done, from the infested locality. When this has been effected, or when the disorder occurs sporadically, as it sometimes does, we may administer drugs with better hope and advantage. Now a great variety of empirical remedies have been recommended for the cure of bronchocele, concerning most of which it would be a waste of time for me to speak at all. The remedy which, as an internal medicine, has of late years superseded all others, and acquired the reputation of a specific against goitre, is iodine: and it certainly has a remarkable influence over the genuine unmixed form of the disease, whether endemic or sporadic—the hypertrophied gland; yet it does not, perhaps, merit the title of specific in the same absolute sense as that in which we say that bark is a specific for the ague, or sulphur for the itch. Dr. Coindet, of Ge-

neva, was the first who made this remedy extensively known. Dr. Straub, however, of Hofwyl, has laid claim to the priority of its use. Probably that happened in this matter, which has often happened in others, viz. that the state of knowledge was ripe for such a discovery, and it was made by each of these physicians independently of the other. Dr. Coindet's name, however, has been inseparably connected with the application of iodine to the cure of bronchocele, in a work which, if he had no other claim to be remembered, would immortalize his memory;—I mean Sir J. Herschel's masterly and beautiful *Discourse on the Study of Natural Philosophy;*—a book which I should advise those among you who have not already read it, to make themselves master of as soon as they have leisure to do so. He thus strikingly illustrates a remark of his own that mighty benefits often accrue to science from the casual observation of even unscientific or illiterate persons. "A soap manufacturer remarks that the residuum of his ley, when exhausted of the alkali for which he employs it, produces a corrosion of his copper boiler, for which he cannot account. He puts it into the hands of a scientific chemist for analysis; and the result is the discovery of one of the most singular and important chemical elements—iodine. The properties of this being studied, are found to occur most appositely in illustration and support of a variety of new, curious, and instructive views then gaining ground in chemistry; and thus exercise a marked influence over the whole body of that science. Curiosity is excited; the origin of the new substance is traced to the sea-plants from whose ashes the principal ingredient of soap is obtained; and ultimately to the sea-water itself. It is thence hunted through nature, discovered in salt-mines and springs, and pursued into all bodies which have a marine origin; among the rest into sponge. A medical practitioner, Dr. Coindet, of Geneva, then calls to mind a reputed remedy for the cure of one of the most grievous and unsightly disorders to which the human species is subject—the goitre—which infests the inhabitants of mountainous districts to an extent that, in this favoured land, we have happily no experience of, and which was said to have been originally cured by the ashes of burnt sponge. Led by this indication, he tries the effect of iodine on that complaint, and the result establishes the extraordinary fact that this singular substance, taken as a medicine, acts with the utmost promptitude and energy on goitre, dissipating the largest and most inveterate in a short time, and acting (of course, like all medicines, even the most approved, with occasional failures) as a specific, or natural antagonist, against that odious deformity."

It is curious enough, and marks the accuracy with which the effects of remedies may be observed, that not only had burnt sponge been found efficacious in removing bronchocele, but the bladderwrack also, the fucus vesiculosus, the plant that, with others of the same family, yields the soda, with which the iodine was found combined. Dr. Gairdner, of Bolton Street, who was the first I believe in this country to write upon the effects of iodine, accounts for the frequent failure of even large doses of the burnt sponge, by showing that it was much adulterated with charcoal. Dr. Manson, of Nottingham, has published the following tabular statement of the result of the employment of iodine by himself. He had treated 116 patients, of whom 15 were men, and the rest women. Of the 15 men, 10 were cured, 3 were improving and under treatment at the time of his publication, 1 was dismissed for irregular attendance, and 1 was much relieved. Of the 101 women, 66 were cured, 9 much relieved, 2 received no benefit, 10 were discharged for irregular attendance, and 14 were improving under treatment. Of the whole 116, therefore, there were 76 positive cures, or two-thirds of the entire number; and only two positive failures. Dr. Coindet was succesful in about the same proportion of cases. This is strong evidence of the power of the remedy. Dr. Manson found that in some, but not in all individuals, after the preparations of iodine had been given internally for a certain time, they were apt to occasion headache, giddiness, sickness of stomach, with some degree of languor, and inaptitude for exertion. His plan in such cases was to suspend the use of the medicine, or to reduce the dose. The following effects of the iodine are stated by Dr. Coindet as having occurred in his practice:—Acceleration of the pulse, palpitation, dry cough, watchfulness, marasmus, and prostration of strength; sometimes swelling of the legs, tremors, painful hardness of the bronchocele, diminution of the breasts, or a remarkable increase of appetite supervened; and he adds that in almost all the instances which he had observed, a very rapid diminution, or a disappearance more or less complete, took place, during these symp-

toms, of even hard, bulky, and old bronchoceles. His doses varied from somewhat less than a grain to somewhat more than a grain and a half. This was certainly, as Dr. Manson has suggested, too large a quantity of this powerful drug for the generality of patients. The management of the remedy is now better understood. I have never seen any of the unpleasant consequences enumerated by Dr. Coindet. These practitioners gave the iodine in the form of a tincture. But this is a bad mode of exhibiting it: for the tincture is decomposed in any aqueous menstruum, and the iodine thrown down to the bottom. Under such circumstances the patient may at one time get no iodine at all, and at another time a dangerous dose: for you are aware that iodine is an active irritant poison. But if you mix iodine with iodide of potassium, it is then *held* in solution; and this is the form in which it is now commonly given. In the last edition of the Pharmacopœia there is a *Liquor Potassii Iodidi compositus*, made by dissolving five grains of iodine, and ten of the iodide of potassium, in a pint of distilled water. This will doubtless, in future, be much prescribed. It is a dilute preparation. There is one grain of iodine in four ounces of the liquor. An ounce would, therefore, be a very *safe* dose. But you may begin with a couple of drachms, and increase the quantity, if need be, gradually: for not only Dr. Manson, but Dr. Copland also, who states that the remedy has been very successful in his hands, advocates *small*, unirritating, what are sometimes oddly called *alterative* doses. Simple friction is said to have had the effect of diminishing the tumour; and friction with some ointment or liniment containing iodine should be employed whenever the internal exhibition of the remedy is contraindicated; or *in addition* to its internal use. There is an authorized form for that purpose also in the New Pharmacopœia—the *Unguentum Iodinii Compositum*. A small portion of this may be rubbed on the tumour night and morning. I need not add that besides great care in watching for the specific ill effects of the iodine upon the system, no less care is to be taken in correcting the state of any other function which may be faulty, and in improving the general health.

Such is the best *medical* treatment of bronchocele: and such is the plan which you will always do well to make cautious trial of in the first instance: and with respect to surgical treatment, I may observe that so long as the disease is merely a deformity, so long as it does not interfere with any of the important functions of the body, nor produce serious discomfort—does not distress the respiration by pressing upon the trachea, nor impede deglutition by pressing upon the œsophagus, nor derange to any great degree the circulation through the head by pressing upon the great blood-vessels of the neck, nor grievously encumber the patient by its weight—we should not be justified (in my opinion) in performing or advising any surgical operation for the removal or the diminution of the tumour. Of such operations the three principal are, extirpation of the whole gland; the passing a seton through the tumour, and so exciting suppuration in it, by which its substance is broken down and destroyed; and tying the arteries which supply it with blood. The first of these methods, extirpation, has been performed when the wen was small; but few patients under such circumstances would consent to the operation; and few surgeons would now-a-days, I presume, advise or undertake it: and in cases where it might seem more expedient, that is, where the swelling is large, and suffocation is threatened by its pressure on the trachea, the execution of this measure would be exceedingly difficult and dangerous; for the arteries are so much dilated in these cases that perilous hæmorrhage might be expected from their division, especially when their extreme vicinity to the carotids is considered. Such large goitres come to involve also, by their lateral extension, very important nerves there situated. In one case where excision was attempted, the hæmorrhage was so alarming that the surgeon was obliged to desist in the middle of his task: and the patient actually died of hæmorrhage a few days afterwards. I believe that this operation may be considered as wholly abandoned by surgeons in the present day.

The introduction of a seton into the tumour has been more successful. This practice was revived somewhat more than twenty years ago by Dr. Quadri, of Naples; who supposed, indeed, that he had been the first to devise it. You will find an account of his mode of proceeding in the tenth volume of the Medico-Chirurgical Transactions, by Dr Somerville. The seton is passed through the substance of the gland, and retained there a considerable time: the chief caution necessary is to avoid the enlarged blood-vessels. Dr. Quadri affirms that the larger trunks of the thyreoïd arteries will not be endangered unless

the seton needle be passed so deeply as almost so touch the thyreöid cartilage; and that hæmorrhage from any of the branches of those arteries that may be wounded when the seton is inserted more superficially, will not be attended with hazard. This plan was followed in one case by ulceration and sloughing, and the patient died. An example of the success of this treatment occurred in the practice of Mr. James, of Exeter; the tumour was almost entirely removed, and the patient was for some time in London for the purpose of showing himself to the medical men here. But he also had passed through a good deal of suffering and peril. The expedient of tying one or more of the thyreöideal arteries, and so *starving* the tumour, has been attended with varied success. It has been done on the continent; and several times in this country: there is a case of it by Mr. Coates, of Salisbury, in the same volume of the Medico-Chirurgical Transactions. His patient was much relieved for a time, and supposed herself cured. But the tumour gradually returned, and caused her death by suffocation. Sir B. Brodie has also performed a similar operation; and I have myself seen one very satisfactory instance of the same thing, the operation having been done by Mr. Earle in St. Bartholomew's Hospital. The patient was a young woman with an immense bronchocele, which for some time previously had obstructed her breathing, and of which the effect on the trachea seemed to be every day increasing. There was loud wheezing, and great difficulty of respiration, and cough, and extreme emaciation: and it was plain that the girl must soon die suffocated if nothing were done for her relief. One of the arteries, I think the superior thyreöideal, was tied, without much difficulty, on one side. After the operation the tumour on that side shrunk considerably; the distress of breathing was removed; and the patient presently recovered so much of her former strength, and flesh and comfort, that she was unwilling to have any thing farther done, and left the hospital. After some time, however, she came back again of her own accord, and requested that the artery on the other side might be tied also. This was accordingly done, and a farther reduction of the tumour took place. The shrinking, however, if I remember rightly, was not so striking or complete after the second operation as after the first: but the patient was certainly rescued, upon the whole, from imminent death, and put into a condition of ease and at least temporary safety; the tumour that remained constituting merely a deformity. I never heard of her afterwards. In a case very similar to this, related by Mr. Crawford in the *Cyclopedia of Practical Medicine*, Mr. Wickham, of Winchester, tied one of the arteries, with much immediate, but no ultimate benefit. The goitre gradually diminished for about six weeks, and then (in consequence, I presume, of the establishment of a collateral arterial circulation) it as gradually enlarged again till it was as big as ever.

Upon the whole, the *average* results of none of these surgical expedients have been sufficiently good to warrant their repetition, except in cases where life is put in jeopardy or made miserable by the swelling: and where other methods, and particularly the treatment by iodine, have been tried, and have failed. One exception, perhaps, I should here make. The tumour sometimes evidently contains a collection of fluid, either in one of its enlarged cells, or in a distinct cyst. Now the cell, or cyst, may in such cases be punctured, and the contained fluid let out, without much risk. This was done in one instance by my colleague, Mr. Arnott. He kept the orifice open; and the cyst shrunk, and was at last obliterated, and the woman was much gratified by this diminution of her load.

LECTURE XLIV.

Cynanche Parotidæa. Spontaneous Salivation. Aphthæ. Cynanche Tonsillaris.

The Greek writers on medicine applied the terms συναγχη and κυναγχη to inflammatory affections occurring about the *throat*, and more or less interfering with the functions of

respiration and deglutition; and the Latins employed the word *angina* in nearly the same sense. And Cullen, in his Nosology, has made a *genus* of *Cynanche*, although the diseases which he has included in that genus have but little connection, except in so far as that the parts they occupy lie *near* to each other. Some of them indeed have their seat in different, though almost contiguous, portions of the *same membrane;* and are apt, sometimes, on that account, to pass one into the other. In general they are allied rather by proximity of situation, than by community of symptoms.

I mention these things, because there being a great disposition in the present day to *rename* diseases, and to affect a more precise and scientific nomenclature than sufficed for Cullen, if I adopted the more modern appellations without adverting to the old ones, which have been current so many years, you might experience some difficulty in your reading, in determining what disease was intended, when it was merely *named.* For my own part, I think there is much inconvenience in altering the established nomenclature; and especially in changing such arbitrary terms as, though they may not be *scientific*, are yet *definite*, and convey no erroneous notions respecting the nature of the disorder. I shall give you, however, in most cases, both the old and the new denominations.

Cynanche parotidæa.—Now one of tho disorders in Dr. Cullen's genus Cynanche—though not the first in the order he follows—is *cynanche parotidæa.* It is an inflammatory affection of the salivary glands, and of the parotid gland in particular. Accordingly it is called *parotitis* now-a-days. It is not, however, mere inflammation of the parotid, arising from any cause whatever; and therefore parotitis, unless some epithet be added, is less exact than cynanche parotidæa. The vulgar have given it just as good an arbitrary name as the learned; and they call it, in this country, the *mumps;* and the Scotch call it, I believe, the *branks.*

This disorder need not detain us long. The parotid swells; tumefaction takes place beneath the ear; and if the submaxillary and sublingual glands are not implicated in the outset, they soon participate in the tumefaction, in most instances; so that the swelling extends from beneath the ear along the neck, towards the chin, and the swelled parts are hot and painful, and tender on pressure. The aspect of the patient becomes curiously deformed. Sometimes one side only is affected; sometimes both sides at once; but most commonly of all, first one side and then the other. These local symptoms are attended with slight fever. But the only function that is materially affected is the motion of the lower jaw, which is impeded by the swelling. The inflammatory condition almost always terminates after a few days, in resolution, under the use of the antiphlogistic regimen, and the application of external warmth. The disease reaches its height in about four days, and then begins to decline; and its whole duration may be stated, on an average, at eight or ten days.

This complaint often prevails epidemically: when it affects one person in a family, or school, it usually affects several others, simultaneously or in succession. It chiefly attacks children and young persons. There can be no doubt that it spreads by contagion; and it seldom happens that the same person is twice affected by the mumps. These are remarkable circumstances, and give the malady a peculiar and specific character. I do not dwell upon them now, because they belong also to a very interesting *group* of diseases, which will require to be particularly considered hereafter.

Another curious circumstance connected with the disease, and one which has some bearing upon its treatment, is that, in many cases, upon the subsidence of the swelling of the neck and throat, and particularly when it subsides *quickly*, the *testicles*, in the male sex, become swollen and tender, and the *mammæ* in the female. It is said, but I do not know whether the observation be constantly true, that the testicle, or the breast, of *the same side* with the inflamed parotid, suffers. Sometimes the testicle wastes away after the swelling recedes; a circumstance which is known occasionally to happen when inflammation of that part arises from other causes. This, however, is not usual. In general the inflammation subsides and ceases in the one gland as it does in the other; the swelling is neither very painful nor long continued. But sometimes a more serious transference takes place, from the testicle to the brain: this I have never witnessed; but then, to say the truth, I have not often been called upon to treat the mumps, and my personal experience of it is limited. I find it stated that the metastasis to the testicle is considered as

rather a fortunate circumstance, because it serves as a sort of protection against metastasis to the brain; but I suspect this to be a mistake. Inflammation of the brain, or of its membranes, has sometimes occurred on the disappearance of the *parotid* swelling; but it has much oftener supervened, I believe, upon the retrocession of the inflammation in the *testicle* or *mamma*. It is said also that the inflammation sometimes returns from the testicle to the parotid, and back again; oscillating thus two or three times between the two glands. Fortunately, the metastasis to the brain is much more rare than that to the testicle.

The treatment of the mumps is simple. It consists in the observance of the antiphlogistic regimen; mild diaphoretics; laxative medicines if the head aches, or the bowels are confined; and warm fomentations, or dry warm flannel, to the neck and throat. The tendency observed in this complaint to a change of place—to metastasis to more important organs—forbids us from using very active measures to *check* or *subdue* the inflammation. Nor are such measures necessary. We are not to bleed, nor violently to purge such patients, nor to apply cold to reduce the swelling. Luckily, hot applications are not only the most safe and proper, but the most grateful also to the feelings of the patient. If suppuration should ensue—which is unusual and unlikely, but which sometimes does occur from extension of the inflammation to the neighbouring cellular tissue—poultices must be substituted for the fomentation. Warm applications, and rest in the horizontal posture, are to be recommended when the inflammation leaves the salivary glands, and attacks the testicles; or if the patient will not, or cannot, lie up, the testicle must be supported by a suspensory bandage—a bag truss. If the inflammation of the testicle or mamma be very violent, we must apply leeches, and afterwards poultices; but this will not often be required, or advisable. Finally, if the inflammation should fly to the brain, we must lay aside our previous caution, and treat the disease in that active manner which the inflammation of so important a part of the body demands. No *worse* metastasis *can* occur on the cessation of the phrenitis. I have fully spoken heretofore of the treatment to be pursued in that disease, and I have nothing to add respecting it now, except that it may be right, as an auxiliary expedient, to try to reproduce the inflammation in the parotid, or testicle, or mamma, by irritating applications—mustard poultices, for example—in the hope of thus producing what is called *revulsion*, and of diverting the disease from the brain to the part which it previously occupied.

Mercurial Parotitis.—You know that there is another specific form of *parotitis*, which is apt to be induced by mercury. Of this I have already spoken. When it is severe, it may be treated by leeches, without any dread of such metastasis as occurs in the mumps. It is usually, though not always, accompanied by a profuse discharge of the secretion proper to the glands affected; and it is attended also by sponginess and swelling of the gums.

I presume that when inflammation of these salivary glands is *not* attended with ptyalism, the parenchyma of the gland, or the cellular tissue which enters into its composition, is principally affected; and that when there *is* much salivation, the membrane lining the secretory and excretory ducts are implicated. We see the same distinctions in other analogous organs.

Spontaneous salivation.—Profuse ptyalism sometimes occurs without any obvious cause; and is then said to be *idiopathic*: and this is a circumstance which it concerns you to be aware of, both as practitioners, and as medical jurists. The same *tenderness and swelling* of the salivary glands, the same copious *secretion and excretion of saliva*, nay, even the *same fetor*, or a smell which can scarcely be distinguished from it—the same collection of symptoms which is familiar to you as indicating the specific action of mercury upon the human system, will arise sometimes (but very rarely) when not a particle of mercury has been administered. Several other substances are well known to have the occasional effect of producing an increased, and even a profuse flow of saliva: preparations, for example, of gold, of copper, of antimony, and of arsenic. The castor oil is said to have sometimes the same consequence. Digitalis certainly has; and the iodide of potassium; and sometimes, I believe, opium. Now and then ptyalism is met with as a symptom, among others, of pregnancy. Occasionally it results from some local irritation within the mouth: from a decayed or misplaced tooth. But what I principally wish to call your attention to is the fact that *salivation* may occur as an *idiopathic complaint*. In the twenty-sixth volume of the *London Medical and Physical Journal*, there is an instance

of it described by Mr. Davies, in which two or three pints of saliva were discharged daily for some time. This flux at length ceased under the use of laxative medicines. In the second volume of the *Transactions of the College of Physicians* is an extraordinary example of the same thing, related by Mr. Power. A young lady, of sixteen, spat from sixteen to forty ounces of saliva daily for upwards of two years. Mr. Power believed that the ptyalism in this case was originally excited by wool, which he found, in a fœtid state, in her ears. In the *Revue Médicale* there is an account given of a patient who was cured of a spontaneous ptyalism after spitting nine pints daily for nine years and a half. You may see another instance as related by Dr. Prout in the old series of the *Annals of Philosophy*. Dr. Pereira states that he has seen a dozen such cases; and he describes one which was fatal, not from the ptyalism however, but from sloughing of the cheek: and this is no uncommon circumstance. In certain cases of idiopathic inflammation and ulceration of the gums or cheeks, from some constitutional unsoundness, there may be extensive sloughing, ptyalism, and a very offensive odour, much resemblimg that which mercury produces. I have met with one example only of well-marked spontaneous ptyalism; and some of its circumstances were so peculiar, that they may be worth relating. I was taken out to Bayswater, by a medical friend, in the beginning of the year 1833, to see a little girl, ten years old, who had been in a state of salivation from the fifth of November in the preceding year. Up to that time she had been a healthy lively child, with nothing very remarkable about her, except that she was habitually subject to profuse perspirations, which had a very acid smell: so that the washerwoman was always aware which were her clothes, when she came to wash them, by this *smell*. She then suddenly became indisposed, had a little headache, and began to spit a good deal. This was noticed by her mother, and pointed out to her medical attendant, before *any* medicine was given her; and mercury, on that account, was religiously withheld. But in spite of all treatment the ptyalism went on increasing. When I saw her she was spitting three pints in twelve hours; transparent, rather dark-coloured, and with a small quantity of foam on its surface. There was nothing amiss with her teeth, or her gums, and no fetor of the breath. She was greatly emaciated, and resembled, in some respects, a person worn down by diabetes. From the very commencement of the spitting, the acid perspiration had ceased, and even the vapour bath failed to make her sweat. A great variety of remedies were tried, under Dr. Nevinson's superintendence, but without the least good effect. At last came the visitation of the influenza, in April of that year. The girl became severely affected by that disorder: and thereupon the salivation disappeared, and has not returned. I heard to-day that she was in excellent health.

Should you meet with cases of the same kind, you will search for some cause of irritation in the neighbourhood of the salivary glands, and especially in the state of the teeth and gums; and finding none, you will seek farther for the cause of the salivation in some deviation from the natural condition of one or other of the principal functions of the system: and you will regulate your treatment accordingly. I do not know of any specific plan of cure to be recommended: but it is certainly of importance that you should be acquainted with the fact, that ptyalism sometimes exists as a separate and independent malady. Astringent washes are found, sometimes, of service; a solution of alum, or the infusion of catechu.

Aphthæ.—Before I proceed to the diseases pertaining to the interior of the fauces and throat, let me take this opportunity of saying a few words in respect to *aphthæ*. They form the characteristic symptom of an especial disease of infancy, and they are apt to occur in the course of other diseases in adult age; and they are then of some importance as guides in forming our prognosis, and even in determining our treatment.

Aphthæ consist in small, irregular, but usually roundish white specks, or patches, scattered over the surface of the tongue, and the lining membrane of the cavity of the mouth and fauces; the angles of the lips, the cheeks, the palate, the pendulous velum, the tonsils, the pharynx. They look like little drops of tallow, or morsels of curd, sprinkled over those parts; they project a little above the surrounding surface; and, in fact, they are mostly formed by elevated portions of the mucous epidermis, covering a small quantity of a serous or gelatinous fluid, which separates the epidermis from the subjacent corium.

These portions of the epidermis detach themselves, and fall off; leaving behind them a reddish raw-looking surface, or sometimes a foul and ash-coloured spot: and successive crops of these aphthæ are apt to be formed.

Now children in arms who exhibit these aphthæ, are said to have the *thrush*. This occurs at an early age; seldom, or never, I believe, after the period of lactation is over. The spots occasion some inconvenience in themselves—the mouth is rendered hot and tender by them. The child may be eager enough to take the breast, but is observed to do so with pain and wailing whenever the mouth is applied to the nipple, and attempts to suck or to swallow are made.

But these aphthæ, thus occurring in infants, are attended with other symptoms of disorder; such as drowsiness, sickness, diarrhœa, and some feverishness. And, I believe, a general notion prevails, that the same aphthous condition which is visible in the tongue and mouth, pervades, in such cases, the whole of the alimentary canal. But this must be a mistake. That *some* morbid condition exists throughout that tract is highly probable, but true aphthæ can only form on those mucous surfaces which are provided with a continuous *epidermis*. This erroneous notion has been strengthened, perhaps, by the observation of aphthous spots on the pharynx and œsophagus. The complaint sometimes appears to be the result of improper diet, in children brought up by hand; or of milk of a bad quality, from an unhealthy or intemperate nurse. It generally lasts eight or ten days. It is not attended with much danger, except in certain cases, when the surface is left brown or bluish after the loosening and separation of the crusts. In such cases, the local affection is apt to run into a bad kind of gangrenous ulceration, and the discharges from the bowels become slimy and shreddy.

In almost all instances of the thrush in children, there is acidity of stomach present. Care, of course, is to be taken to discover and to correct any error of diet; and any unwholesomeness in the quality of the food. And *antacids* are to be administered. I know of no form of medicine better adapted to remedy the diarrhœa of infants, than the *Pulvis Sodæ cum Hydrargyro* of our hospital Pharmacopœia; composed of two parts of the Pulvis Cretæ Compositus, two parts of the dried Carbonate of Soda, and one part of Hydrargygum cum Cretâ. From three to five grains of this powder may be given thrice daily. and for the local affection of the tongue and mouth, the *mel boracis* is a capital application. It may be painted on the aphthous parts with a camel's hair pencil.

Aphthæ occurring in adults, in the course of other diseases, are often the harbingers of dissolution. They denote considerable *debility;* and they point out the propriety of sustaining the patient's strength by bark, wine, and nourishing food. It is remarkable how treatment of this kind will sometimes *tell*. I had a patient last summer who lived for some months, and in tolerable comfort, after a second attack of apoplexy. Every now and then he would have a crop of aphthæ appear, which was always an admonition to us that he not only would bear, but that he required some tonic. A more generous diet, with bark, would dissipate them in a day or two.

Borax is an excellent application for aphthæ, whether they occur in adults or in infants. I have known it afford great comfort to patients who were in the last stage of phthisis, and to whom the aphthous state of the mouth was a considerable source of distress. Equal parts of Mel Boracis, and Syrup of Poppies, is a good form. Or an agreeable as well as useful gargle may be made, by mixing two drachms of Borax, with half an ounce of Mel Rosæ, three ounces of Decoction of Quince Seeds, and four ounces of water.

Aphthæ seem sometimes to depend upon mere derangement of the stomach. A nobleman who is well known as a *bon vivant*, can never eat shell-fish (so I am told by his physician) without finding, within two hours, that his mouth is full of aphthæ. Even lobster-sauce will serve him thus. I look upon this as a sort of internal urticaria.

Cynanche tonsillaris.—Hard by the salivary glands lie the tonsils: and one of Cullen's species of cynanche is the *cynanche tonsillaris:* in more modern language, tonsillitis, or amygdalitis; or, in the vernacular, quinsy, common inflammatory sore-throat: a disease which, though internal, is yet within the reach of our sight, and easily recognized.

The popular term quinsy is in truth traceable—through the French *esquinancie*—to the scientific term cynanche.

This common and troublesome disorder occurs with very unequal severity in different cases. The difference depends upon the extent of the disease, and the number and variety of the parts which it involves: for it is seldom limited entirely to the tonsils, but spreads to the uvula, the velum palati, the salivary glands, the pharynx, and even to the root of the tongue, and the neighbouring cellular tissue. When the inflammation is superficial it does not produce any great distress, even though it may be diffused. When it penetrates through and beyond the mucous membrane, it is apt to end in suppuration, and to harass the patient much: the tonsils swell to an enormous size, and at length deep abscesses form in them. The disease is worst of all when the back part of the tongue, and the muscular and cellular tissue thereabouts, become implicated: it may chance to reach even the larynx, and then it is always and extremely perilous.

Under its more ordinary forms, cynanche tonsillaris generally manifests itself, at first, by a slight degree of uneasiness and difficulty in swallowing, with a constant dryness and sense of constriction in the fauces, and a feeling as if some foreign substance were sticking there. Upon inspecting the throat, more or less of inflammatory redness and swelling is seen of one or both of the tonsils. Sometimes both of them are affected at once. Very frequently one only is first attacked; and the swelling begins in the other as it ceases in the first. This is just what occurs in many instances also of cynanche parotidæa. The uvula is commonly enlarged and elongated, and of a scarlet colour. Sometimes it drags upon the back part of the tongue, or hangs into the pharynx, causing the disagreeable sensation of a foreign body continually present, and provoking, by its mere contact, painful and fatiguing acts of deglutition. More frequently the uvula may be seen to be adherent to that tonsil which is most swollen. The dryness of the fauces soon gives place to a copious secretion of transparent mucus, which is frothy and viscid, and sticks to the inflamed surface. so as to be detached with difficulty; and the patient is tormented by continual and painful efforts to hawk up, or to swallow, this mucus. In an early stage of the disease opaque whitish spots appear upon the red tonsil. They are exudations from its surface, or the discharged contents of the mucous crypts there situate. It is important that you should be aware of this, that you may not mistake such specks for ulcerating or sloughing points, such as occur in some other affections of the throat, but which are not common, at least in the outset, of this.

When the inflammation is violent, the submaxillary and parotid glands sometimes swell, and become tender on pressure; and, less frequently, the patient is troubled by profuse ptyalism. In other words, the inflammation spreads from the tonsils to the salivary glands, and secondary parotitis occurs; sometimes with and sometimes without an augmentation of their natural secretion. Unable, or unwilling to swallow the abundant saliva, the patient allows it to dribble from his mouth.

Now and then, although the act of swallowing is difficult and painful, you perceive, on looking into the fauces, no appearance which can account for these symptoms. The inflammation is seated lower down in the throat; out of sight. This cannot with propriety be called cynanche tonsillaris; indeed, it forms a distinct species, the *cynanche pharyngea* of Cullen. I mention it here because it really does not require any separate consideration.

The pain in cynanche tonsillaris is felt almost solely during the act of deglutition; which is difficult also from the mechanical narrowing of the passage by the enlarged glands. When both tonsils are affected at once, and much swollen, they push forwards the anterior pillars of the velum palati, and project, in the shape of two great balls of flesh, into the arch of the fauces, so as to leave a very small space only between them; and they sometimes even meet and touch each other, and cause ulceration by their mutual pressure. When attempts are made to swallow liquids, they are apt to return through the nose; the backward passage can no longer be shut in consequence of the tumid and fixed condition of the velum palati. The patients are unable to swallow even soft solids; indeed the pain of swallowing is so great, that they are not easily persuaded to try. In severe cases pain shoots from the throat to the ear along the course of the eustachian tube; and this is considered important, as being indicative of suppuration. I believe that suppuration does occur in the majority of the cases which are attended with this symptom. Sometimes there is tinnitis aurium, and partial deafness, from the obstruction produced to the passage of air through the eustachian tube; either by closure of its ex-

tremity in consequence of the swollen state of the parts about it, or by some thickening of its lining membrane from an extension of the inflammation along its channel.

When the inflammation is intense, and involves the root of the tongue, the patient becomes unable to open his mouth sufficiently to allow the fauces to be seen; and the inflamed parts can be examined only by means of one's finger. In some instances the power of separating the jaws is so limited, as not to admit the introduction of the finger: and the tongue is incapable of any motion.

In ordinary cases, however severe the disease may be, there is scarcely any affection of the breathing. But the throat is so blocked up, and the free play of the velum palati so impeded, that the speech is altered; becoming thick, guttural, and inarticulate. You may often recognize the disease by the peculiar tone of the patient's voice, without looking into his throat. When the swelling of the tonsils is very great, the breathing does sometimes become impaired; and it is in such cases alone, or *nearly* in such cases alone, that this disease is at all alarming.

Cynanche tonsillaris is commonly attended, from the very outset of the inflammation, by smart inflammatory fever; severe headache often, and a rapid pulse, rising to 120 beats in the minute, or more. The constitutional disturbance runs higher than we might have expected, considering the limited extent of the local inflammation, and the comparatively small importance of the part inflamed. At the same time there is very little of that debility and anxiety which are observed, as we shall see hereafter, in common continued fever.

The inflammation often terminates by resolution; but when it is violent, or long-continued, it very frequently leads to the formation of pus. When the mucous secretion increases in quantity, and becomes less viscid, when the difficulty of swallowing diminishes, and the febrile symptoms decline, resolution may be expected. Suppuration, on the other hand, may be looked for when the inflammation is unusually intense, when, by the swelling of the tonsils, the breathing is impeded, when a pulsating pain is felt, shooting to the ear, when the patient can scarcely open his mouth, or protrude or move his tongue, when there is more than usual *external* swelling, and when the symptoms increase, or even fail to remit, after five or six days have elapsed. Rigors sometimes accompany and announce the suppuration; and afterwards the pus may be often seen shining through the membrane covering the tonsils. In many cases, however, it lies so deep that it cannot be detected by the most careful examination. At length the little abscess bursts; and the relief thereupon experienced by the patient is sudden and striking. All at once the pain ceases; he can swallow, and he feels himself well; and often indeed he is well, or nearly so. The matter discharged has always a nauseous taste and a remarkably fetid smell: and sometimes the fetor, or the ill taste, is the only sign, besides the improvement in the symptoms, that indicates the rupture of the abscess; the pus being so small in quantity as, when mixed up with other matters, easily to escape notice, or (what frequently happens), passing backwards into the stomach by an involuntary movement of deglutition. Suppuration sometimes, but rarely, takes place externally, in the cellular tissue of the neck, as well as internally.

The termination in gangrene is spoken of in books: and it is just possible, but it never happens, I imagine, except in unhealthy constitutions; or when tonsillitis occurs as a complication of more general disease, such as scarlet fever. It is much more common to see repeated attacks of cynanche tonsillaris produce, what used to be absurdly called scirrhus, a permanent enlargement and hardening of the tonsils. While they are in this state, a low kind of inflammation is very readily re-excited in them. The enlarged tonsils are generally irregular, and notched; something like the surface of a walnut shell; and a white or yellow secretion is often to be seen lying in the hollows. This appearance may easily be mistaken, by an inexperienced eye, for ulceration.

There is not much risk of your confounding cynanche tonsillaris with any other complaint. The various species of cynanche enumerated by Cullen are separated each from the others by certain broad lines of distinction: all derived, however, from different combinations of two symptoms;—*dyspnœa* and *dysphagia*. Thus, in cynanche tonsillaris, deglutition alone is difficult; and when you look into the throat you see at once why it is difficult. There is equal difficulty of swallowing, and equal freedom of respiration, in

cynanche *pharyngea:* but the cause of the dysphagia is not visible. In cynanche *trachealis*, the respiration is much affected; the facility of swallowing not affected at all. In cynanche *laryngea* both these functions are implicated: there is difficulty of swallowing as well as difficulty of breathing. Cynanche *parotidæa* is distinguished by the absence of both the symptoms: its title to the name of cynanche is therefore equivocal. Many of these points will soon come under our notice again.

Cynanche tonsillaris is supposed to be most frequent among the young and plethoric. But it certainly occurs very often also in persons who are pale, and spare, and feeble; and in those of middle age. Some individuals appear to have, by nature, a strong predisposition to the disease; and in them its attacks are more or less periodical; recurring at particular seasons, and commonly during the variable weather of spring or autumn. This peculiarity runs sometimes in families. The liability to the complaint is also very much increased by repetitions of the attacks.

The only exciting cause worth mentioning, almost the sole cause indeed, is exposure to cold. Not unfrequently it assails so many persons at the same time as to be epidemic; and for that reason it has sometimes been thought contagious. But it has no contagious property whatever. Although we often see several members of the same family affected by it at once, yet we may learn, upon careful inquiry, that its commencement in the different cases has been too nearly simultaneous to admit the supposition of its having been communicated from one to another. The patients have all been exposed to the same unwholesome influences, which operating upon similar constitutions, such as those who are sprung from the same parents may be expected to possess, may have produced similar effects. This prevalence of the disorder at certain times and places, is connected, no doubt, with some peculiar conditions of the atmosphere.

The prognosis is almost always favourable. Not but what death may be produced by this disease, under peculiar circumstances, and when the inflammation is unusually violent and extensive. The late Dr. Gregory, of Edinburgh, used to mention in his lectures one instance, the only one he had met with among many hundred cases, of death from cynanche tonsillaris. He did not see the patient till he was moribund; and he suspected that it was combined with typhus fever. The only fatal case that I ever witnessed occurred some years ago at the Middlesex Hospital, in one of my own patients. He was a stout young man, twenty-six years old, a private coachman. The complaint was clearly traced to his having got wet through, more than once, a day or two before it came on. Besides the ordinary symptoms of cynanche tonsillaris, there was great external swelling on both sides of the throat, and the patient was unable either to open his mouth, or to move his tongue. The inflammation involved not the tonsils merely, but the base of the tongue, the salivary glands, and the surrounding cellular tissue. At length suppuration took place. The abscess broke internally, and pointed also externally, just below the symphysis of the chin, where it was opened with a lancet. Two days after there was a sudden gush of blood from the mouth. So immovable were his jaws that it was impossible to determine from which side the hæmorrhage proceeded; it was stopped, however, apparently by the treatment adopted. A fortnight later, the bleeding recurred profusely. It was now evident that the blood was arterial, and that it came from the left side of the throat. Preparations were made for tying the common carotid on the left side; but just as the operation was about to be begun, the patient expired, in our presence. His death was frightful, but full of pathological interest. He did not sink, as you may have supposed, in the way of syncope, from loss of blood; but by suffocation. The blood passed down the trachea and into the lungs; and he had been so weakened by the previous hæmorrhage, that he could not expel the blood so introduced, which actually choked him. A large clot was afterwards found, filling up the windpipe. I felt this man's pulse beat firmly and regularly for a minute perhaps after his last effort to breathe. On examining the body it was discovered that the abscess had opened internally behind and below the left tonsil. The lingual branch of the carotid artery crossed the site of the abscess; and had been severed and laid open by ulceration. From this vessel the fatal hæmorrhage had come.

It should be borne in mind also that cynanche tonsillaris does sometimes, by extension of the inflammation to the neighbouring parts, superinduce that very formidable species of cynanche, of which I am soon to speak, the *cynanche laryngea*. All cases in which the

breathing is in any degree affected, should excite suspicion, and strict scrutiny; although the dyspnœa may be produced by the mere swelling of the tonsils.

You will understand, then, that cynanche tonsillaris *may*, under unusual and untoward circumstances, prove a fatal disease; but that it is so very rarely indeed. In almost all cases we may say that the life of the patient is not in danger.

Treatment.—In the uncomplicated and milder form of the disease, when the inflammation is superficial and the fever slight, no great activity of treatment is requisite. The patient should be kept within doors, and even in bed: for a troublesome tendency to a recurrence of the disorder may be fostered by neglect or imprudence. Cooling saline purgatives will be proper, and the antiphlogistic regimen. A strip of flannel may be put round the neck, and some stimulating embrocation applied to the exterior of the throat, beneath the ramus of the jaw: the compound camphor liniment is well adapted to this purpose. Some such plan as this will generally suffice, not indeed to stop the inflammation of a sudden, nor to put an end at once to the fever, but to cause the complaint to run its course evenly, and to go on to resolution in a few days. Commonly it is not completely over until both the tonsils have been attacked in succession.

When you catch the disorder in its very outset, I believe you may sometimes succeed in cutting it short by an emetic: a scruple of ipecacuanha for example, with a grain of tartarized antimony.

A great variety of astringent, acid, and other gargles, have been employed in this disease; and their good effects have, I apprehend, been much overrated. Many cases would do quite as well, or better, without them: for in the early stages strong astringents, and the straining and movements of the throat that accompany their use, may even be hurtful, and increase the pain and the inflammation. The only gargle which I should consider admissible in the commencement of the malady is a gargle of warm milk and water. I have known of one instance in which quinsy suddenly attacked a gentleman who was extremely anxious to use his throat, in public speaking, the next day. He occupied himself perpetually, for some hours, in this sort of fomentation of the tonsils with hot water; and with such good effect that on the day following he was able to accomplish his object. Still there are cases in which, at certain stages of the disease, detergent gargles are serviceable, by assisting the excretion of the mucus that collects in the fauces, and by correcting fetor. A weak solution of chlorine in water answers well. In more chronic sore-throats, stimulating gargles may often be employed with advantage. When the inflammation is violent a slightly stimulant linctus is preferable; it *cuts the phlegm* as they say, *i. e.* it promotes its detachment and removal. Of this kind, currant-jelly is one of the best.

But far better than any thing else, as a local application to the inflamed fauces, is the steam of hot water: whether we are hoping for resolution of the inflammation, or whether we desire to promote and hasten the process of suppuration already begun. The inhaler introduced by myself into the Middlesex Hospital, and elsewhere, though somewhat clumsy in appearance, is the most convenient and effectual that I am acquainted with. I show it to you. It was invented in Edinburgh by a friend of mine long since dead, Mr. Hercy. It will stand upon a table, or lie upon a pillow; and a large volume of steam is carried inwards against the fauces by the mere natural breathings of the patient. Most of the inhaling machines that I have seen require a sucking effort, like that made in smoking a pipe: an effort that is apt to be irksome and fatiguing, especially in pulmonary diseases; for some of which this method of applying vapour directly to the suffering part is as useful as it is for sore-throats.

Blistering the outside of the throat is a favourite remedy with many. When early applied a blister often does much good, and probably prevents suppuration in some cases. But I have found blisters of uncertain efficacy; they leave a mark which lasts for some time, and which patients of the other sex are apt to complain of. For these reasons I prefer mere rubefacients; the liniment I mentioned before, or the common soap liniment, or a mustard poultice folded between two layers of thin linen.

In more severe cases leeches applied to the upper part of the throat, just below the angles of the jaw, have been found to give sensible relief: and in the worst degrees of the disorder, when there is much outward swelling, and the jaws and tongue are fixed, leeches

are absolutely requisite. It will frequently be proper to take blood from the arm also. The necessity for active depletion must be measured by the severity of the local symptoms, the intensity of the fever, and the general strength and condition of the patient: and of these things a little experience will teach you to judge.

It is not to be expected that either leeches or blisters will be of much use after the process of suppuration has commenced; nay they may sometimes be injurious by retarding it. It is frequently a difficult matter to determine whether pus has yet formed or not.

I have already admonished you to make a careful examination of the throat, and to watch your patient narrowly, whenever he experiences any difficulty of breathing. Dyspnœa may be produced by the mere swelling of the inflamed part: and when it concurs with much enlargement of the tonsils you had better pierce them with a lancet. If they contain matter it will be evacuated; and if not, the bleeding produced by the puncture will generally reduce the swelling somewhat, and relieve the patient. There is an instrument made on purpose for this small operation, consisting of a lancet enclosed in a flat silver sheath, from the end of which it is made to protrude, to a certain extent only, by pressing upon a spring. The instrument should be directed towards the centre of the fauces, and not outwards, in order to avoid wounding important vessels or nerves. Dr. Cullen indeed says "this does not require much caution:" but notwithstanding this high authority I must warn you that puncturing or scarifying the tonsils is an operation not to be carelessly, or rashly, or wantonly performed. Portal mentions a case in which a skilful surgeon in scarifying the tonsil of his patient, wounded as he supposes some ramification of the internal carotid, and the patient was presently dead. That artery lies, as you know, very near the tonsil; and only a very few years ago, in Ireland, it was struck by a surgeon while scarifying a gentleman's tonsil; and the gentleman died in three minutes. This I was told by the late Dr. Barclay. The case I related just now of fatal hæmorrhage from the lingual artery points to the same danger: and since that case occurred two others involving similar hazard have fallen under my own notice, and impressed me with a strong feeling of the necessity of caution. A man was brought into the hospital with profuse hæmorrhage from the right tonsil or its immediate neighbourhood, the consequence of syphilitic ulceration of those parts. He had lost three or four quarts of blood, and was nearly dead. His life was saved by Mr. Mayo, who tied the common carotid on that side.

Last February (1838) a boy, from Harrow School, was placed under my care, in whom cynanche tonsillaris came on during convalescence from scarlet fever. So much swelling was there of both tonsils, that they met, and pushed the uvula outwards before them, and the breathing was much impeded. A surgeon who was in attendance with me punctured the tonsils. The next day a good deal of hæmorrhage took place; and this recurred, several times, to a considerable and even an alarming amount. When the clots that formed were wiped away with a sponge, the blood could be seen welling out in a little stream, with a pulsating motion, from a small incision in the left tonsil. The hæmorrhage was ultimately, after much trouble and anxiety, arrested, by applying a pencil of lunar caustic freely, within the bleeding orifice. Lint, wetted with the muriated tincture of iron, or with a saturated solution of alum, is a fit application in similar accidents.

Mr. Lawrence, who saw this case, told me that he once knew a patient die of hæmorrhage from the tonsillar artery.

I ought, perhaps, here to add, that, very recently, Mr. Joseph Bell, of Barrhead, has strongly recommended the internal administration of powdered guaiacum, in large doses, as being almost specific in the cure of cynanche tonsillaris. He gives as much as half a drachm, suspended, by means of mucilage, in a draught, every six hours. Mr. Bell has no doubt that this remedy, if timely administered, will cut the disease short in ninety-nine cases out of a hundred. It has been found successful in other hands also. I have never had an opportunity of trying it.

The chronic enlargement of the tonsils, to which I have already adverted, is sometimes productive of great inconvenience and distress, and even of danger. Its occasional consequences are—an habitual trouble in swallowing; confused and inarticulate speech; deafness in various degrees, from occlusion of the eustachian tubes; more or less impediment of breathing; and even spasm of the glottis, and impending suffocation. The enlargement

may in such cases be somewhat reduced, I believe, by repeatedly passing a stick of lunar caustic over the surface of the tonsils; but a much readier and better plan is to amputate them, in part at least. This may be done by a ligature; or still better by scissors, or by a sort of small guillotine invented for that purpose. It is not a very painful operation. Mr. Arnott removed one lately for one of my hospital patients; and a very few days ago (Dec. 1838) Mr. Mayo brought two, in a piece of paper, to the hospital. He had just before cut them off for a patient whose respiration they had much embarrassed.

LECTURE XLV.

Acute Laryngitis. Symptoms. Treatment: blood-letting, bronchotomy, mercury, antimony; anatomical characters of the disease. Causes. Secondary Laryngitis. Œdema of the Glottis. Chronic affections of the Larynx.

The disease of which I have next to speak is of far more serious character than those which were considered in the last lecture. *Cynanche laryngea*, or acute laryngitis, has proved rapidly fatal in a large proportion of the instances in which it has been known to occur. Yet, when the patient is seen tolerably early, and the nature of the malady is clearly perceived, and the source of peril thoroughly understood, I believe that our art is sufficient, in most cases, to rescue the sufferer from the fate that hangs over him. It is of the greatest importance, therefore, that you should be able to recognize laryngitis when you meet with it, and that you should comprehend the principles according to which it requires to be treated.

What is laryngitis? It consists, as that term implies, in inflammation of the parts composing the larynx; and especially of the mucous membrane that covers the laryngeal cartilages, including the epiglottis. The inflammation may be, and sometimes is, exactly limited to the larynx; but frequently it extends also to the posterior fauces, the velum palati, and the tonsils.

Symptoms of laryngitis.—The symptoms of acute inflammation of the larynx are these. The patient complains of *sore-throat*. If you look into his throat you will commonly perceive some redness of the velum and uvula, and of the fauces generally. But there is a degree of restlessness and anxiety about the patient more than proportionate to the apparent inflammation. Among the earliest of the symptoms that bespeak danger, and ought to excite alarm, is *difficulty of deglutition*, for which no adequate cause is visible in the fauces; and to this is presently added *difficulty of breathing*, for which no adequate cause can be discovered in the thorax. The mode and character of the respiration are peculiar; it is attended with a throttling noise; the act of inspiring is protracted and wheezing, as though the air was drawn in through a dry narrow reed. If you ask the patient what is the *seat* of his distress, *where* the disease is situated, he points with his finger to the *pomum adami*. If he coughs, he coughs with a peculiarly harsh, stridulous, husky, abortive sound. He either speaks quite hoarsely, or (what is more common) all power of audible voice in the larynx is lost, and he speaks by means of his lips and tongue only, in a whisper. There is tenderness of the laryngeal cartilages; they are painful when pressed externally. The face is flushed; the skin hot and dry; the pulse hard. As the disorder advances, the patient's general distress increases; but some of the symptoms alter: his countenance becomes pale or livid, anxious and ghastly; his eyes protrude; he is miserably unquiet, impatient for some relief, declares or makes signs that he wants air, and begs that the windows may be opened: and if he does not *obtain* timely relief, he perishes—he dies strangled.

The pathology of this terrible disease is extremely simple. The membrane covering the interior surface of the instrument of the voice suffers inflammation. One effect of

inflammation in mucous membranes is a thickening of those membranes; they become turgid and swollen. Another frequent effect is the effusion of serous fluid in the subjacent cellular tissue. By this tumid thickening of its lining membrane, the chink called the *rima glottidis* is narrowed: it is still farther diminished in breadth whenever the membrane is lifted and protruded by infiltration of the tissue beneath it: it is so nearly closed up, that air cannot pass inwards in sufficient quantity to sustain the vital functions: a small portion only of the blood returned to the lungs from the right side of the heart undergoes the requisite change from venous to arterial. The miserable patient grows drowsy and delirious, and dies by a slow process of strangulation. If the rima glottidis becomes *quite* shut up, his sufferings, and his life, are quickly at an end.

This disease affords a good instance of the truth which was announced in a former part of this course of lectures; viz. that the *danger* of a morbid change may depend entirely upon its situation. It is so, eminently, with laryngitis. The inflammation is sometimes limited to a spot of membrane not bigger than a square inch. If a square inch and no more of the same membrane, a little lower down in the trachea, were inflamed in the same manner and degree, the complaint would be quite unimportant. Cynanche laryngea derives all its peril from the circumstance that the inflammation tends to close up what may well be called the *janua vitæ*. The part affected subserves two purposes: it is the organ of speech; and it forms a portion of the channel through which air is conveyed from without into the lungs. Both of these purposes are impeded in laryngitis. Now the animal function of speech may be entirely and permanently suspended without any danger to life. The function of respiration, which, though under the influence of the will, is an organic function, will not bear to be *suspended* even for a few minutes; and life cannot be *long* sustained when it is *much impeded*.

The difficulty of swallowing is a remarkable symptom, and almost always present. Yet it is not absolutely universal; for Mr. Lawrence describes a case in which it did not occur. It appears to depend, in some measure, upon the tumid and tender condition of the whole membrane which is common to the larynx and pharynx, and which is pressed upon as the larynx rises in the act of deglutition: but this symptom depends also, and in a greater measure, upon the state of the epiglottis, which is often enlarged, and fixed by the swelling in an erect position, and unable to execute its natural valvular office: so that when the patient makes efforts to swallow, a portion of the food or drink gets into the larynx, and a paroxysm of choking dyspnœa ensues. By pressing down the back part of the patient's tongue, and getting him at the same moment to make a coughing effort, you may sometimes obtain a sight of the tumid, red, and upright valve.

The dyspnœa is constant: yet there are pauses of comparative ease and quiet; and there are, commonly, periods of severe aggravation and urgent distress. It is probable that the permanent narrowing of the chink by the inflammation and its consequences is from time to time increased by spasmodic contraction of the muscles that close the glottis.

This is the first disease that has come before us, in which the respiration has been *primarily* impeded. If you call to mind what was stated in one of the early lectures respecting death by apnœa, you will be at no loss to understand the manner in which life is destroyed in laryngitis.

This formidable malady has always existed; for you may trace examples of it, under various names, even in the writings of the ancients. But it is only in recent times that it has been singled out from the rest of the anginæ, and made a separate object of study. It has numbered some distinguished medical men among its victims: Dr. David Pitcairn, Sir John Macnamara Hayes, Sir George Tuthill. The celebrated General Washington died of it. When it has caused death it has generally run a rapid course, and proved fatal before the fifth day. It *has* carried the patient off in less than twelve hours.

It is of the utmost consequence to make an accurate diagnosis. Laryngitis is easily distinguished from cynanche tonsillaris by the extreme and peculiar dyspnœa which attends it. There may be difficulty of breathing in the latter disease, from enormous swelling of the tonsils: but then such swelling will mostly be *visible*. In laryngitis the marks of inflammation to be *seen* on inspection of the fauces are generally slight and trifling, and quite inadequate to explain the difficulty of swallowing. Do not, however, forget that laryngitis may *supervene* upon cynanche tonsillaris. Again, cynanche laryngea is readily

discriminated from cynanche pharyngea; in which complaint there is great pain and difficulty in deglutition; but the breathing is quite free. In cynanche trachealis, or croup, which I shall next describe, the breathing *is* affected, and the swallowing *is not*.

Treatment: Blood-letting.—What is to be done for a patient labouring under acute laryngitis? How and when are we to employ the great remedy for acute inflammation—blood-letting? or are we to employ it at all? These are points concerning which it is quite necessary that your minds should be prepared and prompt to decide. If you look merely to the results of the recorded cases of this fearful complaint, you will scarcely find an answer to the question. In some of them copious bleeding appeared to save the patients: in others, it was of no service, but rather seemed to accelerate their death. Sir John Macnamara Hayes suffered two attacks of cynanche laryngea. In the first he was freely bled. Dr. Roberts, of Bishop Stortford, informs us that the first bleeding was attended with *considerable relief;* the second also with *manifest advantage;* and by the third, *his safety appeared to be ensured.* Fifteen years afterwards he died of the same disorder, for which he was again bled and leeched, under the care of the late Dr. Baillie. Washington was largely bled, and died. Again, a Dr. Francis, of New York, recovered from acute laryngitis after copious venesection. It is evidently needful to consider and determine the circumstances under which we are to use, or to withhold, the lancet.

Bleeding, to be serviceable, or safe, must be performed *early*. There is, perhaps, no disease in which the καιρος οξυς, the fleeting opportunity, is more conspicuous than in this. When I say that you must bleed early if at all, I do not mean that you are to reckon so many days or hours from the commencement of the disorder; but you must ascertain what progress it has made; for it travels sometimes at a railroad-pace. You must look to your patient's actual condition: and I apprehend that your practice, in respect to blood-letting, may be safely guided by the following rules. If there be high inflammatory fever present, and the skin is hot, the pulse firm and full, and the cheeks are red, and the lips florid, you may bleed your patient with decision and advantage. But if his powers are beginning to sink under the poisonous influence of imperfectly aerated blood, if his skin be cold, or even cool, his face pale or leaden, his lips blue, his pulse small and feeble, his mind wavering—you will do no good by blood-letting: nay, you will increase the debility which already exists, and hasten the fatal catastrophe.

With regard to *local* blood-letting, and to counter-irritation, there is one remark made by Dr. Farre of much practical importance. It is a common practice, in affections of the throat, to apply leeches over or near the laryngeal cartilages, and afterwards to place a blister there. Now serous infiltration of the neighbouring parts often follows leech-bites; and the effect of a blister in producing serous effusion often extends beyond the skin; and the cartilages of the throat lie very near the surface; and it is possible that œdema of the glottis might be produced, or augmented, in consequence of these topical remedies. It will be better, therefore, when we wish to take blood locally, to take it by cupping from the back part of the neck: and when we desire to produce counter-irritation, it will be prudent to lay a blister on the upper part of the sternum, rather than to the front of the throat.

In the advanced stage of the disease, *medicine*, I fear, can effect but little.

But *surgery* may be more successful.

Bronchotomy.—The danger arises from the *mechanical* obstacle to the entrance and exit of air into and from the lungs; and this state of peril admits of a *mechanical remedy*. If an artificial opening be made between the obstructed part and the lungs, the air is again freely inhaled and freely expelled; the blood undergoes the vital change from purple to scarlet; and the patient is placed in a condition of safety. He continues to respire through the hole thus drilled in the trachea, and the inflammation of the larynx has subsided; the thickening of the membrane disappeared; the submucous infiltration been re-absorbed; and the vocal instrument restored to its natural integrity; and then the aperture in the windpipe may be suffered to heal, and the patient will again draw his breath through its natural channels.

This is one of the triumphs of the healing art. It requires a knowledge of the general *pathology* of the disease, *i. e.* an acquaintance with the facts that acute inflammation may affect the larynx almost exclusively, and that its tendency is to narrow the fissure of the

glottis, and destroy life by suffocation. It requires a knowledge of the *symptoms* of such inflammation: and it requires an accurate knowledge of all the essential circumstances of the particular case. For it is not every case in which the transit of air through the slit in the larynx is hindered, that is a fit case for the operation of tracheotomy. Some years ago there was brought to me by a surgeon a man breathing with considerable labour and constraint, the air passing through the larynx with an audible hissing noise. The surgeon wished to know my opinion of the propriety of opening the patient's windpipe. He had come to the conclusion that there was ulceration of the membrane lining the larynx, with thickening; that the cause of the sibilous respiration was partly mechanical, partly spasmodic; the little muscles that close the glottis acting with injurious energy in consequence of the neighbouring irritation: and he thought that this mischief in the larynx would have a better chance of being repaired, if the functions of the organ could be for a time suspended. He was aware, however, of the necessity of ascertaining what was the condition of the *lungs;* and he had not studied auscultation long enough to trust his own ear to that matter. The patient was pale and thin, and emaciated: and three minutes sufficed to convince me that his lungs were extensively disorganized. His respiration was not so difficult as to threaten suffocation; he was not dying of the laryngeal obstruction; and I recommend that he should not be subjected to an operation which might curtail his existence, but could not affect a cure. The man died soon after; and we examined his body together. There was, as my friend had supposed, ulceration of the membrane near the chordæ vocales, and the lungs were full of suppurating or softening tubercles. I mention this case to show you that it is necessary to ascertain the condition of the thorax generally before we perform or sanction such an operation as tracheotomy. Not that there is any thing very formidable, or painful, or dangerous, in the operation itself. But if we cut a hole in a patient's throat, who is sure to die soon after of some other incurable complaint, we shall incur the risk of being charged with having killed him. Do not misunderstand me, however. If a patient's *life* be threatened by acute laryngitis, or by laryngeal œdema, and we are *sure* of that, and if at the same time we are sure that he carries a mortal disease about him, we are not for that reason to let him die, if we can help it, of the *laryngitis:* any more than it would be lawful for us to administer a drachm of prussic acid to a man condemned to be hanged the next morning. But we must state the whole of the case plainly to the patient's friends, and propose the operation as the means, not o effecting an absolute cure, but of staving off the *immediate* danger.

And here let me repair an omission of which I was guilty when speaking just now of the diagnosis. My object was to guard you against mistaking laryngitis for some other malady: but I must also warn you against the converse error, that of mistaking some other malady for acute laryngitis. I can assure you that such a mistake has been made; and tracheotomy has been performed, too, when there was no disease in the larynx; and the practitioners by whose authority it *was* performed have been ungenerously reproached for their error, although no harm beyond the slight pain and inconvenience of the operation resulted from it. The cases in which this blunder has been committed have nearly all, I believe, been cases of *aneurism of the thoracic aorta*, which, by its pressure on the first divisions of the air-passages, or on the nerves thereabouts distributed, had caused that kind of laboured and stridulous breathing which is characteristic of laryngitis. I may venture to say that no person who has had opportunities of educating his ear for the purposes of auscultation, and has made a proper use of those opportunities, could ever overlook such a complication as this. I have myself seen a woman (I mentioned her case before) whose trachea was laid open by a surgeon while she was suffering under mere hysteria; so closely did that disease mimic laryngitis.

When you have good evidence that a mechanical obstruction to the passage of the air exists in the larynx, and that the tubes *beyond the larynx* are pervious and free; there are two things which I would recommend to you. First, I would most earnestly advise you not *to wait too long* before you propose or perform tracheotomy; and secondly, never to omit performing it *merely* because it may appear to be then *too late*. If, in the acute and limited disease, an artificial opening be made while the patient's strength is yet entire, and before his whole system is poisoned with venous blood, or his lungs are overwhelmed with sanguine congestion and serous effusion, it will almost infallibly save his life. But

if the sinking of the vital power has got beyond a certain point, tracheotomy will not, in that case, rescue him. It is bad and foolish practice to wait, and try other methods, and postpone the operation as *a last resource*, when the circulation is evidently loaded with unarterialized blood. In my own case I should choose to be operated on early; the moment that I found early blood-letting was not *telling* upon the local distress, and that any shade of duskiness became perceptible in the skin; just as I should choose to be operated upon at once for strangulated hernia, after one fair attempt had been made by a skilful hand to return the bowel, without waiting till inflammation set in, or had been *caused* by the taxis. On the other hand, if you do not see your patient until his powers are nearly exhausted, do not abstain from the operation even though you may feel convinced that it will be unsuccessful; for if it does not save life, it will disarm death of its agony. A patient will lie sometimes for hours, painfully labouring for breath in deep and strong catches, at considerable intervals from each other: in fact, he is just in the condition of a man with a cord round his neck, not pulled quite tight enough to suffocate him at once. Besides, it is not always easy to say whether the period of possible recovery *is* yet gone by. I had a female patient in the hospital who had suffered one or two attacks of frightful dyspnœa, in which the main difficulty was referred to the larynx; but she had rallied from them before any steps could be taken for performing tracheotomy. On the next occasion, however, the seizure was so sudden and rapid, that although Mr. Arnott was luckily in the hospital at the time, before he could be found and brought to her bedside, the woman was, to all appearance, dead. Respiration had entirely ceased. This quietude of the larynx rendered the operation more easy. Mr. Arnott speedily made an opening into the trachea; some air was blown in through the aperture, and then pressed out again; and presently the natural respiration was renewed. The woman recovered; the orifice healed up, and she left the hospital. Three or four months afterwards word was brought that she had died at her own home after a short attack, and when there was no one at hand to open her windpipe. We got permission to examine the body, and found a large ulcer in the trachea, near the larynx; which ulcer by its irritation had occasioned, as we presumed, the spasmodic closure of the glottis. The preparation exhibiting the diseased parts is on the table before you. You see that there was enlargement of the thyrĕoid gland. This had probably nothing to do with the symptoms. There was also a large ulcer in the left bronchus.

Mr. Goodeve, surgeon to the Clifton Dispensary, operated on a patient in whom "no pulse could be found at the wrist; his face was suffused with blood, and his lips livid; and it was hard to say whether he breathed or not:" yet he recovered.

It so happens that there is at present (December, 1838) under Dr. Wilson's care, in the hospital, a woman named Slack, who was rescued when almost *in articulo mortis*, by the same expedient. She had chronic disease of the larynx; but a sudden aggravation of the symptoms occurred; she became stupid and comatose, her countenance was cadaverous, her skin covered with a cold clammy sweat, and her breathing, which had been stridulous and laryngeal, had almost, if not quite, stopped. For a little while she had been making efforts to respire, not more than twice in a minute. Her pulse was intermittent, and extremely feeble. In this state the house-surgeon (Dr. William Merriman) made a small incision in the skin over the cricoid cartilage, and then thrust a large trocar into the tube. Air rushed through the opening, the respiration returned, the pulse revived, and the stupor passed away. This happened on the 10th of October. She is still in the ward; the aperture has closed up; and though she is not well, she is *living*.

What is the reason, you may ask, of these different and inconsistent results? How is it that bronchotomy shall reanimate one patient, whose last breath, but for its help, was already drawn, who was already motionless in apparent death; and yet shall fail to save another patient, who is still alive and sensible of his danger, and struggling with his disease? The difference depends, I make no doubt, upon the time that elapses between the commencement of extreme dyspnœa, and the performance of the operation; upon the slow or the speedy completion of the strangling process. And this, again, obviously depends upon the manner and degree in which the passage is narrowed. When the obstruction, though considerable, is incomplete, and does not rapidly augment, the respiration continues to be performed, however imperfectly. Meanwhile the brain gets oppressed,

the circulation tends to stagnate, and, above all, the lungs become gorged with black blood, and clogged up by effusion into their cells and substance. Secondary causes of apnœa are thus established, which do not cease when the primary cause is at length removed, by unbarring the main channel for the admission of air. Whereas, when the access of the atmosphere is suddenly or soon shut out, the lungs are not thus mortally injured, but remain capable of resuming their functions when they are again supplied with air.

Tracheotomy, then, will be the most likely to succeed, while the patient is still lively and strong; and after that the chance of success will be worse in those cases in which the apnœa has been *slow* in its progress, than in those in which it has been *rapid*. I repeat that, in threatening circumstances, the operation should be done *early;* but that it should not be withheld, through despair, at *any* period of the disease.

The effect produced upon the condition of the patient by the timely formation of an artificial glottis, is very striking. The moment that the scalpel penetrates the rings of the trachea, air begins to hiss through the incision; and when a fair opening is established, and a full inspiration is drawn in through the wound, several forcible expirations generally succeed, whereby a considerable quantity of mucus is expelled, which could not pass the contracted aperture of the natural glottis. Then the breathing soon becomes easy, the anxiety and distress are followed by a perfect calm, and usually the exhausted sufferer sinks into a tranquil slumber. This sleep is apt to be from time to time interrupted by the clogging up of the orifice with frothy mucus. It is requisite that some intelligent person should remain by the patient, to assist him in these emergencies, or he may still be throttled, notwithstanding the apparent prosperity of the operation.

When a sufficient hole has been made in the instrument of the voice below the glottis, the voice of course becomes extinct, or nearly so; and the patient is as unable to utter a cough as he is to use vocal language. Now this it is of some importance to notice, for he often wants to cough, in order to clear the air passages of mucus, or of blood, by which they may be embarrassed, and he may be helped to do so, or taught to help himself. First he should draw in a full breath, and then stop the orifice for a moment with his finger, while he makes the expiratory effort. And as the parts within the larynx recover, the patient, by a similar manœuvre, may enable himself to speak aloud.

As actual examples are more interesting and often more instructive than an abstract of results, I will tell you in a summary manner the history of a case of laryngitis, which occurred in one of my hospital patients, in the latter part of the year 1832. He was an old man, about sixty. His name was Kent. He was brought to the hospital bloated with anasarca, which was most conspicuous in his legs and thighs. His breathing was laborious and difficult, and attended with a wheezing noise, audible at some distance. He could not lie down: he had a hard, but not full pulse. The dropsical swelling had come on suddenly five or six days before; and in the outset his face (he said) was so puffed up that he could scarcely see. He had been bled to the amount of a pint and a half, according to his own account, on the previous evening. I had a vein opened immediately, and twenty-four ounces of blood were drawn; and eight ounces more were taken from the chest by cupping. He was thoroughly purged with calomel and senna. The bleeding gave him very little relief, so far as the respiration was concerned; but the next morning the anasarca had totally disappeared. I found him sitting up in bed, breathing with great effort, and with a loud stridulous noise which accompanied both inspiration and expiration. He referred all his uneasiness to two points; one of these was the larynx, the other the ensiform cartilage. He swallowed with great pain and difficulty; and every attempt to do so excited a fit of choking cough. There was no morbid appearance visible in the fauces; every part of his chest sounded well on percussion, and the murmur of healthy respiration could everywhere be heard in the lungs, almost drowned, however, in the louder laryngeal noise. As his strength was entire I had him again cupped, to twelve ounces, at the back of the neck; and prescribed three grains of calomel every three hours. He also inhaled the steam of hot water.

Upon visiting him again the same evening, I found the dyspnœa increased. Each act of respiration was attended with a loud croupy noise. His countenance was beginning to be anxious and ghastly. He was restless; and his pulse was less firm. I was so convinced that the operation of tracheotomy was the only thing that could save him, and that it

could not be safely delayed, that I sent to request that Sir Charles Bell would come and perform it. By the time he arrived the restlessness had increased. The patient was shifting perpetually from one side of the bed to the other, as if seeking some new point of support: his face had become pale; and his lips were turning livid. He spoke with sudden, and as it were convulsive efforts; stating earnestly how thankful he should be to have the obstacle to his breathing removed; and pointing to the larynx as the seat of his distress.

The operation, under such circumstances, is by no means an easy one to perform. Its difficulties were well exemplified in this patient. In the first place he was sitting up; he could not bear to be placed in a horizontal position. Then the dyspnœa caused him instinctively to elevate his shoulders, and sternum, and clavicles, to the utmost, so that the trachea was sunk deeply into the thorax; and the larynx was in constant and rapid movement up and down with a plunging motion, like that of the piston of a steam engine. Sir Charles, after some trouble, succeeded in cutting out a piece of the cartilage; for a mere slit did not suffice, it closed tightly during every inspiration, although it was open enough during expiration. At length, when the air was freely admitted, the breathing became gradually easy. I shall never forget the whole spectacle: there sat the poor man gasping and fighting for breath; his face covered with sweat, and wearing the most anxious expression. By and by what I have called an artificial glottis is opened for him; and presently afterwards, though half a dozen candles (as Sir Charles has himself painted the scene) are held close to his face, to throw light upon the wound, and though the surgeons, their hands covered with blood, are still busy about his throat, making arrangements to ensure the patency of the orifice, the patient falls fast asleep. It was necessary to place an assistant behind him to prevent his head from nodding forwards, and deranging the apparatus in the wound. Nothing can express more strongly than this fact the great distress and fatigue which had previously existed, and the perfect relief afforded by the operation.

This man ultimately got quite well: and he has since shown himself occasionally at the Hospital, in excellent health. There were two or three points about the case which I am unwilling to pass over without notice. It was evident that after the opening was made in his windpipe, he still breathed in part through the rima glottidis also; for the stridulous sound did not wholly cease. The aperture was formed as low as the circumstances of the case appeared to permit: the tube was perforated in the membranous space between the thyreöid and cricoid cartilages. Strictly speaking, *laryngotomy* was the operation performed. I do not enter into the consideration of the best place for making the opening: that point you will be taught by the professor of surgery: but it was observed in the case in question, that the slightest touch of the irritable mucous membrane, with a hook or a probe—especially if the touching instrument was turned *upwards* towards the glottis—produced a fit of coughing, and a paroxysm of still more laborious breathing. For some days after the operation, it was noticed that a part of whatever he swallowed *appeared immediately at the wound.* Now this proved as plainly as if we could have seen the parts, that the epiglottis was thickened, and erected, and incapable of performing its protective function to the larynx: and it accounted for the paroxysms of choking cough produced by efforts at deglutition. At first the lining membrane of the larynx and trachea was so irritable, that the patient could not bear to have a metallic tube inserted; and an ingenious contrivance was adopted for keeping the orifice from being covered over by the lips of the wound. They were held apart by two bent wires, which were tied together at the back of the neck. After twenty-four hours had elapsed the irritation of the mucous membrane had so far abated that he was able to breathe through a canula.

There cannot be a doubt that this man was snatched from the very jaws of death by the intervention of the surgeon. A function indispensable to life was nearly suspended; and a substitute for the faulty organ was provided by art, until the interrupting cause was removed. Scarcely a year passes over our heads without the occurrence of one or two such events in the Hospital. When lecturing upon this subject last season, I was able to show you a female patient whose life had been saved in a similar way. And there is now also (Dec. 11, 1839), in Pepys' ward, a rescued man, with the tube still in his windpipe. The operation was done on the spur of necessity by Mr. Tomes, the present house-

surgeon, with a trocar. The patient, who was previously in a state of extreme distress, said, in a faint whisper, as soon as the opening was effected, "It's all right now."

He had been exposed to rain and cold about a week before; and had suffered pain and tenderness of the larynx. Prior to his admission he had been bled, *and salivated*, and had a *blister on the throat, which embarrassed the operation.*

Within the last eight years the operation of bronchotomy has been performed in the Middlesex Hospital fourteen times. Seven of the patients recovered; seven died. In two of the seven fatal cases, the condition of the patients was hopeless at the time of the operation. In four at least of the five others, much relief from suffering was afforded by it, and life apparently prolonged. One of the patients was a young child: the opening was made by a trocar: much blood got into the air-passages, and the child, which seemed to be sinking previously, died within the hour. Life might, I think, have been preserved in this case, by a *timely* operation, properly done. For a trachea so small, the scalpel is preferable to the trocar.

Mercury.—I have said nothing hitherto about the use of mercury in this acute disorder, because I hold it to be of very secondary importance, and because I have been anxious not to divert your attention from the two great practical points, *bleeding* and *tracheotomy*. Mercury may very fitly be given in those cases and circumstances in which blood-letting appears proper; but we cannot depend upon it: we cannot reckon upon its influencing the system *in time;* nor upon any marked improvement of the symptoms when it does produce its specific effects. After the operation, it is, for the most part, unnecessary.

Tartarized antimony.—Nor do I recommend the employment of tartar emetic, powerful as that drug is known to be in subduing inflammation of the mucous tissues. In the swollen and unpliant state of the epiglottis it would not be prudent to excite, or to hazard, vomiting. The contents of the stomach passing upwards would be apt to enter the unprotected larynx, and to cause hurtful, and distressing, and perilous attacks of suffocative cough.

Anatomical characters.—In the examination of fatal cases, sometimes the thickened membrane forming the edges of the rima glottidis is found covered with viscid mucus, which had formed an additional impediment to the passage of air towards and from the lungs: sometimes pus is discovered, lying in the sacculi laryngis, or scattered among the cartilages and surrounding muscles: and sometimes the chief morbid condition is the infiltration of the submucous cellular tissue. The effect in all cases is the same, that of closing up, wholly or partially, the narrow fissure between the arytenoid cartilages. The state of the epiglottis I have several times described.

This very serious disease is a disease of adult age: it is not often known to occur in children. They again are almost exclusively liable to *croup:* and cynanche laryngea has been called the croup of adults. But as the part occupied by croup, and the event of the inflammation, are both different from those of laryngitis, this name, croup of adults, is objectionable. I may remark, however, that sometimes in true croup, the inflammation, besides specially affecting the membrane of the trachea, extends to that of the larynx also.

Causes.—The main exciting cause of laryngitis is exposure to cold, or to cold and wet. My hospital patient, Kent, was a seller of small wares in the streets, and must therefore have been habitually in the way of such causes. The first attack of the disease in Sir J. M. Hayes was brought on by exposure at an open window to the night air for some time, while he was undressed, and in a profuse state of perspiration, with a strong breeze blowing upon him. Dr. Craigie states that young persons from tropical climates, from the West Indies for example, are apt to be attacked by laryngitis soon after their arrival in Europe.

The disease is liable to be produced also by mechanical violence, or chemical injury done to the larynx. It has been caused, on several occasions, in children, by their attempting to swallow boiling water from the spout of a tea-kettle; and life has been saved in such cases by the performance of tracheotomy. The mineral acids, taken as poisons, have excited the disease. Fatal laryngitis has followed the incautious application of ammonia to the nostrils, in cases of hysteria, and of suspended animation: and I once knew

a man nearly killed by the inhalation of the fumes evolved from cinnabar thrown upon a hot iron, in what is called *fumigation* of the throat for venereal ulceration of that part. I am afraid that I must confess also to have once seen acute laryngitis produced by a bungling attempt to introduce the stomach-pump, in a case of poisoning.

Secondary laryngitis.—In all these cases the laryngitis is primitive. But laryngeal inflammation, and especially laryngeal *œdema*, not unfrequently takes place, and proves suddenly fatal in the course of other diseases. I have apprised you that in cynanche tonsillaris, the inflammation sometimes steals onward to the larynx. I have seen two or three cases of erysipelas of the head, attended, as it almost always is, with sore throat, wherein death took place suddenly and unexpectedly, and where the epiglottis, and the edges of the fissure of the glottis, were found to be œdematous: the inflammation of the throat had extended to the cellular tissue beneath the mucous membrane of those parts, and had led to the effusion of serous fluid there. The very same thing is apt to happen in other forms of exanthematous disease attended with sore throat, and especially in small-pox, measles, and scarlet fever. I have known a similar condition of inflammatory œdema arise from a *mercurial* sore throat in a broken down constitution. In these cases the laryngeal affection is consecutive; and in all of them the great remedy is the formation of a sufficient aperture beneath the obstructed glottis. In all of them also the essential symptoms, warranting and demanding the operation of tracheotomy, are the same.

Œdema of the glottis.—A distinction has been made between *laryngitis* and *œdema of the glottis;* and it is a just and real distinction. Œdema of the loose cellular tissue subjacent to the mucous membrane of the glottis is indeed one common consequence of inflammation of that membrane: but it may occur independently of inflammation. The lips of the glottis become tumid and *dropsical;* sometimes (as I have just pointed out) in consequence of a low inflammatory action in the throat, but sometimes also from obstruction of the veins leading from that part. When laryngeal dyspnœa accompanies aneurism of the thoracic aorta, it may, in some instances, result from local dropsy thus produced; and then tracheotomy is fully justifiable, and indeed demanded.

The main practical difference between mere œdema glottidis and acute laryngitis, is this; that in the former, there being no fever or inflammation, blood-letting is not requisite; and the operation of bronchotomy becomes the sole resource to which, in the extremity of danger, we can look for help. Mere œdema glottidis is seldomer attended with dysphagia too, than is laryngitis: yet, if the epiglottis be involved in the œdematous swelling, and unable to shut over the glottis, the act of swallowing will be followed by strangling cough, and increased dyspnœa.

Chronic affections of the larynx.—Besides the affections which I have now described or referred to, the larynx is liable to *chronic* disease: to chronic *inflammation;* chronic *thickening* of the membrane; slow *ulceration;* necrosis of its cartilages. Chronic inflammation and ulceration of that part is very common in *consumptive* patients. It is attended first with hoarseness, then with aphonia, a barking or stridulous cough, and all the melancholy accompaniments of tubercular phthisis. There has accordingly been a species of phthisis spoken of as *phthisis laryngea.* But in most, if not in all cases, this laryngeal affection is only a part of the complaint under which the patient labours; and what I have farther to observe respecting it, I shall postpone until we come to the consideration of tubercular consumption.

Again, the membrane lining the laryngeal cartilages is not unfrequently thickened and ulcerated in *secondary syphilis:* giving rise to a hoarse croaking voice, and a noisy and painful breathing. In such cases, or in chronic thickening of the same parts from common inflammation, you may do great good by *gently* introducing mercury into the system, until the gums rise. I have again and again seen the uneasiness about the throat, the noisy respiration, the rough or whispering voice, all cease, as if by enchantment, as soon as the specific influence of the mercury became manifest. There was a woman who used to apply at the Middlesex Hospital for an affection of this kind: whether it was syphilitic or not I could not well determine, but she lost it under the employment of mercury, two or three times: the complaint returning again after the interval of a few months,

upon the reapplication of some irritating cause. In another female patient, who was long under my care in the hospital with similar symptoms, every thing failed to give her permanent relief, till I began to leech the neighbourhood of the larynx repeatedly. She had four leeches applied, I think, every night, and then every other night, for a fortnight or three weeks; the hoarseness or difficulty of respiration gradually diminishing all the time, until at length the perfect use of the instrument of the voice was restored. It is often necessary, in these cases, while using local depletion, or mercury, to uphold the strength of the patient by nourishing but unstimulating diet: and it is always expedient that the organ should be kept, as much as possible, in a state of repose.

It is said that a little practice will enable a person to pass his finger into a patient's throat, and to familiarize his sense of touch with the ordinary condition of the upper part of the respiratory apparatus, so as to be able to detect swelling, or irregularity, or thickening about the chink of the glottis. And great advantage is said to have been obtained from applying remedies directly to the diseased or irritable part. This practice was much followed by the late Mr. Vance, who had been for many years a naval surgeon; and he called it, in naval phrase, *swabbing* the affected organ. A small piece of sponge, secured with a string, or fastened to the end of the finger of a glove, is dipped in a strong solution of nitrate of silver, and then carried down into the throat, as far as the spasmodic state of the muscles which the attempt induces will permit, and pressed downwards against the superior surface of the larynx. I believe other stimulating applications are sometimes employed in the place of the nitrate of silver. Now of this method of cure I do not know much, except by report. I have heard that many cases of chronic hoarseness and cough have speedily been cured by it. But I have more than once had what seemed satisfactory evidence of the beneficial effect of this expedient. The man Kent, whose case I have related, gradually regained the power of easy breathing through the natural passage, and the opening, which Sir Charles Bell had made, closed up perfectly. About a week after this took place, he began again to respire with a wheeze almost as audible as that which existed at the time of his admission; and to speak in a hoarse voice; and a night or two after the return of the wheezing, he had a paroxysm of extreme dyspnœa. I began to be afraid that the whole process of laryngotomy and the metallic tube would be again requisite. However, I got Sir Charles Bell to examine the interior of the throat, and we agreed that it would be advisable to swab the epiglottis and upper part of the air-passages with a strong solution of lunar caustic. For he had no fever, and we thought it probable that the membrane might have been left lax, and in a state to be benefited by astringents. Sir Charles applied the sponge with very little difficulty; and the next day the breathing was greatly improved, and the hoarseness almost gone: and he never had from that time any recurrence of troublesome dyspnœa.

Mr. Arnott has twice or thrice, at my request, swabbed the upper part of the larynx for intractable hoarseness and aphonia: but with no good, nor any bad consequence.

The lining membrane of the larynx is liable also to warty growths, which impede the entrance and exit of air, and ultimately destroy life. There are several examples of that kind on record. I extract the following from my note-book:—

George Tenon la Font, aged 11, admitted March 4, 1828. He speaks in a whisper; complains of difficult breathing, and of cough. Inspires with a loud wheeze. Coughs with a sort of whistling sound, as through a narrow tube. The cough is most troublesome at night. Expectoration mucous, and inconsiderable in quantity.

Has been ill, in this way, all the winter—having had hooping-cough in the preceding autumn. There are marks of cupping on his throat. Little can be heard in the chest, the loud wheeze of his respiration obscuring all other sounds. In about a fortnight his gums were brought under the influence of mercury. No perceptible improvement ensued. A careful examination was again made of the thorax, and the conclusion arrived at was, that the obstacle to his respiration lay in the larynx, or upper part of the trachea, and that the lungs themselves were not concerned. After this, a blister to the throat, a seton near the thyroid cartilage, small doses of ipecacuan, emetics, and iodine were successively tried—but in vain. Towards the end of the month he began to suffer occasional, very violent, and apparently spasmodic attacks of extreme dyspnœa. He died during the night, two months after his admission. For some days before he had been manifestly

worse than usual, was more feeble, wandered somewhat, and complained that his vision was imperfect. No noticeable increase, however, had taken place in the difficulty of breathing, except during the paroxysms of aggravation already mentioned. The death was sudden, and probably took place in one of these paroxysms.

When the body was examined the lungs were found sound as to structure, but copiously infiltrated, especially on the left side of the thorax, with serous fluid. At the very top of the larynx, involving the base of the epiglottis and the vocal cords, was a considerable warty growth, closing the rima glottidis almost entirely. The excrescences sprung *chiefly* from one continuous base, and branched out precisely after the manner of what is regularly called a seedy wart. There were, however, several distinct smaller growths or warts: the main excrescence, having several heads, passed upwards from and through the rima, and so came to act partly as a valve during inspiration, which was always sensibly more difficult than expiration.

Ought tracheotomy to have been performed in this case? I now think so. But supposing it to have been done, and to have been successful, the boy would have been under the necessity of breathing through a tube for the remainder of his life.

There are two excellent specimens of warty growths in the larynx upon the table before you.

I might have referred, when speaking of chronic enlargement of the tonsils, in the last lecture, to the case of a little boy at present in the hospital under the care of one of my colleagues. He was brought to the hospital on account of great dyspnœa, and a hissing respiration, produced apparently by two enormous tonsils. With some difficulty (arising from his unmanageable age) a large part of one of the tonsils was cut off with scissors: but after the operation, (whether from any fresh swelling of the parts, or from pressure made by the remaining tonsil, which grew downwards, I understand, into the throat,) his difficulty of breathing became extreme; and it was thought necessary to perform tracheotomy, which afforded him signal relief. He breathed for some time through the artificial opening in his windpipe. At length the other tonsil was partly removed: and the child is now well, and about to be discharged.

LECTURE XLVI.

Cynanche Trachealis: Symptoms: Pathology: Prognosis: Treatment. Child-crowing, or spurious croup.

I proceed this afternoon to another of Dr. Cullen's species of cynanche; the last that I propose to consider in this part of the course: viz. *cynanche trachealis—tracheitis—croup.*

The essence of this complaint is violent inflammation, affecting the mucous membrane of that portion of the air-passages which lies between the laryngeal cartilages and the primary bronchi; in one word, of the trachea, or *wind-pipe.* That is the genuine seat of the disease: but the inflammation sometimes ascends into the larynx; and not unfrequently it dives into the bronchi and into their ramifications.

Cullen makes no distinction between cynanche trachealis and cynanche laryngea. Yet they are separated from each other by very definite boundaries. They differ in anatomical position: they differ in gravity. Both indeed are serious diseases, but croup is the more serious, because it seldom admits of that mechanical relief which, when rendered in time, deprives cynanche laryngea of its dangerous character. The two disorders differ also in respect to the period of life at which they occur. Idiopathic laryngitis is seldom met with except in adults; croup seldom after the age of puberty. Cynanche trachealis is indeed a very remarkable disease, for it exhibits an event of inflammation which does not usually

belong to that process when it affects the *mucous* tissues. In this too it differs from laryngitis.

I say that croup is peculiarly a disease of early life. The interval that lies between the two periods of *weaning* and *puberty*, is the time during which its visitation is chiefly to be apprehended. Comparatively few cases of it occur during the first year of infantile life. There are more in the second year than in any other. This, in all probability, is connected with the change that ensues in regard to diet, upon the child's being weaned. Dr. Cheyne, whose experience of croup was very extensive, says, that the younger children are when weaned, the more liable are they, *cæteris paribus*, to this malady. From the second year onwards the number of children affected with croup gradually decreases. Of ninety-one instances referred to by Jurin, one only was after the tenth year. But it does occasionally happen subsequently to the period of puberty, and up to the twenty-first year; and even later. A girl of nineteen, one of my hospital patients, died of it. Some of the cases recorded of croup in the adult were probably in reality cases of laryngitis.

It is curious that inflammation should thus, at different epochs of life, fix itself upon limited portions of the same continuous surface, and give rise to consequences so diverse. We are unable to give any account of this.

Symptoms.—Cynanche trachealis is frequently preceded by a slighter and more diffused affection of the membrane lining the air-passages. The child has what is popularly called a cold; sneezes, coughs, and is *hoarse*. Now with respect to this last symptom Dr. Cheyne makes the following practical remark. Hoarseness (he says) in very young children, does not usually attend common catarrh. When noticed in a district where croup is not unfrequent, it ought to put the parents or the medical attendant of the child upon their guard; especially as much depends upon the early treatment of the disorder. With these symptoms the child is feverish and fretful, and does not sleep well. In the course of a day or two the signs peculiar to croup begin to show themselves: they are well stated by Cullen, in his definition of the complaint. "Cynanche, respiratione difficili, inspiratione strepente, voce raucâ, tussi clangosâ, tumore fere nullo in faucibus apparente, deglutitione parum difficili, cum febre synochâ." These are the phenomena that characterize croup. Difficulty of breaching; and sonorous inspiration. This last is often almost enough, of itself, to identify the disease. Hoarseness; a gruff voice; sometimes a total loss of the power of vocal speech. A very peculiar and distinctive cough, to which the epithet "brassy" has been justly applied; the noise resembling that which would be occasioned by coughing through a brazen trumpet. This remarkable sound is always easily recognized when it has been once heard. It is a *ringing* cough; and the expiration has a ringing character; and either of these, the cough or the expiration, is followed by a loud *crowing* inspiration. Then there is the negative symptom; the absence of any difficulty of swallowing: and with all this, inflammatory fever; a flushed face, hot skin, a frequent hard pulse, thirst.

However, it is by taking the symptoms collectively that we judge of the existence of croup, and by the rapid progress of the disease; rather than by any particular or pathognomonic sign. Some of the symptoms may occur, separately, when there is no croup. The brassy or metallic cough, for instance, has been known to accompany some chronic affection of the larynx. Dr. Gregory—the late Edinburgh Professor of Physic—knew a man with a venereal disease of the throat who coughed so exactly the cough of croup, that he was admitted into the clinical wards of the infirmary every session for some years, that the students might have the opportunity of *hearing* this peculiar sound. So also the remarkable crowing inspiration may take place, as we shall soon see, without croup.

In the outset the fever generally runs high; and it is of importance, as respects the diagnosis, to mark the presence or the absence of pyrexia. As the obstruction to the passage of air increases, the blood ceases to be duly arterialized; and then, of course, the skin grows dusky, the pulse feeble and irregular, the extremities cold. The cough also, as the malady thus goes on from bad to worse, ceases to be loud and clanging, becomes husky, and inaudible at a short distance, and the voice sinks into a whisper; the head is thrown back; the nostrils, in perpetual motion, dilate widely; the face is pale and livid, and sometimes bloated; the pupils often expand. When these indications of sinking have come on, the case usually terminates ill: the bottoms of the feet turn black and hard; drow-

siness supervenes; some tossing of the arms perhaps; the breathing becomes gasping and interrupted, and the child dies after an *in*spiration.

In other cases the croupy symptoms make their attack very suddenly. A child shall go to bed apparently well, and in the course of the night have all the worst signs of the disease. And it is observable, that whether the attack be altogether unexpected, or whether it has been preceded by hoarseness, sore-throat, and catarrh, it usually comes on *in the night*.

Croup resembles laryngitis in this respect, that it runs its course rapidly: proving fatal sometimes within twenty-four, and often within forty-eight hours. It may, however, continue for five or six days before it terminates, whether death or recovery be the result. Dr. Craigie affirms that it is never protracted beyond the *eleventh* day; the fatal or the favourable issue having always taken place by that time. Life is destroyed, in pure circumscribed tracheitis, by the accumulation in the windpipe of a concrete membrane-like substance, which so frequently attends this disease, and is so peculiar to it, that it is called *the membrane of croup*. In cases of recovery this substance has been expectorated in the form of a nearly perfect tube, representing a cast of the trachea; at other times it is coughed up in flat or tubular fragments. In fatal cases it is found sometimes lying in close contact with the mucous membrane, and sometimes quite detached from it: so that it might have been expelled without much force or difficulty, if the child could have sufficiently inflated its lungs, and the requisite muscular power had remained, and spasmodic irritability of the glottis had not opposed.

But, in many instances, this albuminous exudation is not confined to the trachea. It often stretches down to, and enters, the ramifications of the bronchi, and reaches even to their termination in the pulmonary vesicles. Sometimes also it is found clothing the mucous membrane belonging to the laryngeal cartilages. This Dr. Craigie denies. But I show you two specimens in which the false membrane, besides filling the trachea, evidently extends into the larynx. One of these comes from the museum upstairs: the other I have borrowed from the Middlesex Hospital museum: it is the larynx and trachea of the young woman whom I mentioned just now as having died there of croup at the age of nineteen. The false membrane reached from the tip of the epiglottis to the bifurcation of the trachea. You see the same thing represented in this excellent plate of Dr. Carswell's, pictured from nature. Usually the adventitious membrane commences just below the larynx, where it is thin and soft: about the middle of the windpipe it is more dense and firm; lower down in the trachea, and in the bronchi, it is generally looser again, pulpy, and broken: it sometimes, I repeat, penetrates to the very air-cells. What are called (absurdly enough) bronchial polypi, branch-like casts of the smaller ramifications of the air-tubes, are then apt to be coughed up. And even when this concrete substance is not formed, we have other evidence, often, of the extension of the inflammation throughout the whole downward course of the membrane.

On the other hand, there are a few cases in which the adventitious membrane is not formed at all; the inner surface of the windpipe is seen to be merely reddened and tumid, and covered with viscid mucus; or perhaps with a shred or two of concrete albumen here and there.

The difficulty of breathing, and the characteristic sounds that accompany it, depend, in part, I believe, upon spasmodic contractions of the small muscles of the larynx: for remarkable aggravations of the dyspnœa are apt to occur, and to subside again; and these aggravations are sometimes brought on by sudden causes—by the movements of deglutition, for example.

I shall have to recur to this spasmodic constriction of the glottis: but I may here remark, that because it has not been (and cannot be) *seen*, doubts have been expressed by some about its having any thing to do with the dyspnœa. Such doubts seem scarcely reasonable. It is easy, at any time, by an effort of the will, to close the glottis, and to prevent the passage of air to and from the lungs. This is mainly effected by the action of the little muscles that bring together the arytenoid cartilages. But those muscles, like the other muscles concerned in respiration, act also independently of the will, spasmodically therefore, through the reflex function of the spinal cord. And it is by a providential and conservative arrangement that they do so act, as janitors, admitting, in the healthy state,

the vivifying air, but barring the door against certain hurtful gases, and against solids and liquids which would be injurious to the respiratory apparatus. We know that if a drop of water, or a crumb of bread, or a whiff of carbonic acid gas, gets past the outer defence, the epiglottis, and into the larynx, spasmodic action of the little muscles in question is instantly excited. We cannot see these intruders, and voluntarily resist their entrance, but the unsleeping sentinel is there to guard the passage. We may well conceive, therefore (and I know not how the supposition can be disproved), that the noisy and difficult respiration of croup may be caused, in part, by spasm.

The presence of the adventitious tubular membrane in the trachea affords a plausible explanation of one of the symptoms observed in these little patients; the tendency they show to throw the head back. The cylinder of membrane is kept open in that position; whereas, if the head were inclined at all towards the chest, the membrane would be bent upon itself, and the passage through it obstructed.

This concrete exudation is often adduced to prove that the mucous membrane may exhibit, under certain circumstances, the phenomena of adhesive inflammation. Similar films sometimes form upon, or are thrown off by, the mucous surfaces of the intestines, and of the uterus. Whether they are to be regarded as essentially identical with the layers of coagulable lymph poured forth in inflammation of the serous and cellular tissues, may be made a question. There are certainly some strong points of distinction between them. The concrete membrane of croup is more brittle, less fibrous, more decidedly albuminous, than the false membranes that cover the inflamed pleura, pericardium, or peritoneum. A still more remarkable difference is this, that it is not *plastic*, in the sense in which that term was formerly explained; it never becomes organized, never connects itself by blood-vessels with the surface from which it proceeds. On the contrary, it is partially detached; and by degrees, if the patient live long enough, it is completely separated from the subjacent parts.

Hypotheses have been framed to account for the limitation of this product of tracheal inflammation to the early periods of life; and for its variation from the usual products of inflammation of the same part. Dr. Stokes thinks that the predominance of the white tissues in young subjects may explain the greater frequency of croup, with its peculiar membrane, in infants. Dr. C. B. Williams starts the very reasonable supposition that the inflammation involves the submucous cellular tissue, which is abundant during youth; and that the natural product of this phlegmonous inflammation transudes readily through the thin, simple, and delicate mucous membrane proper to that age.

The formation of this adventitious membrane, and even its renewal, appears to be sometimes very rapid. I have here a preparation made by the late Dr. Sweatman, illustrative of this. It now belongs to the Middlesex Hospital Museum. Upon a child, on the very brink of suffocation from croup, the operation of tracheotomy was performed, at one o'clock in the morning, by Mr. Chevalier. A tubular portion of membrane, of the shape and size of the trachea, was presently forced through the artificial opening. Immediately the child's respiration became easy, and it fell asleep. In the course of the same morning, Dr. Sweatman was hastily summoned, and arriving at eight o'clock, found the child dead. It had slept six hours, and upwards; and then the distress of the breathing had returned, and was soon fatal. The trachea was found to contain a new tube of lymph, or of concrete albumen. The preparation shows that such a membrane may re-form in that short space of time; namely, in from six to seven hours.

The croup is not contagious; although, like cynanche tonsillaris, and for the same reasons, it is found sometimes existing at the same time, or in quick succession, in more than one child of the same family. Thus two twin children of Dr. Gregory's were seized with croup on the same night. They had both been walking in the evening on the seashore during a cold wind. This is in accordance with what Dr. Cheyne has stated, that the attack is almost always nocturnal, and often when the child has during the preceding day been exposed to the weather. It frequently occurs sporadically; but there are places in which the disease appears to be endemic. Dr. Cheyne found it so on the coast of the Frith of Forth. Indeed, the first distinct account of it that we possess was drawn up by Dr. Home, of Edinburgh, in 1765, from much personal observation of its ravages in Leith and Musselburgh. Cold situations, and damp places, more than such as are merely cold,

are subject to the prevalence of this disease. It is accordingly frequent in the seasons of winter and spring. It is said to be most common near the sea-shore, and in the neighbourhood of large bodies of water generally. It occurs in low, moist, what are called in Scotland *carse* districts, more than in upland situations that are more exposed to cold winds. It is more common at Leith than it is in Edinburgh: and in Edinburgh, it is most frequent in the lowest parts of the town. This I learned from Dr. Alison, who, having long been physician to a dispensary there, had had ample means of observing the disease. Canal Street, and the Cowgate, both low spots, as some of you may know, have long been famous, or rather infamous, for cases of croup. Towns situated on the banks of rivers are more than commonly visited by it; and it has been observed to be particularly frequent among the children of washer-women in such places; and thus evidently connected with exposure to moisture. It has been known to prevail epidemically in towns so situated, after an inundation. And Dr. Alison has made a very curious remark respecting it. He says that it seems to be often produced by the child's sitting, or sleeping, in a room newly washed: and that he has frequently known it to occur on a Saturday night—the only day in the week on which it is customary for the lower orders in Edinburgh to wash their houses.

Like cynanche tonsillaris, and unlike cynanche parotidæa, the croup is exceedingly apt to recur. Relapses may happen within a few days after apparent recovery: and these are very perilous. But besides this tendency to a renewal of the severer symptoms, the little patients are often affected with cough, and hoarseness, and even with aphonia, for a long time. And while these relics of the acute attack continue, it is easily brought back again. The first seizure is generally, I believe, the worst: but to this rule there are numerous exceptions.

Prognosis.—Croup is a disorder which justly excites extreme alarm in the friends and parents of the patient: for the prognosis can never be better than doubtful. It is said that four children out of five attacked by it used to die: but that now, the treatment being better understood than formerly, the number of deaths and the number of recoveries are nearly equal. We judge of the probable issue, in a given case, by the apparent circumstances and progress of the malady. If we could see the interior of the air-tubes, we should know that the chance of escape was small, in proportion as the inflammation, and its albuminous product, descended along the ramifications of the bronchi. But in these little patients, and amid the tracheal noises, it is difficult to ascertain the physical state of the lungs. The prognosis is chiefly to be collected from the general condition of the child. If the distress of breathing seems to remit, and free expectoration to come on, while the strength is yet entire, we venture to hope. On the other hand, we begin to despair when the lips are becoming blue, the skin is losing its heat, the pulse is already feeble and intermitting, and the little patient is drowsy or comatose: in other words, when we perceive the final symptoms of death in the way of apnœa. Some few patients die suddenly and unexpectedly without any previous coma.

Treatment.—The mortality will differ according as the disease is detected early, and treated vigorously—or otherwise. And with respect to treatment, there is no specific remedy for this, any more than for any other inflammation. We must put in force the general principles upon which the treatment of inflammation is founded; adapting them, however, to the malady in question by those particular facts which the experience of the best observers have collected for our guidance.

I need scarcely say that where cough and catarrh, and especially hoarseness or loss of voice, are noticed in a young child, he should be narrowly watched, and protected against all circumstances likely to excite or to aggravate inflammation: he should be kept in the house, and put upon farinaceous diet; and the functions of the bowels and of the skin should be attended to.

The three remedies that most require consideration are blood-letting, tartarized antimony, and calomel.

Bleeding is to be unhesitatingly employed when the patient is strong, and plethoric, and seen in the outset of the disease. In judging of its mode, and of its amount, we must recollect that what is no more than a topical bleeding in an adult, becomes equivalent, in its effect upon the system, to general bleeding, when it is used for a very young child.

Abstraction of blood, by venesection or cupping in the case of older children, and by leeches in the case of infants, should be practised whenever the symptoms are violent, and there is much fever, and the patient is seen within a few hours after the commencement of the symptoms. The relief that is given by this measure, under such circumstances, is often so decided, that no doubt can remain of its usefulness and propriety.

It is impossible to lay down any fixed rules for the quantity of blood that should be taken in this complaint. Under two years of age, it should not, says Dr. Cheyne, exceed five ounces. I should esteem that a *large* bleeding at that age. Upon an average, a moderate bleeding will be produced by the application of a couple of leeches to an infant in its first year: and an additional leech may be employed for every additional year: so that six may be put to a child five years old; or eight if he be stout. Dr. Copland estimates the amount of blood which these patients may, with safety, bear, to be somewhat more than an ounce, or as much as an ounce and a half, for every year of their age. Much, however, must depend upon the special circumstances of the case: the quantity of blood extracted by a given number of leeches is less in one instance than in another; and then, of course, the number must be increased. They should be applied at the upper part of the sternum, and not upon the throat itself; for this reason:—that the pressure which may be necessary to stop the bleeding, or to regulate its quantity, cannot well be borne upon the throat in these cases.

After one sufficient evacuation of blood, whether by means of the lancet, or of leeches, or of cupping glasses applied between the shoulders, it will always be right, before repeating it, to ascertain the effects of other measures; such as emetics and purgatives, the beneficial operation of which in this disorder is often very remarkable.

Full vomiting sometimes affords relief so sudden and so complete, as to lead to the persuasion that the symptoms had been principally owing to spasm. And even when the disease is unequivocally inflammation—sometimes even late, but particularly in the early part of its course—the effect of a vomit is often very striking. It promotes expectoration; and is not unfrequently followed by the expulsion of shreds of the adventitious membrane. When blood-letting is employed, it should precede the emetic; or, at any rate, it should precede the act of vomiting. Dr. Cheyne recommends that the bleeding should be practised ten minutes after the emetic has been swallowed. The loss of blood assists the operation of the emetic, and lessens the risk (which is not a fanciful one) of injurious congestion of the vessels of the head during the straining efforts of vomiting.

It is desirable, not simply to excite the act of vomiting, but to produce and to prolong a state of nausea and faintness; and so to depress and to keep down the increased action of the heart, and to keep empty the capillary vessels of the inflamed part. Now the substance best adapted to this purpose is tartarized antimony. This medicine, as I have often mentioned before, has great power over the inflammation of the mucous tissues: and there is one very great advantage belonging to it in cases of croup; namely, that children may be induced to take it without their knowing that they are taking medicine; for the solution of it has little or no taste: whereas the struggling which is often occasioned by the administration of other emetics may be the cause of much inconvenience, and even of much injury to the patient. It should be dissolved in boiling water, in the proportion of a grain to an ounce: and the cold solution given. A tea or a dessert-spoonful may be repeated every quarter of an hour till some effect is produced. When vomiting in thus excited on the very first appearance of the symptoms, and before the disease seems thoroughly formed, it sometimes puts it off; so that no other treatment remains necessary beyond the exhibition of some purgative medicine. But if this perfect relief does not ensue upon the operation of the emetic, Dr. Cheyne advises (and this is in conformity with the practice of many other persons) that a powder, consisting of two, three, or four grains of calomel, with two or three grains of James's Powder, should be given at short intervals; every two or three hours for example. A dose of castor oil is to be administered occasionally, to clear the bowels. And another expedient, of great efficacy sometimes, and therefore never to be omitted, is the warm bath. This is often properly resorted to just after the act of vomiting, particularly if any tendency to perspiration is apparent. The temperature of the water should not be lower than 98° Fahrenheit; and the child should remain in the bath for ten minutes at least. When taken out, he should be wiped dry, and put imme-

diately into bed again. The change for the better produced by the bath is sometimes so marked and so speedy, as to strengthen the conclusion that the most distressing of the symptoms had resulted from spasm.

The usual effect of calomel thus frequently repeated is not, as in adults, that of causing salivation, but the discharge of a quantity of green fæcal matter, resembling chopped spinach: and when stools of this kind begin to make their appearance, there is often a sensible mitigation of the symptoms. The green colour is a common consequence of mercury given to young children; and will occur, I believe, whatever be the disease, when the full effect of calomel as a purgative is obtained. The green matter has been found, after death, in all the intestines, small as well as large, up to the duodenum. I presume that the colour is owing to some chemical action that takes place between the calomel and the bile. We know that calomel does tinge bile green when mixed with it out of the body. It may be, however, that the calomel provokes a flow of altered bile.

Calomel, thus administered, is the purgative that has received the strongest recommendations. Its usefulness appears to have been fully borne out by the test of experience: and the well-known virtue belonging to mercury, of preventing or arresting the effusion of coagulable lymph in other textures, has formed (I conceive) one cogent reason for its adoption in this disorder, of which the chief peril results from the pouring forth of the albuminous part of the blood. But whether mercury really has the same power of controlling adhesive inflammation, when that process is set up in mucous tissues, which are so commonly exempt from it, may be questioned. On the other hand, the effect of full doses of tartar emetic in restraining active inflammation of those tissues, is well ascertained: and I should certainly make use of it in the early stages of this dangerous malady. The system can be brought to feel its decided influence with much more certainty, and in a much shorter space of time, than that of calomel: and if it fails to make a beneficial impression, it need not long interfere with the mercurial treatment. Let me quote to you the statement of Dr. Cheyne (whose experience of this disease was far ampler than mine has been) respecting the efficacy of tartarized antimony in what he calls the second stage of croup. He recommends that half a grain, dissolved in a table-spoonful of water, should be given to a child two or three years old, every half hour, till sickness and vomiting ensue. In two hours after the last effort of vomiting, the same process is to be recommenced; and so repeated while the symptoms require it, and the strength will permit. This mode of treatment was suggested to him by the accidental observation of a particular case, in which it was remarkably successful. From that time he placed his whole reliance on that remedy in the second stage of croup; especially as he had found that blood-letting in that stage only accelerated the death of the patient. He noticed that the cases were *few* in which he had known children survive the second state, but in *all* of these few, they recovered while using a solution of tartarized antimony. He held that no other medicine was, at that stage of the disorder, entitled to confidence. In short, he declared that tartar emetic, so given as to produce continued nausea, had been his sheet-anchor, in the treatment of croup, since the year 1799. This was written in 1801, in a separate work on the pathology of the larynx and bronchi; and Dr. Cheyne recently has affirmed, in the *Cyclopædia of Practical Medicine*, that he still found reason to adhere to the same opinions and the same practice.

Now what is good for the second stage, would, *à fortiori*, I think, be likely to save life, if employed during the earlier stage of the disease. I therefore should say, take blood in the very outset, as largely as may seem prudent: then give the tartar emetic solution in the way already described. As soon as it causes vomiting, and pallor, and a sinking of the pulse, stop; and suffer the heart to recover itself. And if, with the rallying circulation, the difficulty of breathing returns, have recourse again to the same remedy. The faintness and collapse are sometimes so great as to threaten the extinction of life: the child, with a flying pulse, and a clay-cold surface, seems gasping its last. When this happens, a few drops of sal volatile, or of brandy, mixed with water, will presently bring the little patient round again. If no ground is gained after two or three repetitions of the nauseating treatment, then it will be well to make trial of the calomel plan.

Sometimes the tartar emetic acts severely on the bowels: it may occasionally therefore be necessary to combine it with a small quantity of syrup of poppies.

Blisters are often applied in this disease; but with very questionable propriety. In the outset they are likely to do harm: in the advanced periods they are not likely to do good. When used at all, they should be placed, not on the throat, but across the upper part of the sternum.

When signs of approaching death have come on—lividity of the lips, coldness of the skin, and a tendency to stupor—the question *will* obtrude itself whether there may not still be a chance of saving the patient by performing tracheotomy. In the first place you will consider that the operation is even more difficult to execute upon children than upon adults; and is attended with more perplexing hæmorrhage. But there is a greater objection than this to tracheotomy in such cases—an objection which you will have anticipated —namely, the existence of the preternatural membrane; which often extends so far down, that air would not be admitted into the lungs, even when the incision into the windpipe was made at the lowest possible point. Another consideration, forbidding much hope of success from this expedient at any part of the disease, is that the ramifications of the bronchi and the ultimate air-cells get filled up with serous, or mucous, or puriform matter, or even sometimes with a membranous exudation, whereby suffocation is effected *in the lungs themselves*. The membrane in the trachea, being tubular, does not entirely exclude the air from those organs; but it does not admit it in sufficient quantity. Bronchotomy has again and again been practised in this complaint, to no purpose: and I should be inclined to look upon it as absolutely hopeless, but for two instances of its successful performance, recorded in the *Medico-Chirurgical Transactions;* the one performed by Mr. André, and related by Dr. Farre, in the third volume; and the other by Mr. Chevalier, in the sixth volume. These were both apparently desperate cases. Immediate relief followed the operation in both, and the patients recovered perfectly.

There seem to be just two predicaments in which there is a chance that tracheotomy may be useful. They are perhaps rare: yet they have been noticed by several observers. The one is where the preternatural membrane extends but a very little way down the trachea, and is chiefly confined to the larynx: and the other is where there is no preternatural membrane at all, or only a slight coating in some part of the trachea, the impediment to the breathing having arisen mainly from a thickening of the mucous membrane. And you will observe that an impediment from this cause will always be the greatest at the narrowest part of the canal: and therefore incision of the windpipe in such a case may be expected to bring relief. The effect produced by the tracheotomy in Mr. Chevalier's case was very instructive. Air was fully inspired through the opening, then a strong cough took place, by which a large quantity of viscid reddish mucus was forced out by the natural channel, through the glottis. It was evident that the child could not expectorate before, simply because it could not sufficiently fill its lungs with air to drive the collected mucus out. Dr. Farre gives a circumstantial account of a case in which the adventitious membrane did not reach more than a finger's breadth below the cricoid cartilage; and the rest of the tube was so free that he was convinced that the child's life might have been saved by a timely opening into the trachea. Unfortunately, we cannot tell, before death, to what degree or extent the preternatural membrane exists. All that can be said, I think, is, that when dyspnœa and much croup come on suddenly or quickly, the disease is probaby limited to the larynx and upper part of the windpipe: but that when the progress of the disorder is slower, and the croupy symptoms not so well marked, it is more likely that a greater extent of the trachea, below the larynx, participates in the mischief. Our expectations of success from tracheotomy will vary accordingly. It affords a bad chance at the best; but it affords, also, in many cases, the *only* chance.

There is a sort of bastard croup, with which it is quite necessary that you should be acquainted, for it is not at all uncommon; nay, it is far more common, in this place at least, than the real disease. It has received a variety of names, which shows that it has been recognized, as a distinct malady, by various observers. Yet no doubt can be entertained that it has very often indeed been confounded,—and is still continually confounded, —with the true croup, with cynanche trachealis. In their most obvious symptoms the two affections are much alike. The broad and essential distinction between them, is the absence, in the spurious disorder, of inflammation and of fever—and consequently of any concrete or other effusion from the mucous membrane of the air passages. The child is

seized all of a sudden, roused perhaps from its sleep, by a catch, or interruption of its breathing, more or less complete. It strives and struggles to inspire, but is apparently unable to do so; at length the effort is successful, and the breath is drawn in with a shrill whistling or crowing sound, like that which characterizes the inspirations of croup, or of hooping cough, and depending, no doubt, upon the same cause—a narrowing (in this complaint temporary) of the fissure of the glottis. *Spasmodic croup* is the most common of its names. It is the *thymic asthma* of the Germans. My late colleague, Dr. Ley, who, in a work published a short time before his death, has done much to elucidate the pathology of this curious disorder, adopts from Dr. Mason Good the appellation of *laryngismus stridulus*. Dr. Gooch called it *child-crowing*, a homespun term which I much prefer to the somewhat pedantic and cacophonous title bestowed upon it by Dr. Good. The crowing noise, and its concomitant phenomena, take place in paroxysms, which vary in respect to frequency and severity, and which are separated by intervals of easy and natural breathing.

"When the closure of the chink of the glottis is not perfect, the child struggles for its breath: the respiration is hurried; the countenance generally bluish or livid, the eyes staring; and each inspiration is attended with a crowing noise. When the closure is more complete (and this state was found by Dr. Ley, whose words I am now quoting, to be much the most frequent at the commencement of the paroxysm) the function of respiration is entirely suspended for a while; there is an effectual obstacle to the admission of air. The child makes vehement struggles, by some termed convulsive, to recover its breath. At varied intervals, from a few seconds up to a minute, or upon some occasions nearly two minutes, air is at length admitted through the glottis, now partially open; and this rush of air, passing through a very narrow chink, produces the peculiar sound. To these symptoms not unfrequently succeed a fit of coughing or crying, which terminates the scene: or, if the glottis be not thus partially open, the child, at the end of from two to three minutes at the utmost, will die suffocated. Pallid and exhausted, it falls lifeless upon its nurse's arms; and it is then that the child is generally said to have died in a fit."

Sometimes, but not always, with the symptoms now described there is a contracted state of the flexor muscles of the thumb, fingers, wrist, ankle, and toes; giving to the foot an appearance approaching to that of club-foot.

Now most of those persons who had learned not to confound this child-crowing with true croup, have been of opinion that it was connected with *cerebral* disease, or disorder; and that the state of the glottis, and the flexion of the hands and feet, both depended upon tonic spasm. Dr. Ley doubts the correctness of that notion, and believes that the bending of the limbs results rather from feebleness or paralysis of the extensor muscles, than upon spasmodic contraction of the flexors. It is obviously a great point to make out, whether the disorder depends upon pressure *within the head* or not. Our treatment will be regulated by what we know, or believe, in that respect. But what is chiefly original, and very interesting, in Dr. Ley's views concerning this bastard sort of croup is this, that he attributes the temporary closure of the glottis to pressure made by enlarged glands in the neck or chest upon the recurrent nerve, or upon some part of the eighth pair; "subverting the exact antagonism by which the glottis is automatically and involuntarily kept open, and allowing its margins to come together, and to occasion the peculiar kind of inspiration so much like that of croup." Dr. Ley looks upon the affection altogether as more allied to paralysis than to convulsive movement. This certainly is a very original view of the matter; but I must refer you to his book for the facts and reasonings upon which it is founded. The important practical fact is the connection of the child-crowing with tumefaction of the glands in the neck and chest, and with the entanglement of the pneumogastric nerve or its branches among these glands. "Scarcely an instance (says he) has occurred to me since my attention has been very much directed to the subject, in which there has not been the strongest foundation for the belief that either the glandulæ concatenatæ of the neck or the thoracic absorbent glands, have become morbidly enlarged."

Even in the short period that has elapsed since Dr. Ley wrote, this curious and intricate knot has been somewhat farther (though it is not yet completely) untwisted. Dr. J. Reid has ascertained, by a well-contrived set of experiments, that the inferior laryngeal (or recurrent) nerve, is an efferent or motor nerve, by which nearly all the movements of the larynx are regulated; and that the superior laryngeal is an afferent or incident nerve. We

may easily conceive, therefore, how pressure upon, or irritation of either of these nerves, may affect the aperture of the glottis. If the superior laryngeal nerve be implicated, the impression is communicated to the spinal cord, and thence reflected, through the recurrent, upon the laryngeal muscles. And it seems probable that afferent fibrils of the fifth pair of nerves may have a similar exciting power; for a transient crowing is readily produced in some children, by exposure of the surface of the face and chest to a breeze of wind, or by their being suddenly tossed in the arms of a nurse. We may even suppose that the effect upon the central cord may, by reflection, influence other muscles, and cause the contractions that are sometimes observed in the flexors of the feet and hands. Again, if the recurrent nerve itself be pressed upon, or interfered with, undue contraction or paralysis, according to the kind or degree of interference, will be likely to ensue of the muscles belonging to the glottis. Spasm of those muscles would close the chink, and stop the breath. And Dr. Reid has shown that their palsy, except while the breathing is perfectly quiet and tranquil, sensibly impedes inspiration, and alters its character. We have, I say, in these considerations, the materials for ultimately solving a very curious problem.

It is interesting to observe how Dr. Ley's view of the matter harmonizes with what has been noticed of the predisposing causes of this crowing inspiration. In the first place, it is often manifestly connected with *dentition*. Now one effect of dentition is the production of glandular swellings of the neck; which happen even in the absence of all strumous taint; but with still more certainty if any such taint exist. And this explains the fact, that the disorder has appeared in the most robust as well as in the most delicate infants. This explains also another well-known fact, viz. that, when child-crowing accompanies painful dentition, the symptoms do not vanish instantaneously, as if by magic, the moment that the tooth starts through the gum; but pass off by degrees. The truth seems to be, that, "after the gum and enveloping membrane of the tooth have been relieved from swelling and inflammation by the free use of the gum-lance, *some time* is still required for the irritation and tumid state of the cervical glands to subside."

Again, this child-crowing is found to occur in connection with excoriations behind the ears, and with inflamed and irritable scalp; and these complaints very frequently lead to enlargement of the absorbent glands of the neck, which enlargement forms thus an intermediate link in the chain of events. And upon some occasions Dr. Ley has had strong reason to suspect that bronchitis, or other *disease of the lungs*, has occasioned enlargement of the bronchial glands, and thus given rise to the crowing inspiration.

I think that Dr. Ley has made out fair grounds for his view of the pathology of what is called spasmodic croup. At the same time I am not quite persuaded that the child-crowing does not occasionally spring out of a cerebral affection. And of this I am sure, that disease of the brain, and a temporary affection of the glottis, producing stridulous inspiration, sometimes go together, if they be not connected as cause and effect.

The practical *fact* which you have to remember is, that croupy breathing may occur, and return in paroxysms, when there is no croup. And the practical *lesson* which you have to learn is, how to discriminate between these two similar, yet different disorders. I have already specified the distinctive characters of cynanche trachealis. The complaint that copies it may be known by its sudden accession and its sudden departure; by the freedom of breathing in the intervals between the paroxysms; by the absence of fever, of preceding or present catarrh, of hoarseness, and of any abiding cough. The diagnosis, easily enough reached when these points are sufficiently attended to, will be still more sure, if you discover enlarged glands in the neck.

It must be unnecessary for me to urge the *importance* of the diagnosis. Those active measures which befit the outset of the inflammatory disease, would be misplaced and mischievous in the other. It was the result of Dr. Ley's experience that "those children have upon the whole a much better chance of being preserved, who are not subjected to very severe discipline, than those who, in compliance with the prevailing doctrine, have been treated by very copious bleeding, large doses of calomel, and such other remedies as the supposition of the invariable dependence of the disease upon cerebral turgescence or excitement has suggested."

Do not, however, imagine, from what I have just been saying, that this disorder, child-crowing, is a trifling disorder, and unattended with danger. It is really a perilous, as well

as a terrifying condition. The respiration is sometimes so long suspended that death takes place in the paroxysm. And each paroxysm is accompanied by a tendency to stagnation and congestion of blood, in the brain, lungs, and heart: a tendency which, by its frequent repetition, may lay the foundation of serious and fatal disease in one or other of those vital organs.

The treatment of this affection must depend a good deal upon the nature of the predisposing and exciting causes. To go into it fully would require that I should speak of the mode of cure in painful dentition, in eruptive complaints about the head, in certain pulmonary and cardiac diseases, and in all those conditions which are apt to give rise to enlargement of the catenated or bronchial glands.

In addition to the specific methods of treatment which these several diseases may need, attention to the diet of the child, and to the state of the skin and bowels, and (more than any thing else, perhaps) removal from the air of a city or town to the purer atmosphere of the country, will often prove eminently serviceable.

In the paroxysm itself the warm bath might be useful, if it could be got ready in time. The application to the throat of a large sponge from which hot water has just been squeezed, is a more accessible, and often a very effectual expedient. Sprinkling the face and chest freely with *cold* water will sometimes unlock the spasm, and set the little sufferer free.

I should have stated, that Dr. Ley's views were abundantly supported by the researches that he had made into the morbid anatomy of this affection. Had he lived, I should have had it in my power to show you some very beautiful and interesting preparations, illustrative of the connection of the disorder with enlarged glands, which had come to press upon, or stretch the par vagum, or its branches. As it is, I can only show you the published engravings from some of them, appended to his book.

LECTURE XLVII.

DISEASES OF THE THORAX. GENERAL OBSERVATIONS. DYSPNŒA. COUGH. METHODS OF EXPLORING THE PHYSICAL CONDITIONS OF THE CHEST, BY THE SENSES OF SIGHT, TOUCH, AND HEARING.

FROM the throat—and especially from that part of it with which we were last occupied—the transition is natural and immediate to the *thorax*. Now the interior of the chest is the theatre of numerous and most important morbid changes. In that cavity are lodged two out of the three organs most essential to life. The heart, and lungs, and brain, have been said, by a bold figure of speech, to constitute the tripod of life: and the two former are planted in the thorax. In the same division of the body lie also the great blood-vessels, and many other parts of scarcely less consequence. With respect to the heart, its alternate swinging movement cannot long be suspended, and the patient continue to live: and three minutes' total interruption to the play of the lungs would in most cases be irremediably fatal. And lesser impediments to the free working of either of these two vital organs are productive of great distress, and lead often to consecutive changes of a very serious nature in various other portions of the body.

These parts, of which the function is so indispensable, and of which the disorders are so grave and perilous, are encased in such a manner by the ribs and other boundaries of the chest, that they can neither be seen nor handled. And until a very late period in the history of medicine—until our own times, in fact—the diagnosis of the formidable maladies that befall the viscera of the thorax was exceedingly uncertain and imperfect. Physicians were able indeed, by the observance of indirect symptoms that declared themselves through the system at large—by the presence of inflammatory fever, I mean—to infer that inflammation had been somewhere lighted up; and symptoms that denoted disturbed

function of the respiratory apparatus—cough, difficult breathing, or local pain—might suffice to apprise them that the inflammation was situated in the chest. But what tissue it affected, where was its exact place, what was its extent, or what were its physical effects—these were points concerning which they had no means of obtaining any precise knowledge. "Under the title of pneumonia or pneumonic inflammation," says Cullen, "I mean to comprehend the whole of the inflammations affecting either the viscera of the thorax, or the membrane lining the interior surface of that cavity: for neither do our diagnostics serve to ascertain exactly the seat of the disease, nor does the difference in the seat of the disease exhibit any considerable variation in the state of the symptoms, nor lead to any difference in the method of cure." You will see, as we proceed, how very inaccurate this last statement would be, if it were made under our present mode of investigating these diseases. "Pneumonic inflammation (he continues), however various in its seat, seems to me to be always known and distinguished by the following symptoms:—Pyrexia, difficult breathing, cough, and pain in some part of the thorax."

I state these things to you—who do not recollect the time, as I do, when no medical man in this country could, with truth and candour, say more of his knowledge of diseases of the chest than Cullen said—that you may the better estimate the exceeding value of the discovery of what is called the method of auscultation, in the detection and discrimination of disease; and most particularly of thoracic disease. In the present day we are unable to obtain more accurate information respecting the disorders of the parts contained in the chest, than of any other internal, and therefore invisible parts of the body. Indeed, in a vast number of instances, we can tell, as positively as if we saw them, the actual condition of the thoracic viscera: can follow, step by step, the successive processes of disease or of repair, in which they are involved. We can penetrate beyond the symptoms which denote deranged function, and detect and understand those much less fallible symptoms which arise from alterations of structure. And this vast addition to our pathological knowledge has been given us by the simple application of one of our five senses to the investigation of disease, the sense of hearing, which for so many centuries had been (strangely as it now seems) neglected, or but little used. By the assistance of the ear it has come to pass that those diseases which, besides being the most common and the most destructive, were also the most obscure, are now better understood than the diseases of any other internal part whatever.

The direct symptoms which arise out of the changed conditions of the parts affected in thoracic complaints, are so mixed up with all that we know or can learn of such complaints, that what in all other cases is called the morbid anatomy of a disease, becomes, here, a part of its history. I shall not say, therefore,—as in regard to many other maladies I am obliged or I find it convenient to say—so and so are the symptoms; and afterwards, so and so are the morbid appearances: but I shall describe the morbid conditions in the outset, as the only way of rendering the symptoms which flow out of them intelligible.

But before I enter upon the subject of auscultation, it may be useful to make a few remarks upon those symptoms of thoracic disease which were previously known, and which arise out of, or rather, which express, derangement of the pulmonary functions.

One of the most constant, and obvious, and distressing, and instructive of these symptoms, is embarrassed or laborious breathing: what is technically called *dyspnœa*. You know that in a healthy adult, under ordinary circumstances, the act of respiration is performed, unconsciously almost, about eighteen times in a minute. There is about one act of respiration for every four beats of the heart. In various diseases this proportion is materially altered. The measured succession of reciprocal movements, by means of which air is drawn into and again let out of the lungs, are performed with hurry, or effort, or unusual slowness. Dyspnœa implies some deviation from the natural manner or rate of alternately expanding the thorax, and suffering it to collapse again; of inspiring and expiring: in one word, of breathing. The patient himself may, or may not, be conscious of this deviation. In most of the cases in which dyspnœa claims to be regarded as a symptom, he is conscious of it. Now upon what does this symptom depend? It may ultimately be referred to an altered proportion between the quantity of atmospheric air that reaches the lungs, and the quantity of blood that is sent into them, to be con-

verted from venous to arterial, from the right side of the heart. That, doubtless, is at the bottom of almost every case of dyspnœa. Let me remind you that respiration is an automatic movement; subject nevertheless to the occasional control of the will. The pulmonary branches of the par vagum constitute the principal and constant *excitor*, as the nerves that supply the muscles of respiration are the *motor* links of the nervous chain by which the automatic movements are governed. It is believed that the presence of venous blood in the capillary vessels of the lungs forms the natural stimulus to the pulmonary part of the par vagum. In the ordinary breathing of a healthy person, this stimulus or impression is not felt: perhaps because being slight and habitual, and exactly apportioned to the need of the individual, it is not attended to: or it is at once appeased by the admission of air, and the corresponding change in the blood. But when the change is not immediately or perfectly accomplished, then arises the distressful sensation which every body has felt, but which our own language has no one word to express. The French call it the *besoin de respirer*. The English phrase, *want of breath*, denotes the peculiar sensation equally well. It calls into exercise, frequently, the voluntary power of performing the mechanical acts of breathing—a power which is superadded to the automatic process.

Various are the ways in which the natural manner and frequency of the respiratory movement may be deranged. They were fully considered when I was on the subject of death by apnœa. They all operate, ultimately, by destroying the just equilibrium between the blood and air which meet to undergo chemical changes in the lungs. We have lately been considering certain diseases in which the difficulty and distress of breathing is often extreme. In croup, and laryngitis, the only inlet for the air is narrowed at its very entrance: there is more blood passing through the lungs than can be arterialized under the ordinary motions of respiration: instinctive efforts take place to increase the quantity of air; to make up by more numerous acts of inspiration for the diminished amount of air introduced by each single act. For a time these compensatory efforts may suffice. But if the access of air be still impeded, the blood begins to circulate in the arteries, but half decarbonized; and to linger and stagnate in the lungs; the lips become livid, and the skin dusky. Make, however, a free opening in the pipe that should conduct air to the lungs, and the balance between the blood in the lungs and the air that reaches them being restored, the dyspnœa is soon at an end. The quantity of blood being the *same* then, but the air inspired *too little*, there will be dyspnœa. And the very same thing occurs whenever a portion of lung from being spongy is rapidly rendered solid. No air can then penetrate it; nor *perhaps* any blood: but the same quantity of blood as before arrives at the right side of the heart, and is transmitted thence through the pulmonary artery: and consequently those portions of the lungs, which are pervious to blood and air, are supplied with blood in excess, and require air in excess: *i. e.* dyspnœa is necessitated. And you will perceive that similar consequences may arise from any pressure made upon the lung obliterating in a certain degree its cellular structure; as by fluid collected in the pleura; by enlargement of the heart; by aneurism of the great vessels; by tumours, of whatever kind, within the chest; or by pressure upwards against the diaphragm by reason of a distended abdomen, whether the distension be occasioned by disease, such as ascites, or by obesity, or by a full stomach, or by a gravid uterus. A like disproportion will ensue, if the free expansion of the thoracic cavity be prevented by pain, by disease or rigidity of its boundaries, or by palsy of its muscles through interruption of the nervous circle whereon their contractions depend.

But on the other hand the balance may be destroyed from the opposite quarter: the air admitted during a single ordinary inspiration being the same, the quantity of blood requiring to be converted from purple to scarlet may be augmented; and in that case also, in order to maintain the due equilibrium, more numerous acts of respiration must be performed: in other words, dyspnœa will arise. This is the case under strong exercise: the pressure of the muscles upon the veins propel their contents with greater velocity towards the right side of the heart; the heart contracts more frequently in proportion; it is more rapidly filled with blood; a greater quantity than usual is sent through the pulmonary artery to the lungs; and the individual breathes more quickly, to supply this augmented quantity of blood with air: he is out of breath, in a state of dyspnœa. But this is not

disease. Disease, however, will often have the same effect. The quickened circulation in fevers, any obstacle to the free passage of the blood from the heart into the arteries, will tend to gorge the lungs with blood, to destroy the requisite equilibrium between the air and the blood in those organs, and so give rise to dyspnœa.

Other conditions still may be mentioned, as predisposing to hurry of the breathing—a peculiar state of the nervous system; certain qualities of the blood:—but I need not dwell on these at present.

There are two important corollaries derivable from what I have now been stating. In the first place you must perceive how intimately the functions of the heart and lungs are dependent upon each other; and that disease originating in either of these vital organs may readily be the cause of consecutive disease in the other. We shall have many examples of this before us as we proceed. It would afford materials for an interesting essay, this mutual interdependency of cardiac and pulmonary disease. At present I merely glance at it in passing.

In the second place, what I have said of dyspnœa must have sufficed to show you that, taken by itself, it has not much value as a diagnostic symptom. All that it tells us is, that the healthy and natural relation between the quantity of blood and of air in the lungs is disturbed: but to determine the cause of that disturbance—to decide whether the heart be in fault, or the lungs, or both, or neither—we must have recourse to other sources of information.

Cough is another of the symptoms, mentioned by Cullen, as denoting disordered function of the breathing apparatus. I need scarcely tell you that it is produced by closing the glottis, and then making a sudden and strong expiration. Its purpose is the dislodgment of mucus which may have collected in excess in the air passages, or any other source of irritation to the membrane lining those parts. To be effectual it requires the admission of a certain quantity of air, and the possession of a certain degree of muscular strength. I pointed out to you, in the last lecture, a remarkable exemplification of this: the boy whose case is described by Mr. Chevalier in the Medico-Chirurgical Transactions, was dying of croup; was on the brink of being suffocated by the collection of mucus in his windpipe and bronchi, which mucus he was unable to expel: and he was unable, not from deficient muscular strength, but because he could not inflate his lungs *beyond* the collected mucus, in a sufficient degree. When an ample opening was made in his trachea, he drew in a strong breath, and coughed the mucus up *through the rima glottidis.* In old and feeble persons labouring under chronic bronchitis with profuse secretion from the mucous surface, strength is often wanting to cough the phlegm up; and they die suffocated.

But the sensation which prompts to the act of coughing may arise from many other causes besides the accumulation of mucus in the air passages. Any slight irritation about the glottis; a long and trailing and tickling uvula; the inspiration of irritating vapours; pressure of any kind upon the respiratory organs; may any of them produce cough. Nay, it sometimes is provoked by sympathy with other parts; an instance of which we have in what is called a stomach cough. Some morbid condition, some irritation of the stomach exists, which being appeased, the cough ceases. You will recollect the name and the functions of the *pneumogastric* nerves. We have, in the fact just mentioned, another example, in addition to those which I glanced at in a former lecture, of irritation of the sentient extremities of one branch of a nerve, declaring itself by uneasy sensations excited in other branches of the same nerve. For these reasons, therefore, cough is not more diagnostic of particular diseases situated in the thorax, than is dyspnœa. There are, indeed, certain varieties of cough, as there are certain modifications of the breathing, from which we may obtain very useful information even in respect to the nature and seat of *some* diseases: and these varieties and modifications I will point out as I go along.

Let me admonish you, also, before we come to auscultation, not to fall into an error which has been too common; that of trusting entirely to the ear in the investigation of thoracic disease, to the neglect or exclusion of those phenomena which are discoverable by the eye, or the hand; or of those indirect revelations which are furnished by the condition of other parts and functions, or by the previous history of the patient. Even before the discoveries of Avenbrugger and Laennec, physicians were too remiss (if we may judge

from their writings) in what may be called the mechanical exploration and observance of the actions of respiration. A good deal may be learned, sometimes, by merely placing one's hand upon the chest, or belly, as I shall explain more particularly by and by; and a great deal, also, may be made out, in some cases, by the simple inspection of those parts, when they are uncovered. You may see, for example, that the ribs, in respiration, scarcely move at all, while the belly rises and falls alternately with the descent and ascent of the diaphragm. This is called *abdominal respiration*. It may arise from a painful condition of the intercostal muscles, or of the pleuræ, rendering the patient *unwilling* to elevate his ribs; or it may arise from disease of the spinal cord, between the origins of the phrenic nerve and of the intercostal nerves, rendering the patient *unable* to raise them; or the same *inability* may result from disease of the lungs themselves. The symptom may guide us at once to the seat of the malady. Again, the breathing may be entirely *thoracic*, no motion of the abdomen taking place; and this may depend upon an affection of the diaphragm, or of the pleura which is reflected over it; or upon disease, accompanied with tenderness, within the abdomen—upon peritonitis for example; or upon mere distension of the abdomen. Or by looking at the naked chest, you may see that one side of it moves, and that the other moves less, or does not move at all: and the motionless side may be of the natural size as compared with the other, or it may be pinched up and contracted, or it may be round and bulging; and most important conclusions, and most important indications of treatment, will flow from a knowledge of these circumstances. The general form of the chest is also instructive. Never neglect, then, to examine the thorax, in cases where it is supposable that the disease is seated in that part of the body, by your *eye*, as well as by your *ear*. The eye needs but little training to enable it to perceive and comprehend those signs which are within its sphere: the ear, unfortunately, requires to be carefully educated. I will just remark, farther, that in the case of females no indelicate exposure of the person need be made. In most cases the morbid conditions I have been adverting to may be recognized through a thin linen covering.

Auscultation signifies the investigation of internal diseases through the sense of hearing; and it is especially applicable, for reasons which I either have stated or will state, to the study of *thoracic* diseases. In its full meaning it includes all that we learn by listening to a cough, and all that we gather by striking the chest, and listening to the resulting sound. But in general, the term *percussion* is used to express this last mode of eliciting information, although the information is conveyed through the medium of the ear: and the word auscultation is applied to the art of distinguishing diseases by listening to internal sounds, by means of the ear placed in apposition with the surface of the body; or by means of some conductor of sound interposed between the ear of the listener and the person of the patient. In the first of these two modes the auscultation is said to be *immediate;* in the second, *mediate*. By percussion we ascertain the degree of resonance, or want of resonance, of the part struck: by auscultation we learn the qualities and modifications of the voice, as reflected through the chest; and of the breathing; and of the sounds of the heart. The invention of the method of percussion we owe to a German, Avenbrugger, who wrote an excellent treatise upon it, which was brought into notice by Corvisart, who translated it. For the more brilliant discovery of auscultation we are indebted, as every body knows, to Laennec.

Now it will save us much trouble, and conduce, I hope, to your future progress as practical auscultators, if, before I speak of any of the diseases of the chest, I premise some general observations respecting these modern methods of examining the human body, with the view of detecting and discriminating its diseases. Indeed, I could not make myself intelligible unless I did so.

And first, with respect to percussion, which you will please to recollect is nothing else than auscultation of, or listening to, sounds which we ourselves artificially and purposely produce.

You know, every child knows, by daily experience, that different substances, when struck, give out very different modifications of sound. If you strike a drum, you get one kind of sound; if you strike a brick wall, you get another. The one is loud, trembling as it were, and prolonged; the other dull, short, and flat. But why should I attempt to describe in words things which are familiar to you already? Bodies that are solid, or in-

elastic, give the dull flat sound in proportion to their solid thickness, or their want of elasticity. On the other hand, hollow vessels, *i. e.* vessels containing air, with thin, firm, elastic boundaries, give out a sound more or less approaching in its qualities to that of a drum: the sound is called a *hollow* sound from that circumstance. If you have a wooden cask containing air only, it is resonant when struck; fill it half full of water, and the lower part will render a flat sound, the upper empty portion a hollow sound; less hollow, however, than when the vessel contained no water: fill it up with water, and the *whole* is dull on percussion: pour out the water, and fill it loosely with wool—it will still be resonant, though in a different and less clear note than when it held air alone.

Now this experiment may be transferred to the human chest, which is a cavity, bounded by firm, thin, tense, and elastic walls, and containing, in its natural state, the spongy lungs, which are full of air; and other parts that are solid, whereof the heart is the chief. If you strike the surface of the chest (it requires a little knack to do it properly), and if the blow falls over a portion of healthy lung, you will produce a resonant or hollow sound. If the lung be not there, if it be pushed aside, and its place supplied by some more solid or inelastic substance, by fluid, for example, you will hear a dead sound. So you will if the lung *be* there, but has lost its spongy character, is void of air, and somehow or other solidified. But you may have a resonant sound, though the lung is in a state of disease; nay though the lung is not there: so that percussion alone cannot always be depended upon. I shall tell you, hereafter, how to guard against being misled by it in such cases. Again, if you strike over the region of the heart, you will get a positively dull sound, or at any rate a much duller sound than in most other parts of the chest.

It is really a singular thing, that this method of searching for indications of disease, and of health, should have been so long neglected or overlooked in our profession. I am sure that I had a practical acquaintance with the principle of percussion long before I knew any thing of physic; and so, I make no doubt, have most of you. Many a time, when wishing to know whereabouts to drive a nail firmly into a wall, I have tried with the hammer to find which was brickwork, and which was wooden joist; and percussion is an art in daily use for similar purposes, with carpenters and bricklayers. Yet it does not appear to have been thought of by physicians till the middle of the last century, when Avenbrugger, after studying its results for seven years, as he tells us, *inter tædia et labores*, published at Vienna his "Inventum novum, ex percussione thoracis humani, ut signo, abstrusos interni pectoris morbos detegendi." This was almost totally neglected however, until, as I stated before, Corvisart's work on diseases of the heart brought it into general notice.

Avenbrugger and Corvisart, and indeed everybody who used percussion at all, until a very few years ago, employed *direct* percussion: that is, they struck the chest with the extremities of their fingers. More recently, *mediate* percussion has been introduced into practice, by Mr. Piorry. In mediate percussion some solid substance is placed upon the spot, the resonance of which is about to be explored, and the blow is made upon that substance, which is called a *plexímeter*—a blow-measurer. A round thin plate of ivory, laid flat upon the surface, is the most common sort of plexímeter; or metal, or wood, or leather, or India rubber, may be employed. Many persons, and I am one of them, use no other plexímeter than the fingers of the left hand.

I shall explain, as briefly as I can, the method of employing percussion, and the cautions requisite to render it an effectual and a true interpreter of the state of the parts beneath the stricken surface.

The position of the patient is of some consequence. It should be one that is convenient to the examiner, and not inconvenient to himself; and it should be one calculated to render the part struck as firm and tense as possible. The best position of all is a sitting position, on a firm chair. But you may percuss a patient very effectually as he sits up in bed, or while he stands, or some parts even when he is lying down. A good deal is said,—more, in my opinion, than is necessary,—about the effect of curtains, and so forth, in deadening the sound. I do not believe they will ever interfere with your conclusions, especially as we learn more from comparing the sounds given out upon percussing the corresponding parts of the opposite sides of the chest, than from the absolute resonance or want of resonance of any single part. But there are some exceptions to this;

and if your patient can be made to sit on a chair in the middle of a room, so much the better.

Then, if you are about to percuss the front of his chest, make him hook his arms over the corners of the back of the chair, and throw his head back. If you desire to explore in this way the lateral portion of the thorax, he must place the hand of that side upon his head, and lean a little to the opposite side. If you would know how the posterior part of the chest sounds, he must lean forwards, fold his arms across his breast, and bend down his head.

Next as to the mode of percussing. For direct percussion, the ends of the fingers of the right hand should be brought together, and into a line with each other, so that no one of them projects beyond the rest; and care should be taken, first, to compare the sound produced by striking any part of the chest on one side, with that produced by striking the corresponding part on the other side. It follows from this rule that we should not examine all the points on one side before passing to the other, because we should thus lose the remembrance of, and the power of accurately comparing, the sounds obtained from corresponding points. It is best to strike first on one side of the body, and then on the corresponding spot of the other. It follows also that we are not to compare the result of percussion on one of the *ribs*, with the result of percussion on one of the *intercostal spaces*. The blow should fall *upon* the rib, and *parallel* to it.

A second point requiring attention, is the state of the chest in respect to the act of breathing. If one side be percussed after the movement of inspiration, and the other after that of expiration, some little difference in the resulting sounds will be manifest, even in the healthy condition of the thorax. And this might mislead. Let corresponding spots on the two sides be therefore both struck, either while the chest is expanded, or while it is collapsed, or while the patient holds his breath.

Thirdly, you must take care to strike the corresponding parts at the same angle, and not with the fingers perpendicular to the surface on one side, and inclined obliquely to it on the other: also to strike corresponding parts with the same degree of force. And the blow should not be hard enough to give the patient pain; indeed such a blow would not produce a good sound. It should be smart and quick; the ends of the fingers should not *remain* on the chest. Under some circumstances, however, the patient cannot bear to be percussed at all.

These latter cautions are most necessary when *direct* percussion is employed; over which *mediate* percussion has, however, many advantages. Some of these are obvious. In the first place, the space examined by mediate percussion is very exactly defined and limited. Secondly, you may strike the pleximeter much more forcibly than you could strike the unprotected body, and so produce a more decided sound. Even when the surface is morbidly sensible, or the patient unusually irritable, so that percussion in the ordinary way cannot be performed at all, it may generally be done through the pleximeter. A third and very great advantage is, that mediate percussion is available when made over certain parts where, even although there may be no pain occasioned by it, ordinary or *im*mediate percussion is attended with no useful result. Parts, I mean, where there is much fat, and parts which are fleshy, or œdematous. If the pleximeter be pressed firmly upon these parts, even upon the mamma in females, the hollow sound is attainable; whereas, if they were struck by the fingers, the sound would be perfectly dull. Mediate percussion may be applied also, with effect, through the clothes.

I say that a convenient way, and one that I find quite sufficient, of employing mediate percussion, is by making a pleximeter of the finger, or fingers, of the left hand; taking care that they are closely in contact with the subjacent parts, and *similarly* applied to corresponding spots; and that the backs of the fingers be outwards. Piorry declares, indeed, that the resonance produced by this mode is scarcely one-tenth part so great as that elicited by using a thin, solid, and elastic plate. For all practical purposes, however, I am certain that the finger, as it is the readiest, so also is it a very satisfactory and sufficient pleximeter. It has, moreover, this positive advantage, that the sound made by striking *it* is not loud, and does not obscure or interfere with that which depends upon the condition of the chest.

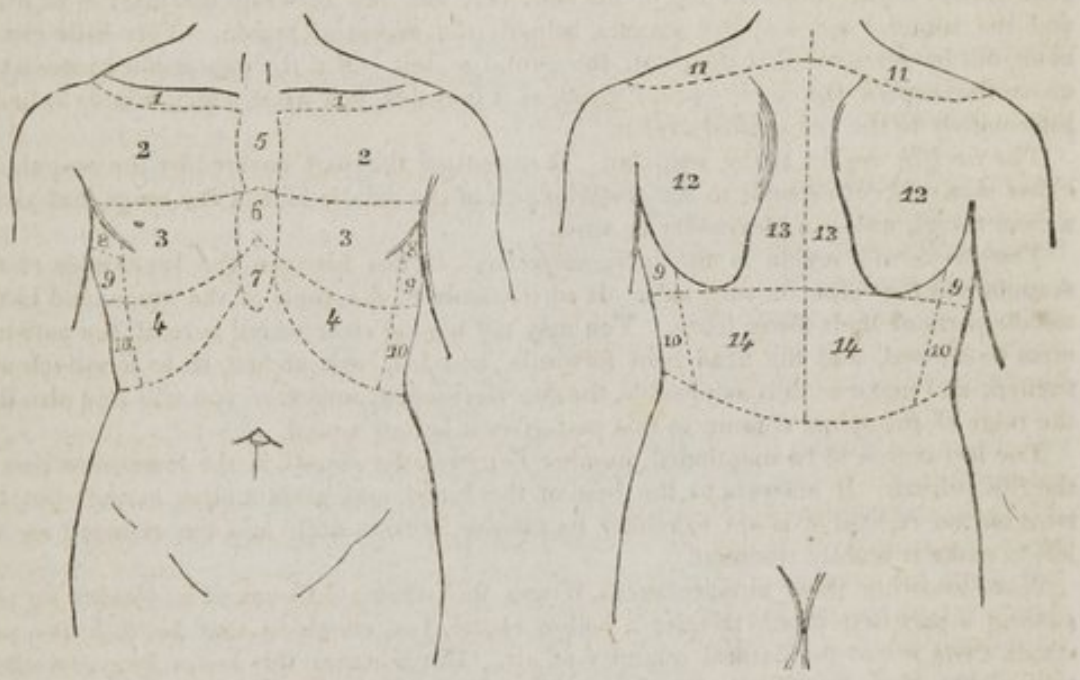
It is right that you should be aware of differences of sound which belong to the indi-

vidual. *Cæteris paribus*, the sound given out on percussion is more resonant during *in*spiration than during *ex*piration; in childhood and youth than in middle age: in middle age than in old age; in females than in males; in thin persons than in fat; and, *they say*, in nervous irritable people than in those of a contrary temperament.

And it is still more necessary that you should be aware of differences of sound given out, in health, by different parts of the surface of the thorax, *in the same individual.* And in order to explain this more distinctly, and for the sake of reference hereafter, let me here exhibit to you the *regions* of the thorax, as they have been artificially mapped out for the purposes of auscultation.

It is unnecessary for me to describe particularly the method followed in this arbitrary division of the thorax into regions. I will state the names that have been commonly applied to them, and the sounds which in health they respectively yield, according to their number in the diagram before you.

The *first* region is that of the *clavicles;* one of course on each side. Upon these bones it is immaterial whether direct or mediate percussion be made. The sound given out should be very clear at their sternal extremities, dull at their humeral extremities, and clear at their



middles. The resonance diminishes from the sternal towards the acromial end of the clavicle. These parts correspond to the summits of the lungs.

The *second* region is the *subclavian*. It lies between the clavicle and the fourth rib on each side. Beneath this superficial region lies the upper lobe of the lungs; and towards the sternum large bronchi are situated. You will understand, therefore, that the sound educed by striking this part ought to be very clear.

A little lower down, number *three*, is the *mammary* region, extending from the fourth to the seventh rib on each side, and answering to the middle lobe of the lungs. Here also the resonance afforded by percussion is clear; but in the lower part of this region, on the left side, we find the heart, which is more or less covered by lung; and on the right side the liver begins to mount. The sound is somewhat modified and deadened by these deeper-seated viscera. In women we can determine the degree of resonance of this region through the mammæ, by *mediate* percussion only.

The *fourth* is the *infra-mammary* region. It comprehends that part of the bony compages of the thorax on each side which lies between the seventh rib and the edge of the cartilages of the false ribs. Into this region, on either side, may descend the thin anterior margins of the lower lobes of the lungs; but in the same region, on the right side, lies the liver, over which the sound of percussion is dull; and on the left side is placed the

stomach. Hence, when the stomach is tympanitic, a preternaturally resonant sound will proceed from this part; and when the stomach contains no gas, the sound will be irregularly dull.

All the regions hitherto described are double. There is still the mesial part of the front of the chest to be subdivided, and we may call the three regions there situated, and numbered 5, 6, and 7, the *upper sternal* region, the *middle sternal*, and the *lower sternal*. In all these the sound on percussion ought to be clear, except perhaps in the inferior portion of the last, which may be rather dull, or which, from its vicinity to the stomach, may be tympanitic.

The *eighth* region is the *axillary;* the axilla above the fourth rib on each side. The *ninth* is the *lateral* region, between the fourth and seventh ribs. In both these regions the resonance should be distinct and clear. The *tenth*, which may be called the *lower lateral* region, below the seventh rib at the sides, gives the same sounds as the infra-mammary, namely, on the right side a dull sound, on the left a sound which at times is preternaturally hollow: on account of the presence of the liver in the former case, and of the stomach in the latter.

But we have yet to look at the hind part of the thorax. Here we have the space (region *eleven*) which forms the top of the shoulder, and lies between the clavicle in front, and the superior spine of the scapula behind; the *acromial* region. Very little can be made out by percussion in this spot; the sound is dull. But the depression immediately above the scapula, the *supra-spinal fossa*, as it is called, is a space which affords valuable information to the *ear* applied over it.

The *twelfth* region is the scapular. It comprises the part covered by the scapula on either side. It corresponds to the posterior part of the middle lobe of the lungs, but yields a dead sound, unless a pleximeter be used.

The *thirteenth* region is the *intra-scapular*. It lies between the inner edge of the scapula and the spine on each side. It corresponds to the roots of the lungs, and to the middle parts of their lower lobes. You may get a good clear sound here, if the patient's arms be crossed, and his head bent forwards, and his back arched, so as to stretch and tighten, and make as thin as possible, the superincumbent muscles; you will find also that the ridge of the spinal column in this part gives a hollow sound.

The last region to be mentioned, number *fourteen*, the *dorsal*, is the lowermost part of the ribs behind. It answers to the base of the lungs, and gives a clear sound: but the liver on the right side is apt to render its inferior portion dull; and the stomach on the left to make it unduly resonant.

Now, knowing these circumstances, if you find that a dull sound is yielded on percussing a part that should render a hollow sound, you conclude that beneath the part struck there is not the natural quantity of air. But whether this arises from consolidation of the lung itself, and the obliteration of its cells, or from fluid effused into the cavity of the pleura, you cannot, by mere percussion, determine. So, again, if the sound be unnaturally tympanitic, you cannot say whether that results from what is called pneumothorax, or from emphysema of the lung. Percussion testifies that *air* is there; but further this deponent sayeth not.

The information which the exercise of percussion may leave thus imperfect, auscultation of the sounds produced by the acts of breathing, speaking, or coughing, will in most cases supply.

If the ear be laid close to the surface of a healthy thorax (or if the instrument called the stethoscope be interposed between that surface and the ear), the ear will hear the air enter and fill the lungs, and then withdraw and leave them again, in perpetual succession. The sound produced by this ingress and egress of air has been called the *respiratory murmur*. I might tell you that this sound, in my judgment, is rather a rustle than a murmur: that it is like the sighing of the wind in the branches of a tree. I might say, with Laennec, that it resembles the sound made in the deep inspiration of a sleeping person; or with Dr. Davies, that it reminds one of the soft murmur of a pair of bellows, of which the valve does not click. But one minute's appliance of your ear to the subclavian region in a child, or even in an adult, will give you a clearer notion of the nature and character of this sound than any verbal description could convey. Yet, respecting

this natural respiratory murmur, there are some things of which it is desirable that you should be previously informed.

In the first place, the *entrance* of the air is much more noisy than the *exit:* which, sometimes can scarcely be heard. You will see, by and by, the importance of noticing this fact.

In the next place, the murmur of respiration is not equally audible in all persons. It may differ much in intensity, though not in kind, in two persons, each of whom is in perfect health. Thus it is much more loud and distinct in children than in grown persons. So remarkably is this the case, that when we meet with an unusually noisy respiratory murmur in an adult, we say that his breathing is *puerile;* it has the character of the respiration of a child. Now, I tell you beforehand, that puerile respiration in the lung of an adult is generally a sign of disease; and it is mostly partial; heard in certain parts only of the chest.

Again, *cæteris paribus*, the natural murmur of respiration is more clearly audible in lean and spare persons than in such as are muscular and fat. Fat and muscle are bad conductors of sound, and act as dampers. Listening to the breathing through a thick layer of adipose tissue, is like listening (as Dr. Latham says) to the respiration of a person through his clothes.

But if you take two healthy men who are alike in respect to leanness and fatness, you may often find that the respiratory murmur is very loud in the one, and very feeble, or almost inaudible, in the other: but in this last it *becomes* audible when he takes a deep and forced inspiration.

The reason of this difference is not very well understood. The breathing may be very indistinct, though the thorax be capacious, and well formed, and healthy. Some persons seem to require less effort than others to maintain the due equilibrium between the air in the lungs, and the blood in the lungs: as if they had not only pulmonary space enough, but to spare. So that the difference in the manner of breathing, and in the sound occasioned by the passage of the air in and out, depends, in all probability, upon individual peculiarities of the circulation. At any rate, it is very important that you should be aware of the existence of these differences.

But the sounds which reach the ear applied to the chest of a breathing person will differ in different parts, and under different circumstances. The sound given out by the air as it passes through the trachea and larger bronchi, differs from that which results from its passage into and out of the smaller bronchi and air passages. I am anxious not to refine too much on these points: and, therefore, I pass by minuter points of difference. Place the stethoscope over the trachea, and you will hear just such a sound as you might expect to hear: the sound of air rushing through a tube of considerable size, a blowing noise. We will call that sound, which you will recognise again when you have once heard it, *bronchial* breathing. It accompanies the outward as well as the inward passage of the breath. Again, place your ear or your stethoscope upon the right mammary region; there you shall hear that rustling sound, which I propose to call *vesicular* breathing, and which is chiefly audible during inspiration. We shall find these distinctions of much use in the discrimination of disease. We shall find, for example, that the breathing sometimes is bronchial, where it *ought* to be vesicular.

Now if in any part of the chest where we ought to hear breathing, we hear none, this may result from solidification of the subjacent portion of lung; or from some obstacle in a large bronchus, preventing the air from entering that part; or from air being in any way shut up and stagnant in that part; or from an effusion of liquid into the pleura of that part. And here again percussion comes into play, and determines for us which of these possible circumstances is the one really present. If the part when struck emits a hollow sound, there is stagnant air beneath it, either in the healthy lung, or in the lung altered by disease, or in the cavity of the pleura. If a flat solid sound be given out, there is solid lung beneath, or liquid in the pleura, between the ear and the lung. All these points, and the conclusions to be deduced from them, will become clear to you, I trust, as we proceed. Minuter analysis of the diagnostic signs would be premature.

A word or two preliminary I must say about the *voice*.

The voice passes outwardly through the mouth and nostrils into the surrounding air; it

passes also inwardly, through the trachea and bronchi into the lungs, and it may be heard through the lungs, by the ear laid flat against the chest. But it gives quite a different sensation to the ear in different places. If you place the stethoscope on the trachea, the voice will articulate itself into your ear as if it came from and through the instrument. This sound, which is natural here, would be unnatural, and a mark of disease, if heard beneath the clavicles. When heard beyond its natural situation, it is called *pectoriloquy*. A less degree of this, a sound like that of a person talking into a tube, and whose words, for that reason, are muffled and indistinct, is called bronchial voice, or *bronchophony*. When to this modification of the voice there is added a twanging vibration, a cracked discordant tremor, resembling the squeak of Punchinello, or (as some think) the bleating of a goat, *ægophony* is said to exist. All these unnatural modifications of the voice are indicative of most important changes within the thorax. I merely introduce them to you now: you will become better acquainted with them in due time.

What is true of the natural voice, is true, *mutatis mutandis*, of that unnatural vocal sound, *cough*. The cough may be so modified by the condition of the internal parts, as to reveal that condition.

There are yet other ways in which some information can be gathered respecting the interior of the chest. If you give certain patients a sudden smart jog while your ear is applied to their thorax, you may hear a splashing sound; like that produced by shaking a barrel or a bottle partly full of water. From this expedient you in fact determine that the cavity of the pleura, or perhaps a large excavation in the lung, does contain both air and a liquid. This is called the method of *succussion*. It was employed long before the other forms of auscultation were thought of. Hippocrates mentions it.

Again (but that is not auscultation), you may sometimes collect useful information respecting the state of the chest by simply laying your open hand upon it. In most persons there is a distinct vibration or thrill produced by their speaking, which thrill is very sensibly felt by the hand. Now this thrill may be felt on one side of the chest and not on the other. And such a difference is an unequivocal sign of disease.

The positions which I described before as being the most convenient for the performance of percussion, are the most convenient also for the purposes of auscultation. You may listen with the unassisted ear, or you may listen through the stethoscope. This, as you know, is a solid or perforated piece of wood, of which one end is adapted to the ear, and the other, which is to be applied to the chest, is hollowed out, or expanded, into the shape of a bell, or funnel. The object of the instrument is to collect and convey to the ear of the observer the vibrating impulse of the air, or of the solid walls of the thorax, occasioned by the motions within. The stethoscope is sometimes useful for examining a circumscribed spot in the thoracic cavity. With it we gauge, more nicely than we could do with the naked ear, the signs furnished by the patient's voice. We *must* use it also when we would investigate the breathing in parts to which the ear itself cannot easily be applied; and in cases where, from the filth of the patient, or because he has some infectious disorder, we desire to avoid any immediate contact with his clothes or person; and in cases of young or old ladies, to whose breasts it might not be seemly or delicate to be putting our faces. But, with these exceptions, the stethoscope, in my experience, is rather a hindrance than a help. Much, however, depends upon custom. I can best distinguish most of the sounds to be heard within the thorax by my unassisted—perhaps I should say my unimpeded—ear: and I therefore employ the stethoscope or cylinder in such cases only as I have just adverted to. I cannot do without the instrument, but I do without it whenever I can. Care should be taken that the end of the stethoscope next to the ear be closely and comfortably fitted to that organ: and that its other extremity be blunt, so as not to hurt the patient; and, further, that it be uniformly and evenly applied to the surface of his chest. If these precautions be attended to, the shape and fashion of the instrument are very unimportant.

LECTURE XLVIII.

CATARRH: ITS VARIETIES. ACUTE BRONCHITIS. DRY SOUNDS ATTENDING THE RESPIRATION; RHONCHUS, AND SIBILUS: MOIST SOUNDS; LARGE AND SMALL CREPITATION: HOW THESE ARE PRODUCED, AND WHAT THEY DENOTE. TREATMENT OF ACUTE BRONCHITIS. PERIPNEUMONIA NOTHA. SUDDEN INFARCTION OF A LARGE BRONCHUS.

IN the last lecture I described to you, in a general manner, the method of auscultation; or the employment of the sense of hearing in the investigation of disease; and particularly of *thoracic* disease. I explained, as well as I could, the different sounds which result from percussing various parts of the chest; and from the entrance and exit of the air during the several acts of breathing, speaking, and coughing. I mentioned certain conditions in which those natural sounds are abolished: but I did *not* speak, except incidentally and very cursorily, of the altered and new sounds to which the presence of disease within the thorax has been found to give rise. Nor do I propose to enter upon any *formal* account of these morbid sounds. I shall endeavour to make you acquainted with their characters, and causes, and signification, as they arise in the progress of the separate thoracic diseases which I am about to consider.

Before you can possibly appreciate the *morbid* sounds, you must make yourselves familiar with the *natural and healthy*. You must have a standard, by which you may measure the numerous deviations from the natural sound, that will reach your ear in disease. Listen to the voice and the breathing of healthy children—or of some of your friends and fellow-students—and you will soon recognize those respiratory sounds which accompany the perfect state and working of the breath-machine.

I intend to commence with those diseases of the lungs in which the *mucous membrane* lining the air-passages is primarily or principally involved. This membrane is often affected *alone*. It is much exposed to known *causes* of disease: to alternations of temperature in the air which is constantly passing over it; to the irritation produced by acrid or noxious matters, solid or gaseous, which are mixed and inhaled *with* the air. And when disease commences in *other* parts of the lungs, it seldom fails to reach, sooner or later, the *mucous membrane*. In diseases also of the *heart* the same mucous membrane is very liable to be consecutively affected, by reason of those alterations in the condition of its capillary vessels which the disorder of the circulation produces.

Now I shall first point out the changes to which this mucous membrane is liable; and then describe the modifications of the natural sounds that result from such changes; and lastly, consider the different forms of disease which these changes constitute, and these altered sounds denote.

In a former part of the course I gave you some account of the peculiarities which differences of texture impress upon the phenomena and process of inflammation: and among the rest, I spoke of the behaviour of the *mucous* tissue when inflamed.

Catarrh.—The mucous membranes, in the state of health, are perpetually *moist*. The exhalation of this moisture, to a certain amount, and *not beyond* a certain amount, constitutes an essential part of their healthy functions. Now their *inflammation* (for I am about to consider first the inflammatory affections of the membrane of the air passages; some of them indeed I *have already* discussed), I say the inflammation of these mucous

surfaces alters their ordinary secretion. An inflamed mucous membrane is in the first instance *dry;* its secretion is suspended. But this is not the only change that takes place in it; it becomes tumid also, swollen, thicker than before: it is redder than natural: and its sensibility undergoes a perceptible modification. *Pain*, in mucous membranes, is *not* a common phenomenon: for their texture enables them to expand or dilate freely, so that they escape much tension, and the pain which is produced by tension: but their natural sensations are blunted, and new and uneasy sensations arise in them: sensations of heat, fulness, itching. It happens that we can *see* a portion of the mucous membrane that belongs to the air passages; and by noticing the changes produced in it by inflammation, we infer those which are apt to take place in the parts we *cannot* see. We have all often experienced in our own persons an inflammatory state of the membranes lining the nasal cavities; the schneiderian membrane. At first the nostril is preternaturally dry: yet though it is dry, you cannot breathe through it: it is stuffed up; not with accumulated mucus, but by the mere swelling of the membrane: the sense of smell is perverted or lost; the part is evidently red; it is tender also and irritable; the contact of atmospheric air a little colder or a little less pure than common, provokes sneezing. The affection extends often into the frontal sinuses; and headache and oppression ensue: or it passes into and through the lachrymal sac, the conjunctiva participates in the inflammation, the puncta lachrymalia become impervious, and the tears flow over the cheek. And with all this there is sometimes shivering or chilliness; and the pulse, especially in the evening, becomes a little more frequent than common. There is slight fever. After the unusual dryness, the membrane begins to secrete a then serous fluid, having acrid properties; for it reddens and frets the alæ nasi and upper lip over which it flows. By degrees, this thin serous fluid becomes thicker, and as it becomes thicker it becomes less irritating also, more viscid, opaque, and yellow: the swelling of the membrane diminishes; it is less raw and sensitive: at length the secretion resumes its natural *quality*, and is reduced to its natural *quantity* again; and the tumefaction of the membrane entirely disappears. This is the course of what is popularly called a *cold in the head.* When the defluxion from the nasal membrane is considerable, systematic writers call the complaint *coryza;* when it is attended with much pain and weight about the *frontal sinuses*, it is named *gravedo.* It is a variety of *catarrh.* In catarrh, sometimes one part, and sometimes the whole, of the mucous membrane of the air passages suffers inflammation. If the disorder goes down into the lungs, it is said to be a *cold in the chest:* or, from one of the most prominent of its symptoms, *a cough:* in medical language, *bronchitis.* It sometimes travels from one part of the membrane to another. Beginning, for example, in the nose, it gradually creeps down into the windpipe and lungs. Sometimes the inflammatory condition passes from the throat into the eustachian tubes, and produces deafness; or down the gullet and to the stomach, causing qualmish or other uneasy sensations, and a loss of appetite. And occasionally this order appears to be reversed. There are some persons who will tell you that whenever any thing disagrees with their stomachs, whenever dyspepsia is produced by some error in diet, they are sure to have catarrh.

Now I have adverted to this *cold in the head*, or coryza, because the phenomena which are open to our inspection in the schneiderian membrane take place also, no doubt, in the bronchial. The membrane is first dry, and tumid, and irritable; the uneasy sensations of which it is the seat prompt to the action of coughing. The chest feels tight, stuffed, constricted. There is some hoarseness, and a sense of roughness and soreness in the windpipe; and a dry cough, which seems to arise from some irritation about the glottis. Sometimes, with these symptoms, pains in the limbs, like the pains of rheumatism, occur; the appetite is impaired; the patient is thirsty; and a general lassitude is felt all over the body.

But what effect has the altered state of the membrane upon the sounds elicited by percussion; or heard within the chest, by the ear, during respiration? Why it brings us acquainted at once with two remarkable modifications of the natural sound of breathing; and these I must describe and explain.

I will take this opportunity of again recommending you to read and study the little book of lectures recently published by Dr. Latham. It contains a very plain and clear account of the auscultatory signs of disease within the chest; and he speaks of these signs in more easy and popular language, with less of over refinement, and a less sub

servient adoption of the French mode of thinking and writing on these subjects, than any other English author that I know of. I recommend his little volume the rather also, because he uses the nomenclature which is the most familiar to myself: in fact, as we some years ago saw and talked of these matters together in the wards of St. Bartholomew's, we were likely to employ the same terms.

When you listen, I repeat, to the breathing of a healthy person, you hear, as the breath goes in and out, but especially as it goes in, a smooth and gentle rustle—the *respiratory murmur*, or the *vesicular breathing*. But when the inner surface of the bronchial tubes, and of their ramifications, is preternaturally dry, and tumid, this sound is altered: you hear a hissing, or wheezing, or whistling, as the breath goes in and out; and this is technically called *sibilus:* or you hear a deeper note, a snoring noise, as the patient inspires or expires—a sound like the cooing of a pigeon, or the bass note of a violin, or the droning hum of an insect in its flight; and this is called *rhonchus*. These two, in their various modifications, constitute the *dry* sounds of respiration; and it will be worth while, once for all, to reflect upon their cause and nature, and the manner in which they are combined, and what they denote. You are aware that when air is propelled through a cylindrical tube of a certain size, and when that tube is narrowed in a particular way at one or more points, a musical note is produced. Now this is what often happens in the larger bronchi; this is what *always* happens in them when *rhonchus* is present. Rhonchus belongs to the larger divisions of the bronchi exclusively; and as these are often, for a time, exclusively affected, so rhonchus may exist *alone*. It will be grave or deep in proportion to the length and diameter of the tube in which it is produced. When the sound *is* grave and deep, the hand placed upon the chest may frequently perceive a trembling or thrill communicated to its parietes. I believe that rhonchus is mostly occasioned by portions of viscid half-solid mucus, which adhere to the membrane, and cause a virtual constriction of the air-tubes, and act as vibrating tongues while the air passes by them. I conclude such to be the case, because it seldom happens that the rhonchus cannot be got rid of by a vigorous cough. It will soon begin again, perhaps, or it will commence in some other part, but the effort of coughing, which detaches and removes the adhering tough mucus, dislodges also, for the time, the rhonchus. Yet, rhonchus in a given spot may be permanent: a tumour, or a tubercle, may flatten one of the air-tubes, and convert it into a musical instrument. For the most part, you will find what I have told you holds true: you may suspend the rhonchus by getting the patient to make a hearty cough. Now in the natural state of the chest, we do not, except in particular spots, hear the transit of the air through the larger bronchi. Whatever sound it makes is damped by the spongy lung, or covered by the vesicular breathing. But rhonchus, in its turn, may overpower the vesicular murmur, and render it inaudible. It does not prevent it, but it outroars it, as it were. Yet this is seldom the case: you hear the rhonchus, and, if you listen attentively, you may in general hear, mingling with it, the vesicular murmur also. Recollect, then, that rhonchus belongs to the larger divisions of the air-tubes; that it denotes their partial narrowing; that it is a dry sound; and that the condition of which it is expressive implies usually no danger: there is no material obstacle to the passage of the air through these larger tubes to the vesicular structure beyond them.

I must further admonish you, that in your earlier essays in auscultation you will be apt to deceive yourselves in respect to the exact place in the lung in which the rhonchus which you hear is produced. It is so loud a sound, that when it proceeds from a single bronchial tube it may be plainly audible over the whole of that side of the chest; and sometimes, more obscurely, over the other side too.

When air is driven with a certain degree of velocity through a small pipe, it gives rise to a hissing noise. It is by forcing air through a cylinder perforated by a slender tube, that Professor Wheatstone obtains the sound of the letter S in the talking machine which he has constructed, after Kempelen's model. Precisely this condition we have in the smaller bronchial ramifications, when the inflammation in catarrh or bronchitis has reached *them*, and rendered the membrane lining *them* tumid. And sibilus is the result of this change. Now sibilus, like rhonchus, may exist alone; and, inasmuch as the sibilus proceeds from the smaller air-tubes, adjacent to the pulmonary vesicles, it *abolishes* the natural respiratory murmur. It does not, like simple rhonchus, merely drown it, but it takes its

place. If you hear the respiratory murmur mingling with sibilus, you may be sure that some of the lesser air-tubes are narrowed, and some free: you cannot have both sounds at once from the same ramifications of the bronchi. Sibilus is a sound of more serious import, therefore, than rhonchus; it bespeaks a condition of greater danger. It belongs to the smaller air-tubes and vesicles, and denotes that they are in the first stage of inflammation, which has diminished their natural calibre, by rendering the membrane tumid. It is a dry sound, but you cannot cough it away.

I say rhonchus may occur alone, and sibilus may occur alone; but very often indeed they both occur together; and may be heard in various parts in different degrees: causing a strange medley of groaning, and cooing, and chirping, and whistling, and hissing, mixed, it may be, here and there, with the natural respiratory murmur. When you hear sibilus over the entire surface of the chest, the mucous membrane is universally affected, and the case is a severe one, and attended with considerable hazard.

It is just possible that a sibilant sound may proceed from a large air-tube, when its bore has been narrowed to a very minute slit or orifice; but this possibility does not interfere with the general distinctions that I have been endeavouring to point out.

Now in these cases we neither obtain nor require any information from percussion, except of a negative kind. Supposing the inflammation confined to the mucous membrane, the resonance on percussion will not be sensibly diminished; the lung is everywhere spongy still, and air reaches every part of it, though not with the usual freedom.

There is one exception to this. Occasionally, though rarely, a piece of tough phlegm may seal up, as it were, the very entrance of one of the principal bronchial tubes, and so prevent the air from passing to or from the portion of lung to which that tube conducts. When this happens, it is very likely to puzzle the auscultator for a time. There is air in the sealed-up portion of lung, therefore percussion gives a natural sound; but the air is at rest, therefore no sound of respiration is audible. An effort of coughing unstops, perhaps, the bronchial tube; and then the air is again heard to enter and to depart from that portion of lung. I shall advert to this sort of accident again.

Finally, I may remark, that these dry sounds, rhonchus and sibilus, are heard during the *breathing;* they have no relation to the voice or to the cough.

After a while, the inflamed membrane begins again to pour out fluid; but it is not the thin, bland, moderate exhalation of health; it is a glairy, saltish, transparent liquid, like white of egg somewhat; and if it be expectorated only after much coughing, it will be frothy also, *i. e.*, it will contain many bubbles of air entangled in it. It is a stringy tenacious fluid, and the more so in proportion to the intensity of the inflammation. With this new condition of the membrane, we have new sounds—sounds which result from the passage of air through a liquid; sounds which are occasioned by the formation and bursting, in rapid succession, of numerous little air-bubbles. These sounds are called *crepitations.* This process may take place in the larger air-tubes, or it may take place in the smaller, or in both. In the larger tubes the *bubbles* will be larger, and the ear can readily distinguish this; we have *large crepitation.* In the smaller air-tubes, we have, in the same way, *small crepitation.* There is no difference between these sounds, except in degree; and they graduate insensibly into each other. But there is a considerable difference in the nature of the intimations which their well-marked varieties convey. If there be merely large crepitation, without any other morbid sound, it is produced in the larger tubes. Air passes, notwithstanding, into the vesicular structure *beyond* the accumulated liquid; and vesicular breathing *exists*, though perhaps it *cannot be heard*, on account of the crepitation. But the state of the patient is not a state of peril. On the other hand, small crepitation has its seat in the smaller air-tubes and cells; it supersedes the vesicular breathing, and, if extensive, it bespeaks considerable danger.

Rhonchus and large crepitation are respectively the dry and moist sounds that belong to the larger bronchi; sibilus and small crepitation the dry and moist sounds of the smaller branches. When the latter sounds are heard over a considerable part of the chest, there is, I say, usually a good deal of distress, dyspnœa, and cough; and the fever which attends the local inflammation is at its height. By and by the expectoration becomes opaque, and more consistent, and of a greenish or yellowish colour; it is brought up with more ease; the crepitation, great and small, diminishes; perhaps rhonchus reap-

pears: but at last the parts return to their original condition; and the natural, smooth, equable rustle of the breathing is again everywhere audible.

These are all the morbid sounds to which active and recent inflammation of the mucous membrane of the air-passages ever gives rise: rhonchus and sibilus; large and small crepitation. Having once described their nature and causes, I need not repeat the description if we find them accompanying other diseases: but their *import* may be different. I may mention here, that as crepitation results from the passage of air amongst and through liquid, from the rupture of the little air-bubbles so produced, the *kind* of liquid may vary. If the air in going and returning, meets with serum, or with pus, or with blood, it will occasion exactly the same bubbling noise. Hence the French term for what I have been calling crepitation, viz. *mucous rattle*, is very objectionable. From the sound itself, we cannot tell whether it proceeds from *mucus* or from some other liquid present in the air-passages; and from this objection the word crepitation, whatever exception may be taken against it on other accounts, is free.

Having thus embraced the occasion of explaining these auscultatory signs, I will now resume the history of catarrh. It implies inflammation of the mucous membrane of the air-passages; and it receives different appellations, according to the district of that membrane which it chiefly plagues: gravedo, in the frontal sinuses; coryza, in the schneiderian membrane of the nose; bronchitis in the trachea and lungs.

Catarrh is the commonest of all disorders. Not one man in ten thousand passes a winter without having a *cold* of some sort. And this name points to its ordinary cause: cold somehow applied to the body. It does not always or often result, I apprehend, from cold air brought into contact with the membrane itself, in the process of breathing; but from cold, and especially from cold and wet, applied to the external integument. It is unnecessary for me again to go over the circumstances under which the application of cold is most likely to prove injurious. Catarrh is usually a trivial disorder, and runs its course in a few days, if abstinence be observed with respect to animal food and stimulating liquor, and if the patient remains in an equable temperature, and avoids re-exposure to the cause of his malady. I am now speaking of the milder forms of catarrh. We are not often consulted for this complaint. Every man, in regard to a cold, thinks himself qualified to be his own doctor. But if you *are* consulted, keep your patient in the house, or even in bed; let him live upon slops; give him a gentle aperient, and then some of those medicines which are esteemed to be diaphoretic; small doses of James's powder; three drachms of the liquor ammoniæ acetatis, with a drachm of the spiritus ætheris nitrici, and an ounce of camphor mixture; or a saline draught with an excess of alkali, and a few grains of nitre, or a little antimonial wine; three or four times a day: and let him take four or five grains of Dover's powder, and put his feet and legs into a warm bath, just before he goes to bed. In this way you may conduce to his *recovery;* and he may be simple enough to believe that you have *cured* him.

Yet I believe catarrhs *may* sometimes be *cured;* and the natural recovery from them *may* be, *sometimes*, accelerated. If you practise the old maxim, which says, "*venienti* occurrite morbo," you may occasionally stop a cold on the threshold, as it were, by an opiate. And to persons who are habitually troubled with slight catarrhs, this piece of practice may prove of the greatest value. A medical man who resides in this neighbourhood, and with whom I was a fellow-student, is exceedingly subject to what he calls a snivelling cold. For many years he used to bear this as he best might; and that, to say the truth, was very ill and impatiently. On one occasion, almost by accident, he took twenty drops of laudanum just as one of his colds was beginning to torment him; and he found that the iniatory symptoms ceased. Since that time he has constantly had recourse to the opiate under similar circumstances; and whereas he used formerly to be very miserable for three or four days, he now is quite comfortable in the course of half an hour. And this is not a solitary case. It is worth trying, if you experience the feelings of an incipient catarrh, to go to bed, and to take a beaker of hot wine negus, with a tablespoonful of the syrup of poppies in it. This will not suit every person; but if it fails on the first trial, it need not be repeated, and no great harm, beyond an increase of headache, will be done by it. I would not recommend this plan, however, to a plethoric person; nor to any one having a tendency to inflammatory disease; for when it does not cure, it makes the complaint worse.

There is also a period in catarrh which has gone on unchecked, when you may accelerate its departure—"speed the going guest"—by a good dinner, and an extra glass or two of wine. But this pleasant method is scarcely to be advised for persons of delicate habits; or in whom any phthisical tendency is suspected to exist; or who are prone to inflammation. And it is not to be tried with any one till the fever is over, and the expectoration thick and loose.

I must not omit to mention the *dry* plan of cure; although (I confess it with some shame) I have never yet tried it either upon myself or upon others. Dr. C. J. B. Williams, who invented it I believe, has a high opinion of its efficacy. It certainly has the merit of simplicity, for it consists merely in abstinence from every kind of drink. No liquid, or next to none, is to be swallowed until the disorder is gone. The principle here concerned is that of cutting off the supply of watery materials to the blood. The wants of the system exhaust, from the circulating fluid, all that can be spared for the natural evacuations; and there is nothing left to feed the unnatural secretion from the inflamed mucous membrane. Its capillary vessels cease to be congested; the morbid flux is diverted, and the inflammation starved away. Such is the theory. Habitual topers might hold the remedy to be worse than the disease; but Dr. Williams assures us that the necessary privation is not very hard to bear: and that it achieves a cure, upon an average, in forty-eight hours. He allows, without recommending, a tablespoonful of tea or milk for the morning and evening meals, and a wine-glass of water at bedtime.

One great advantage of this plan is, that it does not require confinement to bed, or to the house. The man whose business calls him abroad, may, with appropriate clothing, pursue his customary employment, and his cure is all the while going on. In fact, exercise, inasmuch as it promotes perspiration, helps the recovery; whereas the system of warm drinks and diaphoretics renders the body more susceptible to atmospheric vicissitudes; and, to be effectual, implies restrictions which are oftentimes extremely inconvenient.

Dr. Williams observes, that while this dry treatment is serviceable in catarrhal bronchitis, it is *most* successful in coryza, the snivelling cold in the head. It must be put in force in the very commencement of the disorder.

You may often do much by way of *prevention*, for persons who are unusually liable to take colds. I have remarked before upon the great value of the *shower bath* for that purpose. I could mention several instances in which persons have got rid of the tendency to catch cold by the habitual use of this aspersion. It should be begun in the summer, and made tepid at first; but in a short time quite cold water may be employed; and being once begun, the practice may be continued through the winter. I stated formerly, that the effect of exposure to cold was, *cæteris paribus*, in proportion to the intensity and the duration of the *sensation* of cold that it produced. The intensity of the sensation of cold under the shower bath is considerable, but the duration of it is momentary. It operates as a prophylactic in this way: it inures the surface to a lower temperature than it is likely to be subjected to at any other part of the day. The lesser degrees of cold have then no injurious effect, unless they are long protracted. For those who cannot procure a shower bath, or who cannot bear its shock, cold sponging will be found exceedingly salutary.

Acute Bronchitis.—But inflammation of the membrane lining the air-passages may be, and often is, a very acute and dangerous disorder, *i. e.* the inflammation may be both intense and extensive; it may descend into the vesicular texture, and occupy the whole surface of the membrane on one side of the chest, and then it may be a very grave disease; or it may involve the whole lining membrane of both lungs, and then it is always attended with considerable peril.

This inflammation will sometimes, when it is thus *general* over the whole membrane, linger for a considerable period in its first stage; and it may even, after so lingering, subside again without ever passing beyond the first stage. By the first stage, I mean the stage of dryness. Very little notice of this modification of bronchitis has been taken by authors. Dr. Latham has given a distinct and graphic description of it, to the accuracy of which I can testify from my own experience. You will find some cases of it detailed

in his book. Since they were published, some striking instances of this form of the disease have occurred to myself. One, which happened lately, I will relate by way o. example. I was asked by an old pupil of the hospital to see a lady, his patient, in Gordon Square. I found her feverish, and in a state of extreme dyspnœa, sitting up in bed from inability to lie down, labouring for her breath; her face turgid and rather livid, her nostrils working, her shoulders elevated. She could scarcely speak, but expressed, in what she did say, a dread of immediate suffocation. She had been in nearly the same state for a day or two. On listening at her back I could hear the air slowly wheezing and whistling into her lungs everywhere, and then leaving them still more slowly, with a prolonged growl, something like that of an angry cat. There was no true vesicular breathing; there was no crepitation; and there was no part into which the air did not, although with difficulty, find its way. The chest was everywhere resonant on percussion. There could be no doubt that the membrane throughout was tumid and dry, and in the earliest stage of inflammation. Depletion had already been employed in this case, and we had recourse to the tartar emetic. This was given in free and repeated doses, till it produced nausea and sickness. Whenever it did so, the pulse diminished in force, the face became blanched, and the breathing much easier; and the medicine was then suspended until these effects had gone off, when it was repeated in the same manner. The disease was not put a sudden stop to however, by this treatment; it was kept at bay for a day or two longer, and then a copious secretion from the membrane took place, with great relief to all the distressing symptoms. Then, of course, crepitation became universally audible. Except the debility which it left behind, the patient soon recovered of the bronchial inflammation.

But in the great majority of instances the inflammation does not thus linger in its first stage: the membrane soon begins to pour forth glairy mucus; so that we do not often meet with *sibilus*, without finding at the same time, in some part of the same lung, that there is also small and large crepitation. It is of some importance to attend to the characters of the mucus that is expectorated. It is transparent and viscid. If you pour it from one vessel into another, it flows out in one mass of extreme tenacity; it will draw out sometimes like melted glass; and the degree of viscosity is a tolerably accurate measure of the degree of the existing inflammation. Upon the surface of the viscid mucus there is usually more or less froth, the *quantity* of it depending on the facility or the difficulty with which the sputa are brought up. If the patient does not expectorate till after a long fit of coughing, during which air has been many times inspired and expired, and has thus got intimately mingled with the mucus that fills the air-passages, the expectoration will contain numerous little air-bubbles; will be very frothy. Sometimes also, during this stage of the complaint, the sputa are marked with streaks of blood.

While the expectoration possesses the characters I have been describing, the inflammation is still intense, and the fever and dyspnœa considerable. This correspondence between the general symptoms and the matters spat up was well known to the ancients, who said that such expectoration was still *crude*. But in proportion as the inflammation approaches to resolution, the appearance and qualities of the sputa are changed: the mucus loses by degrees its transparency, is mixed with masses that are opaque, and of a yellow, white, or greenish colour: and these masses, few at first, increase more and more in number, until they constitute the whole of the sputa. Such expectoration as this is commonly accompanied by a marked remission in the different symptoms of the bronchial inflammation: it announces that the inflammation is terminating in resolution. It is such as the ancients spoke of as being *concocted*, or *ripe*. However, the characters of the opaque sputa expectorated towards the end of an attack of acute bronchitis are subject to much variety.

It will often happen that the expectoration, after having thus become opaque, and particoloured, will go back again to its former condition of transparency, and stickiness, and froth: and that is a very certain index of a return or increase or extension of the inflammation; so that an observance of the characters of the matter expectorated teaches us, in a certain degree, the progress of the inflammation; and consequently constitutes one point of guidance to our treatment. The nature of the expectoration forms also an important particular in the means of distinguishing bronchitis from pneumonia; as I shall further explain when I speak of the latter disease.

I have described acute bronchitis as it appears when it terminates favourably: in

such cases the inflammation generally begins to abate, somewhere from the fourth to the eighth day of the disease. But acute bronchitis may terminate *unfavourably*. When the inflammation is universal and intense, the fever high, and the labour of respiration great —if the symptoms do not yield to the treatment employed, or if judicious treatment has not been adopted, or has been too long delayed, signs of impending suffocation begin to show themselves: the lips, and cheeks, and tongue, assume a purplish colour; a livid paleness takes the place of the former red flush; the expression becomes more and more anxious; delirium comes on, and rapid sinking. These indicate, you know, the circulation of blood that is in a great measure venous through the arteries; and the venous blood acts as a poison when it so circulates. Profuse, cold, and clammy sweats ensue; and the patient dies of apnœa. His breathing is choked by the morbid secretion which occupies the bronchial tubes, small as well as large, and which he has not strength enough left to cough up.

Accordingly, when we examine the thorax after death so produced, we find, in the first place, that the lungs do not collapse upon the admission of the pressure of the atmosphere to their external surface. We next find the trachea, and bronchi, and their ramifications, blocked up by a frothy adhesive mucus, resembling that which during life had been expectorated: and the membrane which lines them is red and thickened.

The treatment proper for these acute and dangerous forms of bronchitis is a matter of some nicety. Blood-letting, as I formerly stated to you, has not that decided power over inflammation of the *mucous* tissues which it possesses over the adhesive inflammation that takes place in the serous membranes. If there be much fever, a hard pulse, and great oppression of the breathing, and particularly if these symptoms present themselves in a young, strong, and robust individual, we must bleed him from the arm. And you will always find blood-letting *relieve* the symptoms; even when its ultimate effect may be injurious. The patient's distress arises from his inability to supply air enough to arterialize the venous blood which is transmitted to his lungs; and by diminishing the quantity of blood sent to those organs, you will, *pro tanto*, mitigate his uneasiness. But a great part of the danger to be apprehended in the advanced periods of the disease, is that the patient may not have muscular power enough to disembarrass his air-passages of the phlegm that overloads them; to draw a strong breath, and to achieve a vigorous cough. We must not bleed therefore to syncope, and again and again, as we are often justified in doing in cases of pneumonia. Sixteen ounces will be a moderate bleeding at first for an adult, but more or less than that must be taken, and the bleeding must be repeated or not, according to the condition of the pulse: for the pulse affords a better measure of the propriety of pushing the abstraction of blood, than is furnished by the local symptoms.

Great relief is often obtainable by *topical* blood-letting; by cupping over the surface of the chest, or between the scapulæ. If you distinguish sibilus in one portion of the lung more than in another, take the blood rather from that part of the surface which corresponds to the place of the sibilus.

After the bowels have been cleared by a mercurial purgative, calomel and jalap for example, you will find the tartar emetic a very valuable medicine in these acute cases of bronchitis. It should be given in such doses as will excite nausea: and if vomiting be occasioned, you may still go on with the medicine after the sickness has subsided. The depression which this substance produces is great, but it is temporary, and it is effected without expending blood. With the antimony—I mean during the same period—mercury may and ought to be given: to this combination I should be inclined to trust more than to any other internal treatment.

If symptoms of sinking and debility have begun to show themselves, it will be necessary to administer stimulating expectorants. I presume that the carbonate of ammonia, which is often extremely useful in such cases, acts as an expectorant, by giving a fillip to the muscular power. But it is supposed by some persons to exercise some specific influence upon the bronchial membrane. However this may be, five or six grains of it, given in solution every four or six hours, are often followed by free expectoration and a marked improvement.

One of the circumstances of which patients are much disposed to complain, is the violent or importunate cough; and another is, the want of sleep and of rest: indeed, the one of these is often, in a great measure, the cause of the other:—the urgency or frequency of the

cough prevents the patient from sleeping. Now there is nothing so well calculated to allay cough, and to procure sleep, as opium; and you will be strongly tempted to give these patients opiates, and you will probably be encouraged to do so by the success which will follow that practice in many cases. The good effects of a full narcotic at bed-time are sometimes very striking. Patients who for previous nights have been perpetually harassed by cough, and who are worn out by the disturbance of their rest, will sleep tranquilly, and in the morning expectorate largely and freely, and declare themselves wonderfully the better for their opiate. Yet opium is a ticklish remedy in these cases. Many a patient—some within my own knowledge—labouring under general or extensive bronchitis, have been put so soundly to sleep by a dose of opium on going to bed, that they have never waked again. I believe you may receive it as a golden rule in these cases, not to give opium—I mean in a full dose, so as to force sleep—if you see any venous blood mingling in the general circulation,—if the complexion be dusky, and the lips in any degree blue. The circulation of half-arterialized blood through the brain is in itself a powerful cause of coma; and if you add the influence of an opiate, the coma may easily be made fatal. While the cheeks and lips remain florid, and when the first violence of the disease has abated, an opiate will do capital service. It is a common practice to combine it with antimony or some other expectorant. Twenty minims of laudanum, with the same quantity of the liquor antimonii tartarizati; or a third of a grain of the acetate of morphia, with a drachm of oxymel of squills; are convenient forms.

Counter-irritation is frequently of great use, as an auxiliary measure, in the treatment of acute bronchitis. Sensible relief of the cough, and of the oppressed breathing, often follows the rising of a large blister laid across the front of the chest. When the dyspnœa is extreme, and a more speedy counter-irritant is required, you may have recourse to the mustard poultice. Inhalation of the steam of hot water is also very soothing and useful. It is one of the best expectorants I know of when it answers at all; but to some persons it proves irritating, and they derive no comfort from it.

I have been speaking of acute bronchitis, uncombined with any other pulmonary disease; and it is curious how little disposed the inflammation often seems to be to extend itself from the mucous membrane to the neighbouring tissues. The reason, doubtless, is, that this membrane is furnished with a distinct set of blood-vessels, the bronchial arteries, and veins: while the substance of the lungs is supplied by the pulmonary. We could not tell, merely by attending to the general symptoms, whether the inflammation was limited to the inner membrane or not; but by making use of the sense of hearing, we *are* able to determine this. If the inflammation should spread to the parenchymatous texture of the lungs,—*i. e.* if the bronchitis should pass into pneumonia,—this circumstance should be disclosed by physical signs, which I shall in due time describe and explain; and it would demand certain modifications of our plan of treatment.

Peripneumonia notha.—I shall have to speak of some varieties of chronic bronchitis; but there is a mixed form of pulmonary disease that requires to be noticed, in which acute or subacute inflammation engrafts itself upon changes that are chronic and abiding. Sydenham calls the disorder to which I now refer *peripneumonia notha*—bastard peripneumony. *Catarrhus senilis* is another of its names. It may be considered as chronic bronchitis, occurring in old persons, and very apt to be converted into pneumonia, or to be greatly aggravated in degree during winter, or upon any accidental exposure. This is the common complaint of persons advanced in life. I mention it here in compliance with the usual custom, and because this is as convenient a place for its introduction as any. But it would be an error to regard it as exclusively a disease of the mucous membrane of the lungs. An habitually congested state of that membrane, marked by some shortness of breath, and some expectoration, and by the constant presence of some degree of crepitation in the lower parts of the lungs,—these are circumstances which are of daily occurrence as consequences of *disease of the heart;* and it is in persons whose habitual health is of this kind, that what is called peripneumonia notha, which implies a diffused inflammation of the pulmonary mucous membrane, with sometimes an enormous secretion from its surface, is most apt to supervene. Almost all such persons will tell you that there are periods at which they experience slight febrile attacks, and exacerbations of their com-

plaints: they have pain in the breast or side, headache, heat, and thirst; and at these periods the cough and expectoration are always aggravated, and continue for some time to be more than commonly severe. "The disease (says Cullen, who following Sydenham, has given a good description of its general symptoms) has often the appearance only of a more violent catarrh; and after the employment of some remedies, is entirely relieved by a free and copious expectoration. In other cases, however, the feverish and catarrhal symptoms are at first very moderate, and even slight; *but* after a few days these symptoms suddenly become considerable, and put an end to the patient's life, when the indications of danger were before very little evident."

The truth is, (and we learn the truth by the evidence of auscultation,) that in these cases *pneumonic* inflammation is often suddenly set up. There is no security, as Dr. Latham observes, that the portions of lung which yield small crepitation to-day may not be solid and impervious to-morrow. Dr. Latham is of opinion that in this disease the inflammation is apt to travel over the bronchial membrane from place to place, as erysipelas is seen sometimes to wander over the surface of the body. I know not how this may be; but certainly death is often produced in these persons by the sudden spoiling of even a moderate portion of the lung. In their ordinary condition, the patients have just enough, and no more, of the respiratory apparatus in an effective state, whereby to subsist; and when a fresh part of it is rapidly rendered solid, they quickly perish. But they die also from another cause. The nicety of treatment which I spoke of as being required in certain stages of acute bronchitis, is still more apparent and necessary here. We are placed in this dilemma. If we do not take blood in these attacks, we run a risk of losing our patient from the effects of the unchecked inflammation; and if we do bleed, we are in danger of losing him by producing a degree of weakness which will render him unable to expectorate the effused mucus, and so liable to perish by suffocation. Leeches and blisters, and what are called expectorants, are the remedies to which we must chiefly trust. Medicines which are at the same time diuretic are also serviceable—the spiritus ætheris nitrici, the preparations of squill, and of digitalis.

Even in younger patients, in whom the bronchitis is idiopathic, not engrafted upon any previous disease of the chest, and in whom the disorder had not appeared severe, extreme difficulty of breathing will sometimes most unexpectedly arise, and sometimes it will rapidly lead to the extinction of life. Now both Andral and Laennec have pointed out one cause (to which, indeed, I have already adverted in the present lecture) of this sudden change for the worse. It is a cause which was not likely to be even guessed at before the discovery of the method of auscultation. Occasionally it happens, they say, that during the course of an attack of bronchitis, we cease altogether to hear, in a certain extent of the lung, either the natural respiratory murmur, or any of the modifications of rhonchus, sibilus, or crepitation, that have been mentioned; yet over this same portion of lung, in which no sound, healthy or morbid, is heard by the ear applied to the thorax, percussion gives the natural hollow sound. At the same time the patient becomes affected with urgent dyspnœa. This sudden suspension of all sound of respiration they attribute to the temporary, or, in some cases, the permanent, obstruction of one of the bronchi, the ramifications of which are distributed to that portion of lung in which the respiration is no longer audible. In such cases it generally happens that at the end of a strong fit of coughing—the effect of which is to expel, or at least to displace the tenacious plug of mucus obstructing the bronchus—the sound of respiration is re-established as suddenly as it had previously disappeared; and the dyspnœa also ceases. In some rarer cases, however, the noise of the pulmonary expansion does not return, the difficulty of breathing increases, suffocation becomes imminent, and death by apnœa rapidly takes place. The slightest attack of bronchitis may in this way be suddenly transformed into a most serious and quickly fatal malady.

Andral relates two instances of death from this cause; one of which was the following. A coachman, fifty years old, had been several times a patient in La Charité, for obstinate pulmonary catarrh, with slight dyspnœa, and puriform expectoration. Every time he went away relieved, but not cured. On both sides of his chest could be heard all the varieties of rhonchus. In one spot the column of air which penetrated the bronchi imitated the snoring of one in a deep sleep; in another spot it was like a dull and prolonged groan; in a third,

a sound resembling that made by a bellows; and in a fourth the cooing of a turtle-dove was exactly simulated. On the last occasion of his entering the hospital, his respiration was still tolerably free. One morning he was found in a state of unusual anxiety. In the middle of the night, after a violent paroxysm of cough, his breathing (he said) had suddenly become very much oppressed. It was discovered, on auscultation of his chest, that no air penetrated into the upper lobe of the right lung; yet that part sounded well on percussion, even louder than the corresponding part of the other side, which was morbidly dull. The difficulty of breathing went on augmenting, and the man was soon dead.

Besides other marks of disease in the lungs, the primary bronchus leading to the upper lobe on the right side was closed up completely by tough mucus, and exhibited the appearance of a full cylinder.

In the other case, also, the obstructed bronchial tube supplied the upper lobe of the right lung.

It may seem strange that the interruption of the access of air to so small a portion of the lungs should be attended with such serious consequences, when we know that the greater part of each lung may be impermeable by air, and yet the patient may live a long time, and often without any great dyspnœa. The explanation of the apparent difficulty seems to be, that in the one case the prevention of the arrival of air in the part affected is sudden, in the other gradual. Moreover, the remaining portions of the lungs are performing their functions imperfectly.

When once attention has been awakened to the kind of accident just mentioned, the diagnosis would not seem to be difficult. We may suspect obstruction of one of the bronchi when considerable dyspnœa comes on suddenly during the continuance of simple bronchitis: and our suspicion will be confirmed if at the same time respiration ceases to be audible in a certain portion of the lung, while the sound given by percussion over the same part remains unaltered. Emphysema of the lung (which I shall explain hereafter) is the only other condition which could give rise to a similar set of symptoms.

Andral judiciously suggests the employment of emetics, and the inhalation of steam, in such cases.

LECTURE XLIX.

Influenza. Symptoms and progress. Conjectures as to its cause. Treatment. Hay asthma. Chronic Bronchitis. Its varieties. Morbid Anatomy of these affections.

Catarrh, which was the principal subject of the last lecture, occasionally prevails far and wide as an epidemic disease. I speak, indeed (February 4, 1837), during the immediate presence of one such visitation, although the extreme violence of the complaint that has been raging among us is now fast subsiding. You can hardly be without curiosity to know what has been learned respecting an influence which has thus, on a sudden, before your eyes, disturbed and sickened a whole community. I have here used, without thinking of it, the very word by which, in a foreign version, the disorder is denominated. It has received, however, various names; for it has been known and noticed from remote antiquity. Cullen calls it *catarrhus e contagio:* and under that head, in his *Nosology*, you will find a copious reference to recorded accounts of epidemic catarrh, as it has been observed to spread over great portions of the world. In France the disorder thus prevailing, is styled the *grippe*. The Italians, putting the cause for the effect, call it *influenza*, the *influence:* and this last term influenza has now become naturalized in our language. Since Cullen wrote there have been four or five more of these epidemics. One in 1782, which extended over all Europe, visiting every country therein, affecting more than one-half of its inhabitants, and proving fatal to very many of them. You will find, in the

third volume of the *Transactions of the College of Physicians*, a good account of the disease as it then showed itself in this country. In the spring of 1803 another instance of it occurred; and of this the history, as compiled from the testimony of a hundred and twenty-four observers, is preserved in the ninth and tenth volumes of the *London Medical and Physical Journal*. In the month of April, in the year 1833, the influenza again made its appearance, and prevailed extensively, both here and elsewhere: and of the influenza of 1837 you have had, and you still have, the opportunity of being observers. A very good and instructive sketch of this epidemic malady, compiled by Dr. Hancock, is to be found in the *Cyclopædia of Practical Medicine*. To that article, to the publications I just now mentioned, and to the works enumerated by Cullen, I may refer you for much which is curious and interesting in the history of the disease; but which would not be so well adapted to our immediate purpose in this place—namely, that of seizing upon the practical facts which have been ascertained respecting influenza.

Symptoms.—One characteristic feature of this species of catarrh, as distinguished from the ordinary sporadic disorder, is the sudden occurrence, in the outset, of more decided febrile disturbance. The first two patients whom I saw in the epidemic of 1833 had just the symptoms which frequently mark the commencement of an attack of continued fever; and I did not know, at my first visit, what was about to happen to them. The symptoms, taking them altogether, are somewhat as follows. The patient is chilly, and perhaps shivers; presently headache occurs, and a sense of tightness across the forehead, in the situation of the frontal sinuses; the eyes become tender and watery; and sneezing and a copious defluxion from the nose ensue, followed or accompanied by heat and uneasiness about the throat, hoarseness, a troublesome cough, and oppression of the breathing. In short, the symptoms are the symptoms of catarrh; including in that term all the varieties thereof that are sometimes met with separately—gravedo, coryza, bronchitis: and with these symptoms, a sudden, early, and extraordinary subdual of the strength; and, most commonly, great depression of spirits. The debility which comes on at the very outset of the complaint is one of its most singular phenomena, taking place, in some cases, almost instantly, and in a much greater degree than would seem proportioned to the other symptoms of the malady which it thus ushers in. Indeed, this rapid and remarkable prostration of strength is more essentially a part of the disorder than the catarrhal affection, which sometimes (though rarely) is absent, or imperceptible. It is upon the mucous membranes, however, that the stress of the disease generally falls; especially upon the internal lining of the air-passages. Those of the alimentary canal seldom escape entirely; but they suffer in a less degree. The tongue is white and creamy, the palate loses its sensibility, the appetite fails, nausea and vomiting are not uncommon, and sometimes there is diarrhœa. The pulse, in the uncomplicated disease, is soft, and generally weak. The patients complain also of pains in the limbs and back, and of much soreness, a bruised, fatigued, or tender feel, along the edges of the ribs and in various parts of the body.

In its simple form and ordinary course, the disease abates of its violence after two, three, or four days, and the patient is usually convalescent before the termination of the week: but cough and much debility are apt to survive the other symptoms, and while these continue, the complaint is very easily renewed. Pre-existing disease, and peculiar constitutional habits and tendencies, modify considerably the character of the influenza, as it affects different persons. I do not attempt to represent its various phases; they are fit subjects of study for yourselves.

Causes.—I have remarked that Cullen makes this species of catarrh to proceed from contagion. But the visitation is a great deal too sudden and too widely spread to be capable of explanation in that way. I will not say that the disease may not be in some degree infectious; for there is reason to believe that other epidemic disorders, having many points of analogy with the influenza, *are* somehow imparted from one individual to another, although they are mainly produced by some influence which resides in the atmosphere. There are points in the history of influenza which furnish a strong presumption that the exciting cause of the disorder is material, not a mere quality of the atmosphere; and that it is at least *portable*. The instances are very numerous, too numerous to be attributed to mere chance, in which the complaint has first broken out in those particular houses of a town at which travellers have recently arrived from infected places. But this

great and important question of contagion I hope to examine with you more rigorously on a future occasion. What I wish to point out now is the fact that the influenza pervades large tracts of country in a manner much too sudden and simultaneous to be consistent with the notion that its prevalence depends exclusively upon any contagious properties that it may possess. You are aware that it has recently seized upon all parts of this metropolis—and I believe I may say of nearly the whole kingdom—within the space of a very few days. It has been observed to occur also at the same time on land, and on board different ships which have had no communication with the shore, nor with each other. Thus it is stated in the *Transactions of the College of Physicians*, that on the 2d of May, 1782, Admiral Kempenfelt sailed from Spithead with a squadron, of which the Goliah was one. The crew of that vessel were attacked with influenza on the 29th of May; and the rest were at different times affected, and so many of the men were rendered incapable of duty by this prevailing sickness that the whole squadron was obliged to return into port about the second week in June, not having had communication with any shore, but having cruised solely between Brest and the Lizard. This happened in one part of the fleet. In the beginning of the same month, another large squadron sailed, all in perfect health, under Lord Howe's command, for the Dutch coast. Towards the end of the month, just at the time therefore when the Goliah became full of the disease, it appeared in the Rippon, the Princess Amelia, and other ships of the last-mentioned fleet, although there had been no intercourse with the land. Similar events were noticed in the epidemic of 1833. One or two curious instances of the sudden sickening of considerable bodies of men in different places at the same time, were related to me on good authority. On the 3d of April in that year—the very day on which I saw the first two cases that I did see of the influenza, all London being smitten with it on that and the following day—on that same day the Stag was coming up the channel, and arrived at two o'clock off Berry Head, on the Devonshire coast, all on board being at that time well. In half an hour afterwards, the breeze being easterly and blowing off the land, 40 men were down with the influenza, by six o'clock the number was increased to 60, and by two o'clock the next day to 160. On the self-same evening a regiment on duty in Portsmouth was in a perfectly healthy state, but by the next morning so many of the soldiers of that regiment were affected by the influenza, that the garrison duty could not be performed by it. I make no doubt that facts of a like nature have occurred during the present epidemic, and will be made known in due time. They illustrate several important points in respect to the disease: viz., the impossibility of accounting for its prevalence upon the principle of mere contagion—the suddenness of its invasion—and the early and extreme prostration of strength with which it is attended.

The occurrence of epidemic catarrh, as well as of most other epidemics, is unquestionably connected with some particular state or contamination of the atmosphere. What that state is, or what may be the kind of contamination, no one knows. The present epidemic followed hard upon the sudden thaw that succeeded the remarkable snow storm of the last week of the last year. A similar coincidence between the breaking out of the same disorder, and a sudden elevation of the temperature of the atmosphere, happened at St. Petersburg in the epidemic of 1782. "On a cold night (Maertens says), the thermometer rose 30° of Fahrenheit; the next morning 40,000 people were taken ill with the influenza." Now if every epidemic had been preceded by similar changes in the weather, we might resolve the universal prevalence and sudden accession of the complaint, into the effect of the cold and damp state of the air, produced by the thaw. But it is not so; for, as Dr. Hancock observes, there has not been any uniform connection between any one sensible quality of the atmosphere as to heat or cold, rain or drought, wind or calm—and the invasion of the epidemic. "Et tempore frigidiori et calidiori, et flante tam Austro quam Boreâ, et pluvioso et sereno cælo, peragravit hasce omnes Europæ regiones, et omnia loca indiscriminatim." Irregularities and great vicissitudes of weather have however gone before the disease in very many instances: but sometimes one condition of the atmosphere, and sometimes another, has been its immediate predecessor: and the epidemic has frequently been observed to fall partially and capriciously; as a blight falls upon a field or district. Short, in his chronological history of the weather, says that thick ill-smelling fogs preceded, some days, the epidemic catarrh of 1557. Jussieu states

that the grippe of the spring of 1733 appeared in France immediately after offensive fogs, "more dense than the darkness of Egypt." So also in 1775, Petit informs us that in France the disease was ushered in by thick noisome fogs. In the same year it visited the shire of Galloway in Scotland, where, we are told, "a continual dark fog and particular smoky smell prevailed in the atmosphere for five weeks, the sun being seldom seen." Dr. Darwin has recorded that, in 1782, "the sun was for many weeks obscured by a dry fog, and appeared red as through a common mist:" and he supposes that "the material which thus rendered the air muddy probably caused the epidemic catarrh which prevailed in that year." You will call to mind here the dark fog which brooded over this city in the midst of the raging of the distemper about ten days ago, and which was repeated, in a less degree, on Wednesday last (Feb. 1).

It has been observed also, that shortly before, or during, or soon after, the prevalence of these epidemic catarrhs—*epizootic* diseases have raged; various species of brutes, and of birds, have been extensively affected with sickness: while on some occasions prodigious swarms of insects have made their appearance. In short, a great variety of facts concur to render it probable that some peculiar condition of the air existed, which, though it might be favourable to the multiplication of some species of living creatures, such as the insects just referred to, operated as a poison upon the human body, and upon the bodies of many of the brute creation.

It is a very curious circumstance in the history of epidemic catarrhs, and worthy of your reflection, that they *travel;* migrate as it were from one place to another: and moreover, that they hold, for the most part, to certain courses, in spite even of opposite winds, and of variations of temperature. It has been noticed that the influenza generally follows a westerly direction, or one from the south towards the north-west. In this remarkable property it resembles, as you may perhaps be aware, the epidemic cholera.

Although the general descent of the malady is, as I have said, very sudden and diffused, scattered cases of it, like the first droppings of a thunder-shower, have usually been remembered as having preceded it. The disorder is most violent at the commencement of the visitation; then its severity abates; and the epidemic is mostly over in about six weeks. Yet the morbific influence would seem to have a longer duration. In a given place nearly all the inhabitants who are susceptible of the distemper suffer it within that period, or become proof against its power. But strangers who, after that period, arrive from uninfected places, have not, apparently, the same immunity.

The locality does not appear to be thoroughly cleared of the poison for some time: or perhaps a more cautious statement of the fact would be, that the disorder generally shows itself again in succeeding years, but in a milder and less general form. This must depend either upon remaining dregs, or possibly some revival, of the injurious influence; or else upon some abiding predisposition impressed upon the bodies of men by its former visit. You may hear, every year, of Mr. *So-and-so* having the influenza. In many instances, no doubt, common sporadic catarrh is dignified by that name; but it is certain also that many of the colds, and bronchial disorders, of the seasons which immediately follow a period of genuine influenza, are attended with much more languor, debility, muscular aching, and distress, than belong to an ordinary attack of catarrh.

All this is very curious; and very mysterious. All this, or much of it, is also true of all the diseases which are known to prevail occasionally as epidemics. The facts that have now been mentioned respecting the influenza, warrant, I think, the conclusion that it does *not* depend upon any mutations in the ordinary qualities of the atmosphere; upon any degrees or variations, I mean, of its temperature, its motions, or its moisture; upon what is expressed in the single word *weather*. Concerning a calamity so generally felt, and so obscure in its origin, conjecture, you may well believe, has not been idle. One hypothesis assigns the complaint to some change in the electrical condition of the air: to its becoming negatively electric: or to its being such as to cause an excessive accumulation of electricity in the animal economy. The facts adduced in support of these views are of this kind. Meat, sent up by means of a kite, high into the atmosphere, during the prevalence of the disease, has returned putrid. Large heavy separate clouds, in a state of negative electricity, have been observed just before the setting in of an epidemic. Thunder-storms, and tumults of the atmosphere, have occurred at the same periods. During

the raging of one epidemic, 300 women engaged in coal-dredging at Newcastle, and wading all day in the sea, escaped the complaint. It has been thought that this exemption might be accounted for by supposing that the almost constant immersion of the body in a conducting medium prevented any undue collection of electricity.

Again, it has been fancied that the tolerably definite course of the epidemic, in its migrations, might be somehow connected with magnetic currents.

Another hypothesis, more fanciful perhaps, at first sight, than these, yet more easily accommodated to the known phenomena of the distemper, attributes it to the presence of innumerable minute substances, endowed with vegetable or with animal life, and developed in unusual abundance under specific states of the atmosphere, in which they float, and by which they are carried hither and thither. Myriads of these animalcules, or of these vegetable germs, coming in contact with the mucous membranes, and especially with that of the air-passages, irritate (it is imagined) these surfaces, and exercise a poisonous influence upon the system. Now, the sporules of certain fungi, which ruin the health, and destroy the vitality of larger plants, on which they prey, are inconceivably small. I shall prove to you presently, that vegetable effluvia are capable of producing, in the human body, symptoms not very dissimilar from those of influenza. Again, that the waters of this globe swarm with living creatures, which are invisible by our unaided eyes, the microscope has taught us. Others, too small even to be estimated by that wonder-showing instrument, in all probability exist. We cannot doubt that the gaseous fluid which surrounds this planet, equally teems with living atoms. We know that multitudes of insects and of cryptogamous plants, infinite in number in respect to our finite powers of computation, *are* sometimes suddenly hatched or developed, in places which were previously free from them. It is easy to conceive that atmospheric infusoria (so to speak) may rapidly congregate or vivify, in masses sufficient to render deleterious the very air we breathe. If this be so, we can understand how such a cause of disease may first act here and there, and presently overspread large districts; how it may move, or be wafted from place to place, or be carried about by persons; how its course and operation may be circumscribed and definite; and how some germs or ova may remain after the visit, retaining their vitality, and ready in future seasons again to start into life and activity under favouring circumstances. Taking the insect hypothesis, and knowing as we do that some animal poisons (that of small-pox, for example), have the singular property of multiplying themselves in the human body, like yeast in beer, we may conceive that diseases, produced by animalcules, may thus infect the fluids of the body, and become contagious in the fullest sense of that term. Lastly, the uniform duration of these epidemics has been supposed to add probability to the hypothesis that they result from the operation of some organic principle, which has its definite periods of growth and of decay. All this is sheer hypothesis: but it is as good an hypothesis as I am able to offer you; and you must be content to conceive of it as possibly the true one, until a better shall be proposed.

The character of debility which is so conspicuously impressed upon this disease, bears closely upon the treatment required for its cure, or its safe conduct. As in all other epidemics, the severity of the complaint is extremely variable in different persons. In some it proves a very trifling malady, which soon passes off, and requires little or no assistance from medicine. In others it is a very distressing affection, and lays the foundation for other and still more serious, though more chronic diseases: and in some, and more especially in the old and the unsound, it shows itself a very fatal disorder. The absolute mortality under the recent epidemic has been immense: the daily newspaper obituaries have been unusually long; and you may have remarked, that the ages of the persons whose deaths they announced were in almost all cases great. The funerals have been so frequent, that difficulty has been found in performing them without indecent hurry and confusion. One undertaker, of whom I was informed, had at one time 75 dead bodies to inter—*above ground*, as he expressed it; and mourning coaches, and black horses, could not be procured in sufficient numbers to meet the demand for them. The absolute mortality, therefore, I say, has been enormous; yet the relative mortality has been small. You will hear people comparing the ravages of the influenza with those of the cholera, and inferring that the latter is the less dangerous complaint of the two; but this is plainly a great misapprehension. Less dangerous to the community at large (in this country at

least) it certainly has been; but infinitely more dangerous to the individuals attacked by it. More persons have died of the influenza in the present year than died of the cholera when it raged in 1832; but then a vastly greater number have been affected with the one disease than with the other. I suppose that nearly one-half of those who were seized with the cholera perished: while but a very small fraction indeed of those who suffered influenza have sunk under it. The only fatal cases that I have seen have been in persons advanced in life, or in persons whose lungs were previously known to be unsound.

Treatment.--Now the *treatment* of the influenza is pretty well understood. The chief risk of mistake is that of being too busy with the lancet. Certainly those affected by this disorder do not well bear active depletion. Of course no one would think of blood-letting except the symptoms were severe, and the distress great; but even in such cases, much caution is requisite in adopting that remedy. If you find that the inflammation has extended to the pleura, or to the substance of the lungs, it may be necessary to open a vein, or to apply cupping-glasses over the chest; but this is a very unpleasant necessity. Such is the result of all that I have seen, and heard from others, of the present epidemic; and such is the result of the recorded experience of nearly all previous epidemics. You will find abundant evidence of this collected into a summary view by Dr. Hancock. In 1510, Dr. Short says bleeding and purging did harm. In 1557, bleeding was said to be so fatal, that in a small town near Madrid, two thousand persons died after it in the month of September. In 1580, Sennertus, after stating that where blood-letting was omitted, the mortality was not greater than one in one thousand, adds, "Experientia enim *hoc* comprobavit, omnes fere mortuos esse, quibus vena aperiebatur." Dr. Ash observes that, in 1775, it was never necessary to bleed at Birmingham; and that, in a neighbouring town, three died who were bled, and all recovered who were not bled. And a great deal more evidence to the same purpose you may find in the article I have referred to.

I believe the best plan of management—as far as any general plan can be laid down—is to keep the patient in bed, and after clearing the bowels by a mild aperient, to give a couple of grains of James's powder every six hours, with a saline draught, and slops, till the first brunt of the disorder is over; and then, if the cough be troublesome, and the breathing laborious, and much rhonchus, or sibilus, or crepitation, is audible in the chest, to apply a blister, and to give expectorants and diuretics. What I prescribed a great many times was something of this kind: half a drachm of oxymel of squills, a drachm of the sweet spirit of nitre, and sometimes another drachm of paregoric, in almond emulsion. With respect to full doses of opium, when the feverishness is abated and the headache gone, I should recommend the same practice which I described in the last lecture. If there be any lividity of the skin, or of the mucous membranes, it is dangerous to give a full dose of opium. On the other hand, if there be no visible indication in the complexion that venous blood is circulating in the arteries, opium given at bed-time will have sometimes a magical effect in relieving distress, and (by giving rest and refreshing sleep) in recruiting the strength also. In cases in which the powers of the system are prostrate, and the face and lips are livid, and the patient is tugging to expectorate the mucus that is filling up his air-passages, you should have recourse to ammonia, to nourishing broths, and it may be to wine and water: and when all danger from the disease is over, but the patient remains feeble, languid, and out of spirits, then is the fit time to administer tonic medicines; and although snake-root and cascarilla are well spoken of by many practitioners, I know no tonics so good as the sulphate of quina, or of iron, for such patients.

As to external applications, mustard poultices, blisters, and the like—and to the inhalation of the steam of hot water,—these may each and all be very useful; but I have nothing to add concerning the time and manner of their employment to what I said upon the same subject in the last lecture.

There is one point in the treatment which I must not omit to notice, although I cannot tell you much about it from my own experience. Dr. Thomas Davies, an accurate observer, and one well qualified to form a judgment in the matter, states that he found a *mercurial* treatment answer well in severe cases, in the epidemic of 1833. He perceived that active depletion was not well borne, and discovering that in the bad cases there was always crepitation in the lower lobes of the lungs, he thought mercury was one of the most proper remedies to subdue the inflammation, and to occasion absorption of the fluid

effused into the air-cells. He had severe cases to deal with. He says that it happened to be his duty to admit the patients into the London Hospital during the week the epidemic was at its height, and that thirty-two beds which were placed at his disposal were all soon filled with individuals labouring under the severe forms of influenza; so severe, that he believed the greater number of them would have perished if they had been allowed to wander about the streets, or even to have remained at their own homes, with the insufficient attention they could there have obtained. Only one or two of these patients were bled, but they were all put under the influence of mercury. This treatment commenced on Thursday, and all who, by Saturday night, were affected in the usual way by the remedy, safely and gradually recovered, with the exception of two; and one of these had hypertrophy of the heart, and diseased aortic valves. His object was not to salivate, but merely to make the gums tender. It was of course necessary that the action of the mercury should be prompt; and he found that the most quick and efficacious means of obtaining it was by rubbing in the linimentum hydrargyri.

Hay asthma.—There is another kind of catarrh described as depending upon a peculiar local cause, and therefore requiring to be briefly noticed. I have seen only one instance of it; but it has been observed and described by several medical men. Dr. Bostock, in the *Medico-Chirurgical Transactions*, gives an account of this complaint as it is apt to attack himself. It is called the *catarrhus æstivus*, and by some the *hay fever*, or the *hay asthma*. In Dr. Elliotson's lectures, as published in the *Medical Gazette*, there is also a good deal of curious information upon this malady, contained in letters addressed to him from practitioners in various parts of the country, in consequence of some previous remarks he had made upon it in a clinical lecture, which had also been printed. Dr. Elliotson speaks of it as a combination of catarrh and asthma. It consists in excessive irritation of the eyes, nose, and the whole of the air-passages; producing, in succession, itching of the eyes and nose, much sneezing occurring in paroxysms, with a copious defluxion from the nostrils; pricking sensations in the throat; cough, tightness of the chest, and difficulty of breathing, with or without considerable mucous expectoration. This complaint occurs in some persons only, and in them it always takes place at the same period of the year, in the latter end of May, or in June, when the grass comes into blossom, or when the hay-making is going on. It seems, in fact, to be produced by some kind of emanation from certain of the grasses that are in flower at that season, of the irritating qualities of which emanation some persons only,—and a very few persons in comparison with the entire population,—are susceptible. The complaint occurs only at that one particular season; and it then attacks persons who are not remarkably subject to catarrh at other times, or from the ordinary causes of catarrh; and if they avoid meadows and hay-fields, and the neighbourhood of hay-stacks, they escape the disorder. Hence going to the sea-coast,—and especially to those parts of the coast that are barren of grass,—offers a means of protection; and when this cannot be done, such persons obtain refuge, in some measure, from the cause of the irritation, by remaining within doors, and shutting out as much as possible the external air, during the hay-crop. One lady, who suffered annually from this strange affection, states that a paroxysm has been brought on by the approach of her children, who had been in a hay-field; and once this happened when the hay season had been for some time over, upon their joining her at tea, after playing in a barn in which the hay of that year had been deposited. She was in the habit of flying to Harwich, or some other part of the coast, as the dangerous season came on. On one occasion, while walking on the shore at Harwich, she was suddenly attacked by the complaint, to her great surprise, as she was not aware of any grass being in the neighbourhood; but the next day she discovered that hay-making was going on upon the top of the cliff at the time she was walking under it. In another year, she being at Cromer, and an attack that she had suffered having quite subsided, and all the hay-making thereabouts being over, she was suddenly visited by the well-known symptoms, and on going into her bed-chamber perceived that they were making a large stack of hay in a yard near the house, having transferred it from a field five miles distant.

Very lately I was asked by Mr. Cheyne to see with him the wife of a stable-keeper near Regent Street. I found her suffering under what is popularly called "a crying cold:"

pain in the situation of the frontal sinuses, streaming eyes, sneezing and defluxion from the nostrils, and very urgent dyspnœa, which was accompanied by loud wheezing. Symptoms of this kind had come on, suddenly, some days before: and her distress was then so great, that her husband proposed to drive her in a gig to consult a medical friend of his who lived at Islington. On their way thither, every symptom disappeared, and she felt at once quite well. She subsequently stayed a night or two, in comfort, with some relations in the city. Immediately upon her return home the same symptoms recurred, with all their former severity, and resisted the means adopted for their relief by Mr. Cheyne, who had now been called in. He was soon led to suspect the cause of the attack, and of its obstinacy. There was a strong odour of hay in the house. The husband told him that his lofts were filled with a lot of hay which had recently arrived, and which had an unusually powerful smell. We learned that our patient was always worse at night, when the house was shut up; and better in the morning, when a free current of air blew through the open windows. We advised a temporary change of residence: but our advice was not followed until two days afterwards, the disorder meanwhile continuing, and increasing in intensity. Then the patient removed to lodgings not one hundred yards distant; and immediately all the catarrh and distress again ceased, and she passed a perfectly tranquil night. Afterwards she went into the country, and did not return till the odoriferous parcel of hay had been consumed, and a new stock laid in. She was however revisited by some slight cough, and occasional dyspnœa--neither of which troubled her much or long.

Avoidance, then, of the ascertained source of the complaint is the best thing that can be recommended to these persons. You may read almost every year in the newspapers that one of our English Dukes has gone to Brighton to escape the hay fever. But it is not in the power of every one to leave home for that purpose; and it has been found that the system is capable of being fortified in some degree against the pernicious effects of these vegetable effluvia. Mr. Gordon, of Welton, in Yorkshire, had communicated some interesting observations to the profession on this subject, before those of Dr. Elliotson were published. You may find Mr. Gordon's paper in the fourth volume of the *Medical Gazette*. He supposes that the aroma of the sweet-scented vernal grass, the *anthoxanthum odoratum*, is the principal exciting cause of the complaint. He found the symptoms more speedily and effectually removed by the tincture of lobelia inflata, than by any thing else that he had tried at that time; and he recommended the cold shower-bath as the best preservative against the attack. But in a subsequent communication to Dr. Elliotson, he states that the sulphates of quina and of iron, given in combination, had proved completely successful in emancipating the two patients, from whose cases he had principally drawn up his account, from their tormenting disorder: although they had, in spite of all previous treatment, suffered an annual return of it for fifteen or twenty years.

The susceptibility of this troublesome affection of the mucous membrane, from a peculiar cause, which to most people occasions no uneasiness, appears sometimes to run in families; and this is nothing more than one might expect.

Dr. Elliotson, thinking it possible that the chlorides, which have the power of decomposing, and disarming of their noxious qualities, certain *animal* effluvia, might exert a similar control over the *vegetable* emanations that excite the hay catarrh, suggested to one of the sufferers a trial of the chloride of lime or of soda. He desired him to have it placed in saucers about his bed-chamber; to have rags dipped in it, and hung about the rooms of the house; to wash his hands and face with it night and morning; and to carry a small bottle of it with him, to smell to repeatedly in the course of the day: and this plan gave so much relief,—either by destroying the emanations, or by lessening the irritability of the mucous membranes,—that it was tried in other cases; and though it did not succeed in all, it did in most of them. Three patients out of four derived advantage from it. This expedient, therefore, is worth carrying in mind.

There is another vegetable substance, better known to us, which produces in some few individuals symptoms very like those of the hay asthma: I mean the powder of ipecacuan. I recollect a servant employed in the laboratory of St. Bartholomew's Hospital, when I was a pupil there, who had the peculiar ill luck to be liable to this affection. Whenever that drug was under preparation, he was obliged to fly the place. This idiosyncrasy is not very uncommon. A very small quantity of the ipecacuan dust is sufficient, in such persons, to

bring on a paroxysm of extreme dyspnœa, wheezing, and cough, with singular anxiety and great weakness. The distress usually terminates by a copious expectoration of mucus.

These effects of a powdered root, and of certain emanations from grass or hay, lend weight to the hypothesis which ascribes the influenza to subtle vegetable matter floating in the atmosphere.

I would sugest a *trial* of the respirator, as a defence against the particles of ipecacuan, and against the volatile exciting cause (whatever it may be) of hay asthma.

Chronic Bronchitis.—Catarrh is very often met with, in a chronic form; in other words, the mucous membrane of the air-passages is very liable to be affected with chronic inflammation. The accounts which you may read of this are exceedingly puzzling. Authors have endeavoured to draw nice distinctions between different *species* of chronic catarrh; sometimes according to varying qualities in the matter expectorated: thus you have *chronic mucous* catarrh—*pituitous* catarrh—*chronic pituitous* catarrh—and *dry* catarrh, which, after all is *not* dry, but only accompanied by less expectoration than some of the others; and then again there is *symptomatic* catarrh. You will find all these enumerated by Laennec; and the majority of writers since his time have trodden with too much reverence in his footsteps. There are by no means such differences in the symptoms or the treatment of the several varieties of chronic inflammation of the membrane in question, as to make these numerous subdivisions of any practical utility. Chronic catarrh is often a sequel of acute bronchitis; it is a very common accompaniment of disease of the heart; it frequently arises during the course of the febrile exanthemata; it is seldom entirely absent in cases of continued fever; and it is a form of complaint that is full of interest on this account, if on no other, that it has so often been mistaken, and is so liable to be mistaken still, for tubercular consumption; of which indeed it is very frequently the companion.

The *constant* symptoms of chronic catarrh, or bronchitis, are cough, some shortness of breath, expectoration of altered mucus. The *variable* symptoms, those which are oftentimes of the most importance, as determining the slight or the serious character of the disorder, consist in the quantity and quality of the matters expectorated, and the presence or absence of wasting, and of hectic fever.

You will continually be meeting with cases of *this* kind. A person advanced in years has what he calls a slight cold, in the winter. He coughs, and expectorates a certain quantity of gray or transparent mucus. In the summer his cough diminishes, or ceases altogether. The next winter the same thing happens again; and each successive return of the colder seasons of the year brings back in increasing severity the cough and the expectoration; and if you listen to the breathing of such persons while the cough is on them, you will find crepitation at the lower part of their lungs. Now these are examples, I believe, of a chronic state of slight inflammation of the membrane,—or it may be of passive congestion and effusion,—depending upon slowly advancing *cardiac* alterations. Peripneumonia notha is very apt to supervene on this condition.

But chronic bronchitis may take place at any age, as a sequel to the acute: just as active inflammation of other parts of the body is liable to degenerate into the chronic form; and such cases are sometimes very equivocal and deceptive. Several years ago, a lady became my patient, having cough, expectoration of puriform matter, night-sweats, and diarrhœa. She had had hooping-cough a short time before; but though the hooping, and other symptoms proper to that disease had ceased, she continued to cough, and to waste. Gradually she got thinner and weaker, her pulse became like a thread, and beat 120 times in a minute; she took to her bed, the diarrhœa was scarcely restrained by astringents and opiates, and I thought she could not live a week. And, upon being pressed by her brother for my opinion, I said so. She had scarcely allowed me to listen to the sounds in the thorax: but I had once done so fairly, and I could find no morbid sounds, except at the lower part of the lungs. If I had trusted to that circumstance alone, I should have said that she had not tubercular consumption; but I had not then so much faith in the indications afforded by auscultation, nor in my own accuracy of ear in such matters, as I might have now; and I concluded that she *was* dying of tubercular phthisis. Almost on the day, however, on which I ventured to give this prognosis, some slight amendment began: and she did gradually recover, and is alive and quite well at this time. Now it is in cases of this kind that cures are performed by those who boast of curing consumption.

In truth, chronic bronchitis is, in some cases, as incapable of recovery, and as surely and progressively fatal, as tubercular phthisis itself: and even more so than some of the forms of phthisis. So long, however, as *no organic change* has taken place in the air-tubes, or in the mucous membrane lining them, these chronic forms of bronchitis that simulate phthisis in their general symptoms, are within the reach of cure. They are to be treated by counter-irritants to the chest—and by such measures as are calculated to relieve the most urgent symptoms. Opiates for cough, or for diarrhœa. Sometimes the patients bear steel well, and then it is almost sure to have a beneficial effect. Sometimes sarsaparilla appears to do good; but, as far as I have observed, one of the most effectual restoratives in these cases is to be found, when the weather and the strength permit, in frequent change of air and place; in gentle gestation, in a carriage or in a boat; and in a nourishing but bland and unstimulating diet. When the membrane, and the tubes which it lines, become *altered in structure*, and pour forth a fluid which has all the qualities of pus, hectic fever generally is present, and the chronic disease tends, slowly perhaps, but surely, to death.

There are certain cases of chronic bronchitis which are especially remarkable, on account of the great abundance of the bronchial secretion: so great that the patients appear to die principally from the daily exhausting drain thus made upon the system. There are sometimes no other evident signs of inflammation; so that, as Andral observes, one might be led to separate these fluxes from the truly inflammatory affections. They differ from them apparently in their nature, and certainly in the treatment which they require. Andral has detailed two or three instances of this kind in his *Clinique Médicale*. The patients expectorated every day large quantities—a pint or more—of frothy fluid, resembling weak gum-water in colour and consistence. They had no fever; neither frequency of pulse nor heat of skin; but they were exceedingly pale, like persons blanched by hæmorrhage, and their emaciation and weakness were also extreme. Very little appreciable deviation from the healthy state was detectible when the lungs and heart were examined after death.

It does occasionally happen that even larger quantities—three or four pints daily—are, for a considerable period, spat up, without much wasting.

Andral asks, whether, in such cases as these, which certainly occur, though they are not very common, the first indication of treatment should not be to check and diminish the excessive bronchial secretion; to treat it as you might treat a gleet of the other mucous membranes, with balsams, administered either by the stomach, or in the shape of vapour. He conjectures that it might have been in cases of this nature that the vapour of tar, and tar water, were once thought to be so useful. Probably the creasote would be well adapted to such cases. Certainly I have seen the excessive expectoration diminish, and the patients gain strength under the use of the balsams; the compound tincture of benzoes, for example; a form of medicine much employed formerly, and too much neglected, I apprehend, at present. Another remedy from which I have derived great advantage in some cases of the same kind, is the sulphate of iron, given in two or three grain doses, in the compound infusion of roses, thrice daily. When there is fever present, these remedies are apt to augment it: but when the pulse is quiet, and the skin cool, I am quite sure that they are often of the greatest service: and this you will find to be the opinion also of various practical authors.

There is another very remarkable condition of the pulmonary mucous membrane, constituting also, I imagine, a species of chronic inflammation, and characterized chiefly, like the last, by the matters expectorated. I mean that state in which a firm substance, resembling a false membrane, forms in the smaller bronchi and their ramifications, and is coughed up, from time to time, in fragments. I mentioned in a former lecture that the false membrane of croup sometimes descends a long way into the bronchi; even to their extremities. But I am speaking now of a different and less acute form of disease, in which, the trachea being unaffected, concrete masses, evidently moulded in parts of the hollow bronchial tree, are spat up; somewhat like bunches of worms, or the roots of a small plant. This I presume to be uncommon; for I have met with it twice only in my life. It has been described, however, by several observers. The first Dr. Warren has a paper upon it in the first volume of the *Medical Transactions*, where he gives representations of the substances coughed up, which he calls *bronchial polypi*. Dr. Paris has told me that a patient of his coughed up considerable quantities of these branching casts of the ultimate air-tubes, now and then, for a long period. An interesting paper of Mr. North's, on the same

subject, was read at one of the evening meetings of the College of Physicians. That gentleman possesses some beautiful specimens of these miscalled polypi. Dr. Carswell gives a figure representing them. When the affection is extensive, it is attended with great distress, and dyspnœa, and violent fits of coughing; and the symptoms are wonderfully calmed upon each expulsion of the solid matter. The surprise is that such patients should ever recover; but I have never heard of an instance in which the complaint proved fatal.

The two examples of it which have fallen under my own observation, were invested, by the circumstances attending them, with a peculiar interest. They occurred, within less than a twelvemonth of each other, in the persons of two brothers, of middle age, the one a barrister, the other well known to you all as one of my most valued colleagues in this place. Both of them were, and are, remarkably stout, strong, and healthy men. In both cases the expulsion of the so-called polypi was preceded by hæmoptysis, which came in considerable gushes, and was repeated at intervals of a few days, until the solid matters began also to be expelled, and then the hæmorrhage soon subsided.

The barrister, after having been annoyed for nearly a year by some huskiness of the voice, spat up, all of a sudden, a small quantity of bright blood; and soon afterwards expectorated several ramifying masses of tolerably firm consistence, resembling fibrinous coagula of blood, deprived of most of its colouring matter. Some of them, which I saw and examined, were solid; others, I understood, were hollow. I found slight circumscribed crepitation in the lower and posterior part of his left lung. This trivial degree of hæmoptysis, with the expulsion of what looked like casts of the interior of a bronchial tube, was once or twice repeated within a few days. He had no fever—no dyspnœa. Mercury, *inter alia*, was prescribed; but as the patient did not feel in any way ill, I believe he soon became tired of physic, and of medical restraint. Whenever I have since seen him, he has appeared to be in perfect health.

Of the professor's illness I saw more. In the midst of health which had been uninterrupted, save by a solitary fit of the gout some years before, he also spat some mouthfuls of florid blood. He had no cough, but the hæmoptysis was accompanied by a rattling sensation in the right side of his chest.

For about three weeks he continued, at intervals varying from three to six days, to expectorate blood, in gushes. The smallest quantity brought up on any one occasion was two ounces; the largest, eight. Just above the right nipple the respiratory murmur was mingled with large crepitation, which was always sensibly increased, and quite perceptible by the patient himself, during the attacks of hæmorrhage. In the intervals between them his breathing and his pulse were perfectly tranquil and regular.

With the florid blood came up, in general, some black coagula: and at the end of three weeks, or thereabouts, in these black masses ragged shreds of a different and firmer material began to be visible: and presently afterwards, complete branch-like casts of the ramifying air-tubes were expelled; and the bleeding ceased.

Of these casts there were two kinds: the one solid, somewhat coloured, evidently fibrinous, and resembling the branching coagula that may sometimes be drawn out of the arteries in the dead body; the other white, membranous, tubular, but ramifying also. None of them were of very firm consistence.

Till these substances made their appearance, our anxiety about the patient was extreme, and he underwent some rigorous discipline at our hands. He was confined to bed, forbidden to speak, kept strictly to the slenderest slop diet, several times bled, and extensively blistered. Lumps of ice were given him to swallow, and pounded ice was applied to his chest whenever the blood broke forth afresh. He took mercury till his gums were tender, and afterwards the acetate of lead, and other reputed styptics.

To most of this I was a consenting party; but, looking back upon the case now that its nature and result are known, I must confess that the treatment, though fairly justifiable at the time, was unnecessarily active.

Mr. North, in the paper to which I have alluded (you may see it in the twenty-second volume of the *Medical Gazette*), draws a distinction, of which he gives the credit to Dr. Cheyne, between the hollow, membranous concretions, expelled without any blood; and the solid branching masses which accompany or succeed hæmoptysis, and are obviously

mere coagula of blood moulded in the smaller air-tubes, where it had stagnated. He points out the comparatively harmless character of the cases in which the first occur; and the far more serious import of the second: the hæmorrhage denoting the presence of some organic mischief within the thorax, and the "polypous concretions" being simply an accident of the hæmorrhage.

I doubt the accuracy of this distinction. The brothers of whom I have spoken continue to be, as they were before, free from any symptom or suspicion, either of cardiac or of pulmonary disease. Moreover, in hæmoptysis depending upon tubercles in the lungs, or upon organic disease of the heart, these concretions are very rarely observed. I have never seen them in such cases, common as such cases are. The barrister had a husky voice, and the professor was noticed to have been often "clearing his throat" for some time before the first eruption of blood: from which circumstances I infer a previous unhealthy state of the mucous membrane. Upon the whole I incline to the views expressed by Dr. Todd, with whom I had the advantage of consulting in the latter case—that a chronic and limited inflammation of certain of the bronchial tubes first occurred; disclosing itself by no marked symptoms, but leading to the formation of tubular membranes: that, after a while, these membranes began to be detached: that hæmorrhage resulted, and continued till the separation was complete; and that, at the same time, some of the extravasated blood coagulated in, and took the shape of, the air-tubes, and was afterwards expectorated.

I understand that the barrister has since had a recurrence of this strange complaint, which he treated very lightly, and soon got rid of.

He entertained a fixed belief that his attacks were attributable to the presence of one of Dr. Arnott's stoves, in his chambers; the heated atmosphere of which always produced a slight feeling of constriction and distress within his chest. Whether this notion be well or ill founded I cannot pretend to say: but it is curious that the professor also had been using a similar stove, which, placed in his sitting-room, warmed both it and his bedroom adjoining.

Morbid anatomy of the bronchial mucous membrane.—A word or two, before we separate, as to the morbid anatomy of these tissues.

Chronic inflammation of the aerial mucous membrane may lead to changes in its colour; or to thickening of the membrane; or to ulceration; or to dilatation of the bronchi, and their ramifications. And it is proper that you should be informed respecting these morbid conditions.

In general, when chornic inflammation has existed during life, the mucous membrane is found to be red: but it is not a bright redness; it is rather a livid, or violet, or brownish tint. And what is very curious, in some instances in which all the symptoms of inveterate bronchitis, with *puriform* expectoration, had been present, the inner membrane of the air-passages has been found scarcely rosy—or even perfectly white—throughout its whole extent. Of course we are not to infer from this that there has not been *inflammation;* for the same thing is known to occur in the intestinal mucous membrane, in that of the bladder, and even in serous membranes. Where pus is poured forth there must have been inflammation.

One effect of inflammation, as I formerly showed you, is a softening of the membrane; but this is a much less common result of inflammation in the mucous membrane of the bronchi, than in that of the digestive organs. In regard to ulceration likewise there is a great difference between the two mucous surfaces: in that of the air-passages it is comparatively rare.

Thickening of the mucous tissue occurs also in various degrees: but the most remarkable change undergone by the membrane, and the tubes which it lines, is the dilatation of those tubes, and the consequent alteration of the membrame, which expands with them.

There are two or three varieties of this dilatation. In the first of them, one or more of the bronchi present, throughout the whole or the greater part of their extent, an increase of capacity more or less considerable: so that tubes which result from the fourth or fifth, or even sixth division of the principal bronchus of each lung, may equal or exceed in diameter that bronchus itself. Tubes that ought not to be bigger than a crow

quill may become as large as the finger of one's glove. Sometimes this kind of dilatation is seen in a single branch only, sometimes in many. It may affect the bronchial ramifications of an entire lobe. It is more common in the branches of a bronchus than in the bronchus itself.

Is is not very easy to explain the manner in which this sort of dilatation is produced. We might attribute it to simple distension of the bronchial parietes, were it not that these parietes are at the same time thickened, and the circular fibres hypertrophied, as you may see in this preparation, and in Dr. Carswell's plate. But there is another form of bronchial dilatation to which the explanation just adverted to is easily applicable. Instead of the uniform dilatation of one or more bronchial tubes, throughout their whole extent, we find a bellying, or globular expansion, at the extremity of one of them; and the walls of the tube, instead of being thick and hypertrophied, are wasted, and in a state of atrophy. The tissues composing the tube are often so thin, that when the cavity, for such it must be called, is laid open, the colour and structure of the pulmonary tissue may be seen through them. These cavities are generally found filled with a thick, tenacious, straw-coloured, muco-purulent fluid. Now it is easy to conceive how "the straining influence of repeated paroxysms of coughing" may cause dilatation of this kind. The primary branches are more easily cleared of the mucous that fills them: but the pressure which the lung undergoes under a forced expiration, operating on a portion of the same kind of mucus detained in the smaller branches, may be more than the elasticity of the tube is capable of resisting. And, in fact, all dilatations of the bronchial tubes must be, in part at least, owing to the same influence of centrifugal pressure by imprisoned mucus. They are seldom met with except after those affections which are characterized by considerable secretion from the membrane, and by much and repeated cough: as after some forms of chronic bronchitis, and after hooping-cough.

A third variety of dilatation is that in which the same bronchus bellies out in different places; is dilated at intervals; so as to present in its course a series of successive enlargements and contractions. Here, again, the walls of the bronchi, though they may be traced in the parts dilated, do not appear to be thickened, but rather are diminished in thickness. We may suppose, therefore, that these small partial dilatations of the bronchi may result from mechanical distension, by mucus, in those places which offer the least resistance; either on account of diminished elasticity, or of actual thinning. This variety of dilatation is more frequent in children than in adults.

In whatever way the dilatation may take place, one of its obvious and necessary consequences is, the condensation of the pulmonary substance around the dilated tube, the obliteration of some of the cells, and a proportional abridgement of the function of the lung. Accordingly, when it is extensive, dilatation of the bronchi is attended with habitual dyspnœa.

But the most important consideration arising out of this state of the bronchi, is this; that the signs, both general and physical, by which it is accompanied, are apt to be exactly those which are most distinctive of phthisis. And it is on that account that I have now described these changes. I shall revert to them again when I come to the symptoms, revealed by auscultation, of tubercular disease of the lungs.

LECTURE L.

HOOPING-COUGH: SYMPTOMS; DURATION; COMPLICATIONS; PATHOLOGY; TREATMENT. —PNEUMONIA: ITS STAGES AND MORBID ANATOMY; AUSCULTATORY SIGNS.

Hooping-cough.—I have yet to consider one very important disorder, which is usually classed among the catarrhal affections, but which is marked by features so peculiarly its own, as to distinguish it effectually from every other form of disease. I allude to *hooping-*

cough: a remarkable complaint, well known everywhere, I believe, and much dreaded by parents. It has received a variety of names: chin-cough; kink-hoast; coqueluche; tussis convulsiva; tussis ferina; and *pertussis*. This last name, which Sydenham bestowed upon it, and which was adopted by Cullen, is the technical appellation of the disease in this country, as hooping-cough is the popular.

Symptoms.—The phenomena that characterize hooping-cough are, I say, remarkable. It begins with the symptoms of an ordinary catarrh arising from cold. The child (for it is most especially a disease of children) has coryza, and coughs; and mothers and nurses are aware that the disease commences in this way, and express their apprehensions lest it may *turn* to the hooping-cough. After *this*, the *catarrhal* stage, has lasted eight or ten days, or a fortnight, or sometimes a day or two longer, that kind of cough begins to be heard which is so distinctive. It comes on in paroxysms, in which a number of the *ex*piratory motions belonging to the act of coughing are made in rapid succession, and with much violence, without any intervening inspirations; till the little patient turns black in the face, and seems on the point of being suffocated. Then one long-drawn act of *in*spiration takes place, attended with that peculiar crowing or hooping noise, which denotes that the rima glottidis is partially closed, and which gives the disease its name. As soon as this protracted inspiration has been completed, the series of short expiratory coughs, repeated one immediately after the other till all the air appears to be expelled from the lungs, is renewed; and then a second sonorous back-draught occurs; and this alternation of a number of expiratory coughs, with one shrill inspiration, goes on until a quantity of glairy mucus is forced up from the lungs, or until the childs vomits, or until expectoration and vomiting both take place at once. During the urgency of the paroxysms the face becomes swelled, and red or livid, the eyes start, the little sufferer stamps sometimes with impatience, and generally clings to the person who is nursing him for support, or lays hold of a chair or table, or of whatever object may be near him, to diminish (as it would seem) the shock and jar by which his whole frame is shaken. As soon as expectoration or vomiting have happened, the paroxysm is over. The child may pant a little while, and appear fatigued; but commonly the relief is so complete, that he returns immediately to the amusements, or the occupation, which the fit of coughing had interrupted, and is as gay and lively as if nothing had been the matter with him. When the fit terminates by vomiting, the patient is in general seized immediately after with a craving for food, asks for something to eat, and takes it with some greediness.

Each paroxysm may consist of several alternations of the gasping coughs, and the characteristic hoop or kink; but Cullen remarks, that the expectoration or vomiting usually takes place after the second coughing, and puts an end to the fit.

The number of paroxysms that occur in the twenty-four hours is variable also; and they come on at irregular intervals. When the complaint is uncomplicated, the child, during the intermissions, appears to be quite well. This is another striking feature of the disorder. In the earlier paroxysms the mucus expelled is scanty and thin; and in proportion as this is the case, the fits are the longer and the more violent. By degrees the expectoration becomes more abundant; and sometimes it is very copious: at the same time it is thicker, and more easily brought up; and on that account the fits of coughing are less protracted.

The ordinary duration of the disease is from six weeks to three months; but it may run its course, I believe, in three weeks; and it may continue for six months, or more.

In an uncomplicated case, if you listen at the chest during the intermissions, you will probably hear the sounds that are proper to catarrh—some degree of rhonchus or sibilus: and in many parts there may be puerile respiration; and if you percuss the thorax, you get the natural hollow sounds. But what happens when you apply your ear to the chest during the paroxysms of coughing? Why, the information given us in this case by auscultation is very curious. You may perhaps hear, between the short explosive shocks of the cough, some snatches of wheezing, or of vesicular breathing; but during the long-drawn noisy inspiration that succeeds, all *within* the chest is silent. This is supposed to result from the slow and niggardly manner in which the air passes towards the lungs through the chink of the glottis, which is spasmodically narrowed. It may also depend, in part, as Laennec supposed, upon a spasmodic condition of the muscular or contractile

fibres of the bronchi and their branches. When the fit is at an end, the ordinary sounds of healthy, or of catarrhal respiration, are resumed.

Children are very susceptible of this complaint; and it is a complaint which spreads by *contagion*. Hence it follows that few children escape an attack of it. It is also one of those contagious maladies which do not in general affect the same individual twice: and hence again it follows that it is rarely met with in adults. Such is the fact; and such, I apprehend, is the explanation of it. It is not that adults are insusceptible of hooping-cough; for adults that have not had it during their childhood are readily affected when exposed to the contagion. But it is that the disorder, with very few exceptions, protects the system somehow from its future recurrence; and that most adults have *had* it when they were young, and for that reason do not take it afterwards.

During the very early periods of infancy, *i. e.* within the first two or three months, hooping-cough is said to be rare: I mentioned a case, however, before—and I have read of others—in which the disorder appeared to have been contracted before the patient was born. My bedmaker's daughter in Cambridge had a child ill with hooping-cough in the house with her during the last weeks of another pregnancy, and the new comer hooped the first day he came into the world.

As long as this disease is uncomplicated—unmixed with inflammation, and unattended with fever, or only with that slight inflammatory condition proper to mild catarrh—it is not at all a *dangerous* disease. Probably it *will*, under the most favourable circumstances, run a certain course. By degrees the violence and the frequency of the paroxysms diminish; they occur only in the morning and the evening, then in the evening alone, and at length they cease altogether. But for some time after the disorder has apparently come to an end, if the child takes cold, and gets a cough, it is apt to assume a spasmodic character, and to be attended with a hooping noise in inspiration.

Complications.—Unfortunately hooping-cough is, in a great many cases, not simple—not uncomplicated. It becomes mixed up with other kinds of disease in the chest; or in the head. In the chest severe bronchitis supervenes upon it, or inflammation of the substance of the lungs; and then fever is lighted up, and permanent dyspnœa is present. When the disorder has been long drawn out, and has at last terminated fatally, dilatation of the bronchi, such as I described in the last lecture, is often found upon dissection; or, still more commonly, I believe, what is called *emphysema* of the lungs—a change which I have yet to bring before you.

That such effects should follow such violent and continued efforts of coughing, is no great matter for wonder. Neither can we be surprised that the disease frequently leads to cerebral disorder. During the fits there is a great and visible determination of blood towards the head, or rather a detention of the blood in the veins that proceed from the head; —passive mechanical congestion: the transmission of the blood through the lungs being obstructed, and its return from the head being interrupted. Hence, the face becomes turgid, the eyes are prominent, the superficial veins full and projecting, the lips and cheeks turn livid; sometimes hæmorrhage takes place from the nose or ears; or the eyes become blood-shotten; or the patient actually falls into convulsions; nay, apoplexy is occasionally the result of the straining; and when life is not thus suddenly cut short, chronic mischief is apt to be set up in the brain, and the child ultimately dies hydrocephalic.

All this is the more to be feared in proportion as the child is the younger. Head affections are particularly to be dreaded in scrofulous children; and in any children during the first dentition. When the disease occurs within the first two years of life, it is usually attended with convulsions: and many more die within that period than afterwards. And Cullen's remark is undoubtedly true, that the older children are, the more secure they are, *cæteris paribus*, against an unhappy event.

Hooping-cough may be complicated also with a disordered condition of the bowels; and with infantile remittent fever. This complication is more accidental, and less a *consequence* of the hooping-cough than the former; but it may very materially add to its peril.

Dr. Cullen was of opinion that the complaint may exist in even a milder form than that which I have called simple hooping-cough. He thought he had seen "instances of a disease, which, though evidently arising from the chin-cough contagion, never put on any other form than that of a common catarrh." Others again believe that adults may have

it without hooping. But all this seems to me very doubtful. Catarrh is an exceedingly common malady, and I should be slow to consider any case a genuine case of pertussis, unless the characteristic paroxysms of coughing, and the stridulous inspiration, were present.

Pathology.—Divers opinions have been held respecting the seat, and respecting the nature, of hooping-cough. Some suppose it to have its seat in the brain; and that it is essentially a spasmodic disease. Others maintain that it is situated in the air-passages of the lungs, and that it is always an inflammatory disorder. I do not pretend to strike the balance between these conflicting judgments. Certainly the simple form of the disease is often unattended with any appreciable fever: and that is a strong ground for believing that its peculiar phenomena are not *necessarily* connected with inflammation. They who have ascribed the complaint to a morbid condition of the brain have deduced that opinion, I presume, from the cerebral symptoms that are sometimes so plainly marked in hooping-cough. But these symptoms are oftener, to all appearance, the consequence, than the cause, of the paroxysms of coughing. I would suggest it as an interesting point for your future inquiry, whether the pathology of hooping-cough may not receive some elucidation from the researches of the late Dr. Ley, respecting the crowing inspiration of infants. You remember his suggestion, that mere inflammation of the mucous membrane of the air-passages might cause swelling of the absorbent glands of the bronchi or of the neck. This is a circumstance which I have myself long thought probable, from having found enlargement of the cervical glands springing up during the existence of pulmonary irritation. Take notice that the spasmodic fits of hooping-cough are always preceded for some days by mere catarrhal symptoms. Observe further how the parts supplied by the pneumogastric nerve are affected in these paroxysms: the larynx, the lungs, the stomach. This conjecture, that the crowing inspiration of infants, and the crowing inspiration of hooping-cough—though quite distinct affections—may both depend upon irritation of the recurrent nerve, or of the pneumogastric nerve generally; and that even the irritation in both cases might arise out of enlargement of the glands that lie in the course of that nerve: this natural conjecture had presented it to Dr. Ley's mind; for, towards the end of his book, I find this note:—"Recently four children have been brought to my house, labouring under hooping-cough. In all, the glandulæ concatenatæ near the trachea were very considerably enlarged. Is this (he says) merely an accidental combination? or is there any essential connection between the two? May it not be that an enlargement of these glands, from a specific animal poison, similar to that of the parotid glands in mumps, is, after all, the essence of hooping-cough? The subject at least deserves inquiry, and further observation."

In corroboration of this conjectural view of what *may* ultimately be *proved* to be the true pathology of hooping-cough, I may remark that among the morbid appearances described as being met with after death from that disease, "an unusual swelling of the bronchial glands" is set down. It is also stated, by some of the Germans, that that portion of the pneumogastric nerve which lies in the cavity of the chest has been sometimes found red. Yet I should lay no stress upon this; for others have asserted that they have looked in vain for this redness: and even supposing it to exist, it is no sure or safe token that there had been inflammation of the nerve. The nerve, all things considered, would be likely to become tinged of that colour soon before, or even after, death, from the gorged condition of the lungs. In some cases, as you may well believe, serous fluid is met with in the ventricles of the brain, or in the meshes of the pia mater: in others the consequences of inflammation are traceable in the bronchi, the lungs, or the pleuræ. Portions of what is called hepatized lung are not unfrequently seen in the fatal cases.

Treatment.—The object of rational treatment in hooping-cough, supposing the disease to be simple, is to *keep* it simple: to keep it *mere* hooping-cough: to obviate serious inflammation or mischief in the chest and head: and, if possible, to mitigate the severity and shorten the duration of the fits of coughing. I have no notion that any thing we can do in the beginning will materially abridge the duration of the complaint as it appears in its unmixed form. It *will*, I say, in all probability, run a certain course; and our business is to conduct it evenly and safely to the *end* of its course. For this purpose the diet must, in the first place, be regulated and reduced. The child should not be allowed to

eat meat: the bowels should be kept moderately open: and the patient in cold weather should be confined to the equable temperature of the house, or protected by warm clothing. You will find different persons employing and praising different plans of treatment; the object in all cases, however, being the same, viz., to *ward off inflammation*, and to *quiet irritation*. One very good plan, as I believe, is that of giving a grain, or a grain and a-half, of ipecacuan, three or four times a day. This generally keeps the bowels sufficiently open, and seems to have a beneficial operation on the mucous membrane of the air-passages also. Or a few grains of rhubarb and ipecacuan may be given every night: and if the cough be very troublesome and urgent, small opiates may be administered: syrup of poppies: or the extract of hyoscyamus; as many grains *per diem* as the child has years. There is a method recommended many years ago by a namesake of mine, which some people swear by. Sir William Watson's prescription was one grain of tartarized antimony and twenty drops of laudanum in an ounce of water. A tea-spoonful, or a dessert-spoonful, of that mixture was given every evening, or every other evening. I have heard the late Dr. Gooch say that his mother became famous as a village doctress by the help of that prescription. Fothergill's method was to give an emetic every day; or three or four times a week: and this plan answers best, I believe, when the expectoration is scanty, and brought up with difficulty, and after much coughing. The best emetic substance in such cases is, doubtless, ipecacuan. Mr. Pearson—who has had, I fancy, many imitators—used to give, after the operation of an emetic, one drop of laudanum, five drops of ipecacuan wine, and two grains of carbonate of soda, in a draught, every fourth hour, for several days. Under some such treatment as this, the disease will reach its termination in from six to twelve weeks: and it frequently happens that when the child is quite well in all other respects, it still continues to *cough*. The cough would almost seem to be kept up by the mere influence of *habit*. Now, under these circumstances, change of air will often remove the cough, as if by magic: and the shower-bath, and iron in some shape, will sometimes succeed, if change of air be not practicable.

There is a great variety of medicines lauded as *specifics* against hooping-cough: but they are not to be trusted to. Many persons think highly of the prussic acid, as a remedy for the paroxysms of coughing. But this is a gigantic remedy to employ in such young subjects. If you give it at all, you must give it in very small quantities, and watch its effects. The artificial tincture of musk is another substance which some have found useful. Three or four minims of it may be given in the outset, and the dose increased till some sensible effect is produced; and then the dose that has been so reached should be persisted in without further augmentation. I have been assured, by a most intelligent practitioner, that he had got considerable credit by prescribing this medicine, after other persons, with other modes of management, had failed. Belladonna, digitalis, cantharides, are other, and *I think*, *hazardous* remedies. Safer drugs recommended, and, for aught I know, equally efficacious with these poisons, are cochineal, oil of amber, musk, camphor, and the meadow narcissus. Of late the carbonate of iron has been greatly praised by some of the continental physicians.

External applications are also much in fashion in the treatment of hooping-cough. Frictions to the spine and to the chest; and probably as counter-irritants, they are of some service. The tartarized antimony is the least innocent of these applications. It will often cause foul and very troublesome sores upon an adult skin: and till I am better advised than I am at present of its certain efficacy as a remedy for hooping-cough, no one (however authorized professionally *ludere corio humano*), should rub it upon a child of mine. Mothers are many of them fond of using Roche's Embrocation for the Hooping-Cough. This (Dr. Paris tells us) consists of olive oil, mixed with half its quantity of the oils of cloves and of amber.

Such is the plan of management which you will do well to enforce—and such are the expedients which you may, if you please, make use of as auxiliaries to that plan—when the disease is *mere* hooping-cough. But when it becomes complicated with symptoms of inflammation within the chest, or with head symptoms—(and for such symptoms you must jealously watch)—then you must employ antiphlogistic *remedies* (in addition to the antiphlogistic *regimen*) adapted to the circumstances of the case. Now we know that the *bronchi*, or the *lungs*, are affected with inflammation, when we find that the child has

fever, and that there is *permanent dyspnœa* between the paroxysms of spasmodic cough. In such a case we must have recourse to the treatment required in such inflammation: leeches to the surface of the chest, bleeding even from the arm, if the child's age and strength should warrant it, tartar emetic, small doses of nitre, the warm bath, and blistering; and to these measures, modified and combined according to the particular emergency, it will be well to add small and repeated doses of mercury: of the hydrargyrum cum cretâ, or of calomel; the state of the bowels determining which. Some have recommended friction with the tartar emetic ointment to the *chest* in such cases: but I have the same objection to it there, in such young patients, as to the spine.

When any head symptoms come on, threatening hydrocephalus—such as squinting, convulsions, stupor—those remedies must be adopted which I endeavoured to describe to you when I spoke of that disease: leeches to the head, cold applied there, purgatives, the warm bath: but, except in very young children, I believe there is more danger of fatal *pulmonary* changes in this disagreeable, and sometimes intractable disorder, than of cerebral mischief.

Pneumonia.—I might pass, by a very natural transition, from the consideration of hooping-cough, to that of *spasmodic asthma*. But this last complaint is found to exist in connection with *various* organic changes within the chest, few of which have yet been treated of in these lectures. I shall therefore postpone what I have to say respecting asthma, till I have gone through some other thoracic diseases. And I now proceed to *pneumonia*, or inflammation of the *substance of the lungs*. Questions have been raised as to the precise part and texture in which the inflammation begins; and to these questions I may briefly advert as we go: but I hold that in pneumonia *all the textures* composing the pulmonary substance in the part inflamed are involved in the inflammatory process.

Now of pneumonia it is especially true, that we ascertain its extent, its situation, and every step of its progress, by means of the ear. All the symptoms that give us the most sure information respecting the nature of the disease, the event to which it tends, and the remedial treatment which it requires, spring out of the actual changes wrought in the pulmonary substance itself; and these changes are disclosed to us by the method of auscultation. It is necessary, therefore, that you should understand, first of all, what those changes are which are produced by inflammation of the substance of the lungs: that you should know the morbid anatomy of pneumonia, as an indispensable groundwork for a knowledge of its pathology.

Stages and morbid anatomy.—There are three well-marked, and very constant conditions of the lung, corresponding to different degrees and periods of its inflammation. I will describe them in succession, in the order in which they take place.

The first stage or condition is that of *engorgement:* all modern observers agree, I believe, both as to the nature and as to the name of this condition. The substance of the lung is gorged with blood, or bloody serum. It is of a dark red colour externally, and crepitates less under pressure than sound lung does. We feel that there is more liquid than air in its cells. It is heavier also than natural, and inelastic, and retains, in some degree, the impression of the finger. When the engorged portion is cut, we find it red, and we see a great quantity of a reddish and frothy serum flow from it. Its cohesion is at the same time diminished; it is more easily torn; more, in that respect, like the spleen; and accordingly the term *splenization* of the lung has been given to this stage of its inflammation, as hepatization has to that which succeeds it. In this stage of engorgement the mucous membrane of the small bronchial ramifications is of a deep red colour. The portions most engorged, although their specific gravity is increased, will nevertheless almost always float in water.

Now it is necessary to caution you, in the outset, against a very frequent source of fallacy in respect to this condition of inflammatory engorgement. Such a state of the pulmonary substance as I have been describing, you will meet with in half, at least, of the dead bodies which you may have to examine; and you must necessarily infer therefrom that the persons deceased had *inflammation* of the lungs. There is almost always some degree of *mechanical* engorgement of the back part of the lungs; or of that part

which has been undermost during the last hours of life, or after death; and the two kinds of engorgement can scarcely be distinguished from each other by their anatomical characters alone. Andral at one time held, indeed, that if the engorged part were more friable, more easily torn or broken down under pressure than natural, that was a sufficient evidence of its inflammation: but he afterwards saw reason to change that opinion. We judge by the *situation* of the engorgement sometimes: if it be not in a depending part of the lungs, it is surely inflammatory. We judge also by the antecedent symptoms.

If the inflammation continues, the lung undergoes a further alteration, and presents the following characters. It is still red—externally and within: but it crepitates no longer under pressure; and it sinks in water: it contains in fact no air. Its cut surface presents sometimes a uniform red colour; sometimes a slightly mottled or variegated appearance, produced by an intermixture of specks of the black matter of the lung, and of the interlobular cellular tissue, which is less red than the other parts, and more than naturally obvious to the sight: but the spongy character of the organ is lost; it is evidently solid; and the cut surface very much resembles the cut surface of the liver. Hence Laennec, and after him most other writers, have applied to this altered condition of the lung the term *hepatization*. There still flows out, under pressure, from the surface, when a fresh incision is made, some red fluid, but it is much less in quantity than in the former degree; and it is not foamy; and if the surface be gently scraped with a scalpel, you may often perceive in the red fluid so collected, some traces of a thicker and yellower matter, the first indication of commencing suppuration. The hepatized lung is denser and more solid than before, but it is also more friable: more easily crushed and broken: and this results from the softening of the cellular tissue which holds its component parts together.

If you tear a portion of hepatized lung, and examine the torn surface with a magnifying glass, the pulmonary tissue will appear to be composed of a crowd of small red granulations, lying close to each other. These are, I presume, the air-vesicles, clogged up, thickened, and made red, by the inflammation. As no air is contained in the lung in this stage of the inflammation, it follows that if the entire organ be involved in the disease, it will not collapse when the thorax is laid open: and will therefore appear to be increased in bulk. It is swelled, in fact—just as other inflamed parts are swelled—by the congestion of its vessels, and by the effusion of blood, or of some of the constituent parts of the blood, into its hollows and interstices. The marks of the ribs are frequently visible on the surface of the extended lung. The texture of the lung in this condition is sometimes so rotten, that a moderate degree of pressure between the fingers will suffice to reduce it to a state of pulp; and this diminution of consistence has made Andral quarrel with the term hepatization: and he proposes to call this second stage of pneumonia, red softening, *ramollissement rouge*. All this is very unimportant, provided that you recollect the sense in which either nomenclature is employed. But as Laennec and Andral are both great authorities, and both have their disciples in this country, it is well that you should understand their language.

I have been speaking of pneumonia as it is apt to attack the whole, or the larger portion, or a considerable portion, of the lung on one side: but it is a curious circumstance that the changes I have been describing are sometimes exactly limited to certain of the pulmonary lobules, and this state is called, accordingly, *lobular* pneumonia.

In a degree still further advanced, the pulmonary tissue, dense, solid, and impervious to air, as in the last stage, undergoes an alteration of colour: it presents a reddish yellow, or straw, or drab, or stone colour; or it is of a grayish hue, sometimes mottled with red, or with the black pulmonary matter. The little granulations which I just now mentioned are whitish or gray, instead of being red; and the texture of the lung is still more rotten and friable than before. It is full, in fact, of puriform matter, which is sometimes so abundant, that it oozes out plentifully when incisions are made into the lung: or it may be made to exude by gentle pressure. The gray pus shows itself upon the cut surface in the form of minute drops. The more the pulmonary texture is soaked or drenched with this fluid, the softer and more friable it becomes. When crushed between the thumb and fingers, it is reduced to a yellowish gray pulp, exactly like the fluid itself, only rather more consistent. And by gently forcing the finger into any part of the parenchyma in this state, a small cavity may be made which soon fills with pus, and which might readily be mistaken for a recently formed abscess.

Laennec has called this third stage of the process of inflammation in the lung, *gray hepatization*, or *purulent infiltration*. Andral denominates it *gray softening—ramollissement gris*. In fact, it consists in *diffused suppuration* of the pulmonary texture. And it is a very remarkable circumstance, and one which the researches of modern times have brought to light, that inflammation of the lung, going on to suppuration, does not lead to the formation of a circumscribed abscess, as it does when it affects the cellular tissue, or the parenchymatous tissue, in other parts of the body. Abscess of the lung used to be spoken of as a very common thing; but it is a very rare thing. In several hundred dissections of persons dead of pneumonia, made by Laennec during a space of more than twenty years, he only met with five or six collections of pus in the inflamed lung. Once only did he find a *large* abscess of that sort. Once only has Andral seen a real abscess of the lung form as a consequence of pneumonia. You may find collections of pus in the lungs sometimes occurring in connection with the inflammation of veins. Several instances of that kind have happened very recently in patients who have died in the Middlesex Hospital. But these are not ordinary cases of pneumonia. I need scarcely caution you not to take tubercular vomicæ and cavities, containing pus, for genuine abscesses of the lung. These, and the phlebitic deposits of pus, are not *exceptions* to the general statement: they arise from different forms of disease: and you will find a circumscribed collection of pus, surrounded by hepatized lung, as a consequence of common pneumonia, an exceedingly rare event.

Can we account for this in any way? I do not know that any satisfactory explanation of the fact has ever been offered. But I would submit to your consideration what has occurred to my mind on this subject. When I was speaking of inflammation in general, I pointed out to you the remarkable influence which the presence of atmospheric air in contact with the inflamed part has in accelerating, or determining, the event of suppuration. In a recent cut, the admission or exclusion of the air to the cut surface will make all the difference between the adhesive and the suppurative inflammation; and so in other cases which I then mentioned, and will not now trouble you by repeating. Now it seems to me that the same principle obtains in inflammation of the lung. First, there is an effusion of serum and blood, then of lymph and blood; but the air, passing into the surrounding sounder tissue, and mingling for a time even with the inflamed portion itself, causes the suppurative process to supersede the adhesive; and so no wall of circumvallation is formed by the coagulable lymph, as is the case in cellular tissue which is not accessible by the air. Whether this be a sufficient explanation of the *fact* (all explanations being the resolving a given fact into a certain class of other facts more general and comprehensive); I say whether it be a reasonable and satisfactory explanation, you will judge: at any rate it may serve to impress upon your memory that fact which it endeavours to elucidate.

Gangrene is sometimes, but very seldom, the result of acute inflammation of the lung. It is almost as uncommon as the formation of an abscess. Yet it certainly does now and then occur, as a consequence of acute inflammation of the pulmonary substance. It is somewhat more common (though under any shape rare) as an independent and primitive affection. Sometimes it occupies a large portion of the lung, and is uncircumscribed; and sometimes it is more limited. The colour of the part which has thus perished under inflammation, is dark, of a dirty olive, or greenish-brown colour. The gangrenous portion is moist and wet; sometimes of the consistence of the engorged lung; more commonly softer, and even diffluent; and it stinks most abominably. This horrible odour is in truth, during life, the most distinctive character of gangrene of the lung; and it sometimes renders the room in which the unhappy patient is lying, scarcely endurable. I should have stated before that the puriform infiltration of the third stage of pneumonia is attended with no fœtor.

There are some other points, connected with, or learned from investigating, the morbid anatomy of pneumonia, which I may as well take this opportunity of telling you, before we go on to consider the symptoms, physical and general, of that disease.

There are two lungs, just as there are two tonsils, and two eyes; and in the one case as well as in the other, inflammation may affect both organs at once, or it may affect one of them alone. Technically speaking, pneumonia may be either double or single. Again,

the inflammation may occupy a part of one lung, or the whole of it: in other words, it may be partial or general; but it does not affect all parts, or both sides, indifferently or capriciously. In the first place, it is (why I know not) greatly more common on the right side of the body than on the left. I will give you some statistical statements collected by Andral, in respect to this point. Of one hundred and fifty-one cases of pneumonia, noticed at La Charité, ninety were of the right lung alone; thirty-eight only of the left alone; seventeen of both sides at once; and in six the situation was uncertain. He was at the pains of collecting the particulars of fifty-nine other examples of pneumonia, from different authors, so fully described as to leave no doubt about the nature and situation of the disease. Among these, the inflammation existed in the right lung alone in thirty-one patients; in the left alone in twenty; and in both sides at once in eight. Hence, taking both series of observations together, we have two hundred and ten cases of pneumonia; and there were one hundred and twenty-one in which the right side was solely the seat of the disease; fifty-eight in which the left; twenty-five in which the pneumonia was double; and six in which the seat was uncertain. So that, at this rate, pneumonia is more than twice as common on the right side as on the left; and does not occur on both sides together so often as once in eight times.

Again, with regard to the *part of the lung* which is most obnoxious to inflammation, there are remarkable differences. It is well known, and it is a very important fact in respect to diagnosis in some cases, that the lower lobes are more liable to inflammation than the upper. I speak, of course, of active, idiopathic inflammation. But this circumstance, much insisted on by Laennec, and quite true in the main, has perhaps been somewhat exaggerated. I have not had leisure to frame any numerical statement of the cases that have come under my own observation, but the general impression which they have left upon my mind is in favour of the correctness of Laennec's statement—that pneumonia generally commences in the lower lobes, and spreads upwards frequently to the superior lobes. But I may adduce Andral's statistical representation in respect to this question also. Of eighty-eight cases of pneumonia, he found that the inflammation affected the inferior lobe forty-seven times, the superior lobe thirty, and the whole lung at once, eleven.

Inflammation of the bronchi constantly accompanies inflammation of the parenchyma. The mucous membrane presents a red colour both in the large and in the small branches of the air-passages. And when a single lobe is inflamed, it has been observed that the redness of the mucous membrane existed in these bronchial tubes alone which were distributed to that lobe. You may have bronchitis without pneumonia; but pneumonia without a corresponding extent of bronchitis is perhaps never seen.

The majority of cases of pneumonia are attended also with a degree of inflammation of the investing membrane or the lung: there is some pleurisy. So frequently indeed is this the case, that certain writers, Andral among others, call the disease by the compound name of *pleuro-pneumonia*. However, pneumonia may and does *sometimes* occur without any concurrent pleurisy. Of the latter complaint I must speak by itself; and I merely notice now the frequent combination of the two—the occurrence of a slight degree and extent of pleuritis in most cases of pneumonia—that you may the better understand some of the general symptoms.

Auscultatory Signs.—Now such being the changes which the lungs undergo when inflammation affects the pulmonary texture, we may next inquire what signals of its existence the inflammation holds out; and how far we, not having the power of *seeing* what is going on within the cavity of the thorax, may nevertheless ascertain the important processes which are there transacted.

If the ear be applied to the surface of the chest, with or without the intervention of the stethoscope, and the portion of lung subjacent to that surface happen to be in the first stage of inflammation, that of engorgement, what does the lung, so suffering, *say?* what audible notice does it give of its morbid condition? Why it speaks very plainly. You hear a peculiar crackling sound; the smallest and finest possible kind of crepitation: which has been happily illustrated by saying that it resembles the multitudinous little crackling explosions made by salt when it is scattered over red-hot coals. Andral has another resemblance for it, and not a bad one; he says the noise is often like that which is produced

by rumpling a very fine piece of parchment. Dr. Williams observes that a pretty correct idea of this sound may be obtained in a ready way, by rubbing between the finger and thumb a lock of one's own hair, close to the ear. Laennec calls this *crepitant rhonchus:* I would speak of it as *minute crepitation;* or the *crackling of pneumonia.* This may be heard in a very limited spot in the beginning. And what an important sound it is! "It is a direct symptom, having immediate reference to the structure of the part. And (says Dr. Latham) if we consider what the part is, and what the disease; the part the lungs, and the disease inflammation: we cannot too highly value this single symptom (simple and mean as it may seem), which gives the earliest and surest intimation that such a disease has begun, as tends to disorganization, and the inevitable loss of life, unless quickly arrested by its counteracting remedy."

At first, when you catch the inflammation in its earliest stage, this minute crepitation, which announces *commencing* engorgement of the part, is heard mingling with the ordinary vesicular breathing. This obscures the natural sound, though it does not yet entirely cover it—but as the inflammation advances, the crackling becomes more and more pronounced, until at length it totally supersedes it. So long as the natural vesicular breathing prevails over the crackling, we may conclude that the inflammation is slight: and if the crackling should, in its turn, become predominant, if it should ultimately mask the murmur of respiration entirely, that infalliby denotes the progress of the pneumonia, and teaches us that it tends to pass from the first into the second degree. But the crackling sound does not *long* remain in any part. As the case proceeds, the sound is less and less heard, and at length is not heard at all, in that spot; and it may be succeeded by one of two very different things. Its place may be taken by the natural respiratory murmur again. When this is so, it denotes the *resolution* of the inflammation. But the crackling may cease, and either no sound at all be heard in its stead, or another morbid sound which I shall presently describe: and this teaches us with absolute certainty, that the disease is growing more severe and serious; that the lung is becoming, or has become, *hepatized.*

Let us inquire, for a moment, before we go any further, what is the nature and the seat of this minute crepitation, so characteristic of the commencement of pulmonic affection? With respect to its *seat*, I apprehend, there can be no question. It proceeds from the very smallest ramifications of the bronchi, and the air-vesicles themselves. The common opinion is, and such, I confess, is mine, that the sound is the same in cause and kind, only different in degree, with the large and small crepitation described in a previous lecture: that it results from the passage of air through liquid; from the formation and bursting in quick succession of a multitude of little air-bubbles. The bubbles are necessarily minute, for they are formed, and they explode, in very slender tubes. This is Andral's view of the matter. Laennec does not appear to have formed very clear notions on the subject. But a different explanation has been offered by a well-known and able writer on the auscultatory signs of disease, in this country: I mean Dr. Williams. He holds that the distended blood-vessels, and the interstitial serous effusion, press upon the minutest bronchial ramifications, and obstruct, without wholly preventing, the passage of the air through them: that these small tubes are lined by a viscid secretion, such as is expectorated, and such as I shall have to describe: that the sides of the tubes stick together in consequence of the presence of this viscid matter; and that it is the separation of these adhering sides by little portions of air which successively pass in and out, that gives rise to the characteristic sound. However, what it is important to remember is, that the crackling sound proceeds from the minutest divisions of the air-tubes, and from the ultimate vesicles of the lungs.

Sometimes, I say, when this crackling ceases, the ear, applied to the corresponding surface of the chest, feels it heave up in inspiration, but catches no sound at all. Much more commonly, however, a *new* sound reaches the ear. It is not the vesicular rustle; it is not the minute crepitation: but a whiffing sound is audible, like that produced by blowing through a quill. Little gusts of air are puffed in and out; most distinct, often, at the termination of a slight cough or hem. This is the sound to which the term *bronchial respiration* has been given: and the name expresses well the fact. I mentioned before, that in the healthy state we do not hear the air pass through the larger bronchi during inspiration and expiration: the sound doubtless is made, but it is obscured and hidden by the smooth rustle of the vesicular breathing, which comes from the spongy lung surrounding the larger

divisions of the bronchi, and intervening between them and the ear. But that spongy structure is now filled up. The hepatized lung admits air to pass through the larger bronchi, which are still patent, but it admits none into the vesicles and smaller tubes. It crepitates not when pressed between the thumb and finger: in fact, it is converted into a solid substance, and conducts the sound, in the living body, as any other solid substance might do: and therefore the whiffing, blowing, gusty sound of the breath, as it enters and departs from the larger bronchial tubes, which still remain open, is conveyed to the ear, and *bronchial respiration* is heard. At the same time, and in the same place, another auscultatory phenomenon generally arises, and admits of a similar explanation. The *voice* of the patient descends into the pervious bronchi, and is conveyed to the ear of the listener through the solid lung: and it is quite altered by that circumstance. The tone of it is modified; it sounds like the voice of one speaking through a tube. It is totally different from the same voice heard through the healthy lung at the corresponding point on the other side. It approaches in distinctness and quality, but it does not reach, the sound heard in speaking, when the stethoscope is placed over the trachea. A humming and muttering are audible, but the words are not distinctly articulated into the ear. It is hard to describe these things in words. Three minutes, at the bed-side of a patient in whom the bronchial breathing and the bronchial voice were tolerably well marked, would put you in possession of them for ever. They are striking sounds: requiring no fine tact to distinguish; and exceedingly informing sounds. But I must resume this subject when we meet again.

LECTURE LI.

PNEUMONIA CONTINUED: ITS GENERAL SYMPTOMS; PAIN, DYSPNŒA, COUGH, EXPECTORATION. COURSE OF THE DISEASE. PROGNOSIS. TREATMENT.

I WAS describing, at the close of the last lecture, the auscultatory signs which lead us to the knowledge that the inflamed lung, in a case of pneumonia, has passed from the first into the second stage of inflammation, and become solid, or hepatized. The altered condition of the organ gives rise to altered sounds. Instead of the vesicular breathing, which is the natural sound: or of the minute crepitation, which is the sound belonging to the first stage of the inflammation; we either hear no sound at all, although we feel the chest heave up against our ear, or we hear what I described under the denomination of *bronchial respiration;* that is to say, a puffing sound which is conveyed to the ear from the larger and still pervious branches of the bronchi, through the solid portion of lung around them, and through the solid walls of the chest. This is what the listener hears when the patient *breathes.* And when he speaks, his *voice* is heard, much more resonant than is natural, much more resonant than in the corresponding spot on the opposite side of the chest, entering the same open air-tubes, and conducted to the ear by the dense and solid lung. We thus became acquainted with two entirely new sounds; sounds which are never heard in the healthy state of the lungs; *bronchial respiration*, and *bronchial voice*, or *bronchophony:* and you will do well to remember these two sounds, and to familiarize your ear to them; for they speak a most significant language in *other* pulmonary diseases, as well as in pneumonia.

But I say, sometimes we hear these morbid sounds, in the case in question, and sometimes we hear *no* sound at all during the breathing. How is that? Why the existence and degree of the bronchial respiration, and bronchial voice, vary according to the place and extent of the inflammation. These morbid sounds are most plainly marked, where the number and size of the bronchial tubes involved in the hepatization are the greater. They are most distinct, therefore, when the inflammation occupies the upper part of the lung; or the central parts, what are called the roots of the lungs; and when it extends thence to the surface: but when the lower portions alone are inflamed, or the inflamma-

tion is merely superficial or partial, they may not be heard at all. Again, if the hepatization should be so general and complete, as to prevent the chest, on the affected side, from expanding—you will, in that case, hear no bronchial *respiration;* for the air in the larger bronchi must be stagnant. *Bronchophony*, however, may remain.

When we have the bronchial respiration, usually also we have dulness on percussion. The degree in which this is present will depend upon the circumstances of the case. If a portion of crepitant and permeable lung, even a thin portion, should intervene between the inflamed parts and the walls of the chest, there will still be resonance, though it will not be exactly the natural resonance on percussion. If the hepatized part come close up to the ribs, the sound elicited by mediate percussion will be flat or dead. With all this, you will generally hear, in the sound lung, if the whole of the other be engaged in the inflammation;—or in those parts of the inflamed lung that are healthy; you will hear, I say, *puerile respiration:* and this is a strong confirming symptom that a part of the breathing apparatus is spoiled, and that the remaining part is endeavouring to compensate for its deficiency.

Now this period in pneumonia, when no sound but bronchial breathing is audible during respiration, is a period of anxious and painful interest. We cannot tell whether the lung will revert gradually to its healthy state; or whether it is passing into the third stage, that of purulent infiltration. But taking first the most favourable of these two suppositions—what happens? Why, *there*, where for a while we heard nothing but bronchial respiration, a slight crepitation begins again to be distinguishable, especially at the end of each act of inspiration; gradually this increases, in extent and intensity, and as it increases, the bronchial breathing, and the bronchial voice, become proportionally less distinct, in consequence of the texture of the lungs becoming again permeable by air, and therefore a worse conductor of sound. By degrees, the bronchial breathing and voice disappear altogether; the vesicular murmur begins again to mix with the crepitation, and at length supersedes it; and the lung is restored to its previous fitness for the purposes of respiration. The same symptoms therefore recur, over again, but in a reversed order; the *returning* crepitation is however coarser and larger, and less regularly diffused, than that of the *advancing* pneumonia:—and even when, in the ordinary condition of the breathing, nothing is heard but the natural vesicular rustle, some crepitation is found for some little while to mingle with it, towards the end of a full inspiration. This is believed to depend upon an œdematous state of the pulmonary texture, left after the active inflammation has been displaced. Next, let us take the *worst* of the two suppositions. Auscultation has traced the disease *through* its stage of engorgement, and *into* its stage of hepatization. Can it trace it further? I believe not, with any certainty. We cannot say whether the lung remains in the state of hepatization, (as it may remain,) or whether it has passed into the third stage. But at last, if the structure of the lung breaks down, and a portion of it is expectorated, air finds its way into the vacant spot, and gives rise to a large gurgling crepitation. But the other signs sometimes come to our aid when this state has been reached.

We often find, after death, the three degrees of pneumonia existing in different parts of the same lung; and therefore it is not to be wondered at that the different parts of the chest during life should yield sounds indicative of each of those degrees, or at least of the two first; minute crepitation *here*, bronchial breathing, and bronchophony, and dulness on percussion *there*, and in another spot, *no* sound at all, or on the other hand, *puerile* respiration.

Again, it must be confessed—and I am desirous of confessing it, for I am sure that the method of auscultation is brought into undeserved suspicion and disrepute by attempts made to assert its all-sufficiency in all cases—it must be confessed that in some instances, although pneumonia exists, the ear is able to collect nothing of it: nothing indicative of its situation, or of its extent, or even of its existence. The pulmonary expansion is clear, all over the thorax; nay much more strong than is natural; and this circumstance justifies the belief that, from some cause or other, not *necessarily* from pneumonia, a portion of the lung has ceased to discharge its function, and the other portions have taken it up. This failure on the part of auscultation happens when the inflammation occupies only a small portion of the lung, and that portion central, or deeply situated; at a distance from

the walls of the chest. For this reason auscultation may give little or no account of *lobular* pneumonia.

Such are, then, the physical signs that accompany and reveal the successive changes of texture, destructive and reparatory, which take place in inflammation of the lungs. I do not know whether I have made them clear to you; but I know that no very long apprenticeship, if I may so speak, in the wards of a hospital, will be sufficient, with a little guidance, to render you master of them. There are indeed varieties, and modifications, and exceptions, which nothing but such an apprenticeship can ever teach you. Of these it would be idle and unprofitable for me here to speak: and I go on to consider the *general* signs of pneumonia; some of which, either in themselves, or in combination with the *physical* signs, are of no less importance than these.

General symptoms.—In the majority of cases the commencement of inflammation of the lung is marked by shivering, followed by heat and increased frequency of pulse; in one word, by inflammatory fever: and at the same time, or presently after, a stitch in the side comes on, with cough, and a sense of oppression in the chest. In other instances the disease steals on more insidiously, and succeeds to bronchitis; the inflammation appearing to propagate itself by little and little from the larger to the smaller bronchi, and ultimately to reach the air-vesicles themselves, and the interstitial textures; and this may be accomplished with or without the sharp pain or stitch in the side. At first the cough may be dry, but it soon is attended with a very characteristic sort of expectoration. The dyspnœa is sometimes but slight in the outset; sometimes severe.

Apart, therefore, from the physical signs, we may say that the usual symptoms of pneumonia are pain, more or less severe, on one side of the chest; dyspnœa; cough; a peculiar expectoration; and fever.

The pain in pneumonia appears to exist only in those cases in which the inflammation of the lung is accompanied by some degree of pleurisy. But these are the most numerous cases. It is most commonly experienced on a level with, or a little below, one or other breast; but it may exist in almost any other part of the thoracic parietes. Generally it is most severe at the beginning, declines by degrees, and ceases altogether for some time before the *pneumonia* ceases. It is aggravated by cough; by a full inspiration; often by sudden changes of posture; by pressure made upon the ribs or intercostal spaces; or by percussion of that part. For the same reason the patients cannot lie on the painful side. Andral declares that in all the individuals in whom he had noticed this pain, and who died, he found the pleura inflamed, and covered more or less with coagulable lymph; and, on the other hand, that he had constantly known the absence of pain coincide with a sound condition of the pleura. When there is no sharp pain, there is, however, some morbid sensation, of trouble, or tightness, or weight, or heat, on the affected side. He quotes, with approbation of its justness, the ancient observation respecting pneumonia:—"Affert plus periculi quam doloris." When I come to speak of pleurisy as a distinct and substantial affection, I shall revert to this pain.

It is, or it was, a common doctrine that one of the general symptoms of pneumonia relates to the posture which the patient assumes; that the *decubitus*, to speak technically, is on the side affected. The truth, however, is as I have just now stated it. The *breathing*, indeed, is more oppressed when the patient lies on the sound than when on the diseased side; but, in point of fact, patients labouring under this disease almost all lie upon their backs; the decubitus is dorsal. The difficulty of breathing deserves some notice. In general it bears a direct proportion to the extent and severity of the inflammation. But there are many exceptions to this. In some persons the inflammation of even a very small portion of one lung, will embarrass the respiration greatly. In others, who have a much larger portion of the pulmonary tissue intensely inflamed, the dyspnœa is but slight. So that the degree of difficulty of breathing is not a *certain* measure of the seriousness, or rather of the extent and the degree, of the inflammation. It is probable, that if we knew of what kind was the ordinary breathing of the individuals thus differently affected, we should find that they whose respiration is generally indistinct, or noiseless, who do not seem to *want* all their lung for the purpose of breathing, would best bear to have a part of it inflamed; and *vice versâ*. *Cæteris paribus*, inflammation of the upper lobe causes greater dyspnœa than inflammation of the lower. I may observe further with

respect to dyspnœa in general, that you must not trust implicitly to what patients tell you on that head. They will often deny that they have any shortness of breath, when one may see them respiring with unnatural rapidity, or observe that, in their discourse they pause between every three or four words to take breath.

However, the dyspnœa produced by pneumonia varies greatly in its degree in different cases. Sometimes it is so slight that the patient is not conscious of it, and the physician scarcely perceives it: sometimes it is so extreme, that the patient, entirely regardless of what is going on about him, seems wholly occupied with respiring; is unable to lie down; can scarcely speak: his face becomes lividly red or pale, and is expressive of the utmost anxiety; his nostrils are expanded, and in full action; the respiratory movements are very frequent and very short or shallow, as if the air was not able to penetrate beyond the primary divisions of the bronchi. From this state of extreme dyspnœa few patients recover; and between this, and the slightest hurry or embarrassment of the breathing, there are of course many degrees.

Delirium is a symptom which very frequently occurs in the course of an attack of pneumonia; and a very ugly symptom it is. It denotes that the due arterialization of the blood is largely interfered with by the pulmonary affection. It measures, in one sense, the quantity of mischief which is going on within the thorax: and it is a direct evidence that the pectoral mischief is telling, through the circulation of venous blood, upon the *brain*.

The cough, in pneumonia, has no particular character; and affords but little information. It does not usually take place in paroxysms; and its severity and frequency are not always proportioned to the intensity and extent of the inflammation. It is usually dry in the outset; but in a few hours it is accompanied by the expectoration of a peculiar sputa, which constitute one of the most certain indications of the presence of pneumonia: and as this is a symptom which every one can easily recognize, I will describe this characteristic expectoration, and endeavour to explain the cause of it.

The expectoration of pneumonia, when well marked, consists of transparent and tawny or rust-coloured sputa, uniting, in the vessel containing them, into one jelly-like and trembling mass; and of such viscidity that the vessel may be turned upside down, and strongly shaken, without their being detached from its bottom or sides. It cannot be said that when there is no such expectoration as this, there is no pneumonia: but it may be affirmed that where we do find such expectoration, there almost certainly we have pneumonia. At the outset of the disease, either nothing is spat up, or simply some bronchial mucus: but on the second or third day generally, the matters expectorated assume the characteristic appearance: *i. e.*, they come to be composed of mucus, intimately united and combined with blood. It is not that the sputa are *streaked* with blood, as often happens in *bronchitis:* nor have we the *unmixed* blood of *hæmoptysis*. But the blood and the mucus are amalgamated together; and in proportion to the quantity of the former, the sputa become of a yellow colour, or of the colour of rust, or of a decided red: and at the same time they become glutinous and tenacious: they adhere together, so as to form one transparent homogeneous mass. So long as this mass flows readily along the sides of the vessel when it is tilted, so long have we reason to *hope* (judging from that circumstance alone) that the inflammation of the lung does not pass its first degree. But, as I said before, the sputa often acquire an extraordinary degree of viscidity: so as no longer to separate themselves from the vessel when it is inverted: you cannot even shake them out. When this happens, we are obliged to *fear* that the pneumonia reaches its second degree. In fact, when the sputa become thus rusty and very viscid, the stricken chest almost always returns a duller sound, and the vesicular breathing is abolished, and bronchial respiration takes its place. The pneumonia is then at its acme; and the expectoration remains for some time stationary. At length, if the inflammation recedes, the sputa become again less tenacious, less red or yellow, and more like the expectoration of mere catarrh. But if the disease goes on from bad to worse, the rust-coloured sputa may continue to the end. Commonly there is *less* expectoration in that case, or even none at all. Not that the mucus ceases to be secreted, but that its *excretion* is no longer possible: either on account of its extreme tenacity, or on account of the patient's debility. The sputa then accumulate in the bronchi, trachea, and larynx, in succession: they fill up the air-passages; and suffocate the

patient. In some instances the expectoration, in the advanced stages of the disease, consists of a fluid having the consistence of gum-water, and of a brownish red colour: like (as Andral says) liquorice-water, or plum-juice. He states that the mere occurrence of this kind of expectoration has led him to announce the existence of the third stage of pneumonia; and that the subsequent examination of the dead body has seldom failed to justify his diagnosis. Sometimes again, during the third stage, very perfect pus is excreted.

That the colour of the sputa peculiar to pneumonia depends upon an intimate union of *blood* with the altered mucus, is perfectly obvious when that colour is deep. And even when this transparent mucus is yellow, you may satisfy yourselves by the following simple experiment that the source of the colour is the same, and that the yellowness does not result, as some have fancied, from an admixture of *bile* with the matter expectorated:—If to water, rendered viscid by dissolving a certain quantity of gum in it, you add blood, drop by drop, you will obtain, in succession, all the shades of colour that are presented by the pneumonic sputa: first a yellow tinge; then a tawny yellow which loses itself in a red, and comes to represent the colour of the rust of iron; and lastly an intense red. The sputa may indeed, sometimes, but I believe *that* does not often happen, be coloured by bile; but bile is not the source of the yellowness which characterizes them in cases of pneumonia.

Sputa composed of very red mucus, indicate pneumonia less surely than such as are tawny. The very red masses, in which there is more blood than mucus, often belong to pulmonary apoplexy.

Although these rust or orange-coloured sputa are commonly present during the more active period of pneumonia, and as far as my experience goes are peculiar to that disease, you ought to be aware that they do not *constantly* accompany it. Sometimes the matters expectorated are like those of catarrh: and sometimes there is scarcely any expectoration at all.

When the pneumonia passes into gangrene--which I repeat is an exceedingly rare consequence of inflammation in that organ,--the expectoration becomes of a greenish, or reddish, or dirty gray colour: and exhales a fœtid smell, resembling that which proceeds from gangrene of the external parts.

I have now described, *seriatim*, the main symptoms, general and physical, which mark the existence and the progress of pneumonia. And in order to give you a just notion of each, I have spoken of them separately. But they *exist together;* and they must be *studied* together: and some will be found to confirm or to correct the indications that might be drawn from the others. I must briefly therefore run over the phenomena of the disease we have been considering, as it actually presents itself in most cases.

The first symptom felt is commonly pain in the side; which may or may not have been preceded by rigors. At the same time the breathing is constrained; and the patient coughs without expectorating. At this period, the ear may generally detect a slight degree of minute crepitation, which is not strong enough to mask entirely the vesicular rustle; and the stricken thorax still sounds well: and there is fever withal. This assemblage of phenomena constitutes the first period of the disease. From the second to the third day, new symptoms appear. The expectoration, hitherto absent, or merely catarrhal, becomes characteristic; being at first moderately viscid, and having a degree of colour proportioned to the variable quantity of blood which it contains. The minute crepitation increases, and drowns or supersedes the natural respiratory murmur: the clear sound produced by percussion begins to diminish on that side on which the crackling is heard and the pain is felt; and that pain is commonly less sharp than in the beginning. The dyspnœa increases, as is quite apparent from the short and frequent inspirations made by the patient. If the pain be acute, he cannot lie, on that account, on the side affected; neither can he place himself on the sound side, because in that position his respiration becomes more laborious; he remains therefore, almost constantly, lying upon his back.

In this condition of pneumonia, though the disease may be severe, the inflammation is as yet in its primary stage. It often remains stationary for a while, and then recedes, and terminates by resolution. The dyspnœa diminishes, the slight dulness of sound disappears, the crackling is gradually displaced by the natural murmur of the pulmonary ex-

pansion, the sputa again become those of simple bronchitis, the fever subsides, and ceases; and all is well again.

At other times, instead of retrograding towards resolution, the pneumonia becomes more intense, or rather more extensive, without passing beyond its primary stage; and the patient may die while it is still in that stage. But this is unusual. Ordinarily, if the inflammatory engorgement does not cease by resolution, and the symptoms that announce it are exasperated, we must expect that the second stage will be established. And we may be certain that it exists when we observe the following phenomena:—the breathing becomes more and more constrained, short, accelerated; the speech ceases to be free; the patient can do no more than pronounce a few interrupted words in a panting manner. The sputa acquire such a degree of viscidity, that they can no longer be detached from the vessel by shaking it; the sound afforded by percussion, on the side affected, is decidedly dull: at first we still hear a little of the minute crepitation, without the admixture of any pure vesicular breathing; then that little crepitation ceases, and either no sound at all is perceived by the ear, or, in the part where the percussion is dull, bronchial respiration is heard, and this is almost always accompanied with bronchophony. The patient continues to lie on his back.

In this degree of the disease, the prognosis is always uncertain. The patient often sinks rapidly, and dies from apnœa. Yet even in this degree resolution *may* still take place. In that case the dulness on percussion diminishes; the bronchial breathing disappears; we hear afresh the small crepitation, at first alone, then mixed with the natural respiratory murmur, which, in its turn, becomes alone audible. The sputa return to their catarrhal character. In the meanwhile the dyspnœa and fever diminish, and then cease entirely.

It would doubtless be very interesting to determine, in a given case, whether the lung of our patient was in the second or the third stage of inflammation. But there are no certain means for making this distinction. We may *guess* that the third stage is established if the face becomes exceedingly pale and corpse-like; we may be more confident of it if the prune-juice expectoration, or if puriform expectoration, should occur; and our presumption will be strengthened if the disease has existed for a certain *time*. However, this last circumstance will not help us *much;* for sometimes the lung has been found to be in a state of suppuration on the fifth day of the disease, and sometimes it has been found still in a state of red hepatization after fifteen or twenty days.

Whether, when the lung has reached this third stage, it is still susceptible of repair, is a question which no one can answer. We have not the materials for its solution, inasmuch as we have no sure sign of the existence of this third stage during life. I should *think* that recovery from diffused suppuration of the lung is not possible. The rarer form of circumscribed abscess certainly is not of necessity fatal.

The *duration* of pneumonia may be laid, upon an average, at ten days. In a table collected by Andral for another purpose, viz., to determine whether there were any fixed *critical* days in respect to the termination of the disease (a question which I shall not now discuss), the duration, in 112 cases, varied from four days to six weeks. But one only was thus protracted; 23 cases lasted each seven days; and only 15 of the 112 instances continued longer than a fortnight.

I have very little to add to what I have stated already of the morbid anatomy of pneumonia. Of the changes which the *lung itself* undergoes you are now, I hope, fully apprised. The pleurisy, which often attends the disease, is seldom accompanied by much effusion; indeed, when the whole of one lung is solidified by inflammation, it fills the cavity of the pleura, and *prevents* much effusion. The heart is found to be in that condition which I formerly described to you, as being both a consequence, and an index, of death by apnœa. Its right cavities especially are distended by black coagulated blood; and a remarkable degree of venous congestion is frequently met with in the liver, and spleen, and intestines. The amount of this varies according as the process of dissolution—what the French call the *agony*—has been more or less protracted, and the breathing more or less difficult.

Neither need I enter upon any formal discussion of the *causes* of pneumonia. Sometimes *no* cause can be traced; sometimes the disease is clearly the consequence of exposure to cold; especially under those circumstances which were formerly described as aid-

ing the injurious operation of cold upon the human body. Why, in one person, such exposure causes peritonitis, in another pleurisy, and in a third inflammation of the substance of the lungs, we can give no satisfactory account.

Prognosis.—It remains, then, only that I should speak first of the *prognosis*, and secondly of the *treatment*, of pneumonia; and of the first of these matters, of the prognosis, I have already, incidentally, told you nearly all that is made out, or worth knowing. It is almost superfluous to say that the first degree of the disease is less dangerous than the second, and the second than the third. There is no doubt that pulmonary inflammation may still undergo resolution, although a great part of one lung should be hepatized; but there are no facts which prove—indeed there is no possibility of proving—that the lung may recover from the state of purulent infiltration—the third degree.

Something will depend upon the *extent* of the inflammation; I mean that pneumonia, in the first degree and of great extent, is generally as serious as pneumonia in the second degree, but much more circumscribed. Inflammation of the upper lobes is also more perilous than inflammation, to the same extent and degree, of the lower.

Of the *general* symptoms, those which we learn independently of auscultation, the *respiration*, as a prognostic sign, is the most important. Considerable dyspnœa, whatever may otherwise be the condition of the lung, is always a bad omen. We get less help from the state of the *pulse*. If, however, a feeble pulse goes along with great difficulty of breathing, and if it does not develope itself after the first bleeding, we must conclude that the inflammation is intense, and form, therefore, an unfavourable prognosis. The supervention of *delirium* is also a discouraging circumstance. You will have inferred already the information which may be gleaned from the character of the expectoration, in respect to the probable issue of the disease. Great viscidity of the sputa, and a deep rusty colour, announce intensity of inflammation: their return to the catarrhal condition indicates that resolution is going on. Watery and brownish sputa, more or less like plum-juice, should induce us to suspect suppuration of the lung, and are therefore of bad augury.

Treatment.—The *great* instruments to be employed in the *treatment* of inflammation of the lungs, are the same which have so often been recommended by me, in other inflammatory affections, before: blood-letting, tartarized antimony, mercury. Of these, blood-letting is the chief. Both reason and experience attest the especial power of bleeding upon acute pneumonia. In the first place, it tends to restrain or extinguish the inflammation *as* inflammation. But, in the next place, it has the effect of relieving the particular *function* of the lungs. The more blood is sent to them in excess, the more dyspnœa must there be, the more venous blood passing into the arteries, as well as the more risk of the effusion of lymph and the obliteration of the cellular texture of the organ. When we bleed, therefore, in pneumonia, we kill two birds (as the phrase is) with one stone. We do that for the lung, which we do for an inflamed eye when we darken the room, or for an inflamed joint when we keep it absolutely at rest, *i. e.*, we do all that we can to spare the exercise of the organ, and to prevent aggravation of the inflammation from that cause. And the result of the free abstraction of blood in this disease, fully vindicates the value of a practice which has been pursued for ages. The late Dr. Gregory, of Edinburgh, was in the habit of saying, in his lectures, that provided he was called *early* to a case of pneumonia, he would be contented to dispense with all other aids than those of a lancet, and water-gruel. I am far from desiring you to believe that blood-letting is the only expedient required: but certainly the amount of the best experience, ancient and modern, is strongly in favour of its free, and I might almost say, its prodigal, employment. Very lately one, most distinguished, French writer, M. Louis, has endeavoured to show that venesection has not much control over the progress or event of pneumonia; and I advert to his opinion on this subject merely to caution you against being misled by it; as you might otherwise be, considering his well merited reputation as an exact and faithful observer.

I can only lay down *general* rules and indications with respect to the manner and amount of blood-letting in this disease, or in any other disease. The abstraction of blood will be effectual, *cæteris paribus*, in proportion as it is *early;* during the first stage—the stage of engorgement—and before the spongy texture of the lung has been obliterated. The patient should be bled in an upright position, by a large orifice and in a full stream:

and the bleeding should be continued until some sensible impression is made upon the system: until the *pulse* becomes *softer;* or, if it were contracted, until it becomes *fuller;* until the *sensation of constriction* is abated, and the *dyspnœa* relieved; or until *syncope* appears to be at hand.

Bleeding, in this early stage, often gives very speedy relief, both to the pain and to the dyspnœa. Sometimes the pain does not cease at once, but goes off a few hours afterwards; but I believe that if the breathing be not at all relieved at first, the case generally (though not always) does ill. However, you are not to expect that one blood-letting will suffice, even when it is performed early in the disease. Such a favourable case may happen, but not often. The patient should always be seen within four or five hours from the time of the first venesection, that a timely *repetition* of it may take place, if the relief has not been complete, or has not been *permanent*. Many fatal cases have probably *been* fatal from want of this attention; from too long an interval having been suffered to elapse between the bleedings. A vein may be opened, if necessary, two or three times in the twenty-four hours; and the ultimate loss of strength, and even loss of blood, will be less under such treatment than if the blood-lettings were repeated at longer intervals; and the necessity of the repetition must be judged of from the circumstances of the case. As an auxiliary to the lancet, I am much in the habit of taking blood from the surface of the chest itself, by means of cupping-glasses, or of a large number of leeches. I believe that much good is done by this local emptying of the blood-vessels. It is particularly indicated if there be pain; and the part to which the leeches or cupping-glasses are applied should be determined by the situation of the inflamed portion of lung, when that is ascertained by the ear. I scarcely need say that the whole of the antiphlogistic regimen must be rigidly enforced; that the patient must keep his bed; and that all superfluous exertion of his lungs in speaking must be forbidden.

When the inflammation has advanced into the second stage, we cannot expect that the removal of blood will have so decided an influence upon the inflamed and solid parts; but even then, if duly moderated, it will have these good consequences: it will diminish the force of the heart and arteries, and so tend to prevent the *extension* of the inflammatory process; it will lessen the whole quantity of blood circulating through those portions of the lung which are *still pervious*, and thus relieve dyspnœa; and it will put the system at large into the condition most favourable for the reabsorption of the lymph by which the air-tubes and vesicles of the affected parts have been blocked up.

But a time arrives when bleeding is no longer of use, or when it is positively hurtful: when it ceases to have any good influence on the local disease, and has an injurious influence on the whole system; reducing the patient's strength, and incapacitating him for bringing up and ridding his lungs of the tenacious mucus exhaled by the bronchial membrane. This is what takes place in those cases in which the expectoration is said to be *stopped* by a bleeding. I have mentioned Dr. Gregory's reliance on blood-letting for the cure of pneumonia; and I ought to tell you at the same time what I have been informed respecting the result of his practice. He "used to bleed to the verge of convulsion. His colleague, Dr. Rutherford, seldom went beyond three bleedings, and generally accomplished his object by two, judiciously timed and measured. *His* patients recovered quickly; Dr. Gregory's very slowly."

We want some remedy, therefore, to assist the lancet, or to employ alone when the lancet can do no more; and we have two such in *tartarized antimony*, and in *mercury*. The tartar emetic plan I believe to be the best adapted to the first degree of the inflammation—that of engorgement; and the mercurial plan to the second—to that of hepatization.

I need not tell you that the tartarized antimony is not given in this disorder with the object of producing vomiting. It is a very curious thing that although, when administered in a considerable dose, its first effect is usually sickness, followed perhaps by purging, a repetition of the same dose is, in the majority of cases, at length borne without any further vomiting. The stomach comes to *tolerate* the medicine, as our continental brethren say; and then its beneficial influence upon the disease is no less marked than when nausea and retching take place. Some patients do not vomit at all; others, the majority in fact, vomit two or three times, and then *tolerance* is established. If the sick-

ness and purging go on, they may be checked by adding a few drops of laudanum to each dose. Dr. Thomas Davies, who had tried this remedy largely, and, as he tells us, with great success, gives the following as his own plan of administering it, and perhaps it is as good as any. After free bleeding, he begins with one-third of a grain of tartar emetic in half a wineglassful of water, with a few drops of laudanum or syrup of poppies. Two doses of this strength he gives at the interval of one hour from each other. He then, if the patient does not vomit, omits the opium, but continues it if he does, doubling, however, the quantity of the tartar-emetic, giving two-thirds of a grain for two successive hours; and in this way he goes on, adding a third of a grain every two hours, until he reaches two grains every hour. This last quantity he has not exceeded, and he says that he has continued it for many days without producing any injurious consequences.

Under this plan of treatment the symptoms will often undergo a marked change for the better, in three or four hours. Sometimes, however, the relief is not conspicuous for twenty-four or even for thirty-six hours. He states, and this is accordant with my own experience of the remedy, that the tartar emetic always acts best when it produces no effect except upon the inflammation itself; *i. e.*, when it does not cause vomiting, or purging, or a general depression of the powers of the system. This is an important practical remark, because many persons have supposed that it subdues the disease only when it previously gives rise to these symptoms. I consider this testimony of Dr. Davies to the power of the tartarized antimony in controlling inflammation of the lungs the more valuable, because he informs us, that before he had occasion to see its admirable effects in the first stage of pneumonia, he had been in the habit of trusting to the free use of mercury, after due depletion.

When the dyspnœa has been put an end to by antimony thus exhibited, the medicine may be intermitted; and if the inflammation show any disposition to rekindle, it must be again extinguished by a repetition of the tartar emetic.

When, however, the inflammation has reached the second stage, that of solidification, mercury is more worthy of confidence, in my opinion, than tartarized antimony. And I have little or nothing to add to what I formerly said in respect to the mode in which it ought to be administered. The object of giving it is to make the gums tender; and it is expedient to do this as speedily as may be. Small doses of calomel repeated at short intervals—a grain every hour, or two grains every two hours, or three grains every three hours—combined with so much of laudanum or of opium as may be requisite to prevent it from running off by the bowels—offer the most certain way of accomplishing our object. If the bowels are irritable under the calomel, blue pill, or the hydrargyrum cum cretâ, may be substituted for it with advantange: and if the internal use of mercury is any how contra-indicated, or if it appears slow in occasioning its specific effect, the linimentum hydrargyri may be rubbed in, or the strong mercurial ointment.

Many persons, I am persuaded, are saved by treatment of this kind, pushed to slight ptyalism: the effusion of lymph, tending to spoil the texture of the lung, is arrested; and the lymph already effused begins to be again absorbed: and the ease and comfort of the patient, as well as the alteration for the better of the physical signs, attest the healing qualities of the remedy.

After the inflamed lung has become solid and impermeable, the treatment must be regulated rather by the state of the system at large, than by the actual or presumed condition of the lung: we must look more for guidance to the general symptoms, than to the physical signs. If the pulse continue steady and firm, wait patiently the effect of the mercury. But when sunken features, a pallid face, coldness of the surface or extremities, a tendency to delirium, and (above all) a feeble or irregular pulse, proclaim that the vital powers are giving way, it will be requisite, as in other cases where death is threatened by asthenia, to administer cordial and stimulant medicines: the carbonate of ammonia in a decoction of seneka; wine: and to feed the patient well on milk, or beef-tea.

Among what may be called the routine remedies of pneumonia, we must rank counter-irritation by means of *blisters*. When one is called, in consultation, to see a patient labouring under inflammation of the lungs, we may safely speculate upon the conclusion, that bleeding and blistering, and purging by calomel, have all been duly performed. And

I believe that blisters are often applied to the chest much too early in such cases. In the outset, while there is yet considerable fever present, they add to the irritation and distress the patient; and probably tend to aggravate the existing inflammation. They are also attended with this inconvenience, that they interfere with the exploration of the lungs by the ear: and this is not a slight or fanciful disadvantage; for the information we receive, by the sense of hearing, of the state of the lung—whether the inflammation be making progress, or receding, or stationary—is of great use in directing the remedial management of the case. But of course this is a consideration not to be put in competition with the benefit which may be expected sometimes from a blister. When the fever is no longer high, and the skin no longer burning, but the expectoration is still difficult, the dyspnœa considerable, and a sensation of pain, or tightness, or oppression is experienced in the chest, then a large blister is often productive of very sensible benefit; but it should *be* a large one. The patient should have a waistcoat almost, or at any rate a breast-plate, of blistering-plaster. I have never seen such good effects from placing blisters upon distant parts in this disease, upon the thighs or arms for instance, as would lead me to plague the patient with them in those situations.

Purgatives are of less certain value in pneumonia than in many other inflammatory diseases; and less, especially, than in cerebral inflammation. Still it will always be right to give an active aperient at the outset; and afterwards to take care that the bowels be unloaded at least once every day. A continued drain by purgation would not consist at all with the mercurial plan, which promises to be most useful when the inflammation has already reached the state of hepatization.

This, then, is the outline of the treatment which is most likely to save the life of those who are affected with acute idiopathic pneumonia. Different cases will require different modifications of it; for which, I repeat, no particular rules can be laid down.

All that I have hitherto been saying relates to *acute* pneumonia, occurring in a previously healthy person. But pneumonia, having that character, and so occurring, is a much less common disorder than most persons appear to suppose, or than I formerly thought it to be. I have been surprised to find how few cases of pure idiopathic inflammation of the lungs present themselves among my hospital patients. Five or six in the year are as many as I see there. Intercurrent pneumonia, however—pneumonia engrafted upon some other pre-existing disease—is abundantly frequent; and requires, in general, a much less vigorous, and more wary plan of treatment. Inflammation of the pulmonary substance is apt to supervene insidiously upon various disorders which are of every-day occurrence: upon bronchitis; upon phthisis; upon disease of the heart; and upon fevers, especially the exanthematous fevers. In these cases, while the physical signs are necessarily the same as in the unmixed acute disease, the general symptoms are often but slightly pronounced. During the progress of continued fever of a low type, inflammation may steal upon the lung, and run quickly through all its stages, and spoil the organ irrecoverably, without giving any notice of its presence: unless, indeed, you suspect, and search for it with your ear. The pneumonia is said, in such cases, to be *latent*. It seldom needs, the associated disorder would seldom bear, any active depletion. Much benefit often follows the abstraction of small quantities of blood, but they should be taken from the surface of the chest by the cupping-glass, and not by the lancet from the arm: and it is often good practice thus to aim at reducing the local mischief with one hand, while with the other we support the patient's strength by means of ammonia, wine, and nourishing broths. Blisters are also of service: more so than in the sthenic forms of pure pneumonia; and they may be applied at an earlier period. In conjunction with these remedies I should advise the cautious employment of mercury.

When the convalescene from acute pneumonia is decided, and real, it is shorter than might have been supposed. From the period when the pulmonary inflammation is fairly over, the strength returns with unexpected facility, even when large bleedings have been practised and repeated. But we have to guard, more perhaps in this disease than in most others, against false or merely apparent convalescences. A patient can never be pronounced perfectly secure so long as any trace of crepitation remains in the affected lung, and this may often continue long: nay, it not unfrequently ceases only upon the super-

vention of another more surely fatal though less rapid a disorder—viz., tubercular consumption; of which, however, I must treat as a distinct disease.

In the next lecture I shall speak of pleurisy.

LECTURE LII.

PLEURISY. ITS ANATOMICAL CHARACTERS; FALSE MEMBRANES; LIQUID EFFUSION; EFFECTS OF THESE UPON THE SHAPE AND CONTENTS OF THE CHEST, AND UPON ITS HEALTHY SOUNDS. SYMPTOMS OF PLEURISY.

I PROCEED this afternoon to the subject of *pleurisy;* having in the last lecture concluded what I had to say on that of pneumonia: that is, I pass from inflammation of the *substance* of the lung, to inflammation of its *investing membrane*. The two frequently exist together: but when that is the case, the one predominates greatly over the other. Pleurisy, however, without pneumonia, is much more common than pneumonia without pleurisy. When both are present, and the pneumonia predominates, the term *pleuro-pneumonia* is applied to the compound disease. The whole interest of such a case merges in the pneumonic inflammation. Again, when both are present, and the pleurisy predominates, the compound affection is sometimes called *pneumo-pleuritis*.

The pleura, as you know, is one of the serous membranes. Its inflammation is attended therefore with those *events* which I formerly took some pains to describe as belonging especially to that particular tissue. The inflammation is of the adhesive kind: it is accompanied by pain; by the pouring out of serum, of coagulable lymph, of pus, or of blood. I think it will be best, in this instance also, to lay before you some account of the morbid anatomy of the disease, before I consider its symptoms.

Anatomical characters.—The alterations that take place in the inflamed membrane itself are not very striking or important. Experiments upon living animals, made by introducing some foreign substance, or injecting some slightly irritating liquid, into the cavity of the pleura, have proved that, as in other cases, inflammation is attended with *redness* of the part affected. But it is scarcely ever that we observe this effect *alone* of inflammation, in the pleura of a dead person; unless, indeed, he has died of some other complaint while he happened to have *incipient* pleurisy. The pleura has been said to be thickened by inflammation; but that I apprehend to be a mistake. It often *appears* to be thickened, in consequence of the superposition of a false membrane—a layer, or several layers, of plastic lymph. But actual thickening of the pleura itself seldom or never happens. Neither does the pleura easily soften, or readily ulcerate, under inflammation. It peels off, in some cases, from the lung, or from the ribs, with more facility than in the sound state.

The most remarkable effects of pleurisy result from the effusion of coagulable lymph, or of serous liquid, or of both, into a shut sac, having peculiar anatomical relations. One part of the membrane lines the firm walls of the chest: the other part envelopes the soft and compressible lung. The opposed surfaces of this closed and empty bag being *apposed* also, but freely movable one upon the other, very different, and even contrary, effects may be produced by its inflammation. The pulmonary pleura may be glued to the costal pleura, so as to prevent all lateral movement between them, and to obliterate the pleural cavity: or the two surfaces of the membrane which are naturally in contact, may be forced unnaturally apart by a pouring forth of serum between them: or the opposite surfaces of the pleuræ may be united by coagulable lymph in some places, and separated by effused fluid in others. And great differences will arise in the symptoms, and in the gravity and tendency of the complaint, according as one or another of these different conditions of the contents of the thorax is established.

Let us first consider the effect of the throwing out of coagulable lymph only; or of what comes to the same thing, the effusion of coagulable lymph with a small quantity of serum, which last is soon reabsorbed.

One consequence of this is the formation of *false membranes*. These, indeed, are formed whether there be much or little serum poured out. We continually meet with them, sometimes when we least expect to do so, in the dead body. They vary greatly, in different cases, in respect to their thickness, situation, extent, organization, and effects.

When the lymph is first deposited upon the free surface of the inflamed pleura, it is soft, and of a grayish white colour, like paste somewhat. It soon, however, acquires an increase of consistence, and shows marks of vitality; becomes, in short, organized. Red points begin to appear in it, few in number and widely separated at first; but they presently multiply, and lengthen into reddish streaks, which run along the surface of the effused matter. Soon these red streaks may be perceived to be slender vascular canals; and at length they inosculate with the vessels of the pleura, and the lymph, converted into a false membrane, becomes a constituent part of the living frame.

It is curious, and useful too, to know how rapidly this work of organization may go on.

Andral made experiments upon the pleuræ of rabbits, by injecting acetic acid into them. He sometimes found, at the end of nineteen hours, soft and thin false membranes, traversed by numerous anastomosing red lines. In other rabbits, placed under circumstances which appeared to be exactly similar, no such result had taken place at the end of a much longer period; but the pleura contained only a serous or puriform liquid, mixed with unorganized flakes of lymph. Now similar differences have been remarked in the human subject, under disease. False membranes, already vascular, have been found in the bodies of persons who died of pleurisy after a few days' illness; while in other patients, who had lived for many months after the invasion of the disease, there has been no trace of such vascular membranes. It is clear, therefore, that the organization of the lymph does not depend solely upon the length of time that has elapsed from the period at which it was poured forth. It has much more to do with the previous state and habit of the patient. *Cæteris paribus*, plastic lymph and early adhesion are more to be expected in young, strong, and healthy persons; curdy unorganized lymph, granular deposits, with copious and abiding serous effusion tending to become puriform, in such as are old, feeble, cachectic, and scrofulous.

The extent of these false membranes varies, according to the extent of the inflammation which has produced them. When that has been general, they cover the whole lung, and line the whole costal surface, and spread themselves over the diaphragm and mediastinum of the same side. Supposing that there is no serous liquid effused, or that it is absorbed, the lung then becomes everywhere adherent to the sides of the cavity which contains it. The medium of adhesion, which is soft and tender while it is recent, grows firm, and assumes the characters of cellular tissue, when the union is of old standing.

The thickness of the false membranes is also extremely variable. Sometimes it is not more than that of the pleura itself, and the lymph might then, in the absence of adhesion, be almost overlooked. But in the majority of cases their thickness is much greater than this. Frequently several distinct layers or strata are seen, superposed one upon another, to a considerable depth.

Are there any auscultatory signs of this process of adhesion, when it occurs? Yes. There is a morbid sound, not hitherto mentioned by me, whereby it is sometimes disclosed: the sound, namely, of *friction;* the sound produced by the rubbing together of the dry, or inflamed and roughened surfaces. You doubtless are aware that every time a tolerably deep inspiration takes place, the relation between the ribs and the lung undergoes a change. While the ribs are elevated, the lung descends a little; and consequently any given point of the surface of the lung is no longer in contact with the same point as before of the thoracic parietes. You may convince yourselves of this fact by carefully making a small incision through an intercostal space, in a living animal. Now the pulmonary pleura, when that membrane is inflamed, does not slip and glide over the costal in its usual smooth and noiseless manner; but it makes a creaking, or rubbing sound, which the ear, applied to the corresponding surface of the chest, readily catches. I have many times heard this; yet it is not at all a common sound: indeed I have heard it, in one instance, some time before

I knew what the noise meant. The sound has, mostly, an interrupted character, occurring in a series of three or four jerks. The patient is often made aware of the harsh movement, by some internal sensation: and a bystander, who places his hand flat upon the corresponding surface of the thorax, may sometimes feel the grating of the membrane upon itself. You may wonder, as adhesions are so common, that this sound, and these sensations are not oftener heard, and felt. In truth, they are transitory phenomena, and cease, of necessity, as soon as adhesion prevents any further motion of the opposed pleuræ upon each other. If we do not happen to listen during that period, usually a short one, in which the pleuræ, roughened by inflammation and effused lymph, but not separated by liquid, still chafe against each other, we lose the opportunity of hearing the sound at all. This *rubbing* sound, this noise of *friction*, we shall find to be of greater importance in relation to certain diseases of the heart, than in cases of acute pleurisy. In pleurisy the *liquid* matters poured into the membranous sac have far more interesting consequences: and to these I now beg your attention.

In some instances we find, after death, a clear, serous, or watery fluid, without colour, or of a pale lemon-colour, and perfectly limpid and transparent. This may occur independently of inflammation of the pleura; from some mechanical obstacle to the circulation. It then constitutes a species of dropsy; a true *hydrothorax:* and this, though less common than ascites, is by no means an *un*common consequence of disease of the heart. When the effusion does not proceed from a cause of that kind, it is always, probably, the result of inflammation of the pleura itself, although we may find only a slight degree of redness upon its surface, or a few patches of coagulable lymph. More frequently, besides this clear liquid, with flakes of albuminous matter floating in it, there is also a coating of lymph on the inflamed membrane. Very often the thinner fluid is turbid, or whitish, like whey; sometimes it is distinctly puriform; sometimes it is tinged more or less deeply with blood; sometimes it consists of nothing else but blood, which has separated into serum and crassamentum. There being no wound, nor rupture, of large or of small vessels, we conclude, in such cases, that the blood has been exuded, or been *exhaled*, from the membrane.

The different kinds of fluid effused into the pleuræ are always, or almost always, without smell;—provided that it has remained a closed bag: I mean when no communication has been established between the cavity of the pleura and the external air, either through an opening in the walls of the chest, or through a pulmonary fistula leading to the trachea, or through some breach in the œsophagus. I have met with but one exception, and that a doubtful one, to this rule. A patient died in the hospital, who, some years before, had nearly killed himself by swallowing, in mistake for beer, a solution of caustic potass. The result of this had been ulceration, and subsequently stricture, of the gullet. His left pleura was perfectly full of most stinking pus; and we were unable to detect any channel of communication with the outward air, although the circumstances of the case rendered it not improbable that such a channel might have existed.

Sometimes air, or gas, is found in the cavity of the inflamed pleura; either alone, or (what is much more common) together with a liquid. We ascertain this fact, in the dead body, by the hissing sound that takes place as soon as a penetrating incision is made between the ribs; or by opening the thorax under water, and noticing the escape of air in the form of bubbles. It is probable that these gases are sometimes secreted or exhaled from the diseased membrane; sometimes they are the product of decomposition within the cavity; but, for the most part, they are met with only when the sac of the pleura communicates somehow with the external air.

Such being the fluid matters that frequently occupy the cavity of the pleura when that membrane has undergone inflammation, let us next examine the necessary effects of their being collected in that part. These effects will obviously vary considerably according to the quantity of the fluid that accumulates.

Now the quantity of fluid may vary from less than an ounce to several pints. At first it is lodged in the cavity of the pleura solely at the expense of the yielding lung, which is compressed to make room for it. But if the quantity continues to augment, other parts are at length displaced by the increasing pressure, the boundaries of the chest on that side are stretched, and even the abdominal viscera are thrust out of their natural position. The lung is pushed back towards the mediastinum and vertebral column, and flattened, and

brought to lie in the smallest possible compass; the diaphragm is forced downwards, and sometimes gives rise to a considerable prominence of one or the other hypochondrium, the spleen and stomach being displaced on the left side, or the liver on the right. The ribs are separated too; the intercostal spaces become wider, and are pushed out to the level of the bones, and the whole of the affected side is smooth and obviously larger than the other. The mediastinum also undergoes some change of position, being driven more or less towards the side opposite to that on which the effusion exists. If the liquid happens to fill and distend the left side of the thorax, the heart may be moved out of its natural place, and be heard, and felt, and seen to beat on the right of the sternum. Andral mentions having met with only one instance of that kind. I suppose that I cannot have witnessed less than a dozen such. So again the heart may be carried beyond its proper place, to the left, by a large effusion into the right pleural cavity.

I say when the liquid is accumulated in very considerable quantity, the lung is pressed into the form of a thin cake, which occupies a very small space alongside the vertebral column: and if it happens to be covered over and concealed, as it often is, by a strong layer of adventitious membrane, we might fancy, at first examination, that it had completely disappeared. It was in cases of this kind—especially when the effused fluid consisted of pus—that the lung was erroneously represented by the older observers as having been *destroyed* by suppuration. However, you will always find the lung there if you take the pains to look for it, and to divide the false membranes that bind it down: and, in many instances, it is sound also. Its surface may, indeed, be wrinkled, but the lung itself is capable of being restored to nearly its former volume by insufflation, as it is called; by blowing air into it through the principal bronchus of that side. In this compressed state the lung does not crepitate under the finger; it is dense, and sinks in water; in fact, it is wholly void of air, and has been brought, by the pressure of the fluid around and upon it, into nearly the condition of the lung of the fœtus that has never breathed. But its firmness, its resistance to being torn, and its capability of being again inflated, prevent our confounding it with hepatized lung. Sometimes its cellular texture is obliterated; the opposite surfaces of the vesicles and smaller air-tubes adhere together; the lung will not admit air; it looks like a piece of muscle, and is then said to be *carnified*.

Modifications of the healthy sounds.—Such is a general account of the anatomical characters of pleurisy, as they are disclosed to us by an examination of the body after death. We may now inquire what effect these changes are capable of producing on the *sounds* which are heard when the healthy chest is percussed, or listened at. We shall then be the better prepared to appreciate the several symptoms, general and physical, which are known actually to occur in pleurisy. Now it is clear that when the lung is pushed away from the walls of the thorax by fluid between the pleuræ, it will be compressed also; its capacity must be reduced; less air will be able to enter it. There will consequently be a proportional diminution in the intensity of the respiratory murmur; and this murmur will, moreover, be less audible in consequence of the distance from the ear, of the structure in which it takes place. The lung is attached by its roots (so anatomists speak) to the spinal column. A moderate amount of effusion will, therefore, cause it to recede upwards and inwards; and a certain quantity of the liquid will ascend between the lung and the ribs, compressing the spongy pulmonary tissue around the larger and more resisting bronchial tubes. We might expect, in this condition of things, that the passing breath, and the voice would be audible in those tubes, through the partially condensed lung, and through the circumfused layer of liquid: and it is so. We do hear bronchial respiration, and bronchial voice and cough; with some modification, indeed, to be noticed presently. In this respect, therefore, you will observe that pneumonia, which solidifies the spongy texture of the lung around the bronchial tubes by filling it with lymph or with blood, has the same effect, as far as acoustic principles are concerned, as pleurisy, which solidifies a portion of the lung by expressing air from it, and pours round the bronchial tubes a fluid which readily transmits sound. Hence bronchial respiration and bronchophony are not always indicative of the same condition of parts within the chest, but derive their true value and meaning from the context, if I may so say; from the circumstances under which they occur, and with which they are associated.

When the effusion is so copious as to squeeze all the air out of the spongy part of the

lung, to pack the organ up along the vertebral column, to distend the thorax, and to compress strongly the bronchial tubes themselves, *no* respiratory murmur *can* then be heard, nor any tubular breathing: for the dilated chest can neither expand nor collapse, and, therefore, no air can pass along even the larger air-tubes; neither can these compressed tubes vibrate with the patient's voice; wherefore bronchophony also ceases, or is but faintly audible.

Again, if percussion be made over a portion of the chest, where there is incompressible serous fluid beneath, in the stead of healthy and spongy lung, a dull flat sound will be rendered. But a dull sound is rendered also when percussion is made over a solidified lung. Hence the mere dulness of the part struck does not inform us whether we have pneumonia or pleurisy to deal with, or some other disease that has the effect of making the lung solid, without plugging up the larger bronchi.

But an expedient presents itself, by which we may, in some cases, render this experiment of percussion conclusive. The dull sound occasioned by hepatization or other solidification of the lung occupies the same spot in every position of the patient. Not so, necessarily, the dull sound produced by the presence of liquid in the cavity of the pleura. The liquid will gravitate to the lowest part of that cavity, and will carry with it the dull sound. We place the patient, therefore, in different attitudes; and if we find that the chest, when struck, is always resonant in the higher, and always dull in the lower portions of the thorax, whatever the posture may be, then we may be sure that the cavity of the pleura contains liquid. In such a case, when the patient sits up, the dull sound will be elicited from the lower part of the chest, on one side, from the spine round to the sternum. When he lies on his back, the anterior of the thorax sounds hollow; the posterior dull: and when he reverses that position, and lies with his face downwards, these sounds change places also; the hollow sound is still uppermost, in the posterior part of the cavity; the dull sound still undermost, in its anterior portion.

There are just two states which may interfere with the true interpretation of the sounds produced by percussion in the manner now described; and these are, first, partial adhesions of the pleuræ, which may confine and isolate the effused liquid, and prevent its sinking from one part of the chest to another under the influence of gravity: and secondly, so large an amount of effusion as to fill entirely the cavity and fix the compressed and empty lung in one position; for it is necessary, in order to obtain the shifting sounds in different attitudes of the body, that there should be light spongy lung to ascend, as well as heavier fluid to sink down, according to the posture of the patient. In this last case, that of excessive effusion, the whole surface of the affected side will yield a dull sound. It is seldom so in pneumonia; it is seldom that the entire lung on one side is so blocked up, in consequence of inflammation, as to give rise to universal dulness on percussion. But the diagnosis of these two conditions is an important one, and apt to puzzle a student. I hope to elucidate it as we go on.

Symptoms.—We may now consider, with a better chance of understanding some of them, the symptoms which are generally met with in a case of pleurisy under its ordinary form and progress. The general signs, then, of that complaint are rigors, pain in the chest, dyspnœa, cough, difficulty or impossibility of assuming certain postures, and fever. Very much the same, therefore, (as Cullen truly stated) with those of pneumonia, and, it may be added, with those of pericarditis: but auscultation differentiates these diseases. The physical signs I will examine presently. The general symptoms will bear, each of them, a short comment.

The *pain* which the patient feels—or the *stitch* in the side, as it is expressively called—is one of the most striking and characteristic signs of the disease. *Point de côté* the French name it. It occupies a point or spot; and patients feel as if some short stabbing instrument was driven in at that point every time that the act of inspiration goes beyond a certain limit. The Latin medical writers, attending chiefly to this prominent symptom, call pleurisy "morbus lateris."

This pleuritic stitch is subject to considerable variety in regard to its situation, its severity, and its duration. Most commonly it is felt on a level with or just beneath one or other of the breasts, in the part corresponding to the lateral attachments of the diaphragm: and this, even when the inflammation which occasions it is of much greater extent. Why

is this? What is the cause of the pain? Wherefore should it be restricted to one small spot, when the inflammation perhaps pervades the whole of the pleura? Pathologists have made attempts to explain these matters; but perhaps their explanations are not very much to be trusted to. They say that there is a larger degree of motion at the lower part of the thorax, of the pulmonary over the costal pleura; and that the pain resulting from that friction, when the membranes are inflamed, is therefore felt where the friction is greatest. However, the pain is not always confined to that spot. It is occasionally felt in other places, as in the shoulder; in the hollow of the axilla; beneath the clavicle; along the sternum; and sometimes it is complained of as extending over the whole of one side of the thorax. Andral states that he has observed the pain to prevail especially along the cartilaginous border of the false ribs, when the inflammation has attacked that portion of the pleura which covers the upper surface of the diaphragm. He says, too, that in such cases, the pain often affects the hypochondrium, and even extends as far as the flank, so that it might be mistaken for a symptom of abdominal inflammation. This observation is worth remembering. Sharp pain, occupying the right hypochondrium, belongs oftener to the pleura than to the peritoneum. I have known several instances in which such pain was erroneously supposed to be a sign of hepatitis, when in truth it resulted from inflammation of the pleura. Cruveilhier observes, also, that he has known the pain affect the loins, and simulate lumbago.

Whatever may be the situation of the pleuritic pain, it is generally increased by percussion, by intercostal pressure, by lying on the affected side, by a deep inspiration, by cough, and by different movements of the body.

In many patients the pain is exceedingly sharp, whether it be continued, or whether it occur only at intervals: the more *circumscribed* it is, generally the more *acute* it is. The patients are then in a state of great anxiety: they make very short and imperfect inspirations, through fear of aggravating the pain; they dread the least effort of coughing, or of sneezing, and suppress the desire to cough which the disease may occasion. There are other patients in whom the pain is moderate, is felt only when a *deep* inspiration is made, and is scarcely augmented by pressure or percussion. And there are even some cases of pleurisy which are unattended with pain from first to last.

The pain commonly exists from the very outset of the pleurisy: it is sometimes vague and fugitive at first, and becomes fixed and permanent after a day or two. In that case it may be mistaken for simple rheumatic pain; for pleurodyne; or for what is thought to be merely a nervous pain. When the pain is increased by slight pressure made *upon* the ribs as well as between them; when it extends over a large space; when it is unattended with fever; when it is inconstant or fugitive—we may *suspect* that it is situated in the fibrous and muscular tissue; but these circumstances do not afford any *certainty* that such is the case. In fact I have long been of opinion that some at least of the cases which pass under the name of pleurodyne, are really instances of what has been called *dry* pleurisy. You are aware perhaps that adhesions are very constantly found to exist between the lungs and the ribs in persons dead of pulmonary consumption. Such persons are liable to pains in the chest, beneath the clavicles, in the axillæ, between the shoulders, at the upper part of the dorsal region; in short, in those situations where the cellular adhesions are found after death most constantly and in the greatest number. The pains indicated, it may be presumed, the periods at which the slighter forms of circumscribed pleurisy, attended with no other effusion than that of coagulable lymph, took place. And it is probable that many cases of pleurodyne are really instances of the same kind of pleuritic inflammation. How constantly do we find, even when there are no tubercles in the lungs, firm adhesions between the pulmonary and costal pleuræ, in the bodies of persons who were never known to have had any pectoral disease! The pain alone marks the inflammation in those cases; adhesion presently ensues; there is no fever perhaps, or none that attracts much notice; the pain soon subsides, and is soon forgotten; but the adhesion, the consequence of the inflammation, remains: and this is a morbid condition which is neither revealed to the sense of hearing, nor in any other way. I am much disposed therefore to agree with Cruveilhier in thinking that "*pleurodyne is nothing else* (in many cases at least) *than adhesive pleurisy.*"

I need scarcely repeat the fact which has so many times before been mentioned in these

lectures, viz., that the inflammation of membranous parts, and especially of serous membranes, is attended with much more pain than inflammation of parenchymatous parts. We cannot have a better example of it than is afforded in most cases of pneumonia. Most cases of pneumonia are accompanied in the beginning with a stitch in the side; some cases are not. In those cases in which the stitch happens, the pleura also is inflamed to a certain degree, and the pain depends upon the co-existence of the pleurisy: they are cases of pleuro-pneumonia. In pure pneumonia, on the contrary, the pleurisy being wanting, the sharp pain is wanting also.

The *respiration* in pleurisy, at its outset especially, and while there is still pain, is considerably embarrassed: the movements of inspiration in particular are short, hurried, and often interrupted or jerking. And this depends evidently upon the pain, which forbids the free contraction of the muscles that dilate the thorax; and you may often observe that the dilatation is sensibly less on the affected side than on the other. Cruveilhier indeed denies this; or rather he states that he has never observed it: but it certainly is not an uncommon phenomenon. I have noticed it, and drawn the attention of others to it, again and again.

When effusion has taken place—*that*, one can easily understand, will be likely to aggravate the *dyspnœa;* and it will aggravate it in a greater degree, or in a less, according to circumstances. Thus, if the *other* lung happens to be a diseased lung, then the compression of that which is on the side of the pleurisy will have a more injurious effect upon the breathing. The dyspnœa arising from the effusion and consequent pressure upon the lung, will also be in proportion, first, to the *amount* of the effusion; and, secondly, to the *rapidity* with which it has taken place. When the effusion has been slow—or when it has long existed, and the case has become chronic—the circulation through the lung has had time to accommodate itself to the altered condition of the parts, the disturbed equilibrium between the quantity of air and the quantity of blood in the lung is restored, and the dyspnœa is consequently slight.

But there are very singular exceptions met with to all this. Andral states (and I have seen more than one instance confirmatory of his statement) that there are persons, with pleuritic effusion enough not merely to fill but to dilate that side of the chest on which it exists, (and you will observe that we cannot doubt about the presence of the effusion in such a case,) who appear nevertheless to be quite free from dyspnœa; and *that*, not while they are at rest merely, for they talk, get up, walk about, or even take long journeys, without their respiration becoming so short as to make them complain of it. Now this is conceivable enough in old and chronic cases; but Andral further affirms that this absence of dyspnœa is not restricted to those cases in which the collection of fluid has taken place slowly; but sometimes happens, even in patients in whom pleurisy has led to abundant effusion in a few days. He gives a case of this kind, in which the patient was not prevented by an enormous pleuritic effusion from carrying on, without fatigue, in the streets of Paris, his business as a carter. I remember having a butcher in the Middlesex Hospital in exactly the same predicament; and nothing could persuade him that he was otherwise than well, and fit to go out; and out accordingly he went. Remember, therefore, that there are great varieties in this respect. In some patients the dyspnœa never ceases to be urgent from first to last; and these are apt to prove fatal cases. In others the respiration is very much impeded at first; then the difficulty of breathing diminishes; and at length it ceases long before the fluid is reabsorbed. In others again, by some unaccountable idiosyncrasy, the respiration remains at all times very facile, both at the outset and during the progress of the disease.

Cough is another of the ordinary symptoms of pleurisy. It does not occur in paroxysms. It is small, half-suppressed, ineffectual. In some few cases this symptom also is entirely absent, even though the inflammation is intense, and the effusion into the pleura considerable. When cough does exist it is dry; or it is accompanied by the expectoration of slight catarrh. If much frothy mucus should be expectorated, the pleurisy is complicated with bronchitis: if rust-coloured sputa be brought up, it is complicated with pneumonia: and in each case other signs, proper respectively to those two diseases, will be present.

A good deal has been said and written respecting the position which a patient who is

labouring under pleurisy assumes. The manner of the *decubitus* has ever been regarded as one of the pathognomonic signs of the disease. Yet, strange to say, observers are much at variance with each other in respect to this so-called pathognomonic symptom. Some affirm that the patient lies on the side affected; others that he can lie only on the sound side; others again that he lies neither on the one side nor the other; or even that he lies indifferently in any posture. But this dispute is an exact counterpart of that celebrated quarrel which took place about the colour of the chamæleon: "they all are right, and all are wrong." I believe that, if you narrowly inquire into the facts, they will be found to be somewhat as follows:—In the outset of the disease, while there is yet pain, the patient cannot lie on the affected side on account of the pain, which that position exasperates; he lies therefore on the sound side, or on his back; sometimes he is obliged to sit up. At a more advanced period of the disease, when the pain has ceased, and considerable effusion has taken place, he cannot lie on the sound side, because of dyspnœa: the dilatation of the chest on that side would be impeded by such a posture; and what is more, the effusion, lying uppermost, would press upon the mediastinum, and so further tend to restrain the expansion of the sound lung. But he is no longer prevented by pain from lying on the diseased side, and consequently he does, in some instances, take that position: but more commonly still he lies in what Andral calls a *diagonal* posture; *i. e.*, the patient is not on his back, nor on his side, but between the two; on his back, we may say, but inclining towards the affected side. Again, however the fact may be explained, it is certain that there are some few persons who lie indifferently on the back or on either side without augmentation of the dyspnœa in any of these positions, though one side is choke-full of liquid.

Now of the symptoms that we have hitherto been considering, the pain, the dyspnœa, the cough, the accommodation of position, there is not one which, taken alone, can be said to be strictly or absolutely pathognomonic; or which indicates in a positive and certain manner the existence of pleurisy, or of pleuritic effusion. Yet when all, or several of them, occur together, they afford a degree of probability on those points almost equivalent to certainty. There are yet some other, and more conclusive signs, which either in themselves, or taken in conjunction with those already mentioned, render the diagnosis of pleurisy easy and sure. These signs are furnished by the size of the thorax on the affected side; by its form and motions, and above all, as you will have anticipated, by percussion and auscultation.

I have already stated that in some cases, that *side of the chest* which contains the effused fluid *becomes evidently larger* than the opposite side. The ribs and their cartilages present that position which they assume during a deep inspiration: the intercostal spaces are pushed outwards and brought up to the level of the ribs; and occasionally fluctuation may be perceived in those spaces, through the muscles. When these appearances are observable, no doubt (or *scarcely* a doubt) can remain concerning the nature of the disease. This dilatation of the thorax on the diseased side is more common in old chronic cases than in the earlier periods of acute pleurisy; yet it *may* take place in a very short time. Andral declares that he has known it sometimes reach a great degree by the fourth or fifth day of the acute disease. You may satisfy yourselves that the side *is* dilated by measuring it with a string. Carry a string round the chest, upon a level with the extremity of the xyphoid cartilage, then fold it upon itself, and you will find that the half of it will more than encompass the sound moiety of the chest, and will not reach round the diseased. The diseased side may measure an inch, or an inch and a half, or even sometimes two inches, more than the other. But this measurement by a string is seldom necessary. The eye takes a very accurate estimate of the comparative volume of the two sides; and the obliteration of the intercostal spaces can only be ascertained by seeing or feeling them. It is necessary to remember that, in most persons, the right side is naturally somewhat the larger of the two.

I say when this dilatation is noticed *scarcely* a doubt can exist of the true nature of the case. Some time ago I should have said *no* doubt: but having myself mistaken such a case, and seen others mistake it, I introduce this slight qualification, although it is a thousand to one against another such instance occurring to puzzle or mislead the observer. Some time ago Dr. Hawkins had a patient in the hospital, in whom this dilata-

tion of one side of the chest was exceedingly well marked. It was the left side that was enlarged; the heart was evidently pushed over to the right of the sternum. This is another circumstance strongly corroborating our conclusion in such cases. The intercostal spaces were effaced, and the whole of that side was perfectly dull on percussion. The poor fellow had a very unhealthy aspect;—and he had, some time before, suffered amputation of a leg, for what was understood to have been scrofulous disease of the knee-joint. It was not unnatural therefore that every one who saw him should have come to the conclusion that this was a case of empyema; of fluid, and most likely of pus, collected in the pleura, and very probably the result of the extension of scrofulous disease from the lungs. Under these circumstances, and inasmuch as his dyspnœa was not urgent, it was not thought right to take any steps for evacuating the presumed fluid. The case was pointed out to the pupils as a capital example of empyema. At length the patient died; and when his body was examined we discovered—what think you? not pus, nor serum, but a large red *solid* mass, in the centre of which, when it was divided, was still a red, but softer, pultaceous, half-fluid substance. At first it was thought to be cancerous degeneration of the lung; but it was soon noticed that the solider part was arranged in concentric layers, like those which are often seen in aneurismal tumours; and further research showed that the effusion had indeed once been liquid, for it consisted entirely of blood, which had coagulated in the manner I have just described. And the source of the blood was detected. A portion of two of the ribs had been destroyed by ulceration, and one of the intercostal arteries had thus been laid open. The lung was found uninjured, but totally empty of air, and pressed flat up against the mediastinum.

No precaution could guard against such a source of fallacy; and you are not likely ever to meet with just such another case: yet I have thought it sufficiently interesting to relate, in illustration of the subject immediately before us.

It is unfortunate, as far as the diagnosis is concerned (but not in any other sense), that dilatation of the thorax is far from being a constant symptom, even in cases in which the effusion is very considerable.

There is still a condition of the thorax to be described, which is the very opposite to this. When the effused fluid begins to be reabsorbed—and when some cause or other, generally the formation of adventitious membranes, prevents the lung from re-expanding, and approaching the ribs in proportion as the fluid is removed—then of course the ribs *must* sink in, and approach the lung, to prevent that void which would otherwise exist between the ribs and the lung. Consequently that side of the chest on which the fluid has existed becomes narrower than the sound side. And the actual difference between the two will be augmented by the circumstance that, in such cases, an amplification of the sound lung, and of the cavity in which it is lodged, a true compensatory hypertrophy, commonly takes place.

This partial or general *retraction of one side of the chest* is not so much a sign of disease actually in progress, as of disease gone by; and it may exist without evident disturbance of the health of any kind.

Persons who are thus affected have the appearance of being inclined towards the diseased side, even when they endeavour to hold themselves upright: and the deformity, for such it is, becomes manifest to the eye when the chest is uncovered. You see that the side is narrowed and shrunken. All its dimensions are contracted. It measures less, in circumference, by an inch or more, than the other side. The shoulder is depressed; the hypochondrium is tucked up; and the ribs are drawn close together. A patient of mine, whose chest had been punctured (a remedial procedure to be spoken of presently), and who drew off daily, with a syphon, pus which did not otherwise find vent, had such difficulty at last in introducing the tube between his ribs, that excision of a piece of the bone was contemplated by the eminent surgeon who had performed the operation. The effect of the atmospheric pressure is sometimes so great as to crook the vertebral column, and produce lateral curvature of the spine. This I have myself witnessed. And as one of the unseen walls of the cavity, viz., a part of the diaphragm, is carried permanently up under the ribs, so another of the unseen walls, the mediastinum, is liable to be influenced by the tendency to contraction. The heart, which, when the *left* pleura is *distended*, is apt to be *thrust* over, beyond the sternum on the right, may thus, when the *right* pleura is *con-*

tracted, be *dragged* into the same position. In the former case, the dull sound given out by the diseased side when struck, will transgress the mesial line, and encroach a little upon the healthy side: in the latter, the resonance yielded by the healthy will transgress the mesial line, and encroach a little upon the diseased side.

The difference of the two sides is so striking, that, at first sight, an observer supposes it to be even greater than it is actually found to be by admeasurement. Yet Laennec tells us that he had met with this deformity in persons who were not themselves aware of its existence. But all such persons had suffered some long disease, which appeared to be situated chiefly in the thorax.

The conditions I have just been describing are *physical* conditions; and the signs they furnish are *physical* signs. I have still to speak of the remaining physical signs, which are also *auscultatory* signs. What I have already said upon this subject in the present lecture will, I trust, enable you almost to foresee the kind of information which these signs afford in actual practice.

Auscultatory sounds.—As soon as even a slight amount of effusion commences in the pleura, it is announced by a diminution of the hollow sound which percussion elicits in the healthy state. In proportion as the effusion becomes more considerable, the chest, when struck, gives a sound more and more dull. At first this flat sound is rendered opposite the lowermost, depending part only of the cavity; and this, as I showed you before, forms one ground of distinction between the dulness on percussion in pleurisy, and in pneumonia. However, at length, the effusion augmenting, the dead flat sound may proceed from the whole of the affected side; and this forms another ground of distinction: for it is very seldom that the whole lung becomes so solid in pneumonia as to yield a uniform dead sound over the whole of one side of the chest. Either the dull sound is universal on one side, or it is not. If universal, it is not likely to be the result of solidification by pneumonia; or, I may add, by tubercles: if not universal, the dull sound will (except in some rare cases) shift its place as the patient alters his posture.

I may mention another ground of diagnosis, which may be of great assistance when the case is seen from the beginning. The dulness comes on much more quickly in pleurisy than in pneumonia. It has been noticed within twelve hours from the invasion of the disease. In living animals, a considerable quantity of serous effusion has often been very rapidly produced by injecting some slightly irritant matter into the cavity of the pleura. In pneumonia, the dulness is commonly later in its appearance. The induration of the lung is gradual; and so is the pneumonic dulness on percussion: the effusion of serous fluid is early and rapid: and so also is the coming on of the pleuritic dulness. Moreover, as I have just shown you, pleurisy may displace the mediastinum, and cause the *whole* sternum to give a dull sound. A hepatized lung will render *one-half* only of it dull.

The intensity or completeness too of the dull sound is generally greater in pleurisy than in pneumonia. In two days, or even in twenty-four hours, the whole cavity of the pleura on one side may be filled quite full; and the whole of the corresponding surface of the chest, from its base to its summit, will yield a sound (to use one of Avenbrugger's strong expressions) *tanquam percussi femoris*. It is very uncommon for such total and universal *matité*, as the French call it, to result from inflammation of the lung.

Again, in the outset of the disease, while there is yet little or no effusion, but when the pain is acute, the vesicular breathing is heard more faintly and feebly on the painful side than on the other. On that side also the walls of the chest are less forcibly dilated. But percussion, when the pain will permit of its being practised, gives the same sound on each side. It is clear that the sharpness of the pain causes the patient instinctively to dilate the chest on that side as little as possible; and consequently the quantity of air that penetrates the lung in a given time is diminished, and the respiratory murmur is feeble.

As soon as effusion commences, the vesicular rustle is heard still less plainly on the affected side; and in proportion as the fluid increases, that rustle or murmur becomes more and more faint: and at the same time it becomes more distinct and noisy than natural—*puerile*, in fact—on the sound side. And while the respiratory murmur is disappearing on the diseased side, and the spongy lung is becoming empty of air from the pressure of the augmenting fluid, and the larger bronchi are surrounded by compressed

lung and by incompressible liquid, the bronchial sounds begin to be heard, which I formerly described—the bronchial voice, the bronchial respiration. But the sounds are not exactly the same as those which are heard in pneumonia. They are modified by the nature of the substances through which they pass. The voice, for example, is still bronchial, still the voice of a person talking into a tube: but it has a superadded character; it is trembling, quivering, thrilling, cracked, discordant. I strive in vain to convey to you by these epithets a notion of this remarkable modification of the voice. Laennec's happy similitudes may enable you to form a more exact conception of it. It is like (he says) the bleating of a goat; or, happier still, it resembles the voice of Punch. But when once you have heard the sound, you will never forget it again. I presume that this modification of bronchophony (for such it is, and such I would have you consider it) is caused by the rapid undulations communicated to the effused liquid by the vibrations of the bronchi and condensed pulmonary tissue. This sound is usually most distinctly heard near the inferior angle of the scapula, the patient being in a sitting position. It disappears, or merges into pure and distant bronchophony, when the liquid exceeds a certain amount, so as to compress the bronchial tubes themselves, and to *damp* their vibration.

I would have you recollect, therefore, that ægophony, which is the technical appellation of the sound I have just been describing (goat-voice),—ægophony is nothing more than a species or variety of bronchophony; and the two run each into the other by such fine gradations, that it is sometimes difficult to say which it is we are listening to. When the quivering is strongly marked, we may be certain that it denotes effusion into the pleura; when bronchophony only is heard, we cannot be sure, from that sound alone, whether there be indurated lung between the ear and the bronchi; or a liquid, and a portion of compressed and condensed lung: but other phenomena complete the diagnosis.

Do not forget that when any modification of the voice is heard, or thought to be heard, on the suspected side, the sound of the voice in the corresponding part of the other side of the chest must be ascertained also. It is only by a *comparison* of the two sides that we can come to any safe conclusion; and that *comparison* becomes often a striking and most instructive *contrast*.

LECTURE LIII.

PLEURISY CONTINUED. RECAPITULATION OF SYMPTOMS; OF DIAGNOSTIC SIGNS. CAUSES OF PLEURISY. PNEUMOTHORAX; ITS CONDITIONS, AND SIGNS. TREATMENT OF PLEURISY. EMPYEMA. PARACENTESIS THORACIS.

Recapitulation.—In the last lecture I enumerated the symptoms, general and physical, which are met with, more of them or fewer, in cases of acute pleurisy. I then considered them singly; it may be well to take a rapid recapitulatory view of them as they exist together or in succession, and compose the actual disease.

The outset, then, of pleurisy is marked by sharp stabbing pain, most commonly situated beneath one of the breasts, and preceded or accompanied by rigors. These two signs, the stitch and the shivering, are sufficient of themselves to awaken a strong suspicion that pleuritis has set in. At the same time there are usually a dry cough; a dread of breathing; a check or catch in the inspiration, which is curbed, so to speak, by the pain; fever; often a comparatively feeble respiratory murmur on that side on which the pain is felt; and the patient cannot lie on that side. If no liquid effusion takes place, these symptoms ordinarily disappear at the end of a few days, and the patient recovers. The case has been a case of dry pleurisy; and the chances are much in favour of the lung having become permanently adherent to the ribs.

I should have said, with respect to the *fever*, that at the outset of the pleurisy it is often

high. And it was matter of observation long before the method of auscultation was thought of, as well as since, that in the acute period of the disease the *pulse* is remarkable for its hardness, and forms a contrast with the soft pulse of pneumonia, and with the small and contracted pulse of inflammation of the serous membranes of the abdomen. Indeed the older physicians laid great stress upon the quality of the pulse, in their endeavours to distinguish pleurisy from pneumonia.

Symptoms.—But to resume the description of the symptoms of pleuritis. Where effusion takes place (and it does so very early, so as to form a part of the complaint, just in the same sense in which expectoration forms a part of catarrh), the sound elicited by percussion becomes dull on the side on which the effusion exists. While the effusion is moderate, the dulness shifts its place according to the posture of the patient, and is heard only when the lowermost part of the chest is struck. But the fluid may soon increase so much as completely to fill the pleura; and then the whole of that side is dull. Meanwhile the murmur of respiration becomes feeble and faint, and at length, as the effusion augments, ceases altogether; while on the sound side it grows noisy and puerile. Tubular breathing, and that modification of the bronchial voice which medical men have agreed to call ægophony, become audible during the early periods of the effusion. Ægophony is heard, however, only so long as the quantity of liquid poured out observes a specific limit. There must be a certain amount of effusion—and there must not be more than a certain amount. I have sometimes thought that the peculiar sound depended on the undulations produced in the *surface* of the liquid, by the bronchial vibrations. It is somehow certainly connected with the presence of a stratum of liquid between the lung and the ear. When the lung is strongly compressed, and especially when the cavity is stretched and distended by the enclosed fluid, the side is necessarily motionless; no tubular or other breathing can any longer be heard, or even occur: nor is the voice conducted, except, perhaps, very faintly, to the listening ear of the physician.

When the effusion is great, that side of the thorax on which it has taken place becomes, often, more or less, dilated; and I should add, that the integuments on the same side are frequently œdematous. The patient now cannot lie on the sound side: and the most common posture is that which is intermediate between the supine position and the lateral; he lies *towards*, but not, in general, *on*, the affected side.

I observed, in the last lecture, that this inability, after the effusion has reached a certain point, to lie on the sound side, might be accounted for in two ways. Partly it may be owing to the impediment which lying on the sound side offers to that side's expansion. The muscles which dilate the healthy side have then to lift, as it were, the weight of the body, and are, some of them, pressed upon and encumbered in their action, by that posture. But the inability in question is chiefly attributable to another circumstance, viz., the pressure exercised by the effused fluid downwards, through the mediastinum, upon the only lung that is left to perform the function of breathing. Now disputes, or differences of opinion, have arisen as to which of these two circumstances is the most efficient cause in this matter: and therefore it may not be amiss to provide you with the facts which prove that the last mentioned cause is, in reality, the most operative—I mean the weight of the superincumbent liquid, in the supposed position, upon the mediastinum, and upon the healthy lung below it. This is shown by the fact, that patients, to whom the decubitus on the sound side had previously been impossible, on account of dyspnœa, have been able to rest in that position *immediately after* the artificial evacuation of the fluid. Now in such a case the obstruction to the dilatation of the healthy side, produced by placing it under the weight of the body, would remain the same as before, or nearly so. A hospital patient of mine, named Coggs, could not breathe if he attempted to lie on his right side. His left pleura was distended by liquid effusion. I thought fit to have paracentesis performed: and the poor man was greatly delighted to find himself at once enabled by it to assume the posture which his weariness had long made him wish for, but which he had not been capable of enduring. We found, by percussion, that the diseased side was now filled with *air;* the compressed lung had not risen at all; so that the necessity for the free expansion of the sound side was just as great as before the operation. You may find a precisely similar consequence of the same operation related in the fifth volume of the *Dublin Transactions.*

The œdema that is sometimes observed on the diseased side is more or less connected, probably, with the habitual position of the patient.

There is yet another sign of pleuritic effusion, which, as it is very simple, and readily perceived by even the least instructed observer, is too valuable to be neglected. In most persons, one's open hand, laid flat upon the surface of the chest, feels the vibration or thrill which the voice occasions when the person speaks. Now in a case of pleurisy with effusion, you will generally find a remarkable contrast between the two sides in this respect: *i. e.*, the thrill is strong and evident on the sound side, and not perceptible at all on the other. Whereas, when the whole side is dull in consequence of solidity of the lung, the thrill is much *augmented* on that side. But this thrill is not always present in the healthy state, and we can infer nothing from its absence on the diseased side.

After a while, when the fever has ceased, the liquid begins to be reabsorbed: but, as in many cases, the lung is more or less bound down by adhesions, or overlaid by a membranous stratum of lymph, it cannot expand in proportion as the liquid is removed: and the necessary consequence is, *that* shrinking of the affected side in all its dimensions which I fully described yesterday.

Let me now briefly restate the points of distinction between pleuritic effusion and pneumonic consolidation, when the one or the other of these two morbid conditions is proved to exist by dulness on percussion, extending over the whole of one side of the chest. The question is one which frequently arises; and it is one of much interest and importance.

First, then, we distinguish these different conditions, having some physical signs in common, by their *history*. In pleurisy, sharp pain, and a dry cough, or perhaps no cough, precede the dulness: and we have not the crepitation, nor the rust-coloured sputa, which are antecedent to the dulness of pneumonia.

We cannot, however, always learn the previous history of a given case.

Secondly, a lung rendered solid by inflammation does not *distend* the cavity. Copious pleuritic effusion most frequently does. In the first case, therefore, we have not that separation of the ribs, that obliteration of the intercostal depressions, that protrusion of the corresponding hypochondrium, that mensurable enlargement of the side, that extension of the dull sound beyond the middle of the sternum, or that displacement of the heart, which are, some or all of them, apt to result from a collection of liquid in the pleura.

Thirdly, the solid lung transmits the voice from the pervious bronchi to the surface of the thorax; and if any motion of the affected side remains, it transmits also the sound made by the passage of the air through them. These phenomena are wanting when the pleura is so stretched by its liquid contents as to make the side everywhere dull to percussion.

Fourthly, the vibration of the thoracic parietes, caused by the patient's voice, is augmented by consolidation of the lung; prevented when it is strongly compressed by imprisoned liquid. The increase of this thrill can be felt therefore in the one case; its diminution in the other.

This simple test fails to be applicable when, from the feebleness, or the high pitch, of the person's voice, no thrill is perceptible on either side in the healthy state.

Fifthly, a patient having one lung solid, is generally indifferent as to posture. A patient having one pleura quite full of liquid, lies (usually) on or towards that side; and is distressed and suffers dyspnœa if he attempts to lie on the other.

Causes.—It is of more importance that we should inquire into the *exciting causes* of pleurisy than into those which give rise to pneumonia. They are more numerous and complicated, and have a more direct bearing upon the prognosis and treatment in the one case than in the other. I do not desire to refine too much; and therefore I shall restrict myself to those causes which are obvious, and which you are likely to meet with in practice.

I merely say of exposure to cold, as an exciting cause of pleurisy, that it is a very common—the *most common*—cause. You know already all that I can tell you of the circumstances that are likely to render that cause effectual in producing internal inflammations, and, among the rest, pleuritic inflammation.

But pleurisy is often occasioned by mechanical violence; or by the accidental extension

of disease from other parts; and the course, and the event, of the disease, are liable to be considerably modified by the nature of its cause in such cases.

Pleurisy may be excited by the splintered ends of a fractured rib; and if the pulmonary pleura be wounded in that manner, air may get into the pleural cavity, as well as into the cellular tissue beneath the skin; constituting the true and genuine *emphysema* of our forefathers. Pleurisy may be determined also by a penetrating wound of the thorax; or by a perforating ulcer of the pulmonary pleura, the extension of a tubercular excavation. In the one case air will enter from without, if the aperture be sufficiently large; in the other, air will pass from the lung into the cavity of the pleura. In all of these cases of air finding its way into this serous sac while in a state of inflammation, the event of that inflammation is much more likely to be the effusion of *pus*, than when no communication exists between the inflamed membrane and the atmosphere. This I have mentioned, and offered some explanation of, before. But another very curious consequence results from the admission of the air, and its coexistence with puriform or other liquids in the sac of the pleura. New auscultatory signs arise, very easily appreciated, very instructive, and therefore very necessary for you to be acquainted with.

You must know that when the pleura contains air alone, the patient is said to have *pneumothorax;* and when (what is infinitely more common) the air is there in company with liquid, he is said to have *pneumothorax with effusion*. This is the name given to that condition of the chest by Laennec; and it serves its purpose sufficiently well. I shall take leave to employ the simple term pneumothorax, in speaking of either condition; whether there be liquid also in the pleural cavity or not. Pneumothorax, then, often proceeds from one or other of those causes of pleurisy just mentioned. It is sometimes produced too by the operation of paracentesis; by the opening made into the thorax by the trocar of the surgeon, in order to let out its fluid contents; in plain English, by *tapping* the chest.

The modifications of sound that result are particularly curious. Of course the air occupies the higher portion of the cavity and the liquid the lower, in whatever position the patient may be placed. And this being the case, *percussion* will give a remarkably hollow sound when made upon the uppermost part, and a totally dull and flat sound when made upon the lowermost part: and the change from the hollow to the dull sound will often take place quite abruptly, so that you may trace out the exact level at which the surface of the effused liquid stands. And if you reverse the posture of the patient, the resonant and the dull sounds will interchange their respective places: the uppermost part always yielding the clear, and the undermost the flat sound. This is just what you would expect. The result of the experiment is the same, whether you would make it upon the human thorax, or upon a beer-barrel. The resonant part, you are to observe, will be much *more* resonant than it would be in health—more resonant (you have always the other lung to test it by) than the corresponding portion of the opposite side of the chest—tympanitic, drum-like: for the air is not involved in spongy lung, but contained in a free space; and the sound is not damped, as in a healthy chest it is damped somewhat, by the *presence* of the lung. Moreover no respiratory murmur can be heard where this tympanitic resonance occurs: nor can any thrill be felt. Now I say all this is no more than you must have foreseen. But the sounds detected in this new condition of things by the ear applied to the chest, as the patient *breathes*, or *speaks*, or *coughs*, you would *not*, I think, have anticipated. You hear then a sound which I must endeavour to describe in words, but which you will scarcely form a right conception of till you have heard it, and then all further verbal description will be needless. I can describe it by similitudes only. The patient's breathing is like the noise produced by blowing obliquely into an empty flask; and so the French have given the sound the somewhat magnificent title of "amphoric resonance." I have heard, fifty times over, exactly the sound in question when I have been out shooting in a rough day, and the wind has blown sideways into the gun-barrel. It is a ringing metallic sound. When this is present during the *breathing*, the *voice* also has, even *more strongly* in general, this metallic character; and so has the cough; and each of them is apt to be succeeded by a tinkling echo. The voice, and cough, resemble those of a person who speaks or coughs into a deep well; or with his head bent over an empty copper boiler. The same ringing quality is often heard when one speaks in a large vaulted

room; or beneath the arch of a stone bridge. You may perhaps now have some idea of what these metallic sounds are. They are very singular: and they are perfectly decisive (as far as my experience has gone) of the presence of air in a considerable cavity, within the thorax; which cavity mostly contains liquid also: and of the presence of air and liquid in the cavity of the *pleura* in particular. I do not know that the liquid is essential: I do not believe it is; but commonly there is some liquid, and a good deal of air. Almost always too—but *that* is not indispensable—the cavity communicates with the external air, either through the walls of the chest, or through the bronchi. Neither is it necessary that the cavity should be in the pleura, for it may be in the lung: and when we come to speak of phthisis I shall point out the circumstances which will enable you to determine whether the sounds proceed from a tubercular cavity, or from the sac of the pleura. What you will please to remember is, that in actual practice, in ninety-nine cases out of a hundred, these sounds will be found to denote the presence of both air and liquid in the cavity of the pleura; and the probable existence of some passage of communication between that cavity and the *external* air: in a single word, they will reveal the existence of pneumothorax. The voice reverberates in the little cavern just as it does in a large empty room with a stone roof; and this is the best explanation I can give you of the phenomenon. Sometimes, as you are listening, especially if the patient has recently changed his posture, you will hear a sound just like that occasioned by dropping a pin's head into a glass vase, or into a metal basin; and to this sound the name of *metallic tinkling* has been given. It really often closely resembles the distant tinkle of a sheep-bell. This is supposed to result from the dropping of the liquid from the upper part of the cavity; or sometimes from the bursting of a bubble on the surface of the liquid during respiration. You may succeed now and then in hearing a species of the same metallic tinkle by applying the stethoscope over the stomach, when percussion has already taught you that it is distended with gas, and by getting the patient to swallow some drink in successive tea-spoonfuls.

Another auscultatory sound, arising out of the same condition, viz., the presence of both air and liquid in the cavity of the pleura, and known even as early as the time of Hippocrates, is rendered audible by *succussion* of the patient's body. You lay your ear upon his side, and get him to give his body a sudden jerk or jog; or you get some one else to take him by the shoulders and shake him; and you hear the liquid splashing within: just as you hear it when you shake a cask that is neither full nor empty of water. This is an unequivocal indication of pneumothorax; and demonstrates beyond a doubt that there is both air and liquid in the pleural sac; for no sound would arise if there was liquid only. A moderate quantity of liquid will make a greater squash than a large quantity. Unequivocal I say it is, because one could scarcely be misled by the splashing which may sometimes arise from wind and water mingling in the *stoma h*. I wish that a patient, who was under my observation for some months last year in the Middlesex Hospital, and could produce this splashing noise at will, were there now; for he was not a little proud of his fatal gift, and I should have brought him down here to-day, and given you an opportunity of hearing this sound for yourselves, worth a dozen descriptions of it.

It is surprising how long this state of things within the thorax may last, without any great declension of the patient's general health and strength, even when the disease is (as it mostly is) incurable. Two men, patients of mine, both of whom had well-marked pneumothorax in connection with tubercular phthisis, remained in the hospital for several months; till, in fact, I could conscientiously keep them there no longer: and each of them went away in very tolerable plight. I was unable to trace them afterwards, for they returned to their homes, the one in Ireland, the other in the north of Scotland.

You see, then, that the conditions of pleurisy, and the symptoms of those conditions, may be modified by its causes. All those causes that imply the introduction of air into the cavity of the pleura, imply also a more serious state of disease than results from most other causes. The perforation of the pleura, by the extension of a vomica, I have mentioned as one of those causes. But tubercles in the lungs are frequently, very frequently, the cause of pleurisy, when no such perforation has taken place. A tubercle, or a group of tubercles, approaches the surface of the lung, but does not break through. Generally the pleurisy so produced is slight and partial, and ends in the formation of adhesions: it

is *dry* pleurisy. And this very common occurrence of adhesions between the costal and pulmonary pleuræ, in the course of tubercular disease of the lungs, is, in truth, one reason why *perforation* of the pleura, and pneumothorax from that cause, is comparatively so rare. The part where the perforation is likely to take place has generally, though not always, been secured and clouted, as it were, by previous adhesion. So that even here we find that inflammation has a conservative tendency, and helps to postpone the fatal ending of the specific disease.

Pleurisy may terminate in resolution and complete recovery; or in adhesion, which is its next best termination, and which obtains for the patient, at the expense of some trifling embarrassment of his breathing, complete security for the future against the dangers of pleuritic effusion. Again, acute pleurisy may end in chronic disease of the pleural cavity: *i. e.*, in a shrinking inwards of the walls of the chest, attended with total uselessness, or a very imperfect and limited use, of the corresponding lung. Lastly, pleurisy may terminate in death. It may cause effusion so copious, that the patient will die, unless the fluid be removed by art, of actual suffocation. On the other hand, he may die worn out and exhausted by the disease, especially if it be attended with suppuration. In that case he will suffer hectic fever, and all its wasting and mournful accompaniments; and death ultimately by asthenia. It is seldom that simple idiopathic pleurisy proves fatal.

As the matter from a tubercular cavity may break *in* upon the pleural sac, and lead to the admission of air, and the establishment of pneumothorax; so the puriform fluid which has resulted from inflammation of the pleura, and was for some time imprisoned in its sac, may also break *out*, and the result will still be the admission of air, and pneumothorax. This is not a very frequent result of pleurisy, however. When it occurs, an abscess forms externally, generally in front of the chest; and either the abscess bursts, or it is opened by the surgeon, and then it is found to communicate with the cavity of the pleura.

Sometimes air is effused into the sac of the pleura, in consequence of the rupture of dilated air-cells on the surface of the lung: sometimes gas is generated there from the decomposition of effused liquids; and in such cases the gas has a strong odour, like that of sulphuretted hydrogen: sometimes, again, gas appears to be secreted from the membrane itself. All these events are, however, uncommon. When air, from whatever source, is shut up in the cavity of the pleura, and goes on accumulating there, it will compress the lung, just as certainly and effectually as if there was a liquid extravasated. And such compression, if suddenly brought about, may cause speedy death by apnœa: and this is more apt to occur from a puncture of the pulmonary pleura by the extremity of a fractured rib, than from any other cause.

Treatment.—As to the *treatment* of pleurisy, you will have anticipated that in the outset of the disease we must have recourse to the lancet. I have stated, more than once, that blood-letting *tells* more, and is better borne, in inflammation of serous membranes, than in any other case. If you see the patient while the stitch in the side, and the restrained and cautious respiration are present, you will bleed him, in the upright posture, from a large orifice, until the pain is relieved, and he can draw a full breath again with ease and satisfaction; or until he is about to faint. And if the pain and catch in the breathing should return, and the pulse continue firm and hard, you will bleed again in the same way; or cover the painful side with leeches; or abstract blood by the cupping-glass and scarificator. It is best to bleed fearlessly at first; and in proportion as you do so, the chance will be diminished of a repetition of the blood-letting being needed. The blood in pleuritis, is always deeply buffed and cupped.

Tartar emetic, which is so useful when the mucous membrane of the air-passages is inflamed, is *not* adapted to inflammation of the pleura. On the other hand, mercury, from its well-known power to check the effusion of coagulable lymph, is *especially* indicated. Of course it is to be given with a view to its specific effect on the system: *i. e.*, in equal doses, repeated at frequent and equal intervals, and guarded by a small quantity of opium. And in very severe cases, or when the internal employment of mercury is in any way contraindicated, recourse must be had to inunction of the linimentum hydrargyri, or of the strong mercurial ointment.

By the early and vigorous adoption of these measures, the inflammation may generally

be subdued in no long time. If, though the fever diminish, there still be pain in any part of the chest, leeches may be again applied, or the part may be covered with a blister. I do not think a blister does any good,—on the contrary, it is likely, by the additional irritation it causes, to do harm—while the inflammation is yet recent and active.

But though pain may have ceased, and no fever remains, and the patient is not conscious of much dyspnœa, there may be, and there often *will* be evidence, not to be mistaken, of effusion into the cavity of the pleura. Dulness, I mean, on percussion, bronchial respiration, ægophony; and the object of our treatment is now to get rid of the fluid. We seek to do so by keeping the patient on low diet. The more (says Broussais, with some quaintness), the more the patient eats, the sooner he will die. We pursue the same object by keeping his gums tender with mercury; by applying blisters one after another to the affected side; and by purgatives, and diuretics. By keeping the vessels empty we facilitate, as much as in us lies, the absorption of the liquid contents of the pleura. A very good form of diuretic for this state of matters is a combination of squills, digitalis, and mercury. Half a grain of digitalis, one grain of squills, and three or five grains of blue pill, repeated and continued according to the state of the mouth.

Under this kind of treatment the effused fluid will often be completely removed; and the chest restored to its former state. I last week dismissed a lad from the hospital in whom all this was accomplished.

But in other cases, though the fever and the inflammation are at an end, and absorption of the liquid takes place, the parts within the thorax do *not* revert to their original condition. This we know by that shrinking of its dimensions on the side affected, which was described in the last lecture. This shrinking and narrowing is the *necessary consequence* of the absorption of the liquid, *unless* the compressed lung dilates again in proportion as the fluid is taken up. In most cases of this kind the lung *cannot* rise; being bound down by thick and firm false membranes: and then the deformity is irremediable, and lasts for life. If the lung is completely emptied of air, and enveloped by strong bands of lymph, so that it is permanently unable to admit air again—in that case, as the bony framework of the thorax can yield to a certain extent only, there will always remain, I presume, some liquid in the pleural cavity. If, again, the lung recovers a part of its lost volume, and *meets* the contracting parietes of the chest, adhesion may take place; and the cavity of the pleura be obliterated by thick layers of false membrane. And other changes are apt to arise in the lymph which is adherent to the pleura in these cases of imperfect repair. Sometimes tubercles form in it. Sometimes ossific matter is deposited. I show you a fine specimen of this kind of ossification of the pleura. There is yet another supposable case: the investing adventitious membrane may be thin and weak, and yielding; and though the lung may not expand to its full dimensions at first, it may gradually force its way against the binding power of the coagulable lymph, and then the external configuration of the chest may be restored, and the symmetry between the two sides return. That this sometimes takes place I cannot doubt: but I have only once met with a case in which the dwindling of the side was *entirely* recovered from. In May 1834, I was asked to see a child four years old, who had had cough, and had wasted to mere skin and bone, after scarlet fever. I found the whole of the right side of the chest perfectly dull on percussion, and no respiration could be heard on that side. He was taken by his parents into the country, and I did not see him again for some weeks. He then had ceased to cough, and, in a great measure, had regained his strength; but he presented, on the side which had been dull, the most marked and complete example I ever saw of the sinking in of the ribs, flattening and contraction of the chest, and depression of the shoulder, which denote bygone pleurisy, and diminished size of the lung. About a year from the occurrence of the original disease his father brought him to my house, that I might see the change which had again taken place. The boy was plump and rosy, and in perfect health; the right side of the chest was as full and round as the other; the symmetry of the two sides was completely restored; the breathing natural and perfect; and the sound on percussion hollow. His father, to whom the former shrunk state of the side had been pointed out, told me that he had watched, with deep interest, the process of recovery, and that it had been very *gradual*. Whether after once having sunk in, the ribs ever quite return to their natural position in the *adult* subject, I do not know. I have never seen that happen.

There are yet other cases in which the effusion continues and increases, and the side, instead of shrinking, enlarges; the functions of the lung on that side are entirely abolished; nay, the use of the remaining lung is greatly interfered with, by the pushing over of the mediastinum: and the patient is in imminent danger of suffocation. In such cases, whether the effusion has taken place rapidly or slowly—whether the disease has been acute or chronic pleurisy—we must relieve the oppressed lung by *letting the fluid out*—by tapping the thorax; and the sooner that is done, when such a state of things exists, the better.

The operation is not difficult, nor formidable; but a mistake in the diagnosis may be *very* formidable. I have heard of two instances, one in Scotland, and one in this town, in which the operation of paracentesis thoracis was determined on, to relieve the oppression caused by empyema: but the opening was made on the wrong side; and the patient in three minutes was, in each case, a corpse. There was effusion, which had already put a stop to the play of one lung; and upon air being admitted to the surface of the other, it collapsed also, and immediate suffocation took place. I do not mention these mishaps to deter you from performing the operation. They both took place some years ago. Such a mistake would be unpardonable now. But I mention them to show the necessity of our being sure of our ground before we proceed to open the thorax of a living person. A surgeon told me very recently that with the sanction, and at the suggestion, of a physician, who understands auscultation exceedingly well I believe, he passed a trocar into the chest of a patient; but no fluid followed, to the no small mortification of the physician. This proved to be a case of malignant disease of the lung; and fluid was let out afterwards by puncturing the thorax in another place, and much relief afforded; although of course the disease proved ultimately fatal. The surgeon informed me that he had suspected the true nature of the case. from observing a livid protrusion in front; which was, in fact, the specific disease making its way through.

You will take care, then, to survey the chest narrowly before you plunge a trocar into it. If you see by your eye, and ascertain by measurement, that one side is larger than the other; if the intercostal depressions be effaced on that side; if the whole surface affords a dull sound when percussed; if the side does not move at all, or scarcely moves during respiration; if no vibration can be felt on that side when the patient speaks; if no breathing can be heard in the corresponding lung; if the heart be found beating in an unnatural place, down towards the left hypochondrium, or in the other direction on the right of the sternum: and if, at the same time, the other side of the chest moves freely, sounds resonantly, communicates a thrill to the hand while the patient converses, and is full of *puerile* respiration; then you may be sure that the larger side is distended with fluid.

But it does not follow that you should, therefore, open that side. The propriety of doing so will depend upon circumstances.

In my judgment, that operation ought never to be performed unless the life of the patient is, or seems to be in jeopardy, from the continued presence of the liquid within the thorax.

Now life is plainly in jeopardy when the vital functions of the lungs, or of the heart, are greatly hindered; when symptoms present themselves of approaching death by apnœa, or by syncope. If we discover no cause for those symptoms, except the increasing pressure of liquid pent up in the pleura, we are warranted in ascribing them to such pressure, and bound to act upon that persuasion. Whenever, with the physical signs of abundant effusion, we have great labour and distress of breathing; an anxious and livid aspect; a tendency to delirium—or extreme faintness, and a vanishing pulse—there is no time to be lost: it is our duty to propose and to urge the mechanical removal of the pressure which must else be fatal.

Again, when the patient, without suffering much dyspnœa while he lies quiet, is yet evidently losing ground from day to day, and early death by asthenia appears to be inevitable, without the operation: and when all other means for getting rid of the imprisoned liquid have failed; and when no other condition of disease, or of advanced age, exists to account for the progressive sinking; then also, in my opinion, the patient should not be denied the *chance* which the operation may afford.

Thirdly, whenever (no matter how we ascertain the fact) the effused liquid consists of *pus*, it should be let out.

In either of these three predicaments, and in no other, should we be justified (as I think) in making an opening into the living thorax.

But I wish to be understood as giving you simply the impression which my own experience has made upon my own mind. I know that some practitioners recommend the early employment of the trocar: while (they say) the false membranes, which are apt to prevent the compressed lung from expanding again, are yet tender and unorganized. But surely we should risk much, and gain nothing, by admitting air into the pleura while the inflammation is still in progress. Most cases of mere pleurisy with effusion do well. The mortality from uncomplicated pleurisy is exceedingly small. It would, I fear, be vastly augmented if every patient having manifest effusion were to be tapped. The danger of the operation is this;—that it may, and probably will, induce suppuration, or cause the effused liquid to become putrid. Generally the effusion consists of serous fluid, which is at length spontaneously reabsorbed; the lung expands again, or the walls of the chest shrink inwards: and the ultimate state of such a patient is as good as it probably would have been after a successful tapping.

To make assurance doubly sure, it is always right, before proceeding to the operation of paracentesis, to adopt the expedient first suggested, and used, I believe, by Dr. Thomas Davies, of trying the chest by means of a grooved needle; making a tentative exploration of the nature of its contents in that manner. The passage of this little instrument—like the dismissal of a pilot balloon—affords information which is useful in guiding the particulars of the subsequent process. It not only ascertains that there really is liquid within the pleura, but it discovers the kind and quality, and exact place, of the liquid. If it be serous, it will flow readily along the groove, and tricle down the patient's side. If it be puriform and thick, it will not exude so freely, but a drop or two will probably be visible at the external orifice; and when the needle is withdrawn, its groove will be found to contain pus. In the former case it is possible that there may be no false membranes; in the latter they are likely to be thick. You would use a larger trocar to evacuate the thicker fluid.

The puncture thus made is quite harmless; and inflicts very trifling pain. Dr. Davies gives this useful piece of advice in respect to the trocar, that its point should be *sharp:* for otherwise, after the serous membrane has been penetrated, if there happen to be thick tough layers of coagulable lymph, not very closely attached to the costal pleura, they may be driven before the instrument, and so the liquid will not be reached, but the operator will be perplexed and baffled

Connected with the operation itself there are some questions concerning which medical opinions and medical practice are not yet settled. I do not pretend to decide these questions: yet I cannot pass them by. I must point them out to you; and I shall, at the same time, state what my own observation has suggested in regard to them.

1. Should all the liquid be let out at once?

Some say yes: some say no. If we appeal to experience on this point, we obtain no satisfactory answer. I have known patients get rapidly and perfectly well, after as complete an evacuation of the liquid as was possible. On the other hand, I have heard of speedy recovery when, by a sort of accident, very little had been withdrawn: enough to relieve the pressing distress; but much less than the operator intended.

We must try the matter, therefore, by our reason.

I think it very probable that when the serous membrane is *stretched* by the pressure of its contents, its natural absorbing power may be lessened. But we have no reason to suppose that the mere relief of this tension will *often* suffice to renew the process of absorption, and to enable the flattened lung to re-expand.

The theoretic objection to the thorough emptying of the thorax in such cases is (I conceive), that the introduction of air is likely to be hurtful, by converting the adhesive into the suppurative form of inflammation, and by promoting decomposition of the extravasated fluids. No doubt there is this risk; but, in general, it cannot be avoided. Unless the lung freely rises at once, the liquid *cannot* all, nor even much of it, come out, without air getting in. Some attempts have indeed been made, of late, to draw the water

into an exhausted bag, by the help of a pipe and stop-cocks. But it is obvious that, in most cases, very little can be so abstracted. The mere admission of air to the pleura does not necessarily *create* inflammation of the membrane. This we know from what happens sometimes in emphysema produced by a fractured rib. In the only instance of *pure* pneumothorax which I ever saw, the sac of the pleura had become half filled with air, through a very minute opening in the pulmonary membrane, communicating with the air-passages. There was *no* inflammation of the pleura in that case. Except that it was preternaturally *dry*, it seemed perfectly healthy. Neither does the access of air necessarily superinduce suppuration in the membrane already inflamed. Certainly, if pus follows the passage of the instrument, as much should be removed as we can get. And, for my own part, I should take away as much as would come, if the enclosed liquid proved to be serous.

2dly. Is the orifice to be healed up, or to be kept open?

Here, also, practical men differ. I should say, if pus comes out, by all means keep the aperture open; and inasmuch as detention of the pus would be injurious, and the depending point is difficult to hit, and the orifice is apt to clog, I would do more than leave it open: I would draw the puriform fluid off twice a day by a syphon.

If serum is let out, by all means close and heal the wound. Then, if all goes on well, our object is achieved. But should the condition of the patient fail to improve; should hectic fever, after a day or two, set in or even continue; should much constitutional distress or disturbance arise;—under such circumstances I would reopen the wound. There *was* mere serum, or liquor sanguinis: there now *is*, in all probability, puriform matter pent up in the pleura; and even stinking and poisonous gases.

On six occasions I have myself witnessed the evacuation, by puncture, from the human pleura, of a clear transparent liquid. Some of the patients were under my own charge, some under the charge of others. Of these six patients one died the day after the operation; I can scarcely say why. She was an extremely timid and susceptible young woman; and I am inclined to attribute her death to the shock produced, by apprehension of the operation, upon her sensitive nervous system. Two others recovered forthwith, and perfectly. The wound presently healed in the three remaining cases also; but in one of the three it soon broke out again, and a quantity of healthy pus was discharged daily. After some time, the expedient of keeping the cavity free from accumulated pus by the use of a syphon was resorted to. Under this plan the discharge became gradually less and less, and at the end of many months it finally ceased. The patient has a contracted chest, but his general health is quite re-established. He was on the brink of suffocation when the operation was performed. I have been told of a man who, for the last fifteen years, has had a similar thoracic fistula, and who has nevertheless, during nearly the whole of that period, been actively engaged in the various labours of a farm servant.

I have still two of the six patients to account for. They were both much relieved by the operation for a while; but after a few days they again fell off; and after many more days of gradual sinking and distress, they died. The cavity of the pleura contained, in both cases, much puriform liquid, and a quantity of most offensive gas, consisting in great part, as I judged from its odour, of sulphuretted hydrogen. I have since thought that both these patients would have had a much better chance for life, if this corrupt and corrupting mass had been duly removed.

Again, I have twice seen *pus* let out, by the *primary* puncture of the chest. One of these two patients sunk, exhausted, some months after the opening, which never healed, was made. The empyema of the other had been occasioned by fracture of a rib. The discharge continued for a short time, then ceased, the orifice closed, and the lad got well.

This constitutes the amount, or nearly so, of my personal experience of the operation of paracentesis thoracis. You will see, in the statement I have been making, the grounds of those opinions which I have formed and expressed respecting it. A full and final solution of the grave and difficult questions that it involves, would require a much wider field of observation than any one individual is likely to command. Dr. Thomas Davies has published a tabular account of the several cases of operation which he had then superintended. In sixteen cases of empyema, so treated, there were twelve recoveries; that is, the operation was successful in three-fourths of the whole number of cases: a very encou-

raging result. In three of the less fortunate cases, the lung could not expand after the evacuation of the fluid, in consequence of the thickness of the false membranes covering it.

The value of Dr. Davies' table would have been greater, if it had shown in each case the time, after the commencement of the disease, at which the operation was performed; the symptoms that called for its performance, the nature of the liquid evacuated; and whether the orifice made by the trocar was closed or not.

The quantity of liquid which the distended pleura is capable of holding is enormous. I have seen upwards of a gallon let out at once. Dr. Townsend mentions the case of a patient of Dr. Croker's, in Dublin, from whose left pleura Mr. Crampton drew off the almost incredible quantity of fourteen imperial pints of pus. Of course this could not have accumulated there without making injurious pressure in all directions: upon the ribs, upon the heart and mediastinum, upon the diaphragm, and the abdominal viscera beneath it. It is interesting to know with what rapidity the capacity of the diseased side of the thorax will, in favourable cases, diminish. The same writer gives the history of a boy, 12 years old, in whom the circumference of the diseased side was sixteen inches and six lines, while that of the sound side was fourteen inches and one line. Nine days after the operation the circumference of the diseased side had decreased nearly three inches: it measured thirteen inches and nine lines; that is, rather less than the circumference of the healthy side. The side had shrunk somewhat within its natural size. This is common in such cases.

There is yet a third question of some importance. Whereabouts should the opening be made?

If any soft inelastic tumour has appeared, marking a tendency in the effused liquid to make its own way outwards, that tumour should be punctured without loss of time; for there will then be *no* chance of the reabsorption of the pus; and if the swelling be left to itself, troublesome, burrowing sinuses will be apt to form in the thoracic and abdominal parietes. As we have no choice in such a case about the place where the aperture is to be made, authors have termed the operation *the operation of necessity;* and they distinguish the case in which the surgeon is at liberty to introduce his trocar wherever he pleases; they say that then the *operation of election* takes place. Now the question is, what spot is the best for this operation of election?

If there be any part of the surface which is resonant on percussion, or which affords any sound of respiration, that part must be avoided. It is probable that the lung, in that place, is fastened by adhesions to the costal pleura. Of course you would not thrust in a trocar where you saw or felt that the heart was beating.

The object to be kept in view is that of making the opening in the situation which will allow the freest and most perfect vent for the liquid. The intercostal space between the sixth and seventh true ribs, where the digitations of the serratus major meet those of the obliquus externus muscle, is the place usually recommended. Laennec prefers the space between the fifth and sixth ribs. He observes that, on the right side, an enlarged liver frequently reaches as high as the sixth, or even as the fifth rib. When the diaphragm is pushed as high as this (and I believe that Dr. Edwin Harrison, who has paid much attention to this point, will tell you that it is often pushed up even higher) there is an obvious risk of penetrating it with the trocar. In fact, Laennec committed that error himself. After making an incision between the fifth and sixth ribs, he thrust the instrument, as he supposed, into the thorax; and was a good deal surprised to find that no gush of liquid followed its introduction. The patient died; and dissection showed that the trocar had entered the cavity of the abdomen after transfixing the diaphragm, which, having been forced upwards by a large liver, had contracted firm adhesions to the seventh rib. I have myself witnessed a similar mischance, on the other side of the chest. The integuments of the side were œdematous; and it was thought that a little serum issued upon the passage of the grooved needle. The serum must have come from the infiltrated cellular tissue. No liquid was evacuated by the trocar. The patient died a day or two afterwards of peritonitis. The instrument had perforated the diaphragm, and entered the spleen, which was unusually large.

I am tempted to relate the particulars of one of the prosperous cases that I before briefly

adverted to. It occurred in a lad of nineteen; a patient of my colleague, Dr. Wilson. On his admission into the hospital he bore all the marks of copious effusion into the left pleura; the side enlarged and motionless, and dull on percussion; the intercostal spaces tense and even with the ribs: the heart beating to the right of the sternum; respiration puerile on the right side, inaudible on the left; urgent dyspnœa; a tendency to coma, marked by drowsiness and blueness of the cheeks and lips. In short, the boy was on the very verge of suffocation. He had been ill about a month; and had been bled, and cupped, and brought under the specific influence of mercury. Dr. Wilson judiciously directed that the liquid should be let out.

A grooved needle was first passed between the fifth and sixth ribs; and some serum following the puncture, a trocar was then introduced by Mr. Tuson, and nine pints of a clear fluid were drawn off. During the operation the patient became faintish at times, and then the orifice of the canula was stopped for a moment by the finger. The immediate effect of the tapping was most interesting and gratifying. Even while the liquid was flowing, the heart was observed gradually to move over from beneath the right mamma towards its natural situation; and his difficulty of breathing was signally relieved. At the beginning of the operation he respired fifty times in a minute; at its conclusion thirty-eight times only. A good deal of air entered while the liquid was escaping: and for some days after the operation a splashing sound was audible on succussion of the chest; and one part of that side was unnaturally resonant, when struck, and another part unnaturally dull; and whatever was the posture of the patient, the hollow sound was uppermost, and the dull sound was undermost; and when he sat up and spoke, or coughed, a brazen resonance was heard by the ear applied to the scapular region. This lad had got quite well, without the recurrence of a single bad symptom. He afterwards presented himself at the hospital; and I understand that the left side was found to be in a very slight degree smaller than the right.

The liquid evacuated in this case was clear and transparent. It separated, on cooling, into three parts; one of quite watery consistence, one more viscid, and a third which constituted a soft, transparent, jelly-like mass of fibrin.

In this instance no injurious consequences resulted from the free admission of air.

It may sometimes be necessary to puncture the cavity for *mere* pneumothorax: when, for instance, the pulmonary pleura has been pricked by a fractured rib, and air passes from the lung into the pleural sac faster than it can be absorbed; fast enough to compress the lung, and to threaten death by apnœa. The diagnosis of such a state cannot be difficult. The existence of the fracture, the tympanitic sound yielded by the chest on the injured side, the absence of respiratory murmur in the tympanitic part, and the increasing dyspnœa, all point to the same conclusion. Now a trocar of the smallest size—or even an acupuncture needle—may suffice to give vent to the imprisoned *air*, which will escape with an audible hissing noise. In some cases it must have existed in very large quantity, for the stream of issuing air has been strong enough to blow out a candle several times in succession; the flame being each time immediately re-lighted.

The same necessity for puncturing the cavity of the pleura from without may arise in cases of pneumothorax depending on specific disease in the lungs; but we cannot regard the operation as *curative* in such cases. Its value is very different from that which experience has shown to belong to it in empyema from acute or chronic pleurisy. Yet if it saves life for the time, if it prevents impending suffocation, and relieves existing distress, and postpones the fatal event, it is not *without* its value: and it has many times been done, and been followed by very gratifying results; but it has never, that I know of, been followed by entire recovery. Dr. Davies had superintended the operation in nine instances of pneumothorax with effusion: and *all* the patients died from tubercular complications.

There are, indeed, on record examples of recovery after the operation, when pneumothorax had existed, and under very unpromising circumstances. I should have stated before, that as the pus, in empyema, sometimes finds its way outwardly, penetrating between the ribs, and forming an external swelling, which, if not opened by the scalpel, will at length burst; so it also, sometimes, escapes by making a road into some part of the air-passages, and being expectorated. Now the operation of paracentesis, in such a case, *there being no tubercular disease*, has been successful. Le Dran relates an instance in

which he operated for empyema, where "the injection of a small quantity of mel rosarum and barley-water through the wound excited coughing, and part of it was coughed up through the mouth, mixed with pus;" thus clearly proving the existence of a fistulous passage through the lung; notwithstanding which the patient recovered completely. The effusion was probably circumscribed. But you will find other cases of a similar kind referred to by Dr. Townsend, in the *Cyclopædia of Practical Medicine.*

This concludes what I have to say, not only of pleurisy, but also of pneumothorax, and of empyema, which are often treated of as separate and independent disorders. They are more frequently connected with pleurisy than with any other form of disease, and they are almost always *consequences* of disease or of injury. But I believe I have omitted nothing of importance in respect to either of them.

LECTURE LIV.

PULMONARY HÆMORRHAGE: ITS VARIETIES; ITS CONNECTION WITH PULMONARY CONSUMPTION, AND WITH DISEASE OF THE HEART. PULMONARY APOPLEXY. PROGNOSIS IN HÆMOPTYSIS. SYMPTOMS. TREATMENT.

HAVING gone over the *inflammatory* affections of the organ of respiration; having brought before you inflammation of the membrane which *lines the air-passages*, or *bronchitis;* inflammation of the membrane which *invests* the lungs, or *pleurisy;* and inflammation of the *whole substance* of those organs, or *pneumonia;* I proceed next to the subject of *pulmonary hæmorrhage.*

Varieties.—You may remember that, in an early part of the course, I drew your attention to some general facts respecting internal hæmorrhages. I showed you that the blood does sometimes proceed from ruptured vessels, but that it is much oftener poured forth from unbroken surfaces, in the way of exhalation; and that hæmorrhage of this kind takes place from the *mucous membranes* far more frequently than from any other natural surface of the body. I observed also that such hæmorrhage is almost always preceded by congestion; either by active congestion, which is less common, or by passive and mechanical, which is extremely common; and we speak, accordingly, of active and passive hæmorrhage. Hæmorrhage is also sometimes primary, or idiopathic, and then constitutes the whole disease; while at other times it is merely a symptom, direct or indirect, of some other disorder, in which case we will call it secondary.

Now in the lungs we find examples of all these varieties of internal bleeding; but pulmonary hæmorrhage is secondary much more often than it is primary.

In speaking, therefore, of some forms of pulmonary hæmorrhage, I must touch upon certain diseases of which the bleeding is a symptom: but I shall not go further into the consideration of those diseases at present, than may be necessary to elucidate the hæmorrhage. Bleeding from the lungs is a thing of most fearful interest; and it will be useful to take a general view of that phenomenon, whether it be a substantial disease in itself, or merely a sign of other pre-existing diseases.

The blood, then, in pulmonary, as in all other hæmorrhages, may issue through a breach in the walls of some considerable blood-vessel; or it may proceed from innumerable points in the mucous membrane of the lungs, by the process of exhalation: and the latter mode of hæmorrhage is much the more common of the two, although it is the popular belief that the "breaking of a blood-vessel in the lungs" is of very frequent occurrence.

The particular vessels injured in the first class of cases, and the nature and origin of the breach made in their sides, are matters of infinite variety. Sometimes the blood is extravasated through apertures, the results of a disorganizing process which has commenced in the coats of the vessels themselves; as when, for example, aneurisms of the thoracic aorta, or of its primary divisions, burst, and pour their contents into the air-tubes. Having

pointed out this accidental and hopeless form of pulmonary hæmorrhage, I shall postpone any further account of the disease that gives rise to it, to a future lecture.

Sometimes, again, a large blood-vessel is laid open by the encroachment and extension of disease from contiguous structures. Here is represented (Carswell, fasc. vi. plate iii. fig. 5) the perforation of a large branch of the pulmonary artery, and of a neighbouring bronchial tube, by the extension of tubercular ulceration. The blood escaped so abundantly in this case, that the patient was dead in less than a quarter of an hour. And here I show you a preserved specimen of a similar opening made in the pulmonary *vein*.

It will be necessary that I should anticipate somewhat; and in order to include in one view all that relates to pulmonary hæmorrhage, that I should speak cursorily of its connection with tubercular phthisis. No one here can be ignorant that in that terrible disease portions of the lung are liable to be hollowed out by the softening and expulsion of tubercular matter, into what are called *vomicæ*. Now seeing that hæmoptysis occurs very frequently in persons labouring under consumption, and that the expectoration of blood is often copious, and takes place when it is evident that there are tubercular excavations in the lung, it would be very natural for you to suppose that the bleeding in such cases proceeded from large vessels which had been laid open during the softening of the tubercles, or by the subsequent extension of the ulcerating cavities. But in point of fact, this is very *rarely* the case. I shall explain to you hereafter how it happens that this hæmorrhage from the larger vessels is *generally* prevented; still it does *sometimes* happen.

But in a far greater number of instances the blood in hæmoptysis is *exhaled* from the mucous membrane that lines the air-passages. For when this surface is examined in the dead body, and immediately after the occurrence of pulmonary hæmorrhage, it is very often found to be perfectly entire, from the commencement of the trachea to the remotest divisions of the bronchial tubes; as far, at least, as minute dissection can follow them. The membrane in these cases is usually red, as in simple bronchitis; but it is sometimes pale, or with scarcely any traces of vascularity. The former of these appearances results from the continued turgescence of the capillary vessels; the latter is the consequence of their having been completely emptied of blood by the last hæmorrhage. We shall meet with analogous conditions when we come to examine the hæmorrhages that proceed from *other* mucous surfaces; and especially from that of the alimentary canal.

When blood is thus exhaled from the mucous membrane of the air-passages, the hæmorrhage may be strictly primary or idiopathic, *i. e.*, it may be independent of any discoverable alteration of texture, either in the mucous surface itself, or in any other part which, by reason of some intelligible connection of structure or relation, seems capable of influencing the capillary circulation of the membrane. But the occurrence of pulmonary hæmorrhage strictly idiopathic has been more frequently affirmed than proved. Active hæmorrhage from the lungs is stated by systematic writers to be the hæmorrhage of adolescence, as epistaxis is that of childhood. I believe, however, that idiopathic active hæmorrhage from these organs is very rare indeed; unless we may consider as such, certain forms of *vicarious* bleeding, which I shall presently advert to. Andral tells us that in one instance only, in which hæmorrhage from the surface of the air-passages had been the immediate and apparently the sole cause of death, had he ever found the substance of the lungs free from *tubercles*, and perfectly *healthy*. He does not, however, state whether in this one instance the *heart* also was in its natural condition: an important admission, as we shall hereafter perceive. He relates, indeed, as an example of idiopathic hæmoptysis, the case of a young man who suffered profuse hæmorrhage from the lungs on four several occasions, between the ages of twelve and eighteen, without any apparent detriment to his health, which remained excellent. It is consistent, however, with much experience, to suppose that crude tubercles might have been scattered in the lungs of this person, and have sufficed, on the application of some exciting cause, to determine the hæmorrhage, although as yet their presence was not indicated by any other sign. Almost every systematic writer quotes, as an example of idiopathic hæmorrhage from the lungs, the story of the Roman Governor, mentioned by Pliny, who lived to the age of ninety, though he was afflicted with habitual hæmoptysis. Now the frequent citation of this supposed instance is of itself a sufficient proof that spontaneous pulmonary hæmorrhage is far from being common.

Cæteris paribus, the *disposition* to pulmonary hæmorrhage is increased by whatever

tends to diminish the capacity of the thorax, and to compress the lungs, or the heart and great blood-vessels. The mechanical congestion thus produced may become a very intelligible cause of the exhalation of blood from the mucous membrane. And it is partly on this principle that we may account for the frequency of hæmoptysis in persons with crooked spines; in tailors, who sit continually in a stooping posture; in young women who lace their stays too tightly; and even in those who labour under dropsy, or other cause of distension of the belly. Hæmoptysis accompanying ascites has been known to cease at once upon the performance of the operation of tapping, and to recur upon the reaccumulation of the dropsical fluid; and this not on one occasion only, but so often and regularly as to preclude all notion of accidental coincidence. There can be no doubt, however, that in this class of cases, or at least in a vast majority of them, the hæmoptysis is mainly to be ascribed to organic disease of the heart or of the lungs; and that the pressure that precedes and determines the bleeding is simply a *concurrent* cause.

If we cannot properly rank *that* pulmonary hæmorrhage as idiopathic, which is constitutional and vicarious of some other natural or morbid discharge,—and most frequently of all the menstrual discharge in females,—it may be considered as forming a link of connection; as lying midway between secondary and primary hæmorrhages. There are a great number of very curious and well authenticated facts upon record concerning this singular form of hæmorrhage by deviation. I will give you one history of the kind by way of sample; it is related by Pinel, who held that there was no supplemental hæmorrhage more common than the hæmoptysis that is vicarious of menstruation.

A female, fifty-eight years old, born of healthy and robust parents, of strong constitution, of a sanguine and plethoric temperament, and of great sensibility, lived in the Saltpetrière, and was therefore under constant observation, from the age of fourteen. She enjoyed excellent health till she was sixteen years old. In her sixteenth year the menstrual discharge commenced without mishap or difficulty; but this, her *first* menstruation was suddenly suppressed, in consequence of the fright and agitation produced by the sight of an epileptic patient in strong convulsions. From that time the catamenia *never* reappeared, nor did any kind of discharge take place from the genital organs; but at the next period, when regular menstruation ought to have come on, the girl was attacked with violent hæmoptysis. The hæmorrhage was preceded by vague pains in the uterus and loins, and by other symptoms which frequently announce the catamenia. It lasted two days, during which time the girl expectorated nearly a quart of blood. With one interval of exception only, this female continued to menstruate *through her lungs* at each monthly period, from her sixteenth to her fifty-eighth year, *i. e.*, during forty-two years of her life. The coming on of the hæmorrhage was sometimes a little accelerated by strong mental excitement; sometimes a little retarded by causes of a contrary nature. It was suspended during one whole year, without any serious impairment of the general health, or the occurrence of any other hæmorrhage: during this interval, however, the patient suffered most severe headaches. Occasionally the hæmoptysis was complicated with hematemesis. The symptoms by which the pulmonary hæmorrhage in this instance was generally preceded or accompanied were the following:—a sensation of weight and uneasiness in the loins and in the situation of the uterus, soon followed by chilliness of the surface, general lassitude, and a feeling of oppression and heat in the chest, with some dyspnœa. The face became red, and she had intense headache. Then she began to have a distinct sensation of pricking, and a sort of bubbling, in the trachea and about the commencement of the bronchi; then followed sharp cough, and the expectoration of blood, often bright-coloured and frothy, sometimes of a darker hue. The duration of the hæmoptysis was generally confined to a single day, and it never exceeded three days. It recurred with tolerable exactness at monthly periods; sometimes the interval was longer, and then the *hæmorrhage* continued longer, but was less abundant; and upon the whole, about the same quantity of blood was lost on each occasion. This woman continued plump, and otherwise healthy, though liable to some thickness of the breath upon unusual exertion.

Cases of this kind are not at all uncommon; although the vicarious hæmorrhage seldom persists so long and so steadily. They are not usually attended with any peril to life.

It is, however, a melancholy truth, that the hæmorrhage which takes place by exhalation from the mucous membrane of the air-passages, is dependent, in a very large propor-

tion of instances, upon incurable disease. The hæmorrhage is secondary; and the disease of which it is symptomatic is usually a fatal disease. And the complaint of which hæmoptysis is by far the *most frequently* symptomatic, is tubercular phthisis. When the tubercles are found upon dissection to be yet crude and entire, and no breach can be detected in the membrane, then no doubt can be entertained about the source and manner of the bleeding; and even when cavities exist, especially if they are found to contain no blood, it is probable that, in most cases, the hæmorrhage has had a similar origin.

When hæmoptysis is thus actually symptomatic of tubercular disease of the lungs, it is liable to considerable variety in regard to the period of its first occurrence, and the symptoms by which it is succeeded. There are many persons in whom the first attack of hæmoptysis precedes, even for years, the primary symptoms of unequivocal phthisis. There are others in whom the first attack of hæmoptysis is *immediately* followed by all the signs which announce the presence of tubercles in the lungs. Many, again, do not spit blood until the tubercles have acquired a considerable degree of development, and the phthisical symptoms have been for some time clearly marked; and occasionally, in these cases, the first hæmorrhage proves fatal. Lastly, it is far from being an uncommon thing to see pulmonary consumption run its whole course, and terminate in death, without having been attended with *any* spitting of blood.

Andral gives the following statement as the result of his own observation, in regard to the relative frequency of these several modes of connection between hæmoptysis and consumption.

Of the persons whom he had known to die of that disease, one in six never spat blood at all. Three in six (or one-half of the whole number) did not spit blood until the existence of tubercles in the lungs was already made certain by unequivocal symptoms. In the remaining two-sixths the hæmoptysis preceded the other symptoms of tubercular disease, and *seemed* to mark the period of its commencement.

By this comparative statement you will see how very frequently hæmoptysis occurs as one of the symptoms *connected* with tubercular phthisis. Under this physician's observation it happened in five cases out of six. In the experience, however, of M. Louis, the proportion, though very large, is not quite so great as Andral found it. Among eighty-seven instances of consumption, there were fifty-seven, or four in every six, in which hæmoptysis had been present.

It has, however, been made a question, whether the spitting of blood which thus occurs in *connection* with tubercular phthisis, is always to be considered as indicative of the *existence* already of tubercles in the lungs; or whether it may not sometimes precede, and give occasion to, their formation in those organs. This question has evidently been suggested by those cases (constituting, according to Andral, one-third of all that happen) in which the ordinary signs of phthisis *begin* to manifest themselves immediately upon the occurrence of the first hæmoptysis, or within a short time afterwards. Morton, who has noticed this kind of pulmonary hæmorrhage, includes among his *species* of phthisis, the "phthisis ab hæmoptöe;" and Cullen held that *spitting of blood* was *often* the *cause* of pulmonary consumption. It is a very important question, and I shall revert to it again hereafter.

Next to *tubercular* disorganization of the *lungs*, the most frequent source of pulmonary hæmorrhage is to be found in organic disease of the *heart*. It has been stated by Chomel, Bouillaud, and others, both in this country and abroad, that the disease in these cases is most commonly situated in the *right chambers* of the heart. But certainly this is a mistake. The error has arisen from arguing upon erroneous analogies, instead of attending to matters of fact. However, the statement is just as little supported by reason as it is by the result of general experience. The only alteration in the right cavities of the heart which we could suppose likely *à priori* to cause pulmonary congestion, and thereby hæmoptysis, would be increased strength and thickness of their muscular parietes: hypertrophy: a morbid condition which is comparatively rare on that side of the heart, and which, perhaps, would not suffice for the production of hæmoptysis, even if it did oftener exist. The direct effect, on the other hand, of any *obstacle* to the free passage of the blood in the right chambers of the heart, would be to gorge the *liver*, and the system of the *vena portæ;* and to prevent the lungs from receiving their due proportion of blood. But

any material obstruction existing in the *left* auricle or ventricle will impede the return of blood *from* the lungs, lead to its accumulation in those organs, give rise to mechanical congestion, and so dispose strongly to pulmonary hæmorrhage. And, in point of fact, we find that hæmoptysis *is* very frequently the result of disease in the left side of the heart; and this leads me to speak here of one very remarkable morbid condition of the lungs, which is often directly connected both with pulmonary hæmorrhage and with cardiac disease; though it is not *always*, or *necessarily*, associated with either.

The morbid state to which I allude is far from being infrequent; yet it had been scarcely noticed by or known to pathologists, until Laennec described it under the title of *pulmonary apoplexy*. It appears under two forms. In the one form we find an uncertain number of hard knobs, or compact masses, situated here and there in the substance of the lungs, chiefly in their lower lobes, and towards their posterior surface. Their size varies from that of a marble to that of a hen's egg. When cut through they are seen to be very exactly circumscribed, the cut surface being more or less circular, of a uniform and very dark colour throughout, and exhibiting a strong contrast with the surrounding tissue. Careful examination shows that these masses are composed of blood that has coagulated in the pulmonary vesicles. *Occasionally* the pulmonary substance seems broken down, or torn, by the extravasated blood; and in these cases, perhaps, the resemblance between the injury done to the lung, and that which is inflicted on the substance of the brain in *cerebral* hæmorrhage, is tolerably close. Generally, however, there is no such laceration of the pulmonary tissues; but one, or more, of the *lobules* of the lungs, are gorged and crammed with blood, which has been poured out from the surface of the mucous membrane. These lobules, it is well known, have no direct communication with each other; but are isolated (except where they severally open into the bronchial tubes from which they spring) by a distinct investment of cellular tissue; and it is to this peculiarity in *their* structure and disposition, that the exact circumscription of the dark-red indurated masses is to be attributed.

In the other form of pulmonary apoplexy, there are fewer of these solid spots: perhaps one only, large, diffused, occupying sometimes nearly the whole of one lobe, its limits obscurely defined, and its colour gradually deepening to the centre, which is obviously formed by little else than a black clot of blood.

Now the principal *symptom* attending the formation of these masses is *hæmoptysis;* and the principal though not the only *cause* is disease of the heart. The hæmorrhage is often severe and copious in the first, or circumscribed form: sometimes slight and scanty, but commonly slow, oozing, and persistent, in the second or uncircumscribed form. The heart-disease is in its left chambers, and very often consists in contraction of the mitral orifice. No example of pulmonary apoplexy, or of pulmonary hæmorrhage, even *apparently* dependent upon hypertrophy of the *right* side of the heart, has ever fallen under my notice.

In truth the morbid condition of the lungs which I am now speaking of, has been badly named. The application, by Laennec, of the term *apoplexy* to the *lungs* was singularly unfortunate: for it suggests an analogy between two things, which, though resembling each other in the appearances which they leave behind them in the organ affected, are yet, essentially, unlike. I have shown you, in a previous part of the course, that cerebral hæmorrhage depends almost always upon the giving way of a blood-vessel, in consequence of the morbid brittleness of its coats: while what is called pulmonary apoplexy can very seldom indeed be so caused. The notions which I have been led to form upon this subject differ materially from those which you will find expressed in the works of almost every writer on pulmonary apoplexy. The opinions I entertain were stated several years ago, in some lectures which I was appointed to deliver before the College of Physicians; and I have constantly been in the habit of mentioning them to the pupils of the Middlesex Hospital, and to my medical friends. It is a matter of satisfaction to me to find that they are esteemed to be correct by so sound a pathologist as Dr. Carswell, who has alluded to them in one of his fasciculi on the *Elementary Forms of Disease*. Laennec speaks of the pulmonary apoplexy, as if *it* were the *cause* of the hæmoptysis. But this is surely a very incorrect view of the matter. The partial engorgement, and the hæmoptysis, are not mutually connected with each other as cause and effect, but they are *con-*

current effects of the *same* cause; of that cause which gives rise to the extravasation or exhalation of the blood in the first instance. A part of the blood so extravasated passes outwards by the trachea and mouth; while a part is forced in the contrary direction, into the ultimate divisions of the bronchi, so as to fill and block up the whole tissue of a single lobule, or of a bunch of contiguous lobules, and thus arises the *circumscribed* variety. Andral conceives that the sanguine effusion *takes place* in the ultimate air-cells; and *he* applies to this form of disease the term *pneumo-hæmorrhage*, to distinguish it from ordinary hæmoptysis, which he calls *broncho*-hæmorrhage; and this I believe to be the true pathology of the *uncircumscribed* variety. But it seems to me vastly more probable that in the other form of the complaint the seat of the effusion is in one or more of the larger branches of the air-tubes; and that the blood, a part of it at least, is driven backwards into certain of the pulmonary lobules, by the convulsive efforts to respire which the patient makes when threatened with suffocation by the copious explosion of blood, or by a paroxysm of cough and extreme dyspnœa: especially if the blood is poured out from the membrane while the chest is in the state of *ex*piration. It is easy to understand how certain portions of the lungs, without undergoing any actual change of texture, may in this manner be so choked up, and crammed with blood, which afterwards coagulates, as to preclude any subsequent admission of *air*.

This view of the formation of circumscribed pulmonary apoplexy affords an easy explanation of some of the phenomena attending it, which it would be difficult to account for on any other supposition: I mean, first, the occurrence of *several* of the clots or masses of blood, in different, and sometimes in distant parts of the lung at the same time; and secondly, the exact manner in which they are commonly bounded and limited to certain lobules. And if (as is sometimes, though seldom, the case) even the texture of the lung be lacerated, it is easier to conceive that this may happen in consequence of the violence of regurgitation during the struggle of impending suffocation, than that the mere impulsion of a thickened muscle at the centre of the circulation should be capable of driving the blood through the walls of an artery with sufficient force to tear and break down the substance of the lung around it.

In the diffused or uncircumscribed form of pulmonary apoplexy, the congested lung is not relieved by a sudden and copious gush of hæmorrhage, but the impeded and stagnating blood oozes slowly through the vessels containing it into the neighbouring interstitial and vesicular tissues, clogs up a larger and larger space, and is partly expectorated in separate dark-red sputa, combined with a certain quantity of mucus. A similar condition of the lung sometimes occurs in *purpura hæmorrhagica*, without any mechanical impediment to the passage of the blood into and through the heart.

The belief that the dark-coloured, circumscribed spots seen in the lungs, and spoken of as pulmonary apoplexy, are often, if not always, produced in the manner I have just been describing, was suggested to me by the observation of a case, in which these appearences existed, and in which they *certainly were* so occasioned. I told you, when speaking of cynanche tonsillaris, that I had seen one person, and one only, die in consequence of that complaint; and that his death was occasioned by the laying open of the lingual branch of the carotid artery in the progress of ulceration. The phenomena attending that patient's dissolution were of deep, though of painful interest. I described them to you before. He had been taken out of bed, and laid upon a table in the ward, in the middle of the night, in order that Mr. Mayo might more conveniently place a ligature upon the carotid. Suddenly the bleeding burst forth afresh: and he expired, before our eyes, in the course of two minutes. not from syncope or exhaustion, but evidently suffocated. The blood entered and choked up the trachea, and he had not strength enough left to expel it by coughing. I felt his heart and the artery at his wrist pulsate firmly for some little time after the last attempt to dilate the chest had been made. This you know is what always happens when death takes place from the sudden denial of air to the lungs. We found the upper surface of the glottis covered by a clot of blood. There was blood also *in* the windpipe; and scattered through the substance of the lungs there were numerous, hard, and dark, but not very large masses, precisely resembling those described by Laennec as constituting pulmonary apoplexy. This man had shown no symptoms of any pulmonary complaint; nor was there any morbid appearance in his lungs except those which resulted

from the presence of the blood that had been poured into them through the trachea, and rammed home into some of the air-cells, in his convulsive attempts to breathe. All that I have observed since this case happened, has tended to confirm my belief, that what has been erected into a distinct form of disease, under the objectionable name of *pulmonary apoplexy*, is simply an *accident of pulmonary hæmorrhage*. When hæmoptysis has occurred, to any amount, in *consumption*, it is by no means uncommon to find pulmonary apoplexy after death; and Dr. Latham has mentioned to me in conversation, one remarkable instance of that disease, strongly corroborative of the doctrine I have been endeavouring to support. A young female patient of his, labouring under confirmed phthisis, was attacked, for the first time, with hæmoptysis. The bleeding was so profuse as to cause almost immediate death by suffocation. Her lungs were found riddled with small tuberculous cavities; and each of these little cavities contained a little clot of blood. Surely it is more credible that the blood should have reached each cavity by regurgitation from the larger air-tubes, than that each should have been the seat of an independent hæmorrhage at the same moment. It is by a similar reflux of blood that the appearances are produced which characterize the circumscribed form of pulmonary apoplexy.

Prognosis.—Upon the whole, the occurrence of hæmoptysis, considered in reference to the probable duration of life in those who are the subjects of it, is of melancholy omen.

I have long arrived at this conclusion:—that if from any given number of persons who have been known to spit blood, we subtract those in whom that symptom was connected with irregularity in the uterine functions, there will remain but few in whom the hæmoptysis did not depend upon disease, incurable and progressive in its nature, of the lungs, or of the heart; and that if we still further subtract those persons in whom the hæmorrhage was symptomatic of cardiac disease, there will be very few indeed left, in whose lungs the existence of tubercles may not be confidently predicted.

Among these few may be reckoned persons who have suffered hæmoptysis dependent upon the detachment and expulsion of "bronchial polypi;" and who exhibit no other indication of cardiac or of pulmonary disease.

You will, of course, understand that I do not include in this estimate of hæmoptysis as a prognostic symptom, those cases in which (as in simple bronchitis) the expectoration is merely *streaked* with blood:—nor those in which small quantities of blood are intimately combined and amalgamated with the bronchial mucus, and form the rust-coloured sputa so indicative of the presence of pneumonia;—nor those in which the hæmorrhage is the consequence of mechanical injury to the chest.

Of those individuals whom Andral had known to spit blood at some period or other of their lives, there was only one in five whom he did not also *know* to have tubercular phthisis. On the other hand, Louis states that for three years he asked all the patients who came before him, in the practice of a large hospital, and who were *not* affected with phthisis, whether they had ever spat blood; and the answer was always in the negative, excepting only a few instances in which the patients had received violent blows upon the thorax; and the cases of females in whom the menstrual discharge had been suddenly suppressed.

Symptoms.—The quantity of blood which is brought up in different cases of pulmonary hæmorrhage, is extremely variable. Sometimes it is so copious and overwhelming that the patient either dies suffocated, or he dies of syncope, outright: but this is not very common. Sometimes, on the other hand, a small quantity of blood finds its way into the mouth, the patient scarcely knows how. And between these two extremes there is every gradation in respect to quantity.

"When blood is thrown out by the mouth (says Cullen), it is not always easy to determine from what internal part it proceeds; whether from the internal surface of the mouth itself, from the fauces, or adjoining cavities of the nose, from the stomach, or from the lungs. It is, however, very necessary to distinguish the different cases."

Now the diagnosis between hæmorrhage from the lungs and hæmorrhage from the stomach, in other words between *hæmoptysis* and *hematemesis*, I shall not enter upon, until I have described the latter disease. And the diagnosis between hæmorrhage from the fauces or cavity of the mouth, and hæmorrhage from the lungs, can never be very difficult, if once the doubt suggests itself, and the necessary examination of the mouth be

made. And I would advise you not to omit that inspection. I could tell you of cases in which the neglect of this simple precaution has led to needless activity of treatment, and to the ultimate discredit and disadvantage of the practitioner. Blood may ooze into the mouth from spongy gums, or drip from the posterior nasal orifices, and be at length spat out in considerable quantity. Etymologically speaking, these are cases of *hæmoptysis;* but they do not constitute the particular disease or symptom to which alone nosologists have agreed to restrict that term. The sources of the bleeding are manifest as soon as they are carefully looked for.

Patients who are subject to hæmoptysis generally know by experience when it is about to happen. It is frequently preceded by some uneasy feeling within the thorax—pain, or a sense of weight, or of heat or of pricking, beneath the sternum, with anxiety; and they tell you that they taste the blood in their mouths before it comes up, *i. e.*, they perceive a saltish taste; and just before the blood appears, a tickling sensation is experienced about the top of the larynx. To relieve this sensation, the patient coughs or hawks a little, and a certain quantity of frothy and florid blood is expectorated.

In a person disposed to pulmonarry hæmorrhage, the bleeding may be determined by a variety of causes; which ought to be pointed out to him, in order that he may avoid them. Any thing which hurries the circulation will, of course, have a tendency to excite the hæmorrhage. Straining of any kind; great efforts of the body; active exercise; much talking; and more especially public speaking, or singing, or playing on wind instruments. A diminution in the superincumbent pressure of the atmosphere is supposed to be, in some cases, sufficient to bring on hæmoptysis; and blood is said to have been forced even from sound lungs, in persons who have ascended very high mountains, where the atmosphere is rare, and where the pressure upon the surface of the body is sensibly diminished. Perhaps the labour of the ascent may have shared in the production of the hæmorrhage; for I am not aware that any such effect has ever occurred to persons who have much more rapidly reached a very great altitude in balloons.

Auscultation and percussion do not stand us in much stead in cases of hæmoptysis, as far as that *symptom itself* is concerned. Indeed, if they were capable of affording us information, it would in most cases be superfluous; for we *see* the blood, and we can generally satisfy ourselves that it comes from the *lungs*.

But pulmonary hæmorrhage may occur without hæmoptysis. In what is called pulmonary apoplexy there is extravasation of blood: and it is not always attended with the expulsion of a portion of the extravasated fluid through the mouth. Laennec and others pretend to say, that when there is blood in the bronchi, they can distinguish by the peculiar character of the crepitation to which it gives rise that it *is* blood, and not mucus; that the bubbles, passing through a thinner liquid, are larger, and break oftener, than those produced by the passage of air through viscid mucus. This distinction is too subtle for me. If, indeed, there has been hæmoptysis, and especially if the hæmoptysis has been sudden and copious, and if, after it, you hear large crepitation in one or more isolated parts of the lung, it will be reasonable to conclude that the air-tubes contain blood in those parts. Those lobules that are plugged up with blood, to the entire exclusion of air, will not, of course, be the seat of any sound during respiration; but this limited absence of sound will be scarcely appreciable unless the infarcted portion lies near the surface of the lung. Around the spot thus rendered solid the sound of crepitation may be audible.

Yet although the method of auscultation furnishes but little help towards the *detection* of pulmonary hæmorrhage, it will often afford us most precise and valuable information respecting the disease of which the hæmorrhage is a consequence, and an index. Thus, it will frequently teach us, with absolute certainty, that the heart is diseased, or that the lungs are occupied by tubercles. The precise sounds, or deficiencies of sound, which supply the key to this knowledge, I shall describe when I come to those disorders.

Treatment.—Whatever may be the source and organic cause of the hæmoptysis, the bleeding should be stopped as soon as possible: not, however, merely by *suppressing* it, but by relieving the necessity on which it depends. The longer it is suffered to continue, the more likely is it to add to the damage which already, in too many cases, exists in the lungs. If it leads to the formation of blocks of pulmonary apoplexy, the portions of lung so filled up are rendered useless for a long period, and probably for ever. Now as in most

cases the hæmorrhage is a hæmorrhage by exhalation, and depends upon congestion, active or mechanical, we shall stay the hæmorrhage if we remove the congestion.

The congestion may be either mechanical or active when it results from the presence of tubercles in the lungs; it is almost always mechanical when it depends upon disease of the heart. The tubercles may press upon the blood-vessels, and so lead to mechanical engorgement; or they may provoke by their presence an active determination of blood to those organs, just as we know that they often provoke inflammation, which is congestion and something more; and just as any foreign body lodged in the lung may cause either the one or the other of these conditions.

Frequently there is a distinct febrile movement accompanying the hæmorrhage; the heart beats with increased force and frequency, the cheeks are flushed, and the skin is hot; sometimes the pulse is quite hard, and full and bounding, and people speak of such a pulse as a *hæmorrhagic* pulse. Now I mentioned in a former part of the course that hæmorrhage occurring under such circumstances as these often works its own cure; but it is better, when an organ so vital and important as the lung is the seat of the effusion of blood, that *we* should cure the bleeding than that it should cure itself—that we should diminish the congestion with which it is linked, through the safer channel afforded by the veins of the arm. The patient is to be surrounded with cool fresh air. His head and shoulders should be elevated. He should be restricted to the most meager diet; and be forbidden to exert himself, or to speak more than is absolutely necessary. His bowels should be freely purged, in the first instance, and then *kept* lax and open, both with the view of deriving (as it is called) from the thorax, and of preventing costiveness and straining. And, in conjunction with these measures, he should lose blood from the arm. The amount and the repetition of the bleeding must be determined by the circumstances of the case, *i. e.*, by the cessation or continuance of the hæmorrhage, and especially by the condition of the *pulse*. It would be idle to attempt to lay down precise rules on this matter. We do not bleed, however, so resolutely and perseveringly in hæmoptysis as we are often obliged to do in acute inflammation.

A prejudice has been taken (such, at least, I think it) against *local blood-letting* in pulmonary hæmorrhage. Inasmuch as leeches applied to the groins in amenorrhœa appear sometimes to restore the catamenia, so they have been thought likely, when applied over the surface of the chest, to attract the blood somehow to that part of the body, and even to *cause* hæmorrhage when none before existed. Now I have so many times taken blood from some part or other of the exterior of the thorax by leeches, or cupping glasses, without observing any such effect, of causing hæmoptysis, or of increasing it while already present, that I cannot help considering the objection rather a fanciful one.

When the fever and congestion are abated; or when there has been no constitutional disturbance, and the hæmorrhage has shown a passive character from the beginning, and a continuance of it, so far from being curative in its nature, is likely to be injurious; then we are to employ those remedies which have been found efficacious in restraining and suppressing hæmorrhages.

Now of the substances which are held to possess more or less of a specific virtue, when taken internally, in arresting the efflux of blood, the sugar of lead, the *plumbi acetas*, enjoys in this country the highest reputation. And it certainly is a very serviceable remedy. Dr. Paris speaks of it as one of the most valuable resources of physic: and says that in respect to its power over internal hæmorrhage there is nothing *simile aut secundum*. He states also that its use is equally safe and manageable. There is, in fact, no doubt of its efficacy: but most other writers use very curious language in recommending its employment. Physicians have been deterred from giving it by the fear of its poisonous qualities; by the dread of producing the disease called *colica pictonum*. Cullen observes, that the preparations of lead are certainly powerful in controlling hæmorrhage, but that they are otherwise of a character so pernicious as to forbid their use except in cases of the utmost danger. Of late years this drug has usually been administered in small doses, and guarded by opium; and it is to this combination that Dr. Paris refers when he declares it to be a safe and manageable remedy. More recently, however, a statement has been made by Dr. A. T. Thomson, which must be considered of much importance if further experience shall show it to be well founded. He was led, it seems, by some accidental

circumstance, to suspect that lead acted as a poison upon the animal body, only in the shape of its *carbonate*. And the result of a series of experiments upon brutes satisfied him of the correctness of this notion. He holds, that when the acetate of lead produces the well-known symptoms of the painter's colic, it does so in consequence of its being somehow converted, after its reception into the body, *into* the carbonate: that this conversion may be obviated by a very simple expedient; and that the remedy may then be given with perfect safety in large and efficient doses. The expedient is merely that of administering the lead in draughts containing some dilute acetic acid, which prevents the decomposition of the acetate by any carbonic acid that happens to be present in the intestinal canal. In this way he tells me he has given as much as fifteen grains daily for ten days together, without any inconvenience, and with most excellent effect upon the hæmorrhage. I have often exhibited lead in this manner; and I have never known it to give rise to any unpleasant consequences. At any rate this method of administering it deserves further and careful inquiry.

In slight cases of hæmoptysis, the mineral acids are often sufficient; or, if there be feverishness, the saline draught with nitre and digitalis. Of the numberless other drugs which have been vaunted as specific in hæmorrhage I have very little personal experience; at least in pulmonary hæmorrhage. In certain other forms of internal bleeding there are some of them that are worth trying. But in hæmoptysis there are none I can venture to *recommend* but such as I have now mentioned. You may sometimes be urged to give a celebrated quack medicine—*Ruspini's styptic*, which has obtained a high repute, and sells at a high price. This nostrum seems for a long while to have baffled analysis. The late Dr. Wollaston told Dr. Maton that it contained no metallic substance; Dr. Thomson has since announced that it mainly consists of a solution of gallic acid in alcohol diluted with rose-water. But I believe that all the remedial agents which contain gallic acid are more effectual in another form of internal hæmorrhage, to be considered hereafter.

Of *mercury*, as a remedy for pulmonary hæmorrhage, I have already mentioned both my own insufficient experience, and the very favourable report of some excellent judges who have much employed it.

LECTURE LV.

PULMONARY EMPHYSEMA; VESICULAR AND INTERLOBULAR. ANATOMICAL CHARACTERS OF VESICULAR EMPHYSEMA; PHYSICAL SIGNS; GENERAL SYMPTOMS; CAUSES; TREATMENT. INTERLOBULAR EMPHYSEMA: ITS ANATOMICAL CHARACTERS, SYMPTOMS, CAUSE, AND CURE. ŒDEMA OF THE LUNGS. PHTHISIS PULMONALIS.

I HAVE yet one or two morbid conditions of the lungs to consider and to describe, before I go to that which is the most common and most extensively fatal of all its morbid conditions—tubercular phthisis.

Pulmonary emphysema.—There is a state of the lung, or rather there are two or three different states, to which Laennec has applied the name *emphysema*. A very injudicious name it was for him so to impose. We are infinitely indebted to Laennec for the entirely new light which his able researches have thrown upon the morbid anatomy and the pathology of the lungs: but we have to regret that he should have employed, in several instances, a vicious nomenclature. Emphysema is a term that had long been familiar among medical men in a certain sense. It was used to express the inflation of the cellular tissue of the body with air: and surgeons still make much of it as an indication, in cases of fractured rib, that the bone has grazed the pleura, and allowed air to pass into the cellular tissue, and to diffuse itself over the chest and neck, and other parts; so that these parts, when pressed, convey a curious sense of crackling to the finger. But emphysema of the lung, as that term is employed by Laennec, includes dilatation of the air-cells of

the lungs, and rupture of the partitions which separate them from each other; and also the infiltration of the air into the interlobular cellular tissue, or into the subpleural cellular tissue. In strictness of language these last conditions alone should have been called emphysema of the lung. Laennec has distinguished the two species in this way. To the dilatation of the air-cells, with or without a breach of their partitions, he gives the name of *vesicular* emphysema: "the vesicular (I quote the words of Dr. Forbes' translation), or pulmonary, properly so called." Now in truth this is emphysema *im*properly so called. To the infiltration of the cellular tissue in or around the lung with air, *i. e.*, to emphysema of the lung in the old sense of that word, he applies the title of *interlobular* emphysema. We cannot change these denominations now. They have fastened themselves upon medical language. But it is very fit that you should be aware of their inconsistency with the ancient signification of the same word, and have clear notions of what in Laennec's nomenclature they are intended to express.

Vesicular emphysema.—The change called vesicular emphysema was not *unknown*, as a mere morbid condition, before the time of Laennec: but it had been noticed by very few writers, and *practically* it was wholly *unattended to*. Yet it is extremely common: much more so than you would suppose: and when rightly studied, it is of great interest too, in relation to the general pathology of the chest. But it is still so new, and it may so readily escape observation, both in the dead and in the living body, if it be not looked for, that I shall devote a somewhat more minute attention to it, on those accounts.

Laennec was undoubtedly the first to put emphysema pulmonum upon the list of definite and cognizable diseases; to point out its frequency; and to collect its symptoms. But when he affirms that, before his time, the pulmonary change which constitutes the disorder was misunderstood by nearly all those persons who had noticed it, he scarcely does them justice. "All of them (says he) seem to have thought that the derangement in question consisted in the infiltration of the cellular substance of the lungs with air." He inconsistently adds, "Ruysch and Valsalva, are the only authors, as far as I know, who have observed in individual cases, the dilatation of the cells;" and with still greater inconsistency he proceeds to quote, from Morgagni, the following passage, in which this dilatation is very clearly described: "Sinistri pulmonis lobus superior, quâ claviculam spectabat, *vesiculas ex quibus constat mirum in modum auctas habebat;* ut nonnullæ avellanæ magnitudinem æquarent; cæteræ multo minores erant." You will find the same change noted by Dr. Baillie, in his *Morbid Anatomy:* and by earlier writers than he.

Anatomical characters.—Vesicular emphysema then, (to adopt Laennec's phraseology) consists in dilatation of the air-cells. The enlarged cells become misshapen also in many cases. They vary in magnitude from that of a millet seed, to that of a swan shot: nay, the cavities may even reach the size of a nutmeg or of a hen's egg: but when they are as big as this—and *a fortiori* if they are still bigger—the distension and vacuity are, no doubt, the result of the union of several air-cells, broken into one, by the stretching or destruction of the partitions that naturally divide and isolate them. You may see the dilated vesicles very plainly through the pleura if you carefully examine the surface of the lung. They appear to the naked eye as the healthy vesicles appear when seen through a magnifying glass. Sometimes all the vesicles belonging to one lobule are enlarged, while those of the adjoining lobules are of the natural size. In that case the emphysematous lobule is conspicuous both by its peculiar colour, and by its protrusion. The surface of the lung is often rendered quite irregular and uneven by projections of this kind. Sometimes one large globular prominence is seen, like a bubble on the water, or like a little bladder springing from a footstalk: but if you examine it closely you will generally find that the footstalk is merely a constriction at the surface, and that there is as large a cavity beyond it, in the lung, as there is without. These bullæ you cannot slip about, by pressure, from one part of the pleura to another.

The unevenness produced by vesicular emphysema upon the outside of the lung is manifest enough, when looked for; but the same condition of the air-cells exists also within, and *there* it is *not* so readily perceptible. The fluids which the lung contains obscure all distinction of parts when the organ is cut. The best way of getting a fair view of the dilated cells as they appear in the substance of the lung, is to inflate the emphysematous portion, by blowing air in at the bronchial trunk which belongs to it, and

then tying that trunk to prevent the escape of the air. The inflated lung should be hung up in a current of wind, so that it may quickly dry; and during the drying process it should, from time to time, be reinflated: for else the included air gets out somehow, and the piece of lung shrinks and shrivels up. When it is quite dry, if a section of it be made with a thin sharp knife, the altered state of the air-cells, some of which are *more* and some *less* dilated, will be very conspicuous.

No part of the lung is exempt from liability to these morbid changes; but generally they are limited to certain portions of the organ, and they are much more common and more pronounced at its loose anterior borders, and near its summit, than anywhere else. Both lungs appear to be alike obnoxious to the disease; which seldom affects the one without affecting, in a greater or less degree, the other also.

The parts that are emphysematous are usually paler than the rest, and sometimes they are quite white. In extreme cases the surface of the lung presents a sort of pyebald appearance; large patches of it looking as if they had been bleached. This pale colour is oftenest seen towards the free edges of the lung. Sometimes those edges are rounded and thick; sometimes thinner, and folded back; while sometimes the margin is blown out, as it were, into an irregular fringe; some of the inflated portions remaining connected with the lung by slender pedicles, and thus forming *appendices* to it of a light yellow colour. I presume that what was thought and called a fringe of fat, garnishing the edges of the lung, in the body of King George IV., was of this kind. At least I have never seen, nor heard of, any other example of fat deposited in those organs. If you hold the emphysematous border between your eye and the light, you perceive that it is translucent: if you prick it with a pin, the puffy part surrounding the puncture collapses; which shows that the dilated vesicles communicate together.

An emphysematous lung is not only paler, but drier also than ordinary: and for the same reason. It possesses fewer capillary blood-vessels, less blood, and consequently less moisture. It is dry and light, and floats high upon water, like a bladder filled with air.

If you take such a lung out of the body, having its surface embossed with irregular groups of enlarged air-cells—and if you inflate that lung, by blowing into the bronchi—the emphysematous portions will *seem* to sink in, and flatten, and return to the ordinary level as the lung becomes distended. In point of fact, however, these portions remain permanently dilated, and the other parts of the surface rise, as the air enters them, until the whole is smooth and even. Air is shut up in the emphysematous portions, which do not subside, as the adjoining portions do, when left to the agency of their proper elasticity. Hence you will see how it is that, when the vesicular emphysema is extensive, so as to occupy nearly the whole of the lung, the lung becomes apparently too big for the case in which it is contained. Not only does it *not collapse* when the sternum is raised, and the pressure of the atmosphere is admitted to its external surface; but it even *protrudes*, the moment that the opening is made. When you handle such a lung, it gives a very different sensation to the fingers from that produced by pressing a healthy lung. It feels like a down pillow. It crepitates less; the air is less easily forced out of it, and escapes slowly, with a slight hissing noise.

Physical signs.—Such being the state of the lung, as discovered after death, you will naturally be inquisitive to know by what signs the existence of a condition so remarkable is revealed during life. First, then, when the emphysematous distension is considerable in amount, and extensive, it produces alterations in the shape and movements of the chest. The lung having lost much of its elasticity, does not subside as a healthy lung does. The act of expiration is arrested and incomplete. Consequently the thorax remains nearly in that position which it assumes after inspiring. It is prominent and rounder on the diseased side; or on both sides, if both lungs be affected; but it is apt to be irregularly prominent, and unsymmetrical; to bulge here and there in correspondence with the bulging of the lung within. The ribs are less oblique than they should be, and the chest is, therefore, more cylindrical. The clavicles are ill-defined in such persons. They are so, as you know, in *fat* persons: wherefore this aid to the diagnosis is of most value in those who are *spare*. In them it is a valuable sign, for it is simple and obvious. The distended lung presses upwards, as well as in other directions, and tends to efface the impressions which naturally exist both above and below the collar-bone. This symptom is the more to be

depended on if it presents itself on one side only. The manner of breathing is instructive also. The ribs, never receding within their proper limits after *ex*piration, can move but little during *in*spiration; and the breathing is in a great measure abdominal.

Now all these signs are physical signs. But what, in the second place, are the *auscultatory* physical signs? Why, in the emphysematous regions, which commonly are also the most bulging, percussion yields an unnaturally clear and resonant sound; while auscultation discovers a very indistinct vesicular murmur. The two modes check and explain each the information afforded by the other. Percussion ascertains that there is air beneath the part struck: auscultation ascertains that there is little or no *air in motion* beneath that part. It follows, therefore, that there is stagnant air; air shut up in the enlarged air-cells, or air interposed between the ear and the lung in the cavity of the pleura: air at rest, in fact. I say the respiratory murmur is very feeble. This partly depends upon the limited play of the ribs, partly and chiefly upon the circumstance that the air is imprisoned in the dilated cells. When none enters or leaves them during respiration, no vesicular breathing can be heard: and then we must call in the aid of other considerations to determine whether the air be contained in the cavity of the pleuaa, or in the emphysematous lung.

Pure pneumothorax is extremely rare. Pneumothorax with liquid effusion is easily recognised by its proper signs. If these be absent, we conclude that the stagnant air occupies the lung, and not the pleura: and this conclusion is strengthened if the unduly resonant part be prominent also. Almost all writers on this subject follow Laennec in stating that *dry* crepitation may be heard in the emphysematous parts. It may be so; but if so, I cannot distinguish it. I mean that I know no crepitation but that which is moist, and which proceeds from the formation and rupture of bubbles, as air passes through liquids in the bronchial tubes. But other people believe that they can hear a dry crackling, such as may be produced by inflating a portion of dry cellular tissue out of the body, or a dry bladder. You will try this by your own experience. I do not deny that such a sound exists: I only say that if it does exist, I cannot tell when I hear *it*, and when I hear large moist crepitation. But I more than suspect that no such sound is given out by an emphysematous lung; and that the sound heard is really large crepitation in the neighbourhood of the dilated cells: for emphysema is very often accompanied by catarrh; and the sounds in question, authors agree, are not permanently audible.

General signs.—So much for the physical signs of emphysema pulmonum. The *general signs*, when the change is extensive, are an habitual shortness of breath, with occasional paroxysms of extreme dyspnœa; cough, which, however, is far from being a constant symptom; palpitation in most instances as the disease advances, and more or less œdema of the ankles. Usually the appetite remains unimpaired, and the patient does not lose flesh. The disorder is unattended with fever; and is essentially chronic.

The paroxysms of urgent dyspnœa are frequently concurrent with, and apparently excited by, attacks of smart bronchial catarrh; but they sometimes arise without any obvious cause. They are apt to come on suddenly, in the night, and the patient is obliged immediately to sit up, and even to open the doors and windows of his bed-room, that he may breathe more freely. In one word, he undergoes a paroxysm of *asthma*. These attacks become more frequent and more severe as the patient and the disorder grow older. They are attended with much wheezing; and in the lower posterior part of the lungs crepitation is generally audible. At first the palpitation of the heart, and œdema of the feet, abate and cease as the violent dyspnœa goes off: but at length these symptoms become permanent.

We see a reason, in the physical condition of the thorax, why the breathing should be more oppressed, and why the paroxysms of orthopnœa should occur more frequently, *in the night*. Whenever the respiration is principally abdominal, it is apt to be embarrassed by the recumbent posture, which throws a part of the weight of the viscera of the belly upon the diaphragm. The horizontal position is always ill borne by these patients; and, for similar reasons, flatulence or fulness of the stomach, however caused, distresses them.

Cough, as I observed before, is an occasional, but by no means an essential accompaniment of vesicular emphysema: the expectoration, when there is any, is thinnish, like gum-water, and full of foam.

Louis, who has analyzed, with his accustomed care and exactness, a considerable number of cases of emphysema, states that the disease is seldom complicated with tubercles in the lungs. We may suspect that complication, if hæmoptysis, or emaciation occur; which are otherwise rare symptoms.

Causes.—To what *cause* can we ascribe this dilatation of the pulmonary vesicles, whereby the proper function of the lung, in the part affected, is impaired, or annulled? Mainly, I think, in the outset at least, to the imprisonment of air within them, under circumstances of disease. You may often trace back the shortness of breath to the period of infancy. Patients will tell you that from their earliest recollection they have been easily put out of breath: that they never were able to engage heartily, and to the same degree with their companions, in the active sports of childhood. It is easy to conceive that under chronic inflammation, or other disease of the mucous membrane, air may enter the vesicles more readily than it can escape from them again. The act of inspiration is voluntary and strong: the tubes are made patent by it, and air rushes in and finds its way to their extremities. But the act of expiration is passive, and comparatively feeble. Slight tumefaction of the membrane, or a little plug of viscid mucus, may so close up a small bronchial ramification, that the air cannot pass through it in expiration: and more and more air may thus be accumulated and incarcerated in certain air-cells, which yield to its distending force, and losing their elasticity become permanently large. And this process will be accelerated if the original disorder which gives rise to it is attended with violent cough: with *forcible efforts*, that is, of expiration Emphysema is always (in my opinion) a consequence of pre-existing disease or disorder of the chest. There are some who believe it to be occasionally a congenital and idiopathic affection. They build this notion upon the fact that the complaint is traceable, from one generation to another, in certain families; and as it often is present at an early age in children born of emphysematous parents, they conclude that the emphysema, in such cases, is a vice of the original bodily formation. I am not convinced by this mode of reasoning. The facts upon which it rests show simply that the disorder runs much in families, and that the tendency to it is sometimes inherited. The lax or weak fabric, which favoured the production of the disease in the parent, is repeated in the offspring, and imparts the same predisposition. Children are very liable to severe coughs, such as are calculated to strain and overstretch the cells of their delicate lungs. What can be more likely to do so than the reiterated and violent paroxysms of coughing which occur in pertussis? After the cough has ceased, however, the shortness of breath which it leaves behind is easily overlooked, until, with the increase of the emphysema, it forces itself into notice. For when once the morbid process has begun, it tends, slowly often, but surely, to its own augmentation. As the cells dilate, the capillary blood-vessels distributed over their parietes are gradually compressed and emptied: and many of them are, at length, completely obliterated. Hence, not only an exsanguine condition of the pulmonary tissue, but atrophy also of the intervesicular partitions, which become first thin, then tattered and imperfect. In dried specimens of emphysema you see very plainly the remains of the former walls of separation between the vesicles.

It is this interference with the nutrition of the lung which causes vesicular emphysema to be always a *progressive* disease. We see why it is that, speaking generally, the extent of the morbid change is proportioned to the age of the patient: why paroxysms of severe dyspnœa at length supervene; and become more and more frequent and trying. The function of the lung becomes year by year more limited; until it can no longer bear, without a struggle of distress, that further encroachment upon its office and capability which a slight catarrh, rapid movements of the body, a distended abdomen, or even the recumbent posture, may be sufficient to produce.

Laennec attributes the dilatation of the air-cells, in the first instance, to what (with a curious felicity of diction) he terms *dry catarrh*, which is characterized by its tendency to recur, and by the expectoration of small pieces of hard, pearly phlegm. But doubtless the disorder may be produced, and aggravated when produced, by any cause that impedes the free exit of the air from the lungs during expiration: by blowing on wind instruments of music; by pressure made on parts of the lung; by tumours therefore in the thorax, a large heart, aneurism of the thoracic aorta, deformity of the chest from crookedness of the spine,

tight lacing, and even the presence of tubercles; although lungs that are full of tubercles are not, in general, affected by emphysema. This last fact has led to the absurd project of attempting to prevent phthisis by producing emphysema. It is the same disease which exists in broken-winded horses; and Sir John Floyer, in his *Treatise of the Asthma*, published in 1698, sets forth, in the quaint language of that olden time, both the alteration which Laennec thought he had been the first to describe, and the mode in which it takes place. His observations respecting the lungs of horses are equally applicable—and he no doubt intended to apply them—to the human lungs. After speaking of "the broken wind, from the rupture or dilatation of the bladders of the lungs, by which the air is too much retained in the bladders, or their interstices, and thereby produces a permanent flatulent tumour"—and stating that "these horses wheeze much after filling their stomachs, by water or food, because that keeps up the diaphragme"—he goes on thus. "As it happens in external flatulent tumours, they at first go off and return, but at last fix in permanent flatulent tumours; so it is in the flatulent asthma, the frequent nervous inflations induce at last a constant windy tumour or inflation; and it ought to be considered how far holding the breath in hysteric fits, or the violent coughing in long catarrhs, or the great distension of the lungs by an inflammation in the peripneumonia, may strain the bladders and their muscular fibres, and thereby produce the same rupture or dilatation or hernia as happens in the broken-winded. This must be observed by the help of the microscope; and if the air blown into any lobe will not be expelled thence by the natural tone or muscle of the bladders, that the lobe may again subside of itself, 'tis certain some injury is done to the ventiducts; the bladders are either broken, and admit the air into the membranous interstices, or else they are over-distended like a hernia in the peritoneum; and this will produce an inflation of the whole substance of the lungs, and that a continual compression of the air and blood-vessels, which will produce a constant asthma." Really this is a capital piece of pathology for the seventeenth century.

It is, at first sight, a matter of surprise that vesicular emphysema of the lung, and dilatation of the bronchi, do not more often go together. Sometimes, indeed, the smaller branches of the air-tubes do partake of the dilatation of the cells: but this is not commonly the case. Still, the mechanism of both diseases appears to be, in the first instance, the same. The detention of *mucus* in them leads to dilatation of the *bronchial tubes:* the incarceration of *air* in *them* to enlargement of the pulmonary *vesicles.*

Consecutive disorders.—Vesicular emphysema may arise then, and receive increase from, various disordered conditions that precede or accompany it, and of which it is the effect. On the other hand, it is often itself the cause of subsequent disease, not merely in the lung, but in other parts; and, above all, of disease in the right chambers of the heart. The smaller blood-vessels, as I have shown you, are gradually effaced as the dilatation of the air-cells proceeds; the emphysematous lung is evidently in a state of comparative anæmia, and incapable of admitting all the blood which is due to it from the pulmonary artery. In other words, the right side of the heart does not empty itself with its accustomed ease. Hence increased muscular contractions of the right ventricle; and a yielding of its walls to the augmenting pressure of the contained blood. And this embarrassment of the circulation in the right side of the heart is aggravated at those periods when the paroxysms of urgent dyspnœa occur. Now nothing is a more sure cause of anasarca than a permanent dilatation of the right cavities of the heart: and this influence of the emphysematous lung upon that organ is clearly seen in the palpitations to which such patients are liable, and in the œdema of the feet and ankles which often becomes manifest at the same time.

I must beg you to bear in mind that emphysema of the lung is one, and a very common, cause of *asthma.* The asthma so arising is less perilous than that which proceeds from certain other organic changes, to be described hereafter. Vesicular emphysema, indeed, in its simple, uncomplicated form, is seldom attended with much danger. When it proves fatal, it is so in consequence of the superaddition of some other disease. Laennec states very truly that of all the varieties of asthma, this is the one which affords the patient the best hope of a long life.

Treatment.—The condition that I have been describing, when once it has fairly been established, can scarcely admit of a cure. We shall do our patients good, not by any

treatment addressed to the existing emphysema itself, but by guarding them against those circumstances which are likely to aggravate it; and by mitigating or removing those other disorders with which the emphysema is apt to be combined. Whatever is calculated to put the patient out of breath is bad for him. It is observed that they who, having emphysema, are obnoxious also to catarrhs, during which the dyspnœa is singularly increased, are much more free from such attacks in the warm weather of summer, than in the winter. This explains the beneficial influence of a judicious change of climate upon such persons, and it points to the necessity of warm clothing in the colder seasons for those who are obliged to remain in this country. The feet especially should be kept dry and warm; and the liability to catarrh may be sometimes diminished by the use of the cold shower-bath, in the way I formerly recommended. During the fits of extreme dyspnœa, you may hear the expiratory wheeze remarkably loud and protracted; and if, withal, you hear any small crepitation, indicative of pneumonic inflammation, you will do well to cup the patient between his shoulders. This will always give relief to loaded lungs, whether there be inflammation present or not. But the great assuager of the dyspnœa in this disorder is opium; and especially opium combined with æther. Half a drachm of Hoffman's anodyne, with a third of a grain of the acetate of morphia, in camphor julep, will operate like a charm often, in quieting the whole system, and removing the difficulty of breathing. This circumstance would lead us to suppose that the access depended, in part at least, upon a spasmodic state of some of the muscles concerned in respiration. To this question I shall revert when I speak of asthma as a separate disease. At any rate you will find that some such formula as I have just mentioned will stand you in good stead when you have to deal with asthma engrafted on emphysema. And I may add, that this is a case of exception to the rule I formerly laid down. You need not be deterred from giving a full dose of opium by the blueness, which is temporary, of the patient's lips and countenance.

Interlobular emphysema.—The *interlobular and sub-pleural* emphysema of the lungs is a species of *true* emphysema, the air being contained in the meshes of the common cellular tissue. When it appears on the surface of the lung, it may be distinguished from the bladder-like prominences which sometimes form there by the dilatation of the air-cells, in this way; the bullæ which are situated in the cellular tissue connecting the pleura with the lung, may be made to move hither and thither under pressure; whereas those which result from the protrusion of an enlarged cell or cells cannot be made thus to change their place. This sub-pleural effusion of air is sometimes enormous. I have seen it as large as a hen's egg. Bouillaud mentions a case in which the bladder or pouch was equal to the size of a stomach of ordinary dimensions. It proceeds, I presume, from the rupture of a superficial air-vesicle. Sometimes, as I mentioned before, the *pleura also* gives way, and air is poured into the cavity of the thorax. More commonly the membrane remains entire, and then these large bubbles of air may be seen upon the surface of the lung.

Of interlobular emphysema I can give you but little account except from the observations of others. I have never seen more than one well-marked example of it. The lobules of the lungs cohere together by means of cellular tissue, which is dense and close in the natural state, but which admits of considerable expansion when it is inflated with air. If the emphysema be slight in degree, the lozenge-shaped spaces visible on the surface of the lung are defined by little bubbles of air, that look like beads strung upon a thread. But in extreme cases the lobules are fairly blown asunder by the air; the partitions between them increase in width, and are said to be sometimes as much as an inch broad. They are broadest towards the surface of the lung, and narrower towards its roots; and exhibit somewhat of the arrangement seen in the section of an orange, the septa radiating and diverging from a centre. If the cellular tissue could be taken out, there would be left cracks and clefts in the lung. When the interlobular emphysema penetrates to the roots of the lung, the air readily passes into the cellular tissue of the mediastinum, and thence to the subcutaneous cellular tissue of the neck and chest—and then we have the genuine emphysema of authors who wrote prior to Laennec.

There is this material difference between vesicular and interlobular emphysema; that the one is slow and gradual in its formation, the other sudden. The permanent dilatation

of the air-vesicles is the work of time. They yield, and lose their elasticity, and break into one another, only by degrees. The interlobular effusion of air may be effected in a few minutes or seconds. It is caused by *violent* straining efforts; such as those made by a woman in child-birth, or by any one who exerts himself to lift a weight which is too much for him. A deep inspiration is taken: then the glottis is voluntarily closed, and a strong expiratory effort is made. Some rupture must take place and form a communication between the air-vesicles and the cellular tissue; but such rupture has never been *traced*, nor is it likely that it should be.

They say that this form of emphysema is revealed also by large dry crepitation; why it should, is not evident. I can only say of that sound, as I said before: it may exist, and it may be distinguishable from large *moist* crepitation, but my ear is not delicate enough to distinguish it: and to speak the truth, I doubt exceedingly whether any such sound really occurs at any time. But do not let my doubts infect you: try for yourselves; and till you have had opportunities of investigating this point, consider it as *adhuc sub judice*.

Again, they say that the noise of friction denotes the existence of interlobular and subpleural emphysema. On this point I can give you no information of my own knowledge. That you may sometimes hear the costal pleura rub against the pulmonary during inspiration and expiration, I know; I have often heard that sound (as I mentioned to you before) when the membrane has been roughened by pleurisy; but that a soft, smooth, moist lung, though embossed by emphysema, will give rise to a rubbing sound, I do *not* know. It may be so, but it has never occurred to me to hear it.

We may be more certain that interlobular emphysema has arisen when, immediately after some violent straining effort, considerable dyspnœa and oppression ensue, and presently the subcutaneous cellular tissue becomes emphysematous. You will understand how rapidly the inflation of the cellular tissue may take place if you ever saw a butcher blow up that of a calf which he is in the act of skinning.

As interlobular emphysema differs from vesicular emphysema in its seat, and in the suddenness of its formation, and in some sort also in its cause, so does it differ in its curability. Under favourable circumstances it will soon cure itself—the air will be reabsorbed, and the dyspnœa cease. I do not know that we can do much by art to accelerate that process. If the dyspnœa be extreme, it will be relieved by blood-letting; and if the air makes its appearance, and can be felt, crackling, beneath the skin, you may let it out by making a few punctures with a lancet, and the deeper-seated emphysema will be lessened as the air escapes. I believe that this interlobular emphysema is more common in infancy than in any other part of life; on account, I suppose, of the greater delicacy and tenderness of *all* the tissues at that age.

Œdema of the lungs.—The interstitial cellular tissue of the lungs, as well as the air-cells, is liable to be filled not only with air, but with serous fluid; and this constitutes *œdema of the lungs;* a condition which is by no means uncommon, and one of which you ought therefore to be aware; but it need not long occupy our attention at present. When a lung, or a portion of lung, is anasarcous (and you will often find that the œdema is limited to the most depending part of those organs), it is generally of a pale gray or yellowish tint; is heavier than healthy lung, and less crepitant; and pits more on pressure —is *doughy*. And if the œdema is extensive, the lung does not collapse when the chest is laid open. When incisions are made into the lung in this state, a thin watery fluid flows out, more or less spumous; and if the lung be well squeezed, the whole of the liquid may be expressed: and then it will be obvious that the texture of the organ is sound, but that it had previously contained less air than usual, in consequence of the presence of the watery fluid.

This condition of the lung seldom takes place except as a part of general anasarca: and we may discover its existence, first by noticing that there is dropsy of the cellular tissue in other parts; secondly, that the patient has dyspnœa; and thirdly, by hearing crepitation, produced by large bubbles, at the lowermost portions only of the lungs. Into those portions the liquid gravitates; just as it sinks into the ankles when the patient sits up or walks about. There is still air in the œdematous portion; so that percussion still

gives a hollow sound: as hollow at least on the one side as on the other. With the air there is also liquid, which transudes, I suppose, from the cellular tissue, or is exhaled from the surface of the membrane: and the liquid is from time to time coughed up and expectorated. Sometimes, however, there is but little expectoration. What does come up is chiefly aqueous, with occasionally a piece or two of mucus floating upon it; and it is somewhat foamy also.

This œdema or anasarca of the lung is symptomatic of other disease; generally of disease of the heart or great blood-vessels: and it is capable of no other rational treatment than such as is suited to the original disorder; and therefore I have nothing further to say about it now.

Phthisis Pulmonalis.—I proceed, in the next place, to that most prevalent and lamentable disease of the lungs, which is well known to everybody, under the titles of *pulmonary consumption*, and *tubercular phthisis*. And without pausing to make any general reflections, respecting facts which must be familiar to you all—the fatal and almost hopeless character of the disease, and the havoc it produces among the young, the most gifted, and the most beautiful, of the human race—I shall commence by inquiring into the *morbid anatomy* of tubercular phthisis; which will naturally introduce us to the consideration of its symptoms, causes, treatment, and general history.

Phthisis, you know, means a wasting away, or a consuming; but of late years the term has been restricted to that *species* of wasting disease, which consists in the occupation of the lungs by tubercular matter, and the changes which that matter *suffers* and *works*. But it would be an error to suppose that the disease is restricted to the *lungs* in these cases. The lung disease would be sufficient at length to destroy life: but its mortal tendency is aided and accelerated, in most instances, by disease of a similar character, situated in other organs. "The *pulmonary* consumption (as Dr. Latham justly observes) is no more than a *fragment* of a great constitutional malady." But that malady plays its part most conspicuously in the lungs. I shall notice its complications as I go on; but I am desirous of cautioning you in the outset against supposing that tubercular phthisis is *exclusively* a pulmonary disease.

Before I proceed to a more particular description of the changes that, in the progress of consumption, are wrought in the lungs, I must briefly recall to your recollection certain points, relating to tubercular disease in general, which were brought before you in an earlier part of the course. The formation of tubercles is closely linked with the existence of the scrofulous diathesis. Tubercles themselves are composed of unorganized matter, deposited from the blood, of a yellowish colour, opaque, friable, and of about the firmness and consistence of cheese. This is what all pathologists agree in regarding as the true tubercular matter. It is most commonly deposited on the free surface of mucous membranes; and not unfrequently among cellular tissue. You will remember that tubercles are not necessarily, as some have supposed, of a round shape. Their form depends upon the nature of the tissue in which the tubercular matter is deposited. Wherever it is laid down, it is liable to increase in quantity by the continual accretion of fresh matter of the same kind. Hence, when a speck of this peculiar matter is deposited in any soft uniform tissue—such as the brain, or the cellular membrane—there being no inequality of pressure from any quarter, it preserves a spherical or globular form as it grows larger. But taking the lung, with which we are at present chiefly concerned, the round form is sometimes real, sometimes apparent only. It is real when the tubercular matter fills up, or lines, and therefore takes the shape of, the pulmonary vesicles. So it is when a number of these, contiguous to each other, coalesce by the increase of deposit and compose one large globular mass. And you may often catch the tubercles, if I may say so, in the process of forming these large rounded masses; *i. e.*, you may see them arranged in circular groups or clusters, the interstices between them becoming gradually smaller and smaller. But when, as is often the case, the tubercular matter is laid down in the smaller ramifications of the bronchi, it assumes a cylindrical shape. This you may ascertain by carefully following the branching of the air-tubes: but in the manner in which the lung is usually divided by the scalpel, you *see* merely sections of these cylinders; and then the round form is apparent only. If the tubercular matter comes to fill one of these smaller air-

tubes, and also all the vesicles to which that tube conducts, then the new substance, when fairly displayed, represents a branch, with a cauliflower termination; like a twig with a bunch of leaves at its extremity. You may see these appearances delineated, from nature, in Dr. Carswell's admirable lithographic drawings.

If this account of the formation of tubercles, as explained by Dr. Carswell, be the true one;—of which I entertain little doubt;—it will follow, as a matter of necessity, that no alteration *can* take place in the tubercular matter, after it has once assumed the solid form, except through the agency of the parts around and in contact with it. No change can originate in the inorganic tubercle itself.

Besides this true and undisputed species of tubercle, you will often find the lungs more or less thickly studded with a number of small granules, of firmer consistence, almost as hard as cartilage, semi-transparent, and of a bluish-gray colour. Respecting the nature of these granules—which are sometimes called *miliary tubercles*, sometimes the *granulations of Bayle*, who first described them—many different opinions are entertained. Laennec considered them to represent the incipient stage of the opaque yellow substance; and he calls them accordingly *nascent* tubercles. Andral believes that they are simply some of the pulmonary vesicles rendered solid and hard by chronic inflammation. Dr. Carswell explains their formation in this way. The membrane lining the air-passages secretes from the blood, not only the matter of tubercle, but its own proper fluid; whence it sometimes happens that a dull yellowish point of tubercular matter becomes enclosed and set, as it were, in a small pellet of gray, tough, semi transparent mucus. Whatever may be the true theory respecting these little bodies, it is certain that they acknowledge some intimate connection with the true cheesy tubercle. They both occur in the same persons, in the same lungs, in the same parts of the lung. One very seldom occurs without the other. They both belong essentially to the disease we are considering—pulmonary phthisis. Louis, a minute and faithful observer, states, that the granules present, at a certain period of their development, a central opacity. Upon the whole, it seems probable that Laennec's doctrine, in regard to the relation subsisting between the gray semi-transparent granule and the yellow opaque tubercle, was well founded.

He was wrong, however, in some other points, especially in his statement that the softening of tubercles begins in their *centres*. Dr. Carswell has shown how the *appearance* of a central softening arises, first, in the smaller tubercles; secondly, in the larger agglomerated tubercular masses. The morbid secretion is deposited, principally, upon the mucous surface—upon the inner lining of the air-cells, and of the bronchial tubes communicating with them. Now it may so accumulate as to *fill* those cavities; and then, sections of them will represent the crude tubercles of Laennec. But it may only *line* the cavities: it may leave a central vacuity, containing mucus or other secreted fluids; and if the lung be cut across under these circumstances, the divided air-cells will look like *rings* of tubercular matter grouped together; and so also will the divided bronchial tubes. We then have the *appearance* of tubercles with central depressions, or soft central points; and Laennec was deceived by those appearances.

But the larger masses begin also, he says, to soften at the centre. True: we do find the process of softening going on at several points within them at the same time. The masses, you must bear in mind, are formed by the growing together of many smaller tubercles; and the cellular tissue, with the other tissues which originally separated these tubercles, still exists, though it ceases to be visible. At length, under the augmenting pressure, or some other influence, it suppurates; and in this way the tubercular mass is broken down. Now this is the very process by which the tubercles are at length, often, expelled from the body. They increase till the surrounding parts take on inflammation, either from the increasing pressure, or from some accidental cause. The inflammation thus excited, occurring in scrofulous persons, has the scrofulous character. The thin pus which it throws out pervades and loosens the tubercular deposit; a process of ulceration goes on in the surrounding textures; and, at last, the softened scrofulous matter is gradually coughed up and expectorated.

This explanation of the mode in which the tubercles are formed, and increase, and soften, and are removed, has been given to the world within the last few years, by Dr. Carswell; and it is much the most simple and probable of any that I have seen. It is, moreover,

perfectly consistent with the best ascertained facts concerning the progress of tubercular disease. You will find numberless theories broached by different authors on this subject if you like to look for them; but I do not think you will find any so satisfactory as Dr. Carswell's. And having recalled these things to your memory (for it is some time now since I mentioned them before) we may go on to the further consideration of the morbid anatomy and pathology of tubercular phthisis.

It is a remarkable and very important fact, that tubercles, when they affect the lungs, are not deposited at random, or indifferently in all parts of those organs. It is in the upper lobes, and in the upper and back parts of those lobes, that, in nineteen cases out of twenty, and in more than that proportion, we meet with tubercles when they are few. It is in the same part that they are largest and most numerous, when they are scattered throughout the whole lung. It is here, also, that they first ripen, and grow soft, and become ready for expulsion through the bronchi and trachea: consequently, it is here that we have the most frequent, the most numerous, and the largest excavations in the lung—what are technically called *vomicæ*. And the number and magnitude of the tubercles and of the vomicæ gradually diminish from the summit of the lungs downwards.

Now these are not merely *curious* facts: they have a most important bearing upon the diagnosis, in cases that might otherwise be doubtful. It is a rule which has but few exceptions—just enough to establish its claim to be a rule—that the favourite habitat of pulmonary tubercles is the upper part of the superior lobes of the lungs; and I may remind you that the converse of this is true (though with more numerous exceptions) of common inflammation of the lungs. Pneumonia affects by preference the lower lobes; it does sometimes commence in the upper, but that is comparatively rare. When it occupies the superior lobes it generally has arrived there by travelling upwards from the inferior. You will at once perceive the practical advantage of knowing these points of contrast.

It is a curious fact also—less practically useful, perhaps, than the former, but still a valuable fact—that the left lung is much more obnoxious to tubercular disease than the right. Modern observers have collected numerical statements showing that this really is so. Why it should be so, I know not. Thus Louis, whose volume is the result of immense labour in observing, and is full of the most instructive matter, had met with *seven* cases in which tubercles were confined to a single lung; in *two* of the seven cases it was the right lung that was thus exclusively affected, in *five* it was the left. Of 38 instances in which the upper lobe was totally disorganized by the disease on one side, 28 were of the left, and only 10 of the right. Eight times he had known the pleura perforated by the extension of tubercular disease; and seven times out of the eight the perforation happened on the left side of the chest. So also Reynard met with 27 cases of pneumothorax on the left side, to 13 on the right. No less curious is it that here also the facts ascertained with respect to pneumonia are just the contrary of those which belong to phthisis. I mentioned, in a former lecture, Andral's conclusion, derived from the observation and collation of 210 examples, that pneumonia is more than twice as common on the right side as on the left. M. Lombard, of Geneva, found the ratio somewhat less than this, but still great. Of 868 instances of pneumonia, 413 occurred on the right side alone, 260 on the left alone, and 195 on both sides at once. That is, there were three on the right side alone, for every two on the left alone.

The tubercular matter then, being deposited on the mucous surface of the vesicles, and of the small bronchial tubes that conduct to them—groups of these diseased air-cells, lying contiguous to each other, become, more, or fewer of them amalgamated, or fused, as it were, into one large mass: and generally there are tubercles of various sizes, from that of a pin's head, to that of a pigeon's egg, in the same lung. And there is yet another disposition which the tubercular matter is apt to take: it sometimes is diffused uniformly over a considerable space, occupying all the cellular and interstitial portions of the part affected, and having no distinct circumscribing boundary. The part looks as if fluid tubercular matter had been poured into it, and there had hardened. This is what the French call *tubercular infiltration* of the lung.

The tubercular matter, once deposited, may remain for a longer or shorter time in what is called the crude state; surrounded by perfectly healthy lung, undergoing no increase in quantity, and no alteration of consistence. But in a vast majority of cases, scrofulous

inflammation is sooner or later set up around the tubercles—or in the cellular tissue imprisoned within the agglomerated masses—and then the whole breaks down in the way I mentioned before; and the detritus is conveyed through one or more tubes into the primary divisions of the bronchi, and thence to the mouth, to be expectorated. Of course there is an excavation, cavity, cavern, or vomica, left. All these names are given to the void space which the tubercular matter previously occupied. Now there are some curious particulars to be mentioned respecting these cavities; but I must postpone them till our next meeting.

LECTURE LVI.

PHTHISIS, CONTINUED. VOMICÆ; ADHESIONS OF THE PLEURÆ; ULCERATION OF THE LARYNX AND TRACHEA—OF THE INTESTINES; FATTY LIVER; AUSCULTATORY SIGNS OF A VOMICA: GURGLING, CAVERNOUS RESPIRATION, PECTORILOQUY; GENERAL SYMPTOMS OF PHTHISIS: COUGH, EXPECTORATION, DYSPNŒA, PAIN, HECTIC FEVER, DIARRHŒA, WASTING, ŒDEMA, APHTHÆ.

WE were engaged, when we separated yesterday, in investigating the *morbid anatomy* of consumption. Bear in mind how and where the tubercular matter, which is the essence of that disease, is deposited in the lungs: that it occupies by preference their upper lobes, and the upper part of those lobes; invading gradually the lower lobes, from above downwards, as the disease advances. Both lungs are, commonly, affected at the same time, though in unequal degrees. Among one hundred and twenty-three instances of phthisis, Louis found that the tubercles were limited five times to the left lung, and twice to the right. The tubercular matter, once deposited, may remain quiet and unchanged for some time; but in general it increases in quantity, until at length inflammation, of a low and scrofulous character, arises in the pulmonary substance in immediate contact with the tubercles—or in the cellular tissue involved in the larger agglomerated masses—in consequence of which inflammation a sort of suppuration takes place: the tubercular matter becomes soft, and breaks down, and is ultimately expelled through the bronchi, trachea, and mouth. The vacuities left in the lung after this process of expulsion are called cavities, excavations, caverns, vomicæ. And I go on to consider certain points of practical interest connected with these vomicæ.

In the first place, as you may see by the specimens upon the table, they vary greatly in size. They may be no bigger than, or not so big as a pea: or they may be large enough to contain a pint or more of fluid. Sometimes the whole of the upper lobe is converted into a bag of this kind. These large cavities are never met with in the lower lobe. They are formed by the union of several that are smaller; so that they are often of very irregular shape, and divided, as it were, into chambers, by imperfect partitions, or by bands which cross them in various directions. Opening into the cavity there is always one, and there are generally several, pervious *bronchial tubes*, which seem as if they had been cut off just where they enter the cavity. But you never, or very seldom indeed, find a *blood-vessel* thus opening into the cavity. And the reason of these differences is plain enough. It is not, as some modern authors have fancied, that the arterial or venous tissue possesses a low degree of vitality, and therefore resists or avoids the destruction in which the surrounding tissues are involved. That principle may be applicable to other cases, but it is not applicable to this. The opinion I am now referring to proceeds on the supposition that the bands which sometimes cross the cavities are really blood-vessels that have escaped the disorganizing process. Such seems to have been the notion entertained by Bayle; and it has been more recently and more expressly advanced by Cruveilhier. But the truth is, that these bands rarely *contain* blood-vessels: and when they do contain them, the blood-vessels are mostly *impervious*. In one hundred and twenty-three cases, Louis found pervious blood-vessels in the bands no oftener than five times.

The true reason why bronchial tubes open into these cavities, and blood-vessels do not, is to be found in the natural differences between the two sets of vessels, in respect to their structure, and in respect to the fluids that pass through them. The blood-tubes yield readily to external pressure. Many of them are pushed aside and flattened by the progressive increase of the tubercular matter around them; some probably are obstructed by its gradual accumulation within. In either case the stagnant blood coagulates, and the vessel is obliterated to some distance from the place of the original obstacle; just as you know a clot forms, and seals up an artery, which has been tied during life, some little way anterior to the ligature. But the bronchial tubes are neither so easily compressed, nor do they carry any coagulable fluid. In the agglomeration of the tubercular masses, by the softening of which the cavities are formed, the air-tubes included within the mass are filled up by it; and when the whole breaks down in scrofulous suppuration, they are expectorated with the rest of the detritus. Meanwhile their open mouths, on the hither side of the point where the tubercular matter stopped, remain, and afford a channel through which the same matter, after it has become soft, finds its way towards the trachea. Occasionally, indeed—but that, I repeat, is a rare occurrence—a considerable blood-vessel *does* get laid open during the formation of a vomica, and then copious and fatal hæmorrhage ensues. Occasionally also an oozing or exhalation of small quantities of blood takes place from the inner surface of the cavity, tinging the matter expectorated.

When the vomica is first formed, by the expulsion of the tubercular matter, its inner surface is soft and ragged; and if other softening tubercles are in the immediate neighbourhood, the cavity goes on enlarging; that is, two or more vomicæ coalesce. If, however, there happen to be no more tubercles thereabouts, the cavity may remain stationary. Its inner surface then becomes smoother; and something like a membrane forms upon it: and sometimes a puriform fluid is poured out by this surface, and sometimes not. Generally the pulmonary tissue around such a cavity is condensed and solidified; partly perhaps by crude tubercular matter which it contains, partly in consequence of the inflammatory process of which it has been the seat during the softening of the tubercles. It is important to bear in mind this fact of the condensed, solid state of the lung immediately surrounding a vomica; for it explains certain peculiarities met with in the symptoms.

There is another point of much interest connected with these vomicæ. When they occur singly, without other tubercles or cavities (which, though a very rare thing, does sometimes happen); and when occurring thus singly they have been completely emptied of the tubercular matter; they may gradually contract, and ultimately become obliterated. This approach of their sides leads to a puckering of the pleura on the surface of the lung: and, on the other hand, a puckering of the surface indicates that beneath it there is probably a collapsed vomica. Its inner surface becomes converted in that case into a substance resembling cartilage: and the appearance it presents is called a cicatrix: and really it deserves that name. The process which has gone on is a process of natural recovery: and the recovery would be complete, if no fresh deposit of tubercular matter took place. Too often, however, the tubercles multiply, until at length their number, or size, or effects, become incompatible with the further continuance of life.

This, then, is one way in which tubercular disease, *limited to one small* portion of the lung, *may* be eliminated, and the part which it occupied undergo a kind of repair. But the disease, *when so limited*, *may* cease in another way. The more watery parts of the morbid secretion may be absorbed; and the earthy salts it contains may concrete; and the whole be converted into a shrivelled, hard, chalky mass, which sometimes is coughed up, sometimes, in favourable cases, remains for years in the lung, an inert and almost harmless body.

Let me state, while I think of it, that the expectoration of these chalk-like concretions, denoting, as it usually does, the existence of pulmonary consumption, marks at the same time the chronic character of the case. I am acquainted with a gentleman who, though delicate, enjoys a very fair share of health, and who has for years been coughing up, at intervals, little branching fragments, like bits of white coral, consisting principally of carbonate and phosphate of lime, and evidently moulded in the smaller bronchial tubes.

When the tubercles are numerous—or rather when they lie near to the surface of the lung, as, of course, they are likely to do when they *are* numerous—they very generally

give rise to dry or adhesive pleurisy. So that, in a person dead of consumption, it is a very rare thing to find the lungs free from adhesions to the ribs. I mentioned before that this attachment of the lung to the walls of the chest, affords a protection against a much more formidable condition; namely, perforation of the pulmonary pleura, and the escape of tubercular matter and air into the serous cavity: producing that worst kind of pleurisy which constitutes pneumothorax. That the pleurisy and adhesion are consequences of the presence and irritation of the tubercles, appears from this:—that, for the most part, the extent and the situation of the adhesions correspond with the extent and situation of the tubercular disease. Thus, a single spot of adhesion has been seen to unite the costal and pulmonary pleuræ exactly opposite a solitary tubercle which lay near the surface of the lung. As the summit and back part of the upper lobes are most thickly set with tubercles, so here also is the adhesion the most constant, and the most firm. You will often find the upper part of the lung invested with a thick cap of false membrane; and the connection between the pleuræ so tough, that the lung breaks down in the attempt to separate them.

To show you that these statements—which have long been familiar to those much conversant with disease and with morbid anatomy—to convince you that they are strictly borne out by numerical or tabular facts, I may again have recourse to Louis. He tells us that in 112 cases which he himself examined of persons dead of consumption, and having therefore tubercles in their lungs, there was but one in which both lungs were free from adhesions. In eight cases the right lung was exempt from them; and in seven cases the left. Again, in twenty-five other instances, there were either no cavities, or very little ones; and the adhesions were accordingly of small extent, and easily broken down. In the remainder there were large vomicæ, and the adhesions were extensive, dense, and firm.

Such is a sketch of the changes which take place in the *lungs*, in consequence of the deposition of tubercular matter in them, and of the changes which that matter undergoes. But the air-passages that lead to the lungs are very liable to become implicated as the disease proceeds. The mucous membrane of the larynx and trachea ulcerate; and when the morbid condition of the larynx gives rise to prominent symptoms, and especially (as it is apt to do) to hoarseness and loss of voice, the disease is sometimes called *laryngeal phthisis*. But there is no such disease, that I know of, existing by itself. I mean, that *scrofulous* ulceration of the larynx and trachea occurs only when the lungs are affected with tubercles. It is curious that when ulcers are met with in the trachea, they are often found on one side of it only; on the side, viz. which corresponds with the diseased lung, or with that lung which is most diseased. In like manner, when some of the bronchi are found red internally, and even ulcerated, these appearances are confined to those bronchi which communicate with cavities: and do not occur in the bronchial canals leading to crude tubercles. It is towards the back part also of the trachea that the ulcers are generally observed; the floor of that channel when the patient lies supine. And when the epiglottis is involved in the mischief, the ulcers are situated, almost always, on its laryngeal surface alone. We have strong reasons therefore for believing that their formation is influenced by the contact of the matter which is expectorated, in its frequent passage over the mucous membrane. Moreover the little mucous glands wherewith the membrane is provided, are most numerous at the posterior part of the trachea and bronchi; and these glands are especially prone to ulcerate.

In respect to these points also Louis has made comparative observations. Among one hundred and eighty persons who died of some chronic disorder, *not* phthisical, he once only met with ulceration of the larynx; whereas of those who perished of consumption, as many as *one in every five* had ulceration of the epiglottis and larynx, and nearly *one in three* had ulceration of the windpipe. Hence it would appear that, if we except the effects of the syphilitic poison upon the larynx, ulceration of that part is almost peculiar to phthisis pulmonalis.

I have told you that consumption is not merely a lung disease. Its local ravages are most obvious indeed in the thorax; but it leaves in the abdomen also traces of its destructive agency not less definite, and scarcely less constant. You know that the surface of the intestinal canal is strewed, throughout, with separate mucous follicles; and that

the lower portion of the ileum is furnished with other follicles, collected together in oval or circular groups. When I come to speak of continued fever, I shall have much to say about the changes which these little glandular bodies undergo, in one form at least of that disease. These same glands are the frequent seat of tubercular deposit in phthisis. Here and there you may see a solitary yellow tumour, not larger than a hempseed, projecting from the surface of the bowel. In other places, the ripened little tumour has burst, the tubercular matter is gone, and a ragged roundish ulcer remains. More frequently the aggregated glands are affected; and the ulceration, in them, varies much in form and extent. It often involves the whole patch, and then the shape of the ulcer is more or less elliptical. Louis met with ulceration of these glandulæ agmiantæ in five-sixths of all the fatal cases of phthisis that he examined. Ulcers, of greater magnitude, were very nearly as common in the large intestines. And it is worthy of notice that, the disorganizing process being in these cases slow, nature has time (if I may use such metaphorical language) to provide against the threatened perforation of the gut. The tissue that forms the base of the ulcer, whether it be the muscular or the peritoneal coat, is thickened and vamped; or the bowel becomes adherent to some contiguous surface. Once only in my life have I known scrofulous ulceration, in phthisis, penetrate the serous tunic, and allow the contents of the intestine to escape into the sac of the peritoneum.

It is fitting you should know—although the facts possess, as yet, no practical value—that the stomach is often much enlarged and thinned in those who die of consumption; and that the liver is very apt to undergo a remarkable change, almost peculiar, I believe, to that disease. It, too, enlarges, and becomes full of adipous matter, greasing the hands and scalpel of the anatomist, and yielding when heated an oily substance, which makes a grease-spot on paper placed in contact with it. The whole gland partakes in the alteration, is of soft consistence, loses its natural red tint, and assumes a pale fawn-colour. No profession contributes so much as ours to the introduction of new, barbarous, and dissonant words into the English language. We have accordingly invented an epithet of this kind of liver. We call it (not the *fat*, but) the *fatty* liver. In three years Louis met with this fatty liver forty-nine times: and forty-seven of the patients died phthisical. It occurred in more than one-third of the whole number of the victims to consumption; whereas, among two hundred and twenty-three cases, not phthisical, there were two examples only of this hepatic change. Its presence is revealed during life by no symptoms, except that the enlargement belonging to it may sometimes be ascertained by percussion and pressure with the fingers.

Auscultatory sounds.—Let us now inquire what modifications of the healthy sounds arise from the altered conditions of the lungs in phthisis. Most of them are such as you would naturally expect. Whether a portion of lung be rendered solid by common inflammation, or by the presence of tubercles in it, the result, so far as the auscultatory signs are concerned, will be the same. In such a piece of lung, supposing the solidification complete, no vesicular breathing can be heard; but bronchial breathing and bronchophony will be audible, in each case, if the solidified portion encloses a considerable bronchus, and comes near the surface of the chest. And percussion will give a dull sound, whether the lung lying beneath the part struck be hepatized, or blocked up by tubercular matter. On these points, therefore, after what was said in a preceding lecture, I need not dwell. But the excavations, the empty or half-empty vomicæ—these are something new. We have hitherto met with no condition exactly similar to that of a *large* cavity. And accordingly I have to make you acquainted with two or three new sounds: or sounds which are modifications of those formerly described, and in most instances sufficiently distinct from them to have acquired peculiar names. You will remember that what we have called large crepitation depends upon the passage of air through liquids: the liquids being contained in tubes; those tubes the bronchi and their ramifications. But when pus or vitiated mucus, or liquid of any kind, is collected in a *vomica*, which communicates freely with the trachea through pervious bronchi, the bubbles produced by the entrance and exit of air will be still more numerous and large; and a sound is then produced which the word *gurgling* expresses well. Laennec calls it *gargouillement*. This sound is heard, too, in a circumscribed space; and not diffused, as large crepitation usually is. Wherever, therefore, we hear gurgling during respiration, or during the act of coughing, there, we conclude, exists

a *cavity*. But the cavity is not necessarily a *vomica*. In ninety-nine cases out of a hundred it will be so; but in the hundredth case perhaps it will not. Bear in mind what was formerly stated of dilatation of the bronchi: how sometimes they terminate in a considerable globular expansion; sometimes belly out and contract again several times alternately: and you will see that cavities containing liquid, or liable to contain liquid, belonging equally to the one condition and to the other, and the sound in question depending solely on the intermixture and agitation of air with liquid in a cavity, we cannot be sure from *mere* gurgling respiration, or gurgling cough, that we have a *tubercular* excavation beneath our ear; or that the case is one of consumption. Gurgling may also proceed from that very rare morbid condition, abscess, the result of common inflammation of the lung. These constitute the only sources of fallacy in the matter. The fallacy seldom interposes; but it does sometimes interpose; and therefore it must *qualify* our conclusion from this symptom of gurgling, in respect to cases otherwise doubtful.

Again, the vomica may be empty of liquid; and then we hear, as the patient respires, not vesicular breathing of course, nor yet exactly bronchial breathing; it is something more than that when the cavity is large, something different in character from it when the cavity is small: but whatever the character of the sound, as we believe it to take place in a vomica or cavern, we call it *cavernous respiration*. It is a hollow sound, especially when the cavity is of considerable size; an exaggeration of mere bronchial respiration. But the cavity may be small. The moment a portion of tubercular matter is separated and discharged through a neighbouring bronchial tube, the cavity has commenced; and the sound produced in these little cavities during the breathing may be of various kinds. It may be, and it often is, a click, like the opening and shutting of a valve; or a chirp; or a creaking; or like many other well known sounds; but, as all these sounds, under certain circumstances, denote the formation of a vomica, it is best, for simplicity's sake, to call them all by the same name—cavernous respiration.

Dr. Latham explains in a few words the causes of these differences. "The varieties of cavernous breathing are doubtless owing to different sizes, and forms, and situations of cavities, and to different conditions of the surrounding lung. A cavity may be very large or very small. Several bronchi may open into it, or only one. It may be a simple cavity, or it may have many chambers. Its sides may be condensed and equal, or rough and ragged. The lung around it may be solid and indurated, or pervious and vesicular. It may be near the ribs, or far from them: adherent to, or separate from the pleura. It is quite obvious that these different circumstances are calculated to modify the sound, which will, nevertheless, be always such as indicates a cavity."

A tubercular cavity may be *so* large, and of such a kind, as to yield the metallic sounds which are apt to be heard in pneumothorax. I show you a cavity in which those sounds were actually heard, most distinctly, by many persons, during the patient's life, while he was under my care in the Middlesex Hospital. I was certain beforehand that these sounds proceeded, not from the cavity of the pleura, but from a tubercular excavation. Once subsequently, in a patient who was dying of phthisis and diabetes, I have heard the same sounds; also, I am sure, in a tubercular cavity. The patient insisted on leaving the hospital, and I lost sight of him before he died.

I promised when speaking of these metallic sounds as arising (as they much more commonly do) from pneumothorax—a collection of air, or of air and fluid, in the sac of the pleura—I promised to point out the circumstances whereby you may tell which of the two conditions in question the sounds denote. Both of the conditions imply, in general, the existence of tubercular phthisis; and therefore the observation of the ordinary symptoms of phthisis will not help us much.

Now, in the cases seen by me, there were two circumstances that stamped them as being cases in which the sound proceeded from a cavity *in* the lung, and not *exterior* to it. One was the *situation* in which the sound was *invariably* heard. The other was, the absence of excessive resonance when that part was percussed. You know that when the air is contained in the pleura itself, the sound yielded on percussing the chest in the corresponding spot is quite tympanitic, like that of a drum. But it is a well-ascertained fact, (though contrary perhaps to what you would suppose,) that the sound is duller over tubercular cavities, in nine cases out of ten, than over sound lung. The explanation of this fact is

simple enough. It is that the layer of lung which still remains in such cases, thick or thin, is dense and solid, and damps the sound which the vomica might otherwise make resonant. But then again the situation of the metallic sounds was a guide. They occupied the upper part of the chest: the very part where vomicæ are wont to be the most common, and the largest: and moreover a part where pneumothorax seldom or never exists. The summit of the lung is generally covered in phthisis with a cap of false membrane, which binds it to the ribs: and this, as I observed before, is the main reason why perforation of the pleura pulmonalis is so rare in that disease; and it is also the reason why, when it does take place, it seldom takes place at or near the apex of the lung. In truth it is found by experience that (though the rupture of the pleura *may* happen in any part) the place where it usually occurs is in the lower and back part of the upper lobe of the lung, opposite the angle of the third or fourth rib; that is, just beneath the edge of the false membrane by which the summit of the lung is generally adherent. But the sound, in the cases I refer to, was *invariably* heard at the very top of the chest. It did not shift, as that of pneumothorax may often be made to shift, when the patient changed his posture. Attention to these points will always lead you to an exact diagnosis. You may say, perhaps, "The complaint being in each case a mortal one, what is the use of so much refinement?" Why, there is this utility in it. We may sometimes, as I stated before, give great relief to the patient, and save his life for a time, by tapping the chest in pneumothorax. The air may get in with each inspiration, and threaten immediate suffocation; and the thorax being punctured it will issue in a blast. But no one would think of tapping a tubercular cavity.

To give out the amphoric resonance and the metallic sounds, the vomica must, I presume, be a *large* one. That which is before you, the only one I ever *saw* in which those sounds had been heard, is very large. Its inner surface is smooth: it adheres to the ribs externally by at least two-thirds of its circumference: and the medium of adhesion is very thin. Quite low down, a single bronchial tube, of about the third division, may be seen to enter it.

So much, then, for the modification by a tubercular cavity, of the sounds heard during *respiration*. But the *voice* will also be modified, if the cavity be of considerable size, and near the surface, and have dense walls, and be empty. Then we hear, in that part, when the patient speaks, the sound which is called *pectoriloquy:* as if the voice proceeded from the chest. The words are distinctly articulated into the ear of the listener. But I need not trouble myself or you by attempting to *describe* pectoriloquy. You may any day hear the exact sound that word is intended to denote, by placing a stethoscope over the trachea of one of your friends, applying your ear to the other end of it, and getting him to speak: just as you may obtain an exact notion of bronchial respiration by listening then to his breathing.

For some time after the first appearance of Laennec's great and original work on the diseases of the chest, pectoriloquy was deemed to be the pathognomonic and infallible sign of a vomica. "Oh," the young auscultator would say, "I detect pectoriloquy beneath the clavicle. There can be no further doubt about the nature of the disease. My patient has not only tubercles, but a cavity, in his lung." So I long thought; and so some, I fancy, think still. Yet the evidence afforded by mere pectoriloquy of the presence of a vomica, or even of the presence of tubercles, is far from being certain or trustworthy. Experience had taught me this before I knew that many others, studying under the same schoolmaster, had learned the same truth. Among my hospital patients some years ago was a man who laboured under phthisis. Percussion gave a dull sound under his right collar-bone, and in the same spot loud and distinct pectoriloquy was audible. I well recollect inviting the particular attention of the pupils to this case, as affording an exquisite specimen of pectoriloquy; and I predicted very confidently that after the patient's death, which was obviously at hand, a large excavation would be found in the summit of his right lung. My prediction did me no credit. The *left* lung indeed was hollowed by cavities, but there was nothing like a cavity in the right. The upper part of the lung was thoroughly and uniformly solid; filled with hard, gray, tubercular matter. The large bronchial tubes were pervious, and the voice descending into them had been conducted by the solid lung with perfect and almost painful distinctness to the listener's ear. This was a

useful lesson to me: and I mention it that it may be a lesson to you. Remember that solidification of the summit of the lung will modify the sound of the patient's voice, very much in the same manner as a large vomica there situated. It is stated, indeed, and perhaps truly, that a practised ear can discriminate between the loud, diffused, though articulate, resonance of the voice produced by solid lung, and the circumscribed, whiffing, pectoriloquy of a cavity. But the distinction is too nice for the average of practitioners. Now since the pulmonary tissue may be rendered dense and solid by other causes than tubercles, pectoriloquy does not always indicate the existence of consumption. The fallacious condition does not often occur; for common inflammation is seldom limited to the upper part of the lung; and the whole of that part is seldom completely hardened by crude tubercles. But whenever it does occur, it is apt to mislead or puzzle. I was consulted last year about a gentleman in whom this phenomenon of pectoriloquy was strongly marked. Two excellent auscultators had been led, by this symptom, to the belief that a cavity existed in the lung. Remembering the case I have just mentioned, and learning that the patient had been ill for a few days only, and had not previously suffered cough, nor any apparent pectoral complaint, I was of opinion that the summit of his right lung had become hepatized by acute pneumonia. And it was so. The patient died; and the diagnosis I had formed was verified upon the inspection of the lung. Dr. Latham relates one or two examples to the same purpose. Dr. Stokes goes so far as to consider pectoriloquy the least important and most fallacious of all the physical signs of phthisis. Taken alone (he says) it is absolutely without value. Dr. Forbes has come to similar conclusions. Certainly cavernous respiration is a much more alarming sound.

Wherever actual pectoriloquy from a cavity is heard, there also will be heard cavernous respiration. But the converse of this is not necessarily true. There may be, and there often is, cavernous respiration and a cavity, yet no pectoriloquy. The cavity is not large enough, or not near enough to the surface of the chest, or not of such a kind as to reverberate the voice.

Often when pectoriloquy is absent, and cavernous respiration is doubtful, and gurgling even cannot be heard (because the communication with the bronchi is not free), a slight splashing sound will occur when the patient coughs: nay, you may sometimes hear it, if he holds his breath, with every beat of his heart, which causes a little succussion in the cavity: but its contents must then be thin.

Now when the sounds I have been engaged in describing are well marked, they denote the existence of a vomica. The only source of fallacy is that which I formerly mentioned: the same sounds will arise from a cavity in the lung, whatever be its nature: and therefore they will arise when the bronchi are expanded into cavities. But I repeat, that this is a deceptive condition which you cannot calculate upon meeting with often.

When the sounds are not well marked, take time before you pronounce a decided opinion respecting them. Strong bronchophony comes very near to weak pectoriloquy: bronchial respiration may closely resemble some varieties of cavernous breathing: large crepitation, confined to a small spot, may simulate gurgling. It is better, when the sounds are thus equivocal, and when they may denote conditions so very different in their nature and tendency, to suspend one's judgment, and to give a guarded opinion. A little time in such cases will clear away the doubt.

I am afraid of being tedious about these sounds; but really they are of immense importance. Upon their exact appreciation, and correct interpretation, will depend the opinion you will be *called upon* to express; and that opinion will, in many cases, be a sentence of life or death in respect to the dearest friends of those who hear it. A correct diagnosis is also very important, in the early periods of the disease especially, for another reason. It is in those early periods alone that we can hope to arrest the progress of the complaint by art, or by change of climate.

General symptoms.—I must now consider the *general* symptoms of this most afflicting disease: and while doing so, I shall point out how the physical signs confirm or confute their language, in cases which might otherwise be doubtful.

The general symptoms of phthisis are cough, dyspnœa, expectoration, hæmoptysis, wasting, hectic fever, hoarseness, or loss of voice, diarrhœa; and there are some other symptoms which mark often some of its stages, and to which I shall incidentally advert. I shall speak of them all as briefly as is consistent with clearness.

Cough is one of the earliest symptoms of consumption; and it is that which commonly first attracts the attention, and awakens the fears of the patient or the patient's friends. Generally at first it is slight, occasional, and dry: it occurs upon the patient's getting out of bed in the morning; or if he makes any unusual exertion in the course of the day. It feels to him as if it was caused by irritation about the throat. Sometimes it will cease for a while, as in the warm weather of summer, and recur in winter when the external temperature is lower. By degrees it begins to be troublesome in the night: and to be attended with more or less mucous expectoration.

Now when such a cough steals upon a person gradually, and when no reason can be assigned for its occurrence, that circumstance alone is enough to excite suspicion as to its true nature and cause. But chronic cough may exist without any tubercular disease of the lungs; as you well know. It may depend upon a disordered state of the *stomach;* the *pneumogastric* nerve may be irritated *there.* It may be the cough of *chronic catarrh;* it may result from disease of the *heart;* it may be the nervous, barking, importunate, cough which I formerly mentioned as of frequent occurrence in *hysterical* girls. And bearing these circumstances in your mind, you will inquire, and you will generally make out without much difficulty, whether there be any unnatural or deranged state of the digestive organs; or chronic catarrh; or cardiac disease; or hysteria. These are points on which I need not further insist.

I may observe, here, that as chronic cough may exist when there is no consumption; so consumption may sometimes exist, and even prove fatal, and large portions of the lungs may be disorganized, without there having been any cough; or at least without the occurrence of enough cough to draw the notice of the patient or his friends to it. This is not common, however: cough is usually present, more or less, during all the stages of phthisis, and it is often that symptom which most distresses and harasses both the patient and his family.

Great attention used to be paid to the *expectoration* in cases of suspected phthisis. It was thought that if a patient spat pus, he was in a state of confirmed consumption: and whole volumes have been written, and prizes awarded to their authors, respecting the means of distinguishing pus from mucus. But we now know that, so far as the diagnosis of phthisis is concerned, this is a very idle inquiry. The inflamed bronchial membrane may secrete pus; so that the presence or absence of pus in the sputa is no test at all of the presence or absence of tubercles in the lungs. If you are, nevertheless, curious to know how pus may be recognized, one easy criterion is that proposed by the late Dr. Young. You are aware that pus, like the blood, contains globules; and these globules, when examined through transmitted light, will exhibit prismatic colours; appear surrounded by rings of colours, somewhat resembling those of the rainbow, but differently arranged, and often beautifully brilliant. Mucus, having no such globules, affords no such coloured rings. The way to make the examination is, to put a minute quantity of the fluid between two small pieces of plate glass; to hold the glass close to the eye; and to look through it at a distant candle, having a dark object behind it. A yet readier, and I believe a better test is furnished by the *Liquor Potassæ,* which converts pus into a viscid stringy mass, while it liquefies mucus.

Whether the expectoration be puriform or not, has ceased, however, to be a question of much importance as regards the diagnosis of phthisis. A portion of the matter expectorated comes from the surface of the bronchi, and consists of altered mucus: and therefore, the sputa brought up in phthisis, and the sputa brought up in bronchitis, are, in a great degree, the same: a part of it consists of a stringy transparent fluid, in which opaque masses of a yellow or greenish colour are seen to float; and intermixed also with which there may be a good deal of froth. The froth is a measure of the difficulty with which the mucus is brought up: and it is usually less abundant and conspicuous in phthisis than in bronchitis. The heavy, sage-leaf sputa that we sometimes see, belong to both diseases.

You may occasionally find portions of tubercular matter in the expectoration; a circumstance quite decisive, when we are sure of it, of the nature of the case: dull yellow streaks, or little curd-like fragments involved in the mucus. But small opaque specks of that character are sometimes formed in the follicles of the tonsils; and this makes the appearance more equivocal. The sputa *most* characteristic of tubercular disease consist of

globular flocculent masses, which look like little portions of wool more than any thing else. *Nummular* sputa the French call these, because when spat into a vessel not containing water, they assume a flat circular form, like a piece of money, and remain separate and distinct from each other. When they are spat into a glass of water, you will see that some of them subside to the bottom—some float on the top, suspended, apparently, by healthier mucus in which they are entangled, or by bubbles of air—and some remain stationary at different depths. When stirred and agitated in the water, they render it slightly milky. This kind of expectoration commonly marks a confirmed and advanced state of the disease; but it will continue for weeks sometimes. It is not *perfectly* pathognomonic, but *nearly* so. On one occasion I found expectoration of this nature from a man whom I did not very diligently examine by my ear; and I set the case down as one of phthisis chiefly on the observation of that symptom. The patient evidently had not long to live. Our apothecary at the hospital, Mr. Corfe, had more time to explore the condition of the chest: and he came to the conclusion, that the disease was not tubercular phthisis, but extensive chronic bronchitis: and sure enough he was right. When we came to examine the lungs after the patient's death, not a tubercle could be found. I am satisfied that there is no kind of expectoration which indicates phthisis with *perfect* certainty; but that which I have just been describing very *seldom* occurs except there is phthisis. Louis appears to have noticed these round, separated, woolly masses *once* only unconnected with tubercles; and once the same thing has occurred to Chomel: so that, when the other symptoms are obscure and doubtful, this will materially augment the gravity of the prognosis. Flies appear to be more attracted by the sputa of phthisis than by any other.

Hæmoptysis is a kind of expectoration; the expectoration of *blood*. I have already spoken of this symptom as connected with phthisis, and have stated my belief on that subject; viz., that if a person spits blood who has received no injury of the chest, in whom the uterine functions are healthy and right, and who has no disease of the heart, the odds that there are tubercles in the lungs of that person are fearfully high. Excluding cases of amenorrhœa, and of mechanical injury to the thorax, Louis did not meet with a single example of hæmoptysis among twelve hundred patients, except in such as were phthisical.

I touched, at the same time, upon the question, whether hæmoptysis, which sometimes precedes for a while the manifestation of any other symptoms of consumption, is ever really the *cause* of it, as the old authors maintained. You will understand my persuasion to be that, occurring in connection with tubercles, pulmonary hæmorrhage is always the *consequence*, and never the *cause*, of their presence in the lung. Andral relates a curious case, from which the contrary opinion might be argued. "A man, ill of chronic peritonitis, had been for nearly two months in La Charité, and had never presented any morbid symptom which had relation to the organs of respiration. He had no cough, and he breathed easily. One evening, for the first time, he suffered some dyspnœa; and in the course of that night he spat up a large quantity of florid and frothy blood. For the five following days the hæmoptysis continued abundant, then it diminished by degrees, and at length stopped. But the patient continued to cough, and to breathe with difficulty, and at length he died. In the right lung there were found several masses of a brownish-red colour, exactly circumscribed, and constituting, in short, that condition which Laennec has called 'pulmonary apoplexy.' One of these masses contained a considerable number of granulations of a yellowish-white colour, and having all the characters of minute tubercles in an early state. Two other of the red masses contained each a very small number of these white granules; and in the remaining masses no tubercles at all could be discovered, nor was there any trace of them in other parts of the lungs; but they were numerous in the false membranes of the peritoneum."

Andral argues, that in this case the partial collections of blood that were found in the lung could not have been *occasioned* by the presence of tubercles, because in the majority of these masses no traces of tubercular matter could be perceived. On the other hand, their existence appears *connected* with that of the apoplectic masses, because, except in the midst of some of these, no pulmonary tubercles could be seen. But such a case as this hardly bears out the conclusion that pulmonary hæmorrhage is ever the cause of tubercles. There were tubercles in the abdomen before; therefore, the disposition to

tubercular disease pre-existed in this individual, and then tubercular matter was deposited in the places where blood was extravasated; just as we know it is deposited in the blood itself, in the spleen sometimes; or, what I think more probable still, the cluster of granulations provoked the hæmorrhage from the spot they occupied, and other lobules of the same lung became blocked up by the reflux of blood, in the manner formerly explained.

Dyspnœa is not a very important symptom in phthisis. It is seldom extreme till towards the termination of the disease, and not always then. Patients who fear, and yet are unwilling to believe, that they are consumptive, will fetch a deep breath, and bid you remark how thoroughly they can distend their lungs; and they expect you to say that there can be no disease in those organs. I have been told that the late Dr. Baillie died of pulmonary phthisis; and that even he was accustomed to delude himself by this test. However, though phthisical persons do not in general suffer much from dyspnœa, their breathing, though they may not be aware of it, or choose to acknowledge it, is generally, in some degree or other, short, or hurried. You may wonder that a disorder in which so large a portion of the breathing apparatus is often effectually spoiled, should be attended with so little distress in respiration; so little dyspnœa: but your surprise will be diminished if you consider the insufficient manner in which consumptive patients are nourished, in consequence of abdominal disease; and the extent to which their blood is wasted by diarrhœa, and by perspiration. The mass of blood is thus kept down to that measure which, passing through the still pervious portions of the lungs, is capable of being arterialized without any great deviation from the ordinary mode and frequency of breathing.

Neither is *pain* of the chest a very important symptom in consumption. In some cases severe pains are complained of, resembling those of rheumatism; in the sides, or beneath the clavicles. In others, no pain at all is experienced. When sharp pain occurs, it may be supposed that the pleura is inflamed and beginning to adhere in the painful part.

There is, however, one contingency of which the two symptoms last mentioned are sometimes very significant. When, during the progress of phthisis, violent pain of the side, and extreme dyspnœa and anxiety, set in *suddenly*, they denote, with much certainty, perforation of the pleura, and its serious consequences.

The *hectic fever* which accompanies phthisis is of much greater moment. It often creeps upon the patient insidiously. He feels chilly perhaps, towards evening; and in the night his hands and feet will be dry and burning; and in the morning he perspires. The most marked symptoms of the hectic are to be found in the perspiration, and in the state of the pulse. The perspiration is usually out of all proportion to the previous chilliness and dry heat. It seems to have a close connection with the *sleep* of the patient: it seldom comes on while he continues to be awake; but after sleeping he wakes, and finds that he is sweating. The perspiration is generally most copious on the upper part of the body, the chest and head. Sometimes it is moderate; sometimes the patient is drenched and drowned in it. There is a good deal of uncertainty about this symptom, and of obscurity as to its cause. Generally speaking, it belongs to the more advanced stages of phthisis; but occasionally it accompanies its early periods. It will cease without any apparent cause; and recur again with the same capriciousness. A poor friend of mine, who died of phthisis, and was particularly harassed by the nocturnal perspirations, took it into his head that *posture* had something to do with them; and slept for several nights in succession *sitting* in an easy chair: and during those nights he certainly did not sweat, though he had been doing so profusely before. Louis found that one patient in ten escaped this symptom.

This is a symptom which is often very distressing to the patient, making him even dread to go to sleep; it tends also to the rapid exhaustion of his strength; and betokens, it is believed, when copious or persistent, a short duration of the disease.

Frequency of *pulse* is a symptom so generally present in tubercular phthisis, that too much importance has been ascribed to it as a diagnostic sign. I mean, it has been too much the opinion that the lungs are safe, when the pulse does not rise above its natural standard. Sometimes it remains steady nearly up to the period of dissolution. Such cases are, I believe, generally slow in their progress. Very recently I lost a friend whose lungs were full of cavities and crude tubercles. He had been a valetudinarian for years; but the pulmonary disorder had been manifested by decided symptoms during a few months only. At no period did his pulse exceed sixty-eight beats in the minute. Commonly, however,

the pulse is habitually above ninety; and often it is much more. When there is nothing to account for this increased frequency of pulse, it is a suspicious symptom.

Diarrhœa is a common, and an ugly symptom in phthisis. When it occurs early, as it sometimes does; when a patient having habitually costive bowels, becomes habitually relaxed; and you *suspect* only, from other causes, that he may have incipient phthisis; this change often sets its seal upon the nature of his disorder. Usually, however, diarrhœa does not become urgent until the disease is far advanced, and has already declared itself by other and unequivocal symptoms. When it so occurs, it is apt to harass the patient exceedingly; and rapidly to waste his strength and flesh. He appears to melt away under the influence of the purging; which is therefore said to be *colliquative*. It used to be held that the diarrhœa and the perspiration bore an inverse ratio to each other: that when one of them abated, the other always increased. But the more exact observations of Louis and others have proved that this is not so: that neither in phthisis, nor in other diseases, have these symptoms any such regular reciprocal relation. One reason, perhaps, for this error, may be found in the circumstance, that acids, which have the effect often of checking the perspiration, tend also, as is well known, to produce diarrhœa. Louis found that this symptom began early in the disease, and continued through its whole course, in one out of every eight patients; and in one case only in every twenty-eight was it wholly wanting. It depends most commonly, if not always, upon scrofulous ulceration in the small intestines and in the colon. In Louis's experience there were, invariably, *large* ulcers, whenever the diarrhœa had been chronic and abiding, and the stools had been numerous. In the small intestines the ulceration evidently commences in the mucous follicles; the glandulæ solitariæ, or the glandulæ agminatæ: and sometimes, though not often, the ulcer perforates the bowel. It is probable that in the large intestine ulceration begins in the same way, by the deposit of tubercular matter (which is subsequently removed) in the solitary glands: but when once begun the ulcerating process extends itself indefinitely to the surrounding mucous membrane.

I should have stated before that, with this disease of the intestinal canal, there is often found enlargement of the corresponding glands of the mesentery, which are frequently filled also with tubercular matter.

Several of the symptoms that I have been mentioning—the state of the digestive organs, which interfere with the due assimilation of the food, the drain implied in the profuse sweats, and in the habitual diarrhœa—conduce to cause another constant accompaniment of phthisis: and that is *emaciation*. You know that the wasting in this complaint, when it is not cut short by some accidental complication before it has reached what may be called its natural termination, is extreme. It often is one of the earliest, as it is one of the most alarming, of the symptoms which the patient presents: and it frequently becomes excessive before any perspiration or purging have taken place to account for it. If, without any apparent cause, a person grows thin and weak, and his pulse is quick, and his breath at all short—these are intimations which seldom prove unfaithful, that tubercular disease is at work in the lungs, and in the abdomen.

Œdema of the ankles, and even some puffiness of the hands and face, are circumstances which seldom fail to appear in pulmonary consumption: but they are among the latest of the symptoms. Œdema does not tell us what the disease is in such cases. We have been satisfied as to that some time before. But (unless there is some marked disease of the heart) it tells us that the disease is about to terminate. It is worth attention as a prognostic symptom merely.

And the same may be said of aphthæ. This is one of the last of the symptoms: but it does not always occur at all. I have lately described this morbid condition of the mucous membrane of the mouth and tongue, and have nothing more to say of it at present. It has the same relation to phthisis as to other chronic disorders: it marks, for the most part, the approach of their fatal termination.

It is always interesting to couple changes of structure with their appropriate signs. I will therefore take this opportunity of telling you what Louis has observed of this relation, in respect to the larynx and windpipe.

Ulceration of the epiglottis was often latent; gave no appreciable signal of its existence. The symptoms that belong to it are, a raw, or pricking, or burning sensation at the upper

part of the thyreoïd cartilage, with occasional dysphagia, and the rejection of liquids through the nose, while the tonsils and pharynx present no visible alteration.

Ulceration of the interior of the larynx is marked, when slight, by trivial pain in that part, and some variation from the natural voice; when deep, by severer pain, and abiding aphonia.

Ulceration of the trachea is seldom revealed by any symptom. And this is worth remembering; for patients are continually persuaded by medical men who know no better, that their symptoms are all *tracheal*.

There are still a few other circumstances which, when they occur, accumulate conviction as to the nature of the disorder. The catamenia are suspended in women: and the hair falls off. There are certain physical peculiarities too which are strongly indicative of a tendency to consumption; or perhaps I should say of the scrofulous diathesis. Largeness of the pupil, with a sluggish iris—in other words, a not very sensible retina—constitutes one of these. A clubbed state of the ends of the fingers, with convex and adunque nails, forms another. Yet this last is not peculiar to tubercular consumption. I have heard of one case in which it was strongly marked: the patient died after a long illness—chronic puriform discharge from the pleura after paracentesis thoracis: but there were no tubercles.

LECTURE LVII.

PHTHISIS, CONTINUED. DIAGNOSIS. FORMS AND VARIETIES OF PHTHISIS. ORDINARY DURATION. AGE AT WHICH IT IS MOST FREQUENTLY FATAL. INFLUENCE OF SEX; AND OF OCCUPATION. QUESTION OF CONTAGION. TREATMENT.

IN a former lecture, the twelfth of this course, I entered somewhat fully into the *pathology* of scrofulous and tubercular diseases in general. I pointed out the fact, that though such diseases affect vast numbers of persons, and are most extensively fatal, yet that they affect almost exclusively certain *classes* of persons. That while some are so prone to tubercular disease, as to fall into it upon the operation of the slightest external causes, or even spontaneously—nay, in spite of every care to the contrary—others again, who are constantly exposed to influences likely to call scrofulous disease into action, either do not suffer therefrom; or if they do become scrofulous, it is only when the external circumstances most favourable to the production of such disease have been intense in degree and protracted in their application.

At the same time I showed you how commonly the *disposition* to scrofula descends in families: and I told you what observation has collected in respect to the *causes* which may *excite* scrofulous disease in persons *hereditarily disposed* to it. I shall not, therefore, go over that ground again. What I then said of scrofulous disease in general is true of tubercular consumption in particular. I will merely remind you that these exciting causes are essentially causes of *debility*. Whatever tends to depress the vital powers, and permanently to weaken the body, tends also, in a predisposed frame, to engender or to call forth this fearful and most destructive malady.

Diagnosis.—With respect to the detection of tubercular disease in the lungs, it is sometimes very easy, sometimes extremely difficult. It is easy when the tubercles are numerous, large, or far advanced: difficult, sometimes, when they are crude, scanty in number, and thinly scattered, and individually small. In the latter case they may not cause any appreciable deviation from the natural resonance of the chest on percussion, or from the natural smooth equable rustle of the breathing. It would be tedious to travel over again all the auscultatory and other symptoms, with the view of pointing out their bearing upon the diagnosis. I touched upon that point incidentally, when discussing the individual symptoms, in the last lecture. Many of the symptoms tell their story so plainly that any attempt

to expound or interpret them would be quite superfluous. One or two cardinal points, however, which have rather been hinted at before than expressed, I may just advert to.

The fact that tubercles occupy the *upper part* of the lung by preference, is of great moment in relation to the diagnosis. When the symptoms are equivocal; when, as far as *they* are concerned, the case may either be one of chronic bronchitis, or of tubercular consumption; a careful examination of the superior regions of the chest will often decide the anxious question. The sound resulting from the first gentle tap upon or beneath the clavicle, often rings in the physician's ear the knell of his unfortunate patient. Even unusual distinctness of the sound of *expiration*, if heard at the summit of the lung, and *à fortiori* if at the summit of one lung only, warrants the terrible suspicion that tubercles are breeding in that lung. It may, indeed, be laid down as a rule, which is diversified with but few exceptions, that if you find dulness on percussion; or coarse or insufficient breathing; or undue resonance of the voice; or a click or morbid noise of some sort when the patient respires, or speaks, or coughs; if you find this day after day and always, between the clavicle and the mamma in front, or between the clavicle and upper edge of the scapula, over the top of the shoulder, and nowhere else; and more especially if these deviations from the healthy sounds be limited to one side, or greater on one side than on the other, or different in quality on the two sides; you may set the case down as a case of tubercular phthisis. On the other hand, if in the same parts you still distinguish all the natural sounds of the respiration, and can still obtain a clear sound on percussion, you are not to condemn the case, nor to despair of recovery, whatever its other circumstances may be. The worst symptom certainly, when auscultatory signs are wanting, is hæmoptysis.

Incipient consumption is most liable to be confounded with chronic bronchitis. Yet the leading features of the two are well contrasted. The morbid sounds belonging to chronic bronchitis are chiefly audible in the lower lobes of the lungs: those of phthisis in the upper. Some degree of expectoration attends the cough of bronchitis from the first: the cough of phthisis is often, for a long while, dry. Simple bronchitis is not accompanied by hæmoptysis. The pain that occurs in bronchitis is felt beneath the sternum: in phthisis pain most commonly affects the sides, and the space between the shoulders. It is enough, I trust, to have drawn your attention to these points, without dwelling upon them longer.

Forms and varieties.—Dr. Latham, in the little work which I have several times referred to, has laid down certain distinctions most deserving of your notice in respect to the various *forms* of phthisis. This portion of his book is quite original. The facts indeed have long been known; but they have never before, that I am aware of, been made so instructive, by being clearly disposed, and exhibited in their proper bearings.

He first divides phthisis generally into two forms, which he calls *mixed* phthisis and *unmixed* phthisis. And he illustrates what he means by those terms very simply and skilfully. He takes the case of an absorbent gland in the neck, affected with scrofulous disease. The changes which are liable to take place in it are wrought before our eyes: we have the privilege of watching them. Now such a gland will sometimes enlarge, in consequence of the deposition of tubercular matter in its substance: it will grow large and hard without there being any pain, or heat, or redness, observable; and it may remain in that state for weeks, or months, or years.

But in the majority of instances the absorbent gland, after remaining for a certain time in this condition, will undergo, and give rise to, other changes. Pain, heat, and redness, will ensue; the hard gland will soften; the integuments will grow thin, and at length give way; the softened tubercular matter, mingled with pus, will escape; and then the pain and heat and redness—the inflammation, in short—will disappear; and the abscess will heal, leaving behind it nothing more than a slight scar. This process may happen to one such gland, or to more than one *simultaneously;* or to several in *succession.*

In this case there has been no more inflammation than was just enough to accomplish its purpose of removing from the body the tubercular matter. The inflammation has not transgressed what Dr. Latham has called its specific limit.

But again, it may go beyond that limit; it may be both more severe and more extensive than is necessary for the removal of the tubercular matter in the diseased gland. It may

pervade the whole neck, giving rise to diffused redness, and swelling, and pain; and the whole of the subcutaneous cellular tissue between the angle of the jaw and the clavicle may be loaded with effused serum and pus.

All this you may see almost any day in the wards or waiting-rooms of a hospital. And Dr. Latham has happily chosen this affection of the cervical glands to elucidate what happens when the tubercular matter is deposited in the *lungs*, where one cannot *see* the changes it is suffering or producing.

Tubercles in the lungs may remain for an indefinite period of time, in their crude state; never softening at all, or softening only at a very late period. Or they may give rise to just so much of inflammation, and no more, in the pulmonary tissue surrounding them, as is sufficient to bring about their own softening and subsequent expulsion. Or, lastly, the tubercles may excite much more inflammation of the lung around them than is requisite for their elimination: inflammation of every degree, and of any extent.

Now to the first two cases, when they occur, he gives the name of unmixed phthisis: the third he calls, on the other hand, mixed phthisis. We learn from auscultation whether the case be one of mixed or unmixed consumption; *i. e.*, we hear, in the unmixed forms, the sounds or the modifications of sound which result from the presence of tubercles or of vomicæ; and we hear these morbid sounds only: in every part of the lung where they are *not* audible, we hear the vesicular murmur of health. But in the mixed form we also hear these sounds. True, and we hear other morbid sounds beside. The tubercular disease is mixed with common inflammation; and we hear the sounds that denote common inflammation of the mucous membrane, or of the substance of the lungs—sibilus, or large or small crepitation—we hear these sounds *mixing* themselves with the sounds which belong to the tubercular affection.

This distinction is of considerable importance, for it concerns the *treatment* of the malady. The tubercular disease, when established, is beyond our power. The inflammation which is incidental to it we may hope to alleviate or to remove. It is in the stage of vomicæ that the disease commonly assumes the mixed character; and sometimes the bronchial or vesicular effusion upon which the added sounds depend, may be got rid of by the seasonable application of a few leeches, or of cupping-glasses, or of a blister, or by a moderate bleeding from the arm, and the disease be brought back again, for a time at least, within its specific limits; and the patient be relieved from much distress, and imminent danger. It is upon this principle that Dr. Latham explains the fact that most consumptive patients improve considerably, soon after their admission into the wards of a hospital. The poor are necessarily much exposed to those causes which tend to complicate the tubercular disease. The tubercular disease may as yet be slight and limited; but the superadded mischief, the bronchial and vesicular effusion, may be immense; and this being submitted, often for the first time, to treatment, upon their admission to a hospital, is for a while removed.

Now if we had not the advantage of the method of auscultation, we could not ascertain these differences, nor detect them when they existed. You will perceive, I am sure, their practical importance.

Of course the more ready the surrounding lung is to take on inflammation—in other words, the stronger the disposition in the complaint to assume the mixed character—the more rapidly fatal is it likely to be.

But of the unmixed form of phthisis Dr. Latham has made two interesting varieties; and the truth of the distinctions he has drawn will be more manifest to you, the more you see of this terrible disease. In one of these varieties the lungs are apparently tenanted by a multitude of tubercles, which remain crude and unaltered for a considerable length of time. In the other, successive *crops* of tubercles appear to form: or at any rate the tubercles ripen and are expelled in successive crops: and there may be long intervals between each crop and the next.

Dr. Latham thus describes the former of these two varieties:—"An individual loses the complexion of health, and becomes thin; he coughs a little; but perhaps he has no notable fever, and no constant acceleration of pulse." Upon auscultation of his chest it is found that there is dulness beneath one or both clavicles, or about one or both scapulæ, and an indistinct respiratory murmur in those parts; but the vesicular breathing is free and per-

fect in every other part of the lung. Here we have tubercles, crude, and in the upper lobe alone: and this state of things may endure for years, without variation; the patient remaining always a great valetudinarian. "To such a patient (says Dr. Latham) it is a continual puzzle why he does not get well. He consults an infinite number of medical men; and it is remarkable that he gets no comfort or satisfaction from those who understand his disease the best, and the greatest comfort and satisfaction from those who understand nothing about it. Those who know what it is, out of kindness do not tell him the truth, and they cannot asseverate a falsehood stoutly enough to carry any weight with it: whereas they who know nothing about it affirm boldly and unhesitatingly that it *is all stomach:* really believing that the whole and sole disorder is in the stomach, and that it is within the reach of an easy cure."

But at length—perhaps after a very long period—vomicæ *are* formed; and then the patient sinks rapidly, and his lungs after death are found riddled by cavities and stuffed with tubercles; but every part of them not occupied by tubercles or vomicæ is crepitant and healthy. In these cases disease lingers long in the crude stage; new tubercles are added, probably, year after year; but none of them soften. They do not excite inflammation in the lung around them. You recognize the presence of the tubercular matter by the ear; but there are no vomicæ. At last vomicæ are formed, many at the same time or in rapid succession, and the patient presently succumbs.

The other variety of unmixed phthisis may be just as protracted as this; but its character and progress differ materially. In the former case the patient's condition was one of invariable ill health; in the one I am about to mention he has fits of ill health, and fits of comparative good health. He spits for a time considerable quantities of puriform matter, and then ceases from expectorating altogether. He has hectic fever, and then throws it off, and then suffers it again: wastes, and recovers his flesh, and again loses it. You will find such cases common enough; and in these cases the morbid sounds will be correspondent to the symptoms. During the fits of illness you will hear gurgling respiration or gurgling cough at the apex of one or both lungs: and during the fits of good health you will hear cavernous respiration or pectoriloquy in the same parts; but everywhere else you will hear a clear sound of vesicular breathing. Here the tubercular matter excites just enough inflammation around it to achieve its own expulsion, and no more. The lung is destroyed bit by bit. Fresh portions of tubercular matter are deposited; these ripen and soften, and are expectorated, and a vomica is the result: and then there is a period of quiet. And there being still a large portion of each lung to breathe with, the patient regains more health and strength in the intervals of his attacks, than the former patient possessed habitually.

But in this form of unmixed pulmonary consumption, a period at length arrives when the patient does not revert to the former state of apparent health. The quantity of lung that has now been destroyed forbids it. You may hear the sounds proper to tubercular disease over a large space, between the clavicle and the mamma, or anywhere about the scapula, on one or both sides; yet still that part of the lung which is free from tubercles and vomicæ is pervious and healthy: but the hectic continues, the emaciation increases, and the strength declines; and the fatal consummation arrives.

Of these two varieties of genuine and unmixed consumption, the first is the most hopeless. The tubercles are numerous; they probably go on increasing in number though they do not soften; there is not, and there cannot be, any even temporary return to health, either real or apparent.

Whereas when the tubercles come singly, or in successive crops, and rapidly soften, and are expectorated; and where some long time interposes between the crops; the health and strength return, and there is just a chance that no more tubercles may form. It is in this variety of unmixed phthisis that a natural cure, by the contraction and cicatrization of a vomica, may by possibility take place. We cannot expect, and we must not permit ourselves to encourage hope, that the disease *will* cease in that manner: but if it ceases in any form of the malady, it is in this.

This grouping of the different characters under which pulmonary consumption may appear, has been performed by Dr. Latham with perfect fidelity. There is nothing overstrained or fanciful about his sketch; it is after nature; and it is by the hand of a master.

And there is something very refreshing in original views of this kind. Vastly more instructive too they are, than those presented by a dull compilation. I therefore again recommend you to study his little volume. I am sure that *I* have derived much useful and usable knowledge from it: and so also may you.

There is another form still of tubercular consumption which Dr. Latham has not omitted to notice; but his observations on this form are not so new. It is a striking, but not very common form; and it is sure to arrest the attention of the practitioner when it does occur. I have met with three or four examples of it. The phenomena are of this kind. The patient has difficulty of breathing, cough, hæmoptysis perhaps, night-sweats, and much hectic fever: the symptoms in short which constitute the acute phthisis of some authors. But if you listen to the chest, you do not hear the sounds that are peculiar to phthisis: you do not find dulness confined to the upper lobes, or pectoriloquy, or gurgling respiration: but you rather find the superadded sounds which accompany mixed phthisis; small crepitation all over the lungs, succeeded by an absence or deficiency of the proper breathing everywhere. Meanwhile there will be none of the expectoration which is characteristic of phthisis. In short you would not suppose that the disease was phthisis. at all. Yet it must be called such, for after death you find the lungs thickly bestrewed everywhere with what I spoke of before as the granulations of Bayle; nascent tubercles, myriads of them, gray and minute: what many persons call miliary tubercles. The tubercular matter, from some cause which we know nothing of, is thickly and uniformly sown over the whole of the air-passages, or throughout the entire extent of the lungs, and its sudden presence there in such abundance excites inflammation, which masks and conceals the specific disease; and the true nature of the case is not suspected till after the patient's death. All the instances that I have seen—three or four only in number—were supposed by me to be cases of extensive inflammation of the lungs; and so indeed they were, but they were something more. The tubercles, doubtless, were the cause of the inflammation; and not the inflammation the cause of the tubercles.

Ordinary duration.—From what I have been stating you will perceive how difficult it is to say what is the ordinary *duration* of phthisis; concerning which a question was put to me at the close of the last lecture. The disease may be present for some time without declaring itself by any marked or unequivocal symptoms; and therefore without attracting attention. But taking the cases as they occur, and estimating the duration of the malady from the time when it first manifests itself in a decided form, we find there is quite enough of variation to warrant the distinction that has been made by authors between chronic and acute phthisis; or, to use the more popular and more expressive phraseology, between slow and galloping consumption. The following tabular statement of the results observed by Bayle and Louis, will give you a somewhat more precise notion of the general progress and duration of the disease. The whole number of cases noted was 314. Of these 24 died within three months; 69 between three and six months; 69 also between six and nine months; 32 between nine and twelve months; 43 between twelve and eighteen months; 30 within from eighteen months to two years; 12 between two and three years; 11 between three and four years; 5 between four and five years; 1 between five and six years; 3 between six and seven years; 1 between seven and eight years; 3 between eight and ten years; and 11 between ten and forty years.

You will remark that, as far as this account goes, more than one half of the whole number died within nine months from the time when the disease first became manifest. This agrees with the experience of the late Dr. Gregory, of Edinburgh. He used to state that the ordinary duration of phthisis was about six months; that sometimes it lasted only two or three months; and that he had seen one case which proved fatal on the seventeenth day after the symptoms were first observed. On the other hand, he had known one man who was at least 72 years old when he died, in whom symptoms of phthisis first appeared at the age of 18, and who was never free from them during all the intervening period, being often hectic, and frequently spitting blood. The *average* or *mean* duration of consumption has been computed to be about two years. This is a different thing, you will please to observe, from its *ordinary* duration.

There are many other points in the statistical history of phthisis well worthy of attention and inquiry; but I have not time to go into them in any other than a summary man-

ner. This part of the subject is very well worked up in Sir James Clark's lucid and sensible book upon Consumption: but you will have perceived, from the references I have so frequently had occasion to make to M. Louis, that *his* work is the great storehouse or treasury of tabular information, in respect to the facts of tubercular phthisis.

Age at which it is most frequently fatal.—It is an interesting question to determine at what period of human life consumption numbers the most victims. There are two short tables, one drawn up by Louis, containing observations relative to 123 cases, and the other by Bayle, respecting 100—which throw some light on the question. The two tables agree, in the main, very closely. Thus, from the age of 15 to that of 20, Louis met with 11 deaths from phthisis, Bayle with 10: from 20 to 30, Louis met with 39, and Bayle 23; from 30 to 40, Louis 33, Bayle 23; from 40 to 50, Louis 23, Bayle 21; from 50 to 60, Louis 12, Bayle 15; from 60 to 70, Louis 5, Bayle 8. You see from this account how erroneous the common notion is, that consumption does not occur at an advanced period of life: that a person who has reached his thirtieth or fortieth year is thenceforth safe from that disease. From these two tables, and others collected by Sir James Clark, it appears that, taking decennial periods, the greatest number of deaths from phthisis happens between the ages of 20 and 30; the next greatest number from 30 to 40; the next from 40 to 50: and that after these, it is a doubtful matter whether more perish of consumption between 50 and 60, or between 15 and 20, which last is only one-half a decennial period. These calculations refer, as you will remark, to human life after the age of puberty. Before that age, tubercular disease is fearfully common, especially in infancy and childhood. Among 920 children (532 girls, and 388 boys) who died from the age of 2 to that of 15 years, no less than 538 (nearly three-fifths of the whole) were affected, Dr. Papavoine tells us, with tubercles.

Influence of sex and of occupations.—As far as numerical observation has hitherto been extended, it would appear that more women than men die of consumption. Statistical researches are of still greater interest, perhaps, when they elucidate the influence of different trades and occupations in calling phthisis into existence. Sir James Clark has brought together much curious information on this point. There are certain occupations which appear to provoke pulmonary consumption by the direct application of local irritants to the lungs themselves: and there are others which tend indirectly to bring on phthisis, by lowering the tone of the general health: by producing debility and cachexia. But these two causes often go together: and it is difficult to estimate with accuracy their separate effect. The workmen whose employments have a directly irritating operation upon the respiratory organs, are stone-masons, miners, coal-heavers, flax-dressers, brass and steel-polishers, metal-grinders, needle-pointers; and many others who of necessity inhale during their labour an atmosphere loaded with irritating particles of matter. But, then, most of these men work also in towns, and remain for many hours day after day in a constrained position, in crowded or in close apartments. Moreover, some of these occupations, being sedentary, and requiring no great muscular power, are unfortunately selected, for that reason, by persons who are naturally of feeble or delicate constitution. On the other hand, butchers, fishermen, and their families, and farm-servants, are said to be comparatively free from phthisis. Beddoes ascribed this exemption to the use of animal food by these classes; but much of their better health is due, no doubt, to their habits of active exercise in the open air; and to the circumstance that such employments demand a certain amount of bodily strength and energy, and therefore are not likely to be adopted by weak and scrofulous individuals. It is obvious that the whole inquiry is beset with sources of fallacy. We know, however, on the evidence of undoubted facts, that certain occupations do tend to induce pectoral complaints, and to shorten life. Dr. Knight, of Sheffield, informs us that fork-grinders, who are what are called dry grinders, die there of the *grinder's asthma*, or *grinder's rot*, before they are thirty-two years old. Razor-grinders, who grind wet and dry, live a little longer: the moisture diminishes, of course, the number of floating particles of metal. Table-knife grinders work on wet stones, and survive till they are between forty and fifty. I must refer you to Sir James Clark's book for similar facts in respect to the inhalation of silex, of the dust of mines, and so forth. Without pretending to assign to each alleged injurious influence its precise contribution of mischievous effect, we must be content, at present, with the practical inference, that such

employments should, if possible, be avoided by all those who show any tendency to scrofulous disease.

Question of contagion.—Is phthisis *contagious?* No: I verily believe it is not. A diathesis is not communicable from person to person. Neither can the disease be easily (if at all) generated in a sound constitution. Nor is it ever imparted, in my opinion, even by one scrofulous individual to another. Yet in Italy a consumptive patient could not be more dreaded and shunned if he had the plague. And in this country the suspicion will now and then arise that the disease may be infectious. A girl dying of phthisis is nursed by her sister, who afterwards droops and dies of the same complaint. Here the presence of the peculiar diathesis is strongly presumable. But the parties may be different in blood. A wife watches the deathbed of her consumptive husband; and presently sinks herself under consumption: and there may be no traceable or acknowledged example of scrofula in her pedigree. Yet even here the latent diathesis may fairly be presumed to have existed. Very few families are perfectly pure from the strumous intermixture. The predisposition may be slight; it may be dormant for a generation; or, like other inherited peculiarities, it may light capriciously on some individuals only of the kindred. In both the supposed cases there have been other influences at work, more authentic than the alleged contagious property, in calling forth the fatal malady. Watching, the want of rest, confinement in the unwholesome air of a sick chamber; and, above all, protracted mental anxiety, than which no single cause perhaps has more power to foster and forward the inbred tendency to phthisis. The disorder, I am persuaded, does not spread by contagion. Nevertheless, if consulted on the subject, I should, for obvious reasons, dissuade the occupation of the same bed, or even of the same sleeping apartment, by two persons, one of whom was known to labour under pulmonary consumption.

Treatment.—The *treatment* to be adopted, and the plan of regimen to be observed, in respect to tubercular phthisis, resolve themselves into the methods of *prevention* when the disease is *likely to occur;* of *arresting its progress* when that disease is *incipient or limited in extent;* and of *alleviating* the *most distressing symptoms*, when no hope remains of stopping its course, or averting its fatal close.

With regard to the *prevention* of the disease, in those, who, by inheritance, or by circumstances, are predisposed to it, a great deal might be said; but the subject belongs rather to the head of medical police, or hygiene, than to the practice of physic. We deem that a person *has* that predisposition, which is almost a necessary condition of the development of tubercular disease, when we observe those marks of the scrofulous diathesis which I pointed out in an earlier part of the course: or when we know that the parents possess that peculiarity of constitution: or when brothers or sisters have displayed it. It would be well indeed for society if the multiplication, and diffusion, of the strumous diathesis could be checked, by a prudent avoidance of ill-assorted marriages. But we cannot say—no legislature could say—to a scrofulous man or woman, you shall not marry, and propagate scrofula. It is reasonable, however, to conclude, and the conclusion is amply borne out by the observation of facts, that where both parents are strumous, the child will, in all probability, be doubly so; or that, at any rate, its chance of escaping the scrofulous disposition will be small. It is very desirable, therefore, that correct notions on these subjects should be generally prevalent: and that persons who are conscious that scrofula in any of its shapes exists in their family, and *à fortiori* they who know that it exists in their own corporeal frame, should avoid allying themselves with such as are in the same predicament: and this prudence might be enforced if they could be made to foresee the suffering and misery its neglect is calculated to inflict upon their offspring. Intermarriages of persons of the same family, when that family is subject to tubercular disease, are earnestly to be deprecated. But on these points our advice is seldom asked.

We *are* liable, however, to be consulted respecting the mode of *warding off* scrofulous disease in those who have derived an hereditary tendency to it from their ancestors. Now the first, and most effectual prophylactic in such cases, is residence in a warm climate: and next to that is the avoidance of all causes likely to foster the morbid tendency. I need not repeat what I formerly told you on this subject. Pure air; nourishing, but unstimulating food; moderate exercise; early hours; cleanliness; warm clothing; and abstinence from excessive study, from severe bodily toil, and from vicious and exhausting indulgences

of all kinds: these are the topics upon which we must insist, when our advice is sought for respecting the means of preventing consumption in children or others, who are in danger of contracting it.

But when the disease *is present*—when tubercles actually exist, and are ascertained to exist in the lungs—may the progress of the disorder be ever suspended by a change of climate? Indeed I believe it may: but only in certain cases, and in certain stages of the disease. When phthisis occurs in either of its slow and unmixed forms, the question of a change of sky will be worth entertaining. In that form in which tubercles remain long in the crude state, I believe life may be preserved or lengthened by leaving this country, and residing under a higher and more equable temperature, provided that no softening of the tubercular matter has yet taken place. And in the other form—when a vomica or vomicæ have occurred, and the strength is apparently restored, and the remainder of the lungs gives out the sounds of health—in that case also I would recommend a voyage to a milder climate to those persons who could afford to migrate, and to whom it was a matter of importance that they should prolong their earthly existence. I believe there is no place to which such persons could go with more hope of benefit than to Madeira. There are, however, places on our own coast that offer no ineffectual substitute for warmer lands beyond the sea, to those who cannot so conveniently expatriate themselves. Hastings, for instance; the Isle of Wight; and more especially Torquay, on the coast of Devonshire; and Penzance, or its neighbourhood, in Cornwall. In those sheltered spots the patient may sometimes pass the colder weather of our winter and spring months in comparative security. If, however, the lungs are already in a state of rapid disorganization, no benefit, but on the contrary much inconvenience and useless expense, will result from change of place, unless that place in which the patient is residing is notoriously unhealthy. When I am asked about removal, either to another country, or to some distant part of our own, and the state of the patient is such as I have just alluded to, I always advise that he should *not* forego the comforts of his home—and leave his family and friends—to seek advantage which he will not find among strangers, and amid the discomforts of a lodging perhaps, or an incommodious dwelling. I think it cruel, and wrong, to send people away merely to die: and that many are so sent to this place and that, in the almost certain prospect of their never returning, no one, I think, can doubt.

You will find a great discrepancy of opinion among authors, and among practitioners with whom you may converse, with respect to the *regimen* which consumptive patients should follow. One man gives all his phthisical patients beef-steaks and porter; another restricts all his to vegetables and asses' milk: and each will boast, and bring forward most triumphant examples, of the *success* of his system. Now it is quite obvious that for a sick person who receives benefit from the one of these plans of diet, the contrary plan could scarcely be otherwise than injurious; and reason at once suggests that there must be some distinction between the cases that get better under the one system, and those that improve under the other. Doubtless, we must have regard to the constitution and habits of the patient; and sometimes trial alone will show which plan is the most beneficial; but I believe the best clue to lead us out of the difficulty will be found in Dr. Latham's division of phthisis into mixed and unmixed. The object is, to sustain the patient's strength without exciting inflammation in his lungs. If, with the specific disease, there be conjoined an inflammatory condition of the pulmonary substance around the tubercles, or of the bronchial membrane; in such cases an antiphlogistic diet is the proper one. On the other hand when the disease manifests no tendency to transgress its specific limits, then the diet should be generous and full: and it may be so without being over stimulant. Debility, however induced, adds to the disposition to the deposit of tubercular matter; and therefore the debility arising from insufficient nutrition is to be avoided as carefully as is compatible with the other indication, which is, to obviate inflammation of the lung. With these hints, you will be able, I trust, to strike the balance between the risk of augmenting the local mischief directly, on the one hand, and that of depressing the general strength, and so increasing the local mischief *in*directly, on the other. Milk is a sort of animal diet, and it is both nutritious and unstimulating: therefore milk may, in many cases, form the staple of the food, if the patient likes it, and it agrees with him: but there is much variety in this respect in different persons. I repeat, that you will too often find prejudices

entertained, on the one side or the other, in regard to the diet proper for consumptive persons: but the commonest error of the two is, I believe, that of reducing the patient's strength by a needless restriction of his nutriment, lest inflammation should ensue.

Louis, should you refer to him, would be likely to lead you into the opposite mistake: for he affirms, that neither bronchitis, nor pneumonia, nor pleurisy, have any effect in exciting tubercular phthisis. But this opinion is quite opposed to the general sense of most men of experience. Many a case of consumption can be traced back to a severe catarrh, and no further. Many, which ran a short course, were dated, within my own knowledge, from the last visitation of influenza. If M. Louis had meant that thoracic inflammation will not produce tubercles in the lungs of a person who has not the scrofulous diathesis, and that tubercles may and do arise without any previous inflammation, I should quite agree with him. But he draws his conclusions from cases of phthisis. I have no doubt whatever that the dormant predisposition is often awakened into actual disease, and that latent tubercles are often accelerated in their progress, by inflammation of the pulmonary tissues. Whether this happens directly from the local inflammation, or indirectly from its effects in lowering the vital powers, is a question which no one can solve, and of which the solution is not of much consequence. What we are sure of is, that every one who bears a real or suspected taint of scrofula in his frame, should scrupulously guard against every known and avoidable cause of catarrh, pneumonia, or pleurisy. I hold M. Louis's doctrine on this head to be unsound and unsafe: and I mention it only to admonish you against it.

In offering you a few final observations on the *remedies* of phthisis, I shall take leave to abstain from weighing the pretensions of a number of *specifics*, that have from time to time been highly recommended; but which never have come into general use, as they would have done, no doubt, if they had been entitled to such a denomination.

In the first place we must satisfy ourselves as to the kind of case we have to deal with; whether it be mixed or unmixed. We must watch our patient: and keep him on low diet, and take blood, either in small quantities from the arm, or by leeches, or cupping from the chest, whenever inflammatory symptoms arise; whether they are discovered by observation of the general or of the physical signs. The bleedings must of course be small—and palliative only of the symptoms.

Emetics, frequently repeated, have been recommended in the early stage of phthisis: partly on account of their reputed efficacy; partly on theoretical grounds; it being supposed that the tubercular matter may be thus removed from the mucous surfaces as fast as it is deposited. Of the value of this emetic plan, I am unable to speak from any experience of my own.

Counter-irritation is often of undoubted service: mustard poultices to the chest when it is painful; or a blister or succession of blisters, or pustulation by tartar-emetic ointment, to encounter local symptoms. The effect of counter-irritation upon the progress of the tubercular disorder is apparent sometimes by accident. Dr. Abercromby has related an example in which cerebral disease operated in this way; the previous symptoms of phthisis disappearing. In some cases mania appears to have a similar consequence, obscuring the manifestations, and probably retarding the course, of consumption. It has been often remarked—you will find this stated by Sir B. Brodie—that after amputation of a scrofulous leg, phthisical symptoms, very little noticed before, have rapidly increased. And there is another fact, in relation to phthisis, analogous to these, which it is fit you should know and attend to, viz., that the progress of consumption is often suspended by pregnancy:—and when a mother is suckling her child, if the suckling be not too long continued so as to exhaust the mother. I suppose there is no doubt that women disposed to phthisis have been kept alive by successive pregnancies and sucklings. It is a very rare thing for a pregnant woman to die of phthisis. I have known only one instance of it. One of my patients in the hospital, a French woman, died of that disease; and we found suppurating tubercles in her lungs; and a fœtus of about five months in her womb.

Riding on horseback has been strongly advised in the earlier periods of the disease. Its main advantage seems to arise from its allowing the enjoyment of fresh air, and of exercise, without putting the patient out of breath: and these advantages are great. It is affirmed that many phthisical patients remain free from cough, and those affected with hæmoptysis

cease to spit blood, so long as they continue to take exercise on horseback. Gestation in a carriage, or in a boat, has the same good effects, but in a less degree. We are not able, however, to look upon equitation as so certain a cure in consumption as Sydenham did; who says that riding on horseback is as much a specific in phthisis, as the Peruvian bark is for an ague.

Iodine and its compounds, and especially the iodide of potassium, have been much praised of late years, for their reputed efficacy in phthisis. Given in small doses, I believe that they often have a beneficial influence upon the general health. I wish I could tell you that I had ever known them operate a cure of the manifested disease.

Often—too often—all that we can attempt to do is to relieve the most urgent or distressing symptoms: and to make easier the patient's decline. One symptom which is both distressing and weakening is the nocturnal perspiration. The common remedy for this is the dilute sulphuric acid: and a very good remedy it is, but it is not equally adapted to all cases. If the bowels are costive—or if the bowels have not, as they often have, a tendency to be relaxed—then the sulphuric acid may be freely given: and it will often have very good results. It may be exhibited three or four times a day, in doses of from twelve to twenty minims. But when this fails, or when the bowels are irritable and will not bear it, we must have recourse to other means. One of these is sponging the surface of the body, at bed-time, or before the patient settles himself for the night, with tepid vinegar and water: using twice as much water as vinegar. And if the bowels are at the same time purged, I find the compound kino powder of the Pharmacopœia an admirable medicine. It certainly has much power over the perspiration: and it has these further advantages, that (containing opium) it tends to control the diarrhœa, and to calm the cough.

Steel is another substance which exercises a marked influence sometimes over the hectic fever. It was its efficacy in this way that gave celebrity to the famous antihectic mixture of Dr. Griffith, the *Mistura Ferri Composita* of the Pharmacopœia. Certain it is, that when steel is borne in the advanced stage of consumption, it often does a world of temporary good;—but in many cases it is *not* borne well. It increases the cough, occasions headache, and distresses instead of relieving the patient. Nor is it always easy to say beforehand, whether it is *likely* to suit the case or not. I apprehend it will at length be found most applicable to the unmixed forms—the uninflammatory forms, that is—of phthisis. I have frequently, however, succeeded in checking the wasting sweats by the *Tinctura Ferri Muriatis*, given in doses of twenty minims thrice a day, after other expedients had failed me.

When the cough is very troublesome, and especially when it breaks the patient's rest at night, we must endeavour to quiet it; and there is no drug, I fear, that we can *depend* upon for that purpose, but opium. The old paregoric has been, and is, a favourite form for giving opium to appease cough; and old-fashioned apothecaries will tell you that the alteration which was made by leaving the aniseed out of this compound tincture of camphor, in the last Pharmacopœia but one, impaired its efficacy. Whether it was so or not I cannot tell; but Dr. Prout is of opinion that *aniseed* has considerable power in allaying the irritation on which the cough depends. He infuses three drachms, or half an ounce, of the bruised seeds in half a pint of distilled water at a temperature not exceeding 120°; and lets it stand till it is cold. On his strong recommendation I have tried this, as a vehicle for paregoric, when the same dose in other vehicles had failed; and I must say that it has frequently been followed by a marked abatement of the frequency and violence of the cough. The aniseed is restored in the paregoric of the last Pharmacopœia, that of 1836. Hydrocyanic acid has sometimes a very soothing effect upon this harassing symptom. However, at last, opium will be found our sheet-anchor, not merely for the cough, but for the diarrhœa which is so seldom absent in the later periods of phthisis. The diarrhœa depends, as I have told you, upon an ulcerated state of the bowels. In those cases in which it could scarcely be kept in check at all, I have always found very extensive ulceration in the large intestines: but the diseased condition is often seated higher up, in the ileum or jejunum. Catechu is of great service in these cases, combined with laudanum and with the officinal chalk mixture:—or a few grains of the *confectio opii* may be given in peppermint water, after every loose evacuation: or in obstinate cases, a pill composed of a quarter of a grain of the sulphate of copper, and the same quantity of opium, will

often answer well, though it sometimes gripes. I mention these several expedients, for you will often require them all. The injection of a small quantity of starch, as much as the rectum will receive and retain, with ten or twenty drops of laudanum, generally affords the patient most sensible comfort; and suspends the further action of the bowels for a considerable time.

These, I think, are the principal means by which we may endeavour to smooth the pillow of the patient dying of consumption. Sometimes very little pain or distress is felt at all, from first to last; the intellect remains free, and the patients are proverbially sanguine about the issue of their disorder. In other cases, do what we will, the patient suffers greatly. One harassing incidental combination of symptoms is nausea and vomiting. I should have stated before, that when these symptoms last long, and are accompanied by pain and tenderness of the epigastrium, they denote, almost always, a thinned and softened condition of the mucous membrane of the stomach. They may be alleviated by a few leeches—by a blister—by the effervescing draught: or the prussic acid may be used; that is a medicine which certainly tranquillizes an irritable stomach· and it now and then seems to allay an urgent cough. Sometimes, again, the bones of the miserable patient are laid bare, in consequence of pressure upon parts in which the circulation is already very feeble. We cover these with soap plaster; take off the pressure by arranging cushions; or what is much the best of all, we put the patient upon the water-bed invented by Dr. Arnott.

LECTURE LVIII.

Melanosis of the Lung; true, and spurious. Accidental intrusion of solid substances into the air-passages.

I yesterday adverted to certain callings which are unhealthy for various reasons, and among the rest for this;—that the work-people engaged in them breathe habitually an atmosphere loaded with particles of matter which clog or irritate the pulmonary tissues. There is one morbid condition, so produced, which hitherto, or till lately, has scarcely been mentioned by writers on disease, but which deserves a moment's attention; for although it is very uncommon in many parts of this country, it is by no means rare in some others. The texture of the lungs is spoiled by matters carried in with the air, in the acts of breathing. This morbid state has been called *spurious melanosis*. The lungs are found after death to be throughout of a black colour, more or less uniform. Sometimes the pulmonary substance is dry and friable, as well as black; sometimes moist, œdematous, infiltrated with an inky fluid; not unfrequently broken down into irregular cavities of various sizes; and these cavities are often full of the same black liquor.

You are not to confound these appearances, when you happen to meet with them—(and as you will probably scatter yourselves, some here and some there, over various parts of the kingdom, some of you are very likely to meet with them)—you must avoid, I say, mistaking these black appearances and products, for *true melanosis*. The disease so denominated is a singular one. It was first fully described and named by Laennec in 1806. It consists in a morbid product, presenting a black or deep brown colour of various degrees of intensity, moist generally, unorganized, and differing in the form it assumes, and in its consistence, according to circumstances. I shall take this opportunity, for I am not likely to have a better, to tell you the little that has been ascertained in regard to this kind of disease; and having done so, I shall revert to a short account of *spurious* melanosis.

These black deposits take place most frequently of all in the cellular tissue, and in the adipous tissue: and they occur in greater abundance, and in larger masses, according as the reticular tissue is more plentiful, and more lax. They are met with also in the compound organs of the body; especially in the liver. Less frequently in the lungs. Some-

times in the eye. Occasionally in the brain. The serous membranes are obnoxious to the same kind of disease; the mucous very little so. The black or dark coloured matter may also exist, in a liquid condition, in the natural cavities of the body. And lastly, the melanotic material is sometimes mixed up with scirrhous and brain-like malignant tumours.

With respect to the shapes in which it appears—it is sometimes dotted, the surfaces affected by it looking as if they had been thickly sprinkled over with coal-dust or soot. Bu more commonly melanosis assumes the form of solid tumours, of variable magnitude. These tumours are largest, where cellular tissue is most loose and abundant. They may be no bigger than a pin's head, or they may be as large as a man's head. Masses of this kind have been found in the horse, weighing as much as six-and-thirty pounds. In the human subject they may attain the size of an orange. These large tumours (like large pulmonary tubercles) are usually formed by the union and agglomeration of several smaller ones, and hence they have generally a lobulated surface: while the shape of the separate smaller tumours is mostly spherical. Sometimes the cellular tissue lying around the melanotic masses is condensed into a kind of cyst: more generally the black matter is in naked contact with the tissue, whatever that may be, in which it is lodged.

From the serous surfaces, especially from the pleura and peritoneum, knobs of a dark colour are seen in some instances to project; in others, the round tumours, as big as a pea, or a cherry, hang from these surfaces by a sort of peduncle. The omentum is a common *habitat* of melanotic tumours.

Occasionally, I say, the black matter is found spread in a continuous layer upon the serous membranes; or is collected in a liquid state in their cavities. But this, compared with the occurrence of solid tumours, is rare.

When this remarkable disease is met with in one tissue or organ of the body, it is met with in others. It is never confined to one part, but pervades several: resembling in this respect both the scrofulous matter which constitutes tubercle; and the matter of cancer. Müller indeed considers melanosis to be a species or variety of carcinoma.

Scattered notices of these singular and striking changes in the animal frame occur in the works of Morgagni and of Haller; but since the period when Laennec first drew the special attention of the profession to the subject, the black matter has been carefully analyzed by several expert chemists. Without going into any tiresome detail, which you could scarcely remember, as to its exact composition, it is interesting to know that it is very like that of the blood: and no doubt the material is somehow deposited from the blood. Very little however has been ascertained about its primary origin and cause. Some have supposed that the melanotic matter is analogous to the natural pigments which are found in the animal economy; all of which are known to be rich in carbon. It is a curious fact that the disease has been more often observed in white or gray horses than in others. (I should tell you that the complaint is not at all uncommon in various quadrupeds: examples of it have been noted in the horse, ox, dog, cat, rabbit, rat, and mouse.) It has been conjectured that, in white animals, the colouring matter of the surface, and of the hair, has been diverted, by some morbid process, from its proper locality. But the very same disorder occurs also, though not so often, in dark, or bay, horses and cows: and certain pathologists imagine that in these cases there has been an undue accumulation, in the blood, of the carbon which is destined to colour different parts. In the one case, you see, they hold that the pigment is misplaced; in the other that it is excessive. What value these speculations as to the nature and origin of the disease may possess, time alone can determine.

When the tumours are divided, and moist, or when they are rendered moist by admixture with water, they freely impart the colouring matter; staining white paper, and blackening one's fingers, just as Indian ink might do. The disease most frequently happens, when it happens at all, in the decline of life.

The changes to which the melanotic tumours are liable, are very much like the changes which tubercular matter is apt to undergo. In certain situations where the secreted black material is subject to pressure, and is poured out in a soft consistence, the watery parts are sometimes absorbed, and the mass becomes hard and firm. On the other hand, the pressure occasioned *by* the tumour sometimes provokes inflammation in the tissues surrounding it; and then it is liable to be broken down, exactly in the same way as that in which tubercles soften prior to their expulsion from the lungs.

The injurious effects of these collections of black matter arise from the pressure they occasion; and they may evidently thus cause pain, irritation, ulceration; and according to their situation, number, and extent, they may materially interfere with important functions. And in this manner they do, in fact, at length destroy life.

There are no symptoms that I know of, distinctive of this disease, except the appearance of the black masses upon the surface of the body. Nor can I pretend to point out to you any cure for it, when it is ascertained to exist.

Yet it is right that you should be aware of what pathologists have learned respecting this curious morbid state; although that be little, and not very satisfactory. And I have introduced this brief consideration of melanosis here, somewhat irregularly perhaps, to enable you to distinguish from it that pulmonary disease to which I referred in the outset of the lecture, and to which I shall now return. This, I say, has been called *spurious melanosis:* and it has doubtless been mistaken for the specific disease of which I have just given you a sketch; for *real* melanosis.

It is, however, a very different affection.

Laennec had conjectured that certain kinds of black discoloration of the lungs were of extraneous origin; were owing to the introduction of black matters from without in the process of respiration: and Mr. Pearson, in this country, had thrown out the same idea. But this was first ascertained to be actually the case in Edinburgh. Dr. J. C. Gregory had a patient who died in the infirmary of that city, and whose lungs exhibited the following appearances:—They both presented one uniform black carbonaceous colour, which pervaded every part of their substance. The right lung was broken down, in its upper and middle lobes, into irregular cavities; and the walls of these cavities were black; and they contained a considerable quantity of a black liquid like ink. Portions of the pulmonary substance were dense, hepatized, and friable. The rest of this lung was œdematous; and when the serum which rendered it thus œdematous was pressed out, it also, the serum I mean, was quite black. The left lung was infiltered, in the same manner, with black serum. No tubercles could be detected. The bronchial glands were not enlarged, but they were stained of the same sable hue as the substance of the lungs. No other organ of the body presented any trace of this black discoloration.

Dr. Christison, who is known to be a very exact and able chemist, undertook an analysis of the black matter contained in the serum expressed from these lungs. I shall not follow out the details of his researches (you may read them at length in the 109th number of the *Edinburgh Medical and Surgical Journal*), but content myself with stating their result. And I may state it in Dr. Christison's own words. "In the product of this experiment (he says) it is scarcely possible not to recognise the ordinary products of the distillation of coal. A gas of the same quality was procured, and likewise a naphthous fluid holding in solution a crystalline principle, analogous to, if not identified with, naphthaline."

Now the man, whose lungs presented the appearances I have described, had, for the last ten or twelve years of his life, been employed in the coal mines at Dalkeith. He had been exposed therefore to the habitual inhalation of coal dust into his lungs in breathing: and taking this circumstance in conjunction with the result of the analysis of the black matter contained in the lungs, and nowhere else throughout the body, we cannot doubt that the carbonaceous substance so abundant in these organs was introduced from without. In truth we have now a large amount of evidence in proof that it must have been so. In the twenty-first volume of the *Medico-Chirurgical Transactions*, Dr. William Thomson has recorded the results of an extensive inquiry into the subject. Thus he gives ten examples of black sputa going along with pulmonary symptoms during life, and of black infiltration of the lungs discovered after death; and of the ten persons who were the subjects of these observations, nine had been engaged in working coal mines, and the tenth was a moulder at the Carron iron works. He gives also six cases of black infiltration of the lungs, all occurring in persons exposed to the inhalation of carbonaceous matters (one of them was an engineer, and the others were all colliers); but in these six cases there had been no black expectoration noticed during life.

That such carbonaceous particles, floating in the atmosphere, may be, and must be, and actually are, drawn into the lungs during inspiration, no one who has been long in this

smoky town can doubt. Many persons remark that they expectorate during winter, and while in London, a little mass or two of dark gray, dirty mucus, every morning; but when in the country, in the summer, the mucus so spat up is transparent and clean. So I have noticed, and pointed out to some of you, that the *crachoirs* of the patients in the hospital often bear witness that there has been during the previous twelve hours, one of our dense and dirty fogs, which come with an easterly wind, and bring with them a vast quantity of blacks, and soot, and smoke. Immediately after the prevalence of one of those filthy blankets of vapour, we find the contents of each of the little vessels given to the patients whose expectoration is kept for inspection, to be deeply tinged with black. And I had, in the year 1832, a patient whose sputa were remarkably loaded with dark matter. He came into the hospital complaining of cough, and shortness of breath, and a sensation at the lower part of the right side of the chest, as if it were pierced by needles; and he was spitting a considerable quantity of thick mucus, which was almost black. This colour never entirely left the expectoration as long as he remained in the hospital; but it very greatly diminished in proportion as his ailments were relieved. Now this man was a sto ker at one of the gas works. And he attributed his illness, which had come on gradually, to the great alternations of heat and cold to which his occupation exposed him: and the blackness of the sputa he ascribed to the continual inhalation of coal dust. And no doubt he was right.

It may seem strange, if the inhalation of atmospheric air loaded with minute particles of coal or other carbonaceous matter be sufficient to produce the remarkable condition of the lungs, and the characteristic *black spit*, that the change, and its nature and cause, should not have been earlier made out; considering the vast number of men who are employed in our mines, and collieries. It appears, however, that a great repugnance has existed, and probably still exists, among the labourers in the coal mines, to allowing their dead to be opened and examined. And it appears also that the peculiar state of the lungs which I have been speaking of is produced in a comparatively small number of those who are so employed. The precise cause, why some are thus affected, and some are not, is yet to be discovered. Dr. Thomson has circulated among the medical men residing in the coal districts, a list of queries (which he gives in his paper), respecting various points of interest in relation to this pulmonary condition: and we may expect to obtain, by degrees, more exact information about it. I recommend it as an interesting subject of inquiry to such among you as may have opportunities of prosecuting it. It has been conjectured that the specific change takes place, in a marked degree, only in lungs that were previously unsound. It is a question whether the cavities met with in the pulmonary substance, in the fatal cases, were the result of the spurious melanosis: or of the expulsion of tubercular matter which had coexisted with the melanotic state. It is a curious circumstance that the black spit, as it is called in those districts, sometimes does not make its appearance until some time has elapsed after the labour in the coal mines was given up. Certainly this is a complaint that offers several interesting points of research, and requires further investigation.

You will remark that the spurious melanosis is distinguised from the true, by its occurrence in those persons only who are somehow exposed for a certain length of time to breathe an atmosphere which is largely encumbered with carbonaceous particles; whereas true melanosis may occur in any locality. The spurious discoloration never affects any other organs than the lungs and bronchial glands; the true black deposit of melanosis is never confined to a single organ or tissue. Moreover, the one disorder is absolutely beyond remedy; the other, as soon as its presence is rendered probable, by the black expectoration, and the pulmonary distress, may be mitigated, checked, perhaps gradually cured, by removing the patient from the operation of the exciting cause, and pursuing such other measures as the symptoms may seem to require. The distinction is not a matter therefore of mere curiosity: it bears upon the treatment to be followed, which is our proper business. It is connected also with medical police or hygiene, which we should all of us cultivate as extensively as we may; as a science intimately related to our strictly professional pursuits, and to the welfare of the community.

Other forms of disease, unquestionably cancerous, occasionally infest the lungs. Their chief symptoms, when the malady does not reach the surface of the body, result from the

pressure which the carcinomatous masses exercise on the parts in their neighbourhood. I shall postpone a more particular consideration of these effects of intra-thoracic pressure, until I come to aneurismal tumours, which have a similar mechanical influence.

Foreign bodies in air-passages.—There is yet another affection of the breath-machine, to which I must briefly direct your attention: a casualty that is apt to befall the air-tubes. I said nothing of this, indeed, last year; but having since witnessed an example of the accident to which I allude—the entrance, namely, of some solid substance into the windpipe—I have thus been reminded of my former omission, and taught at the same time the necessity that every medical man should have well considered such cases. I was taken to Kentish Town, in the autumn (1837), by a professional friend, to see a child, into whose trachea a small nail, what is commonly called a *tack*, was thought to have passed. When I saw the boy, he seemed to have nothing the matter with him: but he had been subject, ever since the accident, to paroxysms of most violent choking cough; alarming the parents and his attendants for his life. There was good reason for concluding that the nail, which was missing, and which he said he had *swallowed*, had really got into the windpipe, and was still there, or in the lungs; and the question was much discussed, what ought to be done in such a case? The result was, that nothing was done: but that, after the lapse of several weeks, the nail was at length coughed up.

Now there are some interesting points arising out of this sort of mischance. The instance I have just referred to will probably be published; and therefore I dwell upon it the less. Dr. Stokes has devoted a short chapter to the consideration of foreign bodies in the air-passages: and examples of that accident are more common than you might suppose.

It is, at first sight, a surprising circumstance, that a solid body of any considerable magnitude (a molar tooth for instance), should be able to pass at all through the narrow chink of the glottis. But, supposing the chink to be plugged by the sudden entrance of a passing substance, just at the commencement of a forcible endeavour to inspire, when, of course, the opening is at the widest, that substance must necessarily sustain, as the chest expands, a strong degree of pressure from the external atmosphere: strong enough, often, to force it through. If you cork a bottle that contains air only, and sink it sufficiently deep into the sea, the pressure of the water will push the cork into the bottle. The condition of the lungs, in the case supposed, and the condition of the bottle, are analogous. A vacuum beyond the plug is attempted by the act of inspiring, and obviated by the displacement of the plug, inwards. There are no such powerful forces called into action to drive the intruding substance out again.

The matters which have been actually thus caught in the rima glottidis, and forced through, are, as you might almost expect, oddly various in kind. Morsels of food: the stones of fruit; of these there are many instances: teeth; three such are referred to by Dr. Stokes: portions of bone: pebbles: a piece of money: a nut: a nut shell: a button: a musket ball: a large shot: a fragment of nutmeg: iron nails: kidney beans: ears of grass or corn; of these, four examples at least have been noticed; one is mentioned by Dr. Stokes, two are recorded in the *Gazette Médicale*, and I show you a monument of the fourth, in this interesting preparation, for the history of which I am indebted to Mr. Mayo. The young son of an English nobleman was riding in a carriage, in or near Paris, and had an ear of rye in his mouth. The carriage made a sudden jolt, and the ear of corn disappeared. Little was thought about this at the time: but soon afterwards symptoms of pulmonary irritation set in, attended with hectic fever, and with the most fœtid expectoration. The boy gradually sunk. The ear of rye lay, as you may perceive, in an abscess which was common to the right lung and to the liver, through the diaphragm.

If any of you have tried the boyish trick of slipping beneath your wristband an ear of bearded corn, you will have no difficulty in understanding how and why, with every movement of the parts connected with it, the ear will travel onwards; and how improbable it is that such a substance should ever be expelled from the lungs by coughing. Yet, in one of the cases, recorded in the *Gazette Médicale*, by a physician whose sister was the subject of the accident, an ear of barley was so rejected, seven years after its entrance. During that long period she had suffered repeated attacks of copious hæmoptysis. Her recovery was perfect.

The very enumeration which I have just made may convince you that the accident is not a very unfrequent one; and it is more than probable that fatal cases happen, the nature of which escapes detection.

The results of the accident are various also.

In the first place, it sometimes causes speedy death by apnœa.

2dly. It may be followed by inflammation of the lung, and perhaps abscess; and *so* destroy life.

3dly. Death may ensue, after symptoms resembling those of chronic phthisis.

4thly. The "foreign body," as we oddly enough call it, may be expelled through the glottis, after a variable period of time. Sometimes, yet not always, its expulsion is the condition and the harbinger of the patient's recovery; but he is never safe while it remains.

Death may take place in a few seconds when the substance sticks in the glottis. Death *has* occurred within three days, when the substance has passed the glottis; and in eleven days when it had reached the lung. The intruding piece of matter has escaped, through the natural passages, after remaining imprisoned for seventeen years. In that case, the patient died, hectical and emaciated, a year and a half afterwards.

It may be worth our while to consider these particulars somewhat more closely; and to inquire what, in different cases, becomes of the foreign body which thus, to use a common phrase, "goes the wrong way;" and what the symptoms are to which it gives rise.

First, then, I say, it may get wedged in the slit of the glottis, and produce immediate suffocation. I mentioned, before, the frequency of this kind of death by misadventure. If you are summoned to any one whom you find comatose, or apparently just dead, and you learn that he had been suddenly attacked with choking during a meal, lose no time in examining his pharynx and gullet. You may chance to save a life so. The accident often happens to persons who are drunk. No doubt it happens oftener than we are aware of. The attack is very likely to be mistaken for an apoplectic seizure.

In these cases of sudden choking, the morsel of food is not always caught in the rima glottidis. If it be large enough to stick fast in the *pharynx*, it may provoke, through a reflex action, an abiding spasm of the little laryngeal muscles, and so produce death by apnœa. The remedy for such an emergency, as Dr. Marshall Hall truly observes, must be *immediate:* and this is what he tells us should be done.

"Pressure being made on the abdomen, to prevent the descent of the diaphragm, a forcible blow should be made by the flat hand on the thorax. The effect of this is to induce an effort similar to that of expiration; the larynx being closed, œsophageal vomiting takes place, and the morsel is dislodged."

"If this plan fail, not an instant being lost, the pressure should be kept up on the abdomen, the finger should be introduced into the throat, and the same smart and forcible blow made on the thorax as before. By the irritation of the fauces the cardia is opened, and by the blow on the thorax (firm pressure being made on the abdomen) an effort similar to that of expiration, with a closed larynx, is made, and a direct vomiting ensues, and the morsel of food is carried away."

2dly. The substance, if small, may, after it has passed the chink, remain in the larynx; entangled in its ventricles, or between the chordæ vocales. In that case it usually occasions very severe laryngeal symptoms—spasmodic gasping cough, choking sensations, croupy respiration, and pain in the larynx—symptoms which harass the patient without intermission, until death ensues, or until the substance is driven upwards into the pharynx, or passes downwards into the wind-pipe. There is, however, one instance on record, in which a piece of gold was lodged for years in the ventricles of the larynx, without these distressing consequences.

3dly. Having passed the upper part of the larynx, it may stop, and become fixed beneath the cricoid cartilage, or in the trachea. In these situations, unless it quite blocks up the passage, its presence may be productive of but little distress. A wheezing or croupy sound during one or both of the movements of respiration, and some degree of pain and tenderness of the part where the substance was lodged, have constituted all the evidence of its position in the air-passages, in more than one instance. A very singular and whimsical case of this kind, related by Professor Macnamara, is referred to by Dr. Stokes. A boy

had made a whistle, by perforating a plum-stone, and extracting the kernel. This, during a strong inspiration, passed from between his lips, through the glottis, and became fixed transversely in the larynx. So little inconvenience did it create, that the boy, finding that he still whistled as he breathed, went about for some hours, pleased to display this new accomplishment. For three days he continued to occupy himself in his childish amusements, suffering now and then a seizure of suffocating cough. He was then taken to the Meath hospital. He had no pain in deglutition; but he said that when the cough was severe, it caused pain in his throat. He had also uneasiness in the epigastrium, a bloated countenance, and a frequent pulse. The chest sounded well on percussion, and the vesicular murmur was natural. The fits of coughing were followed by white frothy expectoration. Laryngotomy was performed; but during the struggle and the convulsive cough which took place when the opening was made, the stone (so the patient declared) was coughed up, and swallowed. The symptoms were relieved; and the whistling ceased. But it was found that, as the wound healed, the distress and the whistling sound returned; which showed that the stone lay above the opening; and that the disappearance of the symptoms had been owing, not to its dislodgement, but to the admission of air below the point where it was fixed. Soon after this, however, it changed its place, passed down into the right bronchus, and then up again towards the larynx. By a second operation it was extracted; and the lad recovered without any bad symptom.

4thly. The substance may get beyond the trachea, into one of the bronchi, and stay there. And it is a very curious fact, and one which has evident importance in respect to diagnosis, that it is almost always the *right* bronchus which the substance enters. Dr. Stokes has explained why it is so. The septum that divides the extremity of the trachea into two branches is not placed in the middle of the channel, but decidedly towards the left; so that any solid body falling down through the windpipe, is naturally directed into the right bronchus. Perhaps this tendency is aided by the more vertical direction, and by the somewhat greater capacity of that tube, compared with its fellow. Now you will readily apprehend what sort of symptoms would be likely to result from the impaction of a solid body in either of the primary bronchi. It would be very apt to excite inflammation of the corresponding lung, which inflammation would reveal itself by its proper signs; but it would produce peculiar auscultatory phenomena, prior to and independent of such inflammation. It would prevent, partially or altogether, the entrance of air into the lung of that side. Hence, when we have other reasons for thinking that a solid body has passed the glottis, if we find the vesicular murmur suspended or enfeebled in one lung, while percussion gives out its usual clear sound, we may conclude that the intruder is lodged in the bronchus belonging to that lung.

Dr. Stokes believes, and his opinion is fortified by his own experience on the subject, that smooth bodies (beans or shots, for example), are more calculated than such as are rugged and uneven to cause urgent distress when impacted in one of the bronchi; inasmuch as they more completely plug and obstruct the tube, thereby depriving the patient at once of the use of half his lungs. An irregular substance, which can neither seal the passage up, nor be closely grasped by its spasmodic contraction, will probably occasion less dyspnœa, and at the same time will be less likely to be dislodged by the effort of expiration. Under these circumstances we look for more chronic symptoms.

5thly, and lastly. The intruding substance may not be fixed anywhere, but may shift its place from time to time; and this, in fact, is what most frequently happens: and when it does happen, it gives rise to a very striking and distinctive series of symptoms. Paroxysms of suffocating cough and extreme distress, when the substance is driven up into or near the larynx; with intervals of comparative quiet, and sometimes indeed of apparent health, when it subsides into the trachea or bronchi. But during these intervals, the signs that sometimes mark its situation in those tubes may perhaps be discoverable.

There are, then, a set of general symptoms, which lead us to believe, or to suspect, that some solid body has entered the air-passages: and there are other sets of particular symptoms, which inform us, with more or less certainty, whereabouts it is fixed, or that it is not fixed at all. A person, previously in good health, is seized with violent cough and choking dyspnœa, suddenly, during a meal, or while he had in his mouth some loose substance, which he fancies he has swallowed. This is a sufficient clue to the probable

nature of the case: and we next inquire for a sense of soreness in the windpipe, and wheezing respiration; for signs of bronchitis or of pneumonia, especially in the right lung; for signs of obstruction of the bronchus on one side, and especially on the right side; or for alternations of suffocating cough, with intervals of outward calm. In the last case, we may expect to find the bronchus unstopped during the periods of laryngeal irritation; and *vice versâ*.

When we know that a solid body has been entrapped in the air-tubes, our business is plain; there is no room, in my opinion, for hesitation; we must let the substance out through an artificial wicket. There is danger, so long as it remains in these vital passages, of speedy suffocation; of fatal damage to the larynx, or to the lungs; of cerebral mischief during the violent paroxysms of coughing. Convulsions and apoplexy have, under such circumstances, actually occurred. Against these perils there is no security, except in the early performance of tracheotomy. If the included substance be loose and smoothed it will presently be shot forth at the new orifice; if it be fixed, or angular it may generally be extricated by a skilful and delicate hand.

Even while this sheet has been passing under the press, (May, 1843,) another instance has occurred of the same accident, and excited a degree of anxious interest in the public mind, scarcely less than is accorded to a royal illness. It befell a gentleman whose name was previously famous. Mr. Brunel, in amusing the children of a friend with some tricks of legerdemain, put a half-sovereign into his mouth; and the coin slipped, as from its size and shape it might easily do, through the chink of the glottis. It seems to have occasioned no very urgent distress. The patient was made aware, by some internal sensation, that it lay towards the right side. After more than three weeks had passed, the trachea was opened: but the piece of money did not come forth. Probably its weight prevented its being driven up and down in the windpipe; and its form, when it lay edgeways, did not oppose much impediment to the breath. The same weight, however, brought it back to the larynx whenever Mr. Brunel placed himself with his head downwards. In some of these experiments, coming crossways I suppose, it produced most violent cough, and feelings of impending suffocation: but in a final and happier trial, at the end of six weeks, it dropped out again, through the natural passage—just as a coin may sometimes, by good luck, be shaken out of a box through a slit in its lid.

LECTURE LIX.

Diseases of the Heart: usually partial. Changes in its Muscular Texture. Mechanism of those Changes. Natural Dimensions of the Heart. Natural Sounds. Modifications of these by Disease. Review of the Physical and General Signs that accompany Cardiac Disease.

You will perhaps accuse me, gentlemen, of a disposition to magnify the importance of every new class of diseases at which we arrive, in our survey of the morbid conditions of the various parts of the body in succession. There are few complaints, in truth, which are not important; either from the discomfort to which they give rise, or from their tendency to abbreviate the span of human existence. Yet of the strictly vital organs the derangements are necessarily the most perilous; and therefore, to us, the most interesting. Two props of the tripod of life we have passed in review, and seen how they may be weakened, and how they may fail altogether. The office of the *heart* is not less essential to life and health, than that of the brain, or of the lungs. The well-being of every portion of the frame depends upon its being duly supplied with arterial blood, and duly relieved of that which has become venous: and this supply and relief require that the central organ of the circulation should be sound in its structure, and perfect in its working. But

it is frequently otherwise. I can remember, indeed, the time when disease of the heart was thought to be a very rare thing; but it is now well known to be one of the commonest of disorders, and it connects itself with a variety of other affections, with which it was formerly supposed to have no relation.

Like other organs that are complex of structure and formed of different tissues, the heart is subject to *partial* disease. Its lining membrane alone may, in the first instance, become the seat of inflammation, with its various effects; or its investing membrane only may undergo morbid alterations; or the muscular substance that constitutes the organ itself may be gradually changed in its qualities, in its bulk, or in its proportions.

But the morbid conditions of the investing and lining membranes do not always, or immediately, compromise the life of the patient. They are fatal at length in ninety-nine cases out of a hundred, through the alterations to which they lead in the muscle wherewith they are connected. It may be practically useful therefore to consider, first, these ultimate morbid states which are incompatible with the continuance of life; and then to trace them back to the next link in the chain of their causes, which will be found, in a very great number of instances, to consist in some antecedent morbid state of the exterior or of the interior membrane.

The heart, you know, is a living forcing pump; a hollow muscular engine, with its chambers and their outlets, its contractile walls and their strength and thickness, so admirably adjusted, that the healthy balance of the circulation is continually maintained, under many varying outward influences and inward emotions which tend to destroy it. In treating of disease of the heart we have to consider, therefore, the modes in which its mechanism may be spoiled or deranged; and the effects of such derangements.

Not only the component tissues, but different portions also of the organ may be separately diseased. It seldom happens, indeed, that the whole heart is affected; although that is probably the vulgar belief. The left side is much more obnoxious to morbid changes than the right: and when both sides are implicated, the alteration is almost always more decided and conspicuous in the left than in the right chambers.

In the rapid sketch which I attempted of general pathology, in the outset of the course, I pointed out the various kinds of alteration to which the tissues and organs of the body, and therefore the heart among the rest, are subject. One or more of the chambers of the heart, you will remember, may become larger or smaller than is natural; or have their walls increased or diminished in thickness, and consequently in power; or one or more of its outlets and orifices of communication may be widened or contracted: and the purposes and function of the organ will be more or less impaired by these changes.

Hypertrophy.—In order, then, to have a clear conception of cardiac disease, it is necessary to analyze it, and to investigate the derangements of the several parts of the heart. And I begin with *hypertrophy:* augmentation of bulk in its muscular substance. And I must first of all define one or two phrases which are current among pathologists in respect to this condition.

The muscular tissue of one, or more, of the chambers of the heart may become thicker and stronger than natural, while the capacity of that chamber, or of those chambers, remains unaltered. The hypertrophy in that case is said to be *simple.*

But, while the muscular parietes are thickened, the corresponding chamber may become unnaturally large. This constitutes the *active aneurism* of the heart of Corvisart, the *eccentric* hypertrophy of more modern writers.

On the other hand, it has been supposed that the capacity of a cavity of the heart may diminish in size as its walls increase in thickness: that the hypertrophy may take place *at the expense* (as it were) of the chamber. This has been called *concentric* hypertrophy.

Now of these three reputed forms of hypertrophy, considered in their relation to *disease*, two only, the simple and the eccentric, have any real existence. The third, or concentric form, never occurs, I believe, except as a congenital malformation. And of the two genuine species of hypertrophy, the eccentric, which is plainly a compound affection, consisting of *hypertrophy with dilatation*, is much the most common. The reason of this is to be found in the physical cause of the morbid condition, in most instances. The physical cause, in nineteen cases out of twenty, is some obstacle, mechanical or virtual, to the perfect accomplishment of the function of the chamber; some obstruction opposed

to the free and thorough exit of the blood from it; or something which hinders the easy play of the organ: hence, in the first place, a gradual yielding, or tendency to yield, in the sides of the affected chamber, from the continual and unwonted pressure of the accumulated blood against them, and in the second place, a *striving* action of the muscle to overcome the hindrance, or to counterbalance the obstacle; and consequently, according to the law formerly announced, an augmentation in the bulk of the muscle the function of which is thus increased. If the hypertrophy, which is the result of a truly conservative process, keeps pace exactly with the amount of the obstacle and *exactly balances* it, no dilatation happens, or next to none. But this is comparatively seldom the case. According to the principles of mechanics, a little distension of the spheröidal cavity must require an increase of force to propel from it a given quantity of blood, in the same time, through a given discharging orifice. So that incipient dilatation becomes (in addition to the supposed obstacle) an efficient cause of hypertrophy: and the two, the dilatation and the hypertrophy, commonly make progress together.

Cruveilhier appears to have been the first to reject *concentric hypertrophy* from the catalogue of cardiac diseases. The smallness of its cavity, with a proportional increased thickness of its walls, was regarded by him as a transient condition of the ventricle, depending upon the mode of death. He found these phenomena very strongly marked in the hearts of all those whose bodies he had examined after decapitation by the guillotine; "Les parois ventriculaires se touchaient dans tours leurs points." It is therefore his opinion that the hearts which had been thought by others, to present examples of concentric hypertrophy, were in reality "hearts more or less hypertrophied, which death had surprised in all their energy of contractility."

This question has since been considered by Dr. Budd, in a communication to the Medical and Chirurgical Society, which you may read in the twenty-first volume of its *Transactions*. He has lately favoured me with a statement of his matured views upon the subject.

The semblance of concentric hypertrophy is most common in the left ventricle; and depends upon the ventricle being nearly empty at the time of death, and upon the corpse being examined while the heart is contracted by the *rigor mortis*. The fallacious appearance is accordingly noticed in cases where, from the manner of death, the left ventricle, or the entire heart, contains but little blood, and where, from the muscular power not having been previously exhausted, the rigor mortis is of long duration.

"In all these concentrically hypertrophied hearts (writes Dr. Budd) the ventricle may be readily dilated by means of the fingers, and always dilates of itself when the rigor mortis goes off."

"In the published cases of concentric hypertrophy, in which there was no disease of the valves (I have given eight such cases in my paper, and could now add a long list to them) there were no signs, or only very slight signs, of disease of the heart, during the lifetime of the patient. This circumstance is sufficient proof that the cavities of the heart in these cases could not have been during life permanently in the contracted state in which they were found after death. A left ventricle that could scarcely contain an almond (a common form of expression in the description of these cases) would surely have caused a great impediment to the circulation."

Moreover, concentric hypertrophy could answer no mechanical purpose; nor could its formation be accounted for on mechanical principles. But "concentric hypertrophy of a ventricle, in a high degree, with obstruction at its discharging orifice, and an extraordinary channel for the passage of the blood, occasionally exists as a congenital malformation; and in most cases, the right is the ventricle so affected."

To resume. Recollect that there may be two distinct kinds of physical cause of excessive action of the heart, and therefore of hypertrophy. In the one kind, there is some mechanical obstruction to the exit of the blood from one or more of the cavities; a constricted state of the *orifices*, is the most common condition. In the other kind, without any such mechanical bar or dam to the fluid, there is something to hinder the free and sufficient play of the organ; an adhering pericardium it may be, or malposition of the heart. If the heart be pushed, for instance, out of its proper place and posture by effusion into the pleura, or by distortion of the chest, it will not work with the same ease as when

all is perfect and symmetrical; and the unusual labour imposed upon it fully to execute its office, will lead to hypertrophy. The causes of hypertrophy may therefore be situated *within* the heart itself, or *without* and beyond it: but in all those cases in which the effect of the hindrance or obstacle is to *detain* the blood in one or more chambers, the hypertrophy will be likely to be accompanied by *dilatation:* and, generally speaking, the hypertrophy and dilatation result from disease in some part which lies *beyond* the affected chamber, in the order of the circulation. Thus either a narrowing, or a dilatation of the aorta at its commencement, will tend to cause hypertrophy and dilatation of the left ventricle.

That *contraction* of the aorta, or of the aortic orifice, may have this consequence, you will have no difficulty in perceiving. The blood cannot so readily pass through the narrowed channel; hence it will tend to accumulate in undue quantity in the ventricle, and therefore to stretch and dilate it; and the increased muscular efforts necessary to drive the delayed blood onwards, tend also to thicken the muscle itself. But it may not be so obvious that *dilatation* of the mouth of the aorta—a wider channel of egress—would also virtually prove an obstacle to the emptying of the ventricle. Yet it certainly would, in two ways. In the first place, dilatation of the entrance of the aorta implies a diminution in the elasticity of that vessel; and the blood after it has left the heart is urged onwards by the healthy elasticity. But again, dilatation of the mouth of the aorta commonly implies an imperfect closure of that vessel by the sigmoid valves; so that during the diastole, a part of the blood is apt to regurgitate from the aorta, and to keep the ventricle morbidly full. You see, therefore, that a deviation from the healthy state of the aorta and of the valvular apparatus which lies at its mouth, may obstruct the course of the blood, and lead to hypertrophy and dilatation, whether the deviation be in the one way or in the other; whether, I mean, the natural size of the vessel be increased or diminished. Again, disease of the mitral valve, obstructing the flow of the blood at *that* point, will lead to an accumulation in the left auricle, in the pulmonary veins, and in the lungs themselves. The auricular action is always less regular and energetic than the ventricular, so that we less frequently meet with *hypertrophy* of the auricles; but very often with dilatation. And if we go to the other side of the heart, we find hypertrophy with dilatation, and more especially dilatation of the right ventricle, when, from some reason or other, the blood passes with difficulty towards or through the *lungs:* either from disease of the pulmonary artery, or from disease in the substance of the lungs—emphysema, for instance: and if the difficulty be great, the accumulation and distension will affect successively the right auricle, and the venæ cavæ; and then we have, in most cases, general dropsy. So that, I repeat, disease in the heart tends to propagate itself in a direction contrary to that of the circulation. Furthermore, if the muscular tissue of the heart be pale, flabby, soft, and weak—as it frequently is in feeble, ill-nourished, cachectic persons—it will the more readily yield to the centrifugal pressure of the blood it embraces. In this way we may have dilatation without any hypertrophy. I am anxious that you should in the outset comprehend the mechanism by which the natural dimensions and relative proportions of different parts of the heart, may be altered in disease.

Natural dimensions of the heart.—One reason why disease of the heart used formerly to be overlooked, was that these natural dimensions and relative proportions were not ascertained or much attended to. It is not easy to form any very precise estimate of the size of a healthy heart. It is commonly held that if the heart be about the same size with the closed fist of the subject, its general dimensions may be considered as being natural. Bouillaud, who has taken much pains with this matter, weighing and measuring a great number of different hearts, states that the *mean* weight of that organ, with the origins of its large vessels, and empty of blood, in adults from twenty-five to sixty years old, is from eight to nine ounces; that in subjects from sixteen to twenty-five years old it may be one or two ounces less; and that, in very large and robust persons, it may rise to ten or eleven ounces. Also, what we should expect, that the weight is less in women than in men.

So much for the general bulk of the heart. And we must have some standard whereby to estimate its relative proportions. Every one knows that the walls of the left ventricle are thicker than those of the right. Bouillaud found that the mean thickness of the walls of the left ventricle *at its base* was seven lines, while that of the right ventricle was two-and-

a-half lines. And taking the thickness generally, he says that the thickness of the parietes of the right ventricle, has not a greater ratio to that of the parietes of the left, than two to five, or even than one to three.

So again of the auricles; he lays it down that the mean thickness of the walls of the left auricle, is to the mean thickness of those of the right, as three to two.

He holds also that the mean capacity of the right ventricle exceeds, by a little, that of the left: and that the right auricle is larger than the left. You must always make allowance in actual cases, for the possible distension of these cavities with blood, beyond the size to which they would have contracted if they had contained no blood.

I may add, that the same author declares the rule I just now mentioned, which had been proposed before his researches were instituted—the rule, viz., which makes the bulk of the healthy heart equal to the fist of the subject—to be tolerably correct. By keeping in mind these general facts, you will be better able to appreciate the appearances presented by the heart when it is taken from the body to be examined: but you will recollect that they relate to *averages* only.

Now having pointed out the modes in which the natural proportions of the heart and of its several parts may be morbidly altered; and given you a rough standard which may enable you to estimate these proportions in the state of health, and the deviations from them in the state of disease: I will go on to consider the symptoms, by which the altered conditions are accustomed to declare themselves. And it is with respect to the heart, as with respect to the lungs, there are *general* symptoms or signs, and there are *physical* symptoms or signs: and the information derived from these sources respectively is of variable utility. Neither of them can be safely neglected; and it is often found that the indications afforded by one of these sets of symptoms are confirmed or corrected by those furnished by the other. I believe it will be best to pursue the same course in both cases, and to speak, in the first place, of the signs that are brought within our notice by the sense of hearing.

But, in order that we may comprehend the *morbid sounds* of the heart, we must first make ourselves acquainted with those that belong to its healthy condition.

Natural sounds of the heart.—The heart may be heard by the ear laid flat against the præcordial region, or through a stethoscope, to beat over a certain space. That space, in ordinary circumstances, corresponds to the inferior half of the sternum, and to the cartilages of the ribs, from the fourth to the seventh, on the left side. The apex of the organ may often be *seen* to pulsate between the cartilages of the fifth and sixth left ribs; about two inches below the nipple, and one inch from it towards the sternum.

This is the space over which, in the sound state of the heart and lungs, the pulsations of the former are plainly audible. But there are several diseased conditions both of the heart itself, and of the parts around it, which interfere with this rule.

In the first place, if the heart be larger than natural, it will be heard to beat over a proportionally larger space. In this way it may come to be heard all over the chest in front; and behind on the left side of the spine; and even, in extreme cases, on the right side of the spine.

Again, the extent of space over which the heart may be heard to beat will be increased in proportion to the thinness of its walls; and diminished, *cæteris paribus*, according to the thickness of its walls. So that when the heart is nearly its proper size, if its walls be thin, it will be heard beyond its natural limits; and if its walls be morbidly thick, *i. e.*, if it be affected with considerable hypertrophy, it will not be heard beyond, nor even to the extent of its natural limits. I will endeavour, presently, to explain the reason of these differences.

Again, and this it is of great importance to remember, the heart may be heard far beyond its natural limits, even when it is perfectly healthy, in consequence of the lung between the ear and the heart having become solid, and therefore a better conductor of sound: and the solidification may have resulted from hepatization, or from the presence of a number of crude tubercles. The sound of the heart's action will also be conveyed to a distance by the liquid effusion in pleurisy. If we are not aware of these circumstances, we are continually liable to fall into mistakes.

The heart is likewise heard more distinctly, and over a space which is comparatively

larger, in children than in adult persons; and I need scarcely say that it may be heard over a wider extent of the chest whenever its action is augmented by exercise, by emotion of the mind, or by febrile excitement.

The *impulse* of the heart is another point which you must attend to. In healthy persons who are thin, you may generally feel the stroke which the heart gives to the ribs, by placing your hand on the præcordial region. In persons who are fat, you often cannot feel the heart at all in this manner. For obvious reasons, it is felt more distinctly, over a larger space, and higher up, while the person is stooping forwards, or makes a forced expiration; less distinctly, over a smaller space, and lower down, when he makes a deep inspiration, or is lying on his back. In proportion as the heart is enlarged by disease, it can be felt more extensively: and when there is hypertrophy, the force with which it strikes the parietes of the chest is sometimes extraordinary, and very instructive. You will see the ear and head of the listener distinctly lifted at every pulsation; sometimes the whole of the patient's body, nay his very bed, is shaken by the strong shock of the heart during its systole. There is no sign of hypertrophy so sure as that afforded by the heart's impulse. You feel, not a smart, quick, and sudden knock, but a steady, heaving, irrepressible swell, which is perfectly characteristic. You may always infer increased thickness of the walls of the organ, when you meet with this regular heaving motion; and the extent to which the whole heart is enlarged in such cases may be discovered by the extent of space over which the heaving impulse is perceptible.

The *sounds* which we hear are two. One of them coincides, in point of time, with the impulse; and barely precedes the beat of the radial artery. It happens, therefore, when the ventricles contract; during the systole. It is called, accordingly, the *systolic* sound, or the *first* sound of the heart. The other of the two sounds coincides with the diastole, and is spoken of as the *second* or the *diastolic* sound. It takes place at the instant when the heart reverts to that place and condition in which it had been prior to the systolic movement. These two sounds occur in quick and regular succession, and then follows an interval of silence, after which the two sounds are repeated; and so on.

The two sounds are not, however, exactly alike. They differ somewhat, both in quality, and in duration. The first is a dull, prolonged noise; the second a shorter and smarter sound, having more of a clacking or flapping character. Attempts have been made to assign the respective duration of each sound, and of the period of repose. I confess that I have never succeeded in measuring them satisfactorily in my mind. Probably Dr. C. J. B. Williams' estimate is as near the mark as any. He divides the whole period, from the beginning of one pulsation to the beginning of the next, into five equal parts: and allots two of these to the first sound, one to the second, and the remaining two to the interval of silence. This order of succession is called the *rhythm* of the heart: and it may be perverted.

Respecting the physical *causes* of these natural sounds there have been much recent discussion and research. Our time, however, will permit me to do little more than tell you what I believe to be the facts of the matter. And I take, first, the diastolic sound, as being the simpler of the two. It used to be ascribed to the contraction of the auricles: but that was quite a mistake. The contraction of the auricles, such as it is, happens *immediately before* each systole of the ventricles; whereas the sound in question occurs *immediately after* it, and is succeeded by the period of silence. This we know from the visible movements of the organ when exposed in a living animal. In truth, the auricular contractions are very feeble, and not attended with any appreciable noise. I have no doubt that the second sound is produced mainly, if not altogether, by the sudden shutting of the floodgates placed at the mouths of the two great outlets of the heart. The recoiling blood forces back the semilunar valves of the aorta and of the pulmonary artery, as one unfurls an umbrella; and with an audible check as they tighten. There is no other tenable mode of accounting for the sound. Experimenters have contrived, by hooks and wires, to prevent these valves from unfolding; and then the flapping sound has been converted into a hiss. Disease of the same valves demonstrates the same things; as we shall presently see. Nevertheless, it is both possible and probable that the relapse of the whole organ to its former place may contribute an ingredient towards this second sound.

The first, or systolic sound, is more complex. Partly it is owing, as I believe, to a

similar cause with that which occasions the diastolic sound, viz., the abrupt closure of the orifices of communication between the auricles and ventricles, by the reflux of the blood against the ventricular surfaces of the tricuspid and mitral valves; partly, sometimes, to the blow of the heart's apex against the ribs; *chiefly*, however, it consists of the sound that results from the muscular contraction of the ventricles. The systolic sound commences with the tightening of the walls of the ventricles, including the valves; and it is prolonged by the muscular noise. You are aware, I dare say, that the vigorous contraction of a large muscle is accompanied by audible sound. If, during the stillness of night, when lying in bed, with your cheek and ear upon the pillow, you set your teeth firmly, you will hear a continuous dull rumbling, like the noise of carriage wheels in the street, and evidently caused by the action of the masseter and the temporal muscles. Dr. Williams states that, with the help of a flexible stethoscope, one may hear the voluntary jerking contraction of his own abdominal muscles; the sound being as loud as that of the heart's systole and very like it in character. That the systolic sound is essentially due to muscular contraction is proved by the fact, that when a heart is taken from the living thorax and placed upon a table, its contractions (which persist for a while) are still attended with a noise similar to the natural first sound, though weaker. Here there is neither collision of the blood, nor valvular reaction, nor impulse against the ribs, to render the experiment ambiguous.

If you acknowledge and comprehend this source of sound, you will understand without difficulty why the heart, *cæteris paribus*, is heard more clearly and extensively when its walls are thin, less widely and loudly when they are thick. "The transition (says Dr. Williams) of a thick muscle from slack to tight can never be so complete and sudden as that of a thin one. Where there are many fibres they choke and muffle each other's vibrations; hence the sound is dull and prolonged, rather than loud and clear. If we observe the different sounds produced on tightening thin silk and thick baize or cloth, we find that the thinness of the silk gives a unity and briefness to the impulse which it receives, and the sound is short and loud; whilst in the baize the impulse is divided and prolonged in the complexity of the fibres, and the sound is dull and less brief: so, under similar circumstances, a thin ventricle will give a louder, sharper sound than a thick one.

Modifications of the natural sounds of the heart by disease.—The natural sounds which I have been describing are liable to be changed, or modified, by disease. Some of the modifications, indeed, I have adverted to as I went along. But others, of a more striking and extraordinary character, are yet to be explained. Either sound, or both, may be accompanied by a noise, which, in its commonest type, very closely resembles that produced by the blowing of a pair of bellows. Four persons out of five, I should think, if they were asked what this sound resembled, when they heard it accompanying each systolic movement of the heart, would say that it was exactly like the repeated blowing of bellows in an adjoining room. It is called, accordingly, by the French, the "bruit de soufflet;" and, in homely English, a *bellows sound*. This is the generic sound. It may be divided into species; but it is scarcely worth while so to divide it. We are only likely to confuse our notions by over-refinement. So I will only add, that, when this bellows sound is very harsh or rough, persons will tell you that it is more like the noise of a rasp, or a file, or a saw: but all the while it is some kind of bellows sound. These sounds are often denominated *murmurs* also.

Now what is the cause of this singular deviation from the natural noises made by the successive contractions and relaxations of this hollow muscle, the heart? The whole matter may, I believe, be briefly thus expressed. The blowing sound may be occasioned by any change which alters the due proportion between the chambers of the heart, and their orifices of communication with each other, and with the blood-vessels that respectively enter or leave them; it may also be occasioned by a preternatural velocity in the passage of the blood through a healthy and well adjusted heart. Dr. Elliotson, I think it is, who has offered this apposite illustration of the phenomenon. If the arches of a bridge have a certain relation to the quantity of water in the river, and to the force of the current, the water passes through them quietly, and without any noise. Diminish the size of the arches, and the water begins to go through them with an audible rushing or roaring sound. The very same thing will happen if the arches remain unchanged in size, but the quantity of

water in the river, and therefore its velocity and force, be augmented by heavy rains. So it is in the heart. If one of its orifices—say the aortic orifice—be narrowed, by disease of the valves, or in any other way, the blood will not, as before, glide through it smoothly and without noise, but will yield that sound which we call a bellows sound. So, also, if the orifice retain its natural dimensions, but the capacity of the cavity from which the blood is driven be augmented. Nay, the same blowing sound may be produced though the cavities and orifices be all healthy, and duly proportioned to each other, if the velocity of the circulating blood be increased beyond a certain measure. If you bear this explanation in mind, it will be found applicable, I think, to almost every case in which there is a blowing sound accompanying the *systole* of the organ. If, at the same time, the valves over which the blood must pass be rigid, or rough, or even loose and vibrating, those circumstances may modify the blowing sound, and render it louder, or hoarser than it would otherwise be, and justify the appellations of *bruit de scie*, and *bruit de rape*, with which you will find the French books, and many of our English also, full.

But this explanation applies to a *systolic* blowing sound only. What are we to say when there is a similar sound attending the diastolic movement of the heart? Why a diastolic bellows sound will mostly, if not always, be found to accompany and denote some organic disease affecting the valves of the heart. Thus, if the mitral valve be converted, as it often is, from a loose flapping valve into a bony and rigid unvarying chink, the blood which passes through it from the auricle to the ventricle, during the diastole, may (though it seldom does) cause a rushing or blowing sound. On the other hand, the reflux of blood through the unshut mitral orifice, during the ventricular contraction, may also be attended with an audible noise; and thus we have another and not unfrequent source of a *systolic* murmur. Again, if the aortic valves are imperfect, as they often are, and do not effectually close that vessel, blood will regurgitate through them during the diastole, and produce a bellows sound. That this is the true explanation of the diastolic murmurs, I am convinced, both by the observation of disease, and by the results of experiments on animals. In some of Dr. Holt's experiments, which he was good enough to allow me to witness, the short clack of the diastole was at first distinctly audible; then hooks were introduced, so as to prevent the perfect closure of the sigmoid valves during the diastole, and then the short smart clack was converted into a prolonged bellows murmur; and, upon letting them go again, the short smart clack recurred. The presence of a diastolic bellows sound has repeatedly enabled me to foretell some disease of the sigmoid valves, interfering with their proper function—that of forbidding the re-entry of the blood into the ventricle from the aorta; and what I have thus predicted during life, has been verified by observation after death.

Such are the principal sounds, natural and morbid, which are audible to the naked eye, applied to the præcordial region, or which may be heard through the stethoscope. But we derive assistance, in respect to cardiac disease, from percussion also. It enables us to measure, in some cases, the bulk of the heart; in others, to ascertain that the pericardium is distended by fluid. In the perfectly healthy state of the viscera of the thorax, the heart is somewhat overlapped by the thin edge of the lungs; and the sound elicited by percussion over a part of the præcordial region is intermediate between the hollow sound rendered by lung, and the flat sound yielded by the solid heart. In the centre of the præcordial region, where the heart is not covered by lung, the sound is decidedly dull. When, however, the heart is enlarged by disease, a larger part of its surface is exposed, and a larger portion of the præcordial region yields a dull sound on percussion. And when the pericardium is full of liquid, which distends and expands it, you will sometimes find that not less than a third part of the anterior and lateral portion of the left side is quite dull: and it is interesting often to measure, by percussion, the diminution or extension of the limits of the dulness, as the amount of fluid effused decreases or augments.

What I stated before, concerning the effect of different positions of the body upon the space over which the healthy beating of the heart may be heard, felt, and sometimes seen, applies, *mutatis mutandis*, to the natural dulness which it causes when the præcordial region is percussed. This dulness comprehends a space of between one and two square inches, reckoning from the spot where the impulse is felt, towards the left edge of the sternum. The dulness should diminish or disappear, in the supine position, and when a full breath is drawn; and increase in degree and extent upon a forced expiration, and when the posture is prone.

There is another physical sign which is much dwelt upon by Laennec, and which is sometimes very striking. In certain conditions of disease, the hand placed over the situation of the heart perceives a peculiar thrill or vibration accompanying its movements. The sensation conveyed to the hand is really very much like what Laennec compares it to, viz. that tremor which you feel, when coaxing the back of a cat while it is purring with pleasure. Accordingly he calls this sensation "fremissement cataire," the purring thrill. You feel this vibration often when there is present also a loud and strong bellows-sound; and Dr. Davis is of opinion, that the bruit de soufflet, and the fremissement cataire, constitute, in fact, but one phenomenon, which is rendered evident to the touch by the vibrations communicated to the hand; and to the ear by the vibrations communicated to it through the solid walls of the chest. I know, however, that the fremissement cataire does accompany other sounds, as well as the bellows-sounds: sounds of which I have not yet had any occasion to speak, but which I shall make you acquainted with when we come to the subject of pericarditis. And I pass from this general account of the sounds belonging to the action of the heart, in health and in disease, to consider the other symptoms by which we judge that such disease is present.

General symptoms of cardiac disease.—Among the general symptoms, then, of cardiac disease, some are direct—as pain; palpitation or excessive action of the heart perceptible by the patient; irregular or intermittent action, which the patient may or may not be conscious of: and some are indirect, declaring themselves through the medium of other parts ond organs—such are dyspnœa; cough; dropsical accumulations; hæmorrhages; various affections of the nervous system, especially an increased and morbid sensibility, what is usually called *nervousness:* and some others, which I will cursorily notice as we proceed.

I shall take this opportunity of considering, once for all, some of these symptoms; whether they really proceed from organic disease of the heart or not: for the determination of the question, whether they do or do not indicate such disease, is often of great moment, and is not always easy.

We are not, in general, sensible of the beating of our hearts: but when the pulsations become inordinately forcible, they make themselves felt, and the sensation is, in many cases, a most troublesome and distressing one. Palpitation implies increased force, or increased frequency—or an increase both in force and in frequency—of the contractions of the heart. Every one has experienced palpitation in his own person who has run himself out of breath. The pulsations are sometimes tumultuous also, and irregular, as well as unduly frequent and forcible; but this is by no means always or necessarily the case. There may be great palpitation with perfect regularity of the heart's action. The increased beating not only can be felt internally by the patient, but it may often be heard both by himself and by others. However, we do meet with persons whose hearts throb with excessive violence without their being at all aware of it. Such cases are always, I believe, cases of disease; whereas the palpitations that annoy and harass the patient are very often connected with functional disorder only.

Irregular action of the heart consists in some derangement or discord of its rhythmical movements, and is discovered by the condition of the arterial pulse—by unnatural fluctuations in the strength, or in the number of its beatings, or in both. Sometimes a few rapid and feeble pulsations occur at uncertain intervals, and are followed by others that are fuller and slower. Sometimes one or more beats are left out, and the next beat, as if to make up for this pause, is unusually strong. The pulse is then said to *intermit*. The intermissions may be unperceived by the patient himself; but in general they are attended with a singularly disagreeable fluttering, or trembling sensation in the breast. The pulse may intermit though the heart does not: the ventricle may now and then contract so faintly as not to propel a wave of blood so far along the artery. Intermission implies irregularity; but the action may be irregular and disorderly without intermitting.

Now, any of these deviations from the natural rhythm and action of the heart alarm people very much, and impress them with a belief that they have some fixed disease of that organ; and you will continually be appealed to for your opinion on this point. I suppose there are few medical students who have not, at some time or another, admitted into their minds the apprehension that they had disease of the heart; an apprehension

engendered by its occasional palpitation or irregularity. For though there may be palpitation without irregularity, yet it is practically convenient to consider the two together.

These deviations certainly belong both to organic disease and to mere functional disorder of the heart; but I repeat, that in a great number, nay, in a great majority, of the cases in which they so distress and alarm the patient as to lead him or her to complain of them, they are unconnected with any change of structure; and this it is of much importance that you should be aware of.

Palpitation of the heart, and intermission or irregularity of the pulse, are often dependent upon some disordered condition of the stomach, and will cease at once when that disorder is rectified. It is curious that this may happen although the gastric affection does not manifest itself by any other symptom: and it is curious too, how slight a cause may suffice to produce the irregular action. A friend of mine, a barrister, used to be very anxious about himself, because a fluttering sensation frequently occurred at his heart; an intermission of one or two beats, and then a violent throb when the organ again resumed its play. This is a sensation very familiar to myself, and probably most persons have occasionally experienced it. However, it happened so often to the gentleman I speak of, that it made him very unhappy. He persuaded himself that he had disease of the heart, and that he should some day suddenly drop down dead. But there was no other symptom of cardiac disease direct or indirect, general or physical. He was accordingly told that the intermission depended upon some fault in his digestive organs; and he was advised to leave off different articles of food and drink in succession, in order to discover whether any one thing in particular offended the stomach, and gave rise to the symptom. He began by abstaining from tea, which he had been in the habit of drinking in considerable quantity; and thereupon the fluttering of the heart ceased. After a while he took to tea again, and then the fluttering returned. He repeated the experiment many times, and always with the same result, till at length his mind was satisfied; and by renouncing tea altogether he got rid of his palpitation and of his apprehensions. I mention this instance, because it came within my own cognizance; but it is only a sample of many such, and *tea* is frequently found to be the disturbing substance.

I must caution you, however, against the mistake, which is often made, of inferring that the heart is free from organic change because its regular movements are accompanied by dyspeptic symptoms. Structural disease of that organ is very apt to derange the digestive functions. You will commonly find that patients who labour under such disease are exceedingly liable to flatulence of the stomach; and free eructation of the gas which plagued them mitigates wonderfully the cardiac distress. It does so, no doubt, by relieving the diaphragm from that upward pressure which embarrassed the motions of the heart.

We judge that palpitations and irregularities are merely symptomatic consequences of gastric disorder when they occur occasionally only; when the rhythm of the heart is perfect during the intervals; and when we fail to discover any other physical or general signs that its texture has undergone alteration.

Besides these overstrong or irregular movements, which are symptomatic of disorder of the stomach, and are remedied by correcting that disorder, there are palpitations of a purely nervous kind. I mean, that they depend upon a peculiar and highly sensitive condition of the nervous system; which condition is itself dependent, in general, upon a particular state of the vascular system. Persons of a "movable" constitution, whether male or female, are subject to these palpitations: but especially young women; and, of these, such as are pale, exsanguine, hysterical, in whom the menstrual functions are deficient, or excessive, or somehow unnatural. Anæmia, if not a constant, is certainly a frequent and most remarkable feature of this nervous state. The blood is aqueous; poor in fibrine, and red particles. The age, and frequently the sex, of the patient form leading points in the diagnosis. Nervous palpitations are apt to come on when the patient is quite at rest: palpitations that result from organic disease are, on the contrary, always mitigated by repose. The occurrence of palpitations *in the night*, however, is but an equivocal circumstance, for nervous persons who dream, awake often with palpitation; and the recumbent posture generally excites or aggravates the palpitations that are organic. Neither, in forming our diagnosis, can we trust entirely to the presence or absence of physical signs. The heaving impulse of hypertrophy is indeed wanting; but, as I told you

formerly, the short abrupt knock of chlorotic palpitation is often attended with a bellows sound. The weak and flabby muscle dilates (I fancy), and the natural proportion between the chamber and its outlet is for a time destroyed. The sound may partly depend upon the thinness of the blood in such patients; and this reminds me of another diagnostic clue which you should be acquainted with. In nervous susceptible persons, especially if they exhibit the pallor of anæmia, very curious noises are often audible, by means of the stethoscope, in the *neck*. Continuous rushing or roaring sounds, very like those which are to be heard in shells, and which poets feign, and the vulgar believe, to be the noise of the distant sea.

> Shake one, and it awakens; then apply
> Its polished lips to your attentive ear,
> And it remembers its august abodes,
> And murmurs as the ocean murmured there.

Sometimes the sound is more like the hum of a gnat, or the sighing of the wind through a crevice. Dr. Hope very truly states that it may be imitated, by a prolonged whispering pronunciation of the syllable *who*. Bouillaud, from its resemblance to the whizzing of a well-known toy, calls it the "bruit de diable." He supposed this singular sound to reside in the arteries of the neck: but it is quite distinct from the true arterial bellows murmur, and it has been clearly shown (first by Dr. Ogier Ward, of Birmingham), that it is produced by the descent of the attenuated blood through the great cervical veins. The sound, though continuous, has often a marked and regular increase, or swell, which keeps time with the heart's systole, and is believed to depend upon the pulsating pressure of the contiguous artery. It is best heard on the right side of the neck, just below the clavicle, and just behind the posterior edge of the sterno-mastoid muscle. You must take care not to *produce* these sounds, as you easily may, by pressure with the stethoscope. So, also, you may suspend them at your pleasure, by pressing, above the stethoscope, upon the track of the veins, so as to stop the current of blood through them, without arresting the pulsation of the arteries. This proves that the murmurs are *venous*. I have no leisure to go more into particulars concerning these sounds; but when you meet with them, concurring with cardiac palpitations, in a young, nervous, anæmic subject, the palpitations, ninety-nine times in a hundred, will turn out to be simply functional—independent of any organic disease. No doubt there may be coexisting change of structure; but that is a rare exception, and when it does occur, the signs proper to structural disease will be present, and will betray it.

Now these palpitations, and these musical or rushing sounds in the jugular veins, are to be cured by remedying the state of the blood. And the remedies are preparations of steel, aloetic purgatives, animal food, the cold shower-bath, and exercise, short of producing great fatigue, in pure air.

I have further to remark, with respect to intermissions of the heart's action, and therefore of the pulse at the wrist, that they are frequently connected, both in health and in disease, with feebleness, and also with unusual slowness, of pulsation. So that a slow pulse which is likewise feeble is often converted into an intermitting pulse by depletion; by blood-letting, for example, or by an active purgative; and the intermittence may be removed again by a stimulant. I mention this now, because there is another and very different state of disease, in which the pulse is apt to intermit. I mean when there is *plethora capitis*, and cerebral mischief is present or impending. But then the pulse will be full and strong, and labouring. In these cases a stimulant treatment would of course be injurious; while blood-letting, which would *cause* the other form of intermission, is the *remedy* of this.

Some assistance in determining between organic disease and mere functional disorder of the heart may perhaps be derived from observing the position of the patient. It is stated that when there is mere nervous palpitation, the patient lies as well, and perhaps better, on the left side: whereas, when the heart is actually diseased, the decubitus on the right side is more comfortable than that on the left. If there be any tenderness of the heart, or of its enveloping membrane, the posture on the right side is supposed to be the easiest, because the heart is further removed from the ribs, and impinges upon them during the systole with less force. However, no great stress can be laid upon this symptom.

Of the general remaining symptoms of heart-disease there is not much to be said. Dyspnœa and cough are indirect symptoms declared through the lungs, between which and the heart there is a close and obvious reciprocal influence. But dyspnœa and cough are direct symptoms of pulmonary disease; and even of pulmonary disease they scarcely help the precise diagnosis. That disease of the heart may materially alter the quantity of blood that is sent to, or transmitted from the lungs, is too plain to require any formal proof; and where the quantity of blood in the lungs is affected, the quantity of air necessary to ventilate that blood must vary: in other words, dyspnœa must ensue. Hæmoptysis is also an equivocal symptom.

One very common effect of cardiac disease is an impeded and sluggish transmission of venous blood from the abdominal viscera. Hence congestions of various parts, and especially of the liver, which enlarges and grows tender; and the biliary secretion and functions are deranged. These symptoms are a fruitful source of mistake, leading the unwary practitioner into the belief that the whole of his patient's malady is hepatic; whom he comforts accordingly with the assurance, that "it is all liver."

The circulation through the *brain* is also apt to be much disturbed in heart diseases: and to this circumstance we must attribute the headaches and giddiness that often accompany them; the dread and causeless apprehension which such patients frequently exhibit; the cowardice and irritability which disease of the heart engenders in men who previously were intrepid, and of strong and firm nerves; also that propensity to dreaming, and especially to distressful and frightening dreams, so commonly observable in them; and the sudden starting from sleep in horror and alarm. The relations that exist between apoplexy and organic disease of the heart, were fully discussed in a former lecture.

One of the most common indirect symptoms of cardiac disease is dropsy; yet sometimes the disease of the heart may continue long, and even prove fatal, without giving rise to any dropsy. It will produce that symptom or not, according as it leads to venous congestion or not. Hence dropsy is more particularly connected with dilatation and attenuation of the right cavities of the heart. But these are points to which I must revert.

Having thus run over, Gentlemen, the morbid changes to which the heart, as a muscular organ, is liable; the alterations of thickness in its walls, and of capacity in its chambers, and the derangements of the natural relations between the several chambers and their orifices; having considered, also, in a brief and cursory manner, the sounds which the heart gives out in its different movements during health, and the modifications to which these sounds are subject in disease; and having, moreover, passed in review the general symptoms which frequently display themselves in connection with cardiac disease, we shall be the better prepared, I hope, to investigate, when we next meet, some of the specific diseases of that important organ.

LECTURE LX.

DISEASES AFFECTING THE MUSCULAR TEXTURE OF THE HEART; AND THEIR TREATMENT. CHANGES TO WHICH THE VALVES OF THE HEART ARE SUBJECT. EFFECTS, AND DIAGNOSIS OF THOSE CHANGES. ANGINA PECTORIS.

I KNOW not how I can so well put you in possession of what I know, or think, concerning particular structural diseases of the heart, as by taking them in succession, and offering a sort of running commentary upon them. The *mechanism* of those structural changes, and the altered sounds, and the other physical signs, arising out of them, I endeavoured to explain in the last lecture. Bear in mind that, in this place I can do no more than draw broad outlines.

Simple hypertrophy of the left ventricle. This sometimes occurs when we can discover no mechanical obstacle to the passage of the blood out of the ventricle, which might

account for it: none, I mean, by the closest scrutiny made even after death. Is it, then, possible that this change may be brought about by physical causes which are not permanent, and have no place within the body: such as undue action of the organ for a length of time, in consequence of habitual bodily exertion? A runner, for example, we may conceive to keep his heart beating with a degree of force and frequency beyond what is natural, for the greater part of the day; and that for many days, or weeks together. Again, can simple hypertrophy grow out of that excessive action of the heart which may be kept up, day after day, for a long period by protracted mental emotion? It is difficult to answer these questions. But I presume that causes of this kind—that any cause, in short, which implied long-continued increase in the function of the organ,—*would* suffice to generate hypertrophy. What is certain, however, is that such causes seldom do act with sufficient intensity and constancy to produce these effects: and simple hypertrophy of the left ventricle, with no physical obstruction to the flow of blood through the heart, and no impediment to the free play of the organ, is rare.

We ascertain its existence when it does exist, first, by the account which the patient gives of himself. He has a *sensation* of beating of his heart which he ought not to have; he feels it, and hears it beating as he lies awake in bed; or even at other times when he is at rest. The pulsations are regular. Hypertrophy has no tendency in itself to cause the pulse to intermit or to become irregular. There is no marked dyspnœa: the circulation of the blood through the lungs is not much affected by this alteration of the left ventricle; they are in fact protected by the mitral valve: there is seldom any dropsy: but the arterial circulation being forced, there is a tendency to active congestion in the capillary vessels. As there is no mechanical obstacle to bridle the excessive power of the muscle, the pulse is full and strong; the face is florid; the patient is liable to headache, to bleeding from the nose, to active hæmorrhage, and to local inflammation. If you listen to the heart in such a case, you find that the systolic sound is less loud and clear than natural. It is not heard beyond the præcordial region, nor even perhaps over its whole extent: but there is no bellows sound. And if you place your hand upon the left breast, you feel that steady, swelling, incontrollable impulsion, which I spoke of in the last lecture as the surest sign that I am acquainted with, of hypertrophy. Sometimes the præcordial region is manifestly bulging and prominent.

If I were to preach for an hour concerning the treatment of such cases, I could say no more than this: that they require perfect quiet of mind and body; undeviating abstinence; in short, the strict observance of the antiphlogistic regimen as formerly described; and some of the antiphlogistic remedies: particularly moderate topical bleedings, often repeated; with a close attention to the functions of the digestive organs. These are the cases in which, if in any, we may expect to *cure* hypertrophy.

If simple hypertrophy of the left ventricle be rare, hypertrophy of the same chamber from a mechanical obstacle, or from some hindrance to the easy working of the hydraulic machine, is exceedingly common. What difference, then, let us inquire, is made in the symptoms, in the treatment, and in the prospect of recovery, by the presence of a physical impediment, out of which the hypertrophy has grown?

The mechanical impediment will frequently signify its existence, by causing some unnatural sound: a systolic bellows sound most commonly, which is audible over the sternum, along the course of the aorta. And the mechanical impediment will *tend* to cause faltering of the pulse; but generally the hypertrophy corrects that tendency. So, on the other hand, the mechanical obstacle corrects the tendency of the hypertrophy to cause active capillary congestion: and when the obstacle is considerable, it will prevent the pulse from being so full and strong as in the former case. If to the physical signs of hypertrophy of the left ventricle there be added a systolic bellows sound, and a disproportionate smallness and feebleness of the pulse at the wrist, we may safely conclude that there is some impediment to the escape of the blood from the left ventricle into the aorta; and that this impediment has given occasion to the hypertrophy.

Now, in this case, the hypertrophy is really an endeavour towards health. The increased power of the ventricle compensates for the bar which is opposed to the current of the blood. The blood would not be able to go on without the hypertrophy. There would ensue a tendency to stagnation in the circulation, a faltering pulse, imperfect arterializa-

tion of the blood, blue cheeks and lips, dyspnœa, dropsy; but the augmentation of bulk and force in the impelling muscle obviates this: obviates it at least for a while: puts off the evil day to a distance. Since this is the case, and since we have no means of removing the mechanical impediment, we should be mad to desire the cure of the hypertrophy, which is to a certain degree a remedy for the impediment; nor indeed could we cure it if we would. But we have to endeavour to keep it within due bounds. If the beating be troublesome to the patient, we may alleviate that symptom, and check what there may be unnecessarily active in the contractions of the morbid chamber, by abstracting blood from the præcordia by leeches; and by soliciting the action of the kidneys, by means of cooling diuretics, among which small doses of digitalis may find an appropriate place. The labouring action of the heart is sometimes calmed by the application of a belladonna plaster. In this variety, also, of the disease, it is of primary importance that no undue efforts of the body be made, and that the patient be protected, as much as possible, against all causes of mental emotion; that scrupulous temperance be enforced; and that all the functions of the body be carefully watched and regulated.

These are not cases in which we can look for recovery: but they are cases which bad management and imprudent habits may hurry on to a fatal termination; and which judicious treatment and a well-regulated course of living may render tolerable, and carry forwards for a considerable period.

Under the same condition of mechanical impediment, we oftener have eccentric hypertrophy of the left ventricle; hypertrophy, *i. e.*, with dilatation. Of course the bulk of the whole organ is augmented by both of those conditions; and sometimes it becomes enormous, as big as that of a bullock. The symptoms will differ somewhat, according as the hypertrophy, or the dilatation preponderates, and therefore it will be as well to state here what are the symptoms of simply dilated ventricles. They are a diminished *impulse* of the heart's action; and therewith a clearer sound than is natural. The first sound approximates to that of the heart's diastole; to the clacking second sound, and it is heard extensively. There is more or less tendency to fluttering palpitations and irregularities of the pulse, which is usually weak and small; to faintness and debility, and to coldness of the extremities: and when the *right* ventricle is dilated, there are some other symptoms which I shall notice presently.

Now, I say, there will be a mixture or modification of the symptoms, when the left ventricle is both dilated and hypertrophic. The dilatation will aid the mechanical impediment in giving a tendency to irregularity and intermission of the pulse; and the hypertrophy will tend to rectify that disposition. And we must *trim* our management of such cases accordingly. If the pulse flutters, we cautiously administer tonics, or stimulants: if it is steady, and the signs that belong to simple hypertrophy predominate, and are excessive and troublesome, we must starve the patient, take blood from his side, purge him, and give him diuretics; but at all times keep him as *tranquil* as we can.

Simple hypertrophy of the right ventricle is not a common disease. When it occurs, it results from some actual or virtual impediment to the passage of the blood from the ventricle into the lungs. The most extreme instance of it that I ever saw, was in the heart of a medical friend's son, who died at the age of seventeen; having been for many years affected with the *morbus cæruleus* as it has been called, *i. e.*, an habitual blue state of the cheeks, lips, and tongue, finger nails, and the skin generally; attended with shortness of breath, and augmented by every kind of exertion. It is seldom that persons thus affected live so long as this poor boy did. The heart, as is usual under such circumstances, was malformed. The septum between the ventricles was imperfect at its upper part; and the aorta belonged as much to the one ventricle as to the other. The pulmonary artery would not admit a goose quill; the walls of the right ventricle were as thick as those of the left.

Authors tell us that hypertrophy of the right ventricle of the heart is a cause of pulmonary apoplexy. I explained to you in a former lecture why I cannot believe in this doctrine. In the first place I say that the increased thickness and strength of the walls of that chamber supply a measure of the *difficulty*, and not of the *freedom* and *force*, with which the blood is conveyed to the lungs. In the second place pulmonary apoplexy does not result from rupture of vessels by the *vis à tergo*, nor in general from rupture of vessels at

all; and therefore is a quite different lesion from *cerebral* apoplexy. It is simply an *accident* of pulmonary *hæmorrhage*. And lastly, I never met with pulmonary apoplexy coincident with mere hypertrophy of the right ventricle. The right ventricle lies *on this side* the lungs, in the order of the circulation; and accordingly, following the rule I mentioned in the last lecture, its morbid states are for the most part *effects*, and not *causes*, of *pulmonary* disease.

The commonest affection of the right ventricle is dilatation without any increase of thickness, but with attenuation even, of its muscular parietes. This is in general the consequence of *long standing* pulmonary disease; which has prevented the easy passage of the blood out of the right ventricle. It is often, or ultimately, attended with dilatation of the right auricle, and of the jugular veins, which stand out in relief from the sides of the neck, and exhibit an undulating sort of pulsation, produced by the regurgitation of a part of the blood, whenever the ventricle contracts. I have taken from the neck of a person dead of such disease, veins into which I could pass my forefinger. With all this there is a fluttering action of the heart, an irregular pulse, great distress and shortness of breathing, a dusky skin, and blueness of the countenance, which is bloated and anxious: and a tendency to delirium and drowsiness; while, sooner or later, the whole cellular tissue of the body becomes charged with accumulated serum. Some degree of this may now and then be noticed towards the fatal close of phthisis. Much oftener it accompanies the latter periods of extensive pulmonary emphysema. The same condition of the right chambers, producing the same afflictive consequences, is the very frequent sequel of organic changes that originated in the left side of the heart.

Disease, such as I am now describing, in its advanced stages especially, is difficult to treat. If you stimulate, you run the risk of increasing the patient's distress; if you deplete, you incur the hazard of producing fatal syncope, of bringing the heart to a pause from which it is never able to recover. Here, again, you must try to keep the kidneys active; you must enjoin that as far as may be possible, all causes of agitation or hurry, every thing which has previously been found prejudicial to the patient, may be sedulously warded off. I have found more benefit in these cases from the cautious employment of steel, than from any other drug. Without forcing the heart's action, it appears to have the effect of increasing the tone of its muscle; which it thus enables, *for a time*, to compete more successfully with the load it has to carry, and the impediment which it cannot overcome. We can do no more in such cases than to palliate.

Sometimes the parietes of one or of the other ventricle become so thin--either in consequence of dilatation with tenuity, or through ulceration of the muscular tissue—as to bulge out into a pouch, or even to crack; in which case the patient almost always dies suddenly, the motion of the organ being stopped and strangled by the effusion of blood into the pericardium; so that to die of a broken heart, is not a mere metaphor. A clergyman from the country, whom I previously knew, called at my house in the autumn, and waited some time in my absence; but went away at last without seeing me: and after consulting Dr. James Johnson, set out for his home, ten miles on the other side of Colchester. He had been unwell for some time; had suffered occasional attacks of dyspnœa; and was unusually nervous and irritable. He must have been conscious of some severe distress, for he was extremely anxious to get home, and bribed the post-boys to drive fast. As soon as he reached his own house, he took some supper, and went to bed, apparently comfortable. Half an hour afterwards one of his servants went to him, and found him asleep. At the expiration of another half hour, he was again visited, and was then a corpse. Among other changes, the pericardium was full of blood, which had escaped from the heart through a rent in the left ventricle, large enough to admit one's finger. That part of the ventricle which surrounded the laceration, was unnaturally thin, to the extent of a crown piece. There are several specimens of rupture of the left ventricle in the Museum at St. Bartholomew's Hospital. George II. died of rupture of the heart. It is curious enough that a Duchess of Brunswick, of the same family with George II. died of the same disease. In her case an ulcer penetrated the parietes of the *right* ventricle, which in other respects was healthy. In most instances, the rupture has taken place in the left ventricle. The same chamber is liable also to *aneurisms:* that is, to partial distension of its walls into lateral cells or pouches. This form of disease has not been met with in the right ventricle.

Hypertrophy—or dilatation—or dilatation and hypertrophy—may affect, in their various degrees and combinations, one chamber only of the heart; or several at the same time; or all of them together. It would be vain to attempt to represent, in verbal description, these complicated changes. Enough, I trust, has been said, to enable you to unravel them when they come before you; and to ascertain, with sufficient exactness, the general indications which they severally furnish, and the plan of treatment which they require.

You will often find the muscular substance of the heart pale, soft, and flabby; easily broken down, or penetrated by pressure. This may occur with general debility and looseness of tissue; it sometimes accompanies a plentiful deposit of fat about the organ: and it is supposed to be sometimes also a consequence of inflammation affecting the muscle. Walls thus soft are likely to yield under pressure: but I know of no particular symptom by which we can detect such a state of softening.

The morbid conditions of which I have been speaking, involving the muscular substance of the heart, spring very frequently indeed from pre-existing morbid conditions of the membrane lining the heart, or of the membrane investing the heart. It is necessary therefore, in the next place, to inquire into the nature and history of *these* morbid changes: and I will first request your attention to the diseases of the lining membrane. The investing membrane is familiar to you as the *pericardium*. Of late years, since the diseased states of the internal membrane have been more studied and understood than they formerly were, *it* has been called the *endocardium:* a convenient enough name, which may occasionally spare us circumlocution. Now, certain parts of the endocardium are much more obnoxious to disease than others: those parts which enter into the fabric of the valves and orifices of the organ. The membrane is here in close contact with a dense fibrous tissue; and participates in the changes to which that tissue is subject. And it is an important fact, that the valves and orifices of the left side of the heart are much more frequently affected with disease than those of the right side. I have adverted to this fact before. What is the prevailing cause of it I cannot tell; but it seems to be a portion of a more general fact; namely, that the arteries are more liable to chronic morbid changes than the veins. Some explain the difference by alleging that the left side of the heart has the heavier task to accomplish. But nature seldom executes her purposes so clumsily, as not to adjust the strength of her machinery to the labour it is destined to perform. Others remark that fibrous tissue is more abundant, and therefore the changes proper to that tissue are more numerous and extensive, on the left side. And this may be the true explanation. Others, again, have conjectured that the arterial blood is more irritating than the venous. But there is no evidence of this: and it is better to content ourselves with noticing the fact, without attempting to account for it by mere gratuitous hypothesis.

Valvular changes.—You are not, however, to suppose that the right side is exempt from valvular disease. When there is much change on the left, we often find a less degree of the same kind of change upon the right. The valves of the pulmonary artery are, perhaps, the least frequently of all the valves found otherwise than healthy.

Many of the alterations that take place in the internal lining of the heart result, apparently, from inflammation, which causes a deposit of lymph upon or beneath the serous membrane. The valves are apt to lose their thinness, and transparency and pliancy. They become thick, stiff, puckered, curled up, or glued to each other, or to the opposite walls of the channel. On the other hand, quite independently of inflammation, they may become morbidly thin, riddled with holes, and even rent asunder. What are called vegetations or excrescences may also project from them, very much resembling warts. Or they may be converted wholly or partly into bone.

Alterations of some kind or another are very frequent in the *semilunar valves* of the *aorta*. When they are of such a nature as to diminish the orifice during the systole, they commonly occasion a systolic bellows sound. When the diseased valves offer no obstruction to the exit of blood from the ventricles, but do not close immediately afterwards, so as effectually to prevent the reflux of that fluid from the aorta, they commonly give rise to a diastolic bellows sound. When both these defects of function occur, there is often a *double* bellows sound; a sawing alternate noise; one murmur during the systole, another, distinguishable in tone and quality, as well as in time, during the dias-

tole. These sounds are conveyed along the tube in which they are formed, and are therefore most audible in the track of the aorta, as it leaves the heart. If the sound be diastolic, it will *take the place* of the smart clack of the second sound of the heart, or perhaps succeed it. Sometimes the new sound is very loud and curious. I had a patient in the hospital last year, in whom this diastolic sound was, in character and intensity, like the cooing of a pigeon. The patient could plainly hear it; nay, it could be heard by a person standing near him, but not touching his body, even with a stethoscope. In that instance we found one of the aortic valves irregularly thickened, with its free edge loose and flapping, and unable to fulfil its function of closing the aperture. During the diastole it was retroverted, and vibrated in the regurgitating stream of blood; and thus, no doubt, the musical note, heard alternately with the first sound, was produced. In March, 1837, I heard in a man (Henry Milton) who was under Dr. Latham's care in St. Bartholomew's Hospital, and who had acute rheumatism, a very shrill diastolic sound, like the repeated whining of an imprisoned puppy dog wishing to be released. This remarkable sound was audible, by means of the stethoscope, even in the radial artery. The patient died at last in St. George's Hospital, and his case is mentioned in Dr. Hope's book on the Heart. One of the aortic valves was torn downwards to some distance from its edge, and formed a flap, which was perforated by a round hole.

I need not again point out to you the manner in which such disease of these semilunar valves tends to produce hypertrophy and dilatation of the left ventricle.

The *mitral* valve is often thickened; and it is particularly subject, more so even than the aortic valves, to ossification. And the effects of the ossification are to prevent its closing the auricular orifice during the systole; and to prevent its lying flat against the walls of the ventricle, and allowing a free passage of the blood out of the left auricle, during the diastole. The orifice is often converted into an unvarying oval slit, with puckered edges, and resembling a button-hole; or the valve projects, like a thimble of bone, into the left ventricle. And it is remarkable how small the chink, which is thus permanent, may be, and yet life go on. The heart, having been taken out of the body, and the auricle filled with water, I have seen the water pass into the ventricle, by its gravity, *stillatim;* drop by drop.

Let me just remind you, that the direct and necessary consequence of constriction of the mitral orifice, is an accumulation of blood *behind* it; *i. e.*, in the left auricle, in the pulmonary veins, in the lungs. Hence so much mechanical congestion, that the blood bursts at length through the bronchial membrane; hæmorrhage, slow or copious, ensues from the air-passages; and *pulmonary apoplexy* is formed.

In extreme cases, where the mischief is chiefly confined to that valve, the blood necessarily reaches the ventricle in a penurious manner; that chamber contracts unsteadily and irregularly; and its cavity sometimes diminishes. This I think I have seen. But far more commonly there is disease of the aortic valves also; and the condition of the left ventricle is that of hypertrophy with dilatation.

When there is a permanent chink in place of the limber valve, there may be a double bruit. The first heard during the systole, and produced by the regurgitation of blood from the ventricle into the auricle, through the rigid slit. This is common. The second accompanying the diastole, and resulting from the mechanical impediment to the free passage of the blood from the auricle into the ventricle. This is uncommon. The constriction must be great for the diastolic murmur to occur at all: and when it does occur, it is faint, from the comparative feebleness (I presume) of the auricular contractions.

The form and the consistence of the altered valves being the same, no difference whatever in the sounds, or in the general symptoms, will arise from the particular nature of the changes. It will, I mean, make no difference whether the obstacle to the flowing blood, or the imperfect closure of the orifice, depends upon mere thickening of the valve by cartilaginous deposition, or upon ossification, or upon wart-like vegetations. These last may be found upon any of the valves, but, like other morbid states, they are less frequent on the right than on the left side of the heart; and they are most common of all on the aortic valves. Ossification—the deposition of the phosphate of lime—is almost confined, I believe, to the left side. I never saw the tricuspid valve, or the semilunar valves of the pulmonary artery, converted into bone.

The warts, or wart-like excrescences, which are so often found upon the valves of the heart, are very curious things. Sometimes they are separate, like rows of beads. Sometimes several appear to spring from a common base, which spreads out so as to exhibit a cauliflower appearance. And occasionally they hang in long strings from the valve into the adjoining chamber of the heart. In a patient of Dr. Hawkins's, I saw a cylindrical excrescence of this kind which measured an inch in length. The valves presented slit-like perforations; and from the edge of one of these slits in the mitral valve, this long, vegetation dangled into the ventricle. The whole of the valves of the aorta were covered on their ventricular surface, with similar but shorter excrescences.

They vary much, these vegetations, in consistence. Sometimes they are soft, easily crushed, and capable of being readily detached from the smooth surface of the valve. Others are more firm, and yet separable from the valve without injury to it. Others, again, are so adherent, so rooted into the valves, that they can be removed only by tearing or cutting them off. They are found sometimes on the free edge of the valves; sometimes on their surface, or even on the inner membrane of one of the chambers, especially of the left auricle.

Much difference of opinion has prevailed respecting the nature and origin of these singular appearances. It was a common notion among the French, at one time, that they were really, what they so much resemble, venereal warts. What seems to be certain is, that they are somehow connected with *inflammation* of the internal lining of the heart; and of that which covers the valves in particular. But, then, are they lymph *poured out from* the inflamed membrane? or are they fibrin deposited from the blood *upon* an inflamed membrane? It is probable that the last is, sometimes at least, the true explanation of their origin. You know, that when the membrane lining a vein becomes inflamed, the blood in contact with it has a strong tendency to coagulate upon it, and to adhere to it. The fleshy excrescences found on the valves are often attached to the edges of slits in the valve: the broken surface having probably been the especial seat of inflammation. When the formation of vegetations is recent, they are very soft and frangible. But the most interesting fact that I am acquainted with, in evidence of the mode in which these little projections arise, is one that accidentally came to light in one of Dr. Hope's experiments upon an ass, at which I was present. The aortic valves had been held back by a wire passed into the vessel with the view of ascertaining the physical cause of the second sound. The animal was previously rendered insensible by a narcotic poison; and the circulation was kept up—languidly, however, towards the last—by artificial respiration. Upon the final cessation of the heart's motions, the organ was removed from the body and examined: and the valve, that had been mechanically irritated by the wire, was found studded with these little wart-like appearances, which were so soft as to admit of being readily brushed off from the subjacent membrane. Here the deposit took place after the death of the animal, and while some of the functions of organic life alone were kept up by the artificial breathing.

There are still some curious circumstances to be mentioned, connected, in some instances, with the formation of these warty vegetations. I shall not, however, enter upon them in the present lecture: but when I speak, at our next meeting, of rheumatic inflammation of the heart and its membranes.

Effects of morbid changes.—Any or all of the lesions that I have been describing may and must lead, at length, according to their places and magnitude, to some of those changes in the actual and relative dimensions of the heart that were considered in the last lecture. They obstruct the stream of blood when moving in its natural course, and when its passage ought to be free; or they allow of its *refluent* course, when it ought to be effectually opposed: and the necessary results, in either case, are dilatation of one or more of the chambers of the heart, with thickening, or with attenuation, as the case may be, of its walls. I have already spoken of the symptoms, physical and general, to which these secondary changes give rise; and of the treatment which they admit and require.

There being valvular disease, and that valvular disease giving rise to a bellows sound, can we distinguish the particular valve affected? Generally, we can. Our skill in diagnosis outruns here, as indeed it too often does, our skill to cure. A few simple rules and considerations enable us, in most cases, to satisfy our natural curiosity to penetrate the

exact conditions even of changes that are incapable of repair. These rules relate chiefly to the time when the murmur is heard, to the direction in which it is most audible; and to the state of the arterial pulse.

When a bellows sound accompanies the systole, it must be caused by a current passing *out* of a ventricle. But serious disease of the valves, sufficient to occasion a murmur, on the right side of the heart, is very rare. In nineteen cases out of twenty, valvular murmurs belong to the left side; so that practically the distinction lies, almost always, between two orifices, the mitral and the aortic, the inlet and the outlet of the left ventricle. The natural inlet has become an outlet also: or the natural outlet is obstructed. Now if the sound be heard at the base of the heart, and along the track of the thoracic aorta, up towards the right clavicle, and even in the carotids; and if it be less audible towards the apex, and if the pulse be steady and regular, the mischief is seated in the semilunar *valves of the aorta:* there is some obstacle which produces a ripple in the onward stream of the blood.

On the other hand, if the pulse be irregular, and if the sound be heard better towards the apex of the organ, on the left, it is owing to regurgitation through a diseased *mitral* valve. Such regurgitation is often attended with a purring thrill.

When, what scarcely ever happens, the sound does result from injury of the semilunar valves of the pulmonary artery, it is heard plainest in the track of that vessel, up towards the *left* clavicle. So also, a murmur produced by change in the tricuspid valve would be loudest towards the apex, on the right. The arterial pulse for obvious reasons is but little influenced by disease affecting the orifices of the right heart.

Again, if the morbid sound be diastolic, it accompanies the entrance of blood *into* a ventricle; and for similar reasons as before, the fault is most probably in the *left* ventricle. It may be owing to the direct, but impeded passage of the blood from the left auricle through a narrowed mitral orifice: yet this *very* seldom occasions any audible noise. Or the diastolic murmur may proceed from regurgitation through the defective aortic valves: the natural outlet having become an inlet also: and this is exceedingly common. We attend, as before, to the situation and the track in which the sound is the loudest. We listen also for the smart clack of the natural second sound; and if it is not to be heard, or is very indistinct, we have, in that circumstance, corroborative evidence of an imperfect aortic flood-gate. Moreover, we are again assisted by the pulse. The pulse of aortic regurgitation is very striking and peculiar: sudden, like the blow of a hammer, without any prolonged swell of the artery. It always reminds me of the well-known chemical toy, formed by including a small quantity of liquid in a glass tube, exhausted of air, and hermetically sealed. On reversing the tube, the liquid falls from one end of it to the other with a hard short knock, as if it were a mass of lead. The sensation given to the finger by the pulse, when there is much regurgitation through the aortic valves, is very similar to this. It is as if successive balls of blood were suddenly shot along under the finger. Dr. Hope calls this pulse a *jerking* pulse; the pulse of unfilled arteries. And this abrupt pulse makes itself *visible* in the arteries; the wave of blood lifts, and moves, and sometimes contorts the vessel. When this kind of pulse occurs with a diastolic bellows sound heard in the track of the aorta, and the short clack of the second sound is absent or diminished, you may be quite sure that the aortic orifice is patulous during the diastole. The reflux of the blood, when the patency is great, is strong enough sometimes to produce a palpable shock or jog, called the diastolic impulse. And this refilling of the ventricle from the artery may even provoke it to a supernumerary contraction.

In a patient by whom I was lately consulted, the hard, sudden, hammering pulse led me to conclude that the blood regurgitated from his aorta; and accordingly, upon applying my ear to his chest, I discovered a loud murmur, coincident with the diastole, and most distinct in the direction of the right clavicle. The shock of this man's artery was plainly to be felt, through his clothes, by one's hand laid lightly upon the bend of his arm. His wife told me that, for five years past, this jarring blow had made it uncomfortable for her to take his arm when they were walking together. The same kind of jerking impulse was strikingly perceptible in the femoral arteries, and in the carotids.

Of regurgitant sounds belonging to the right side of the heart I can tell you nothing. I never heard one, that I know of, from the pulmonic valves. Through the tricuspid ori-

fice the blood is believed to be often refluent; causing, as I stated before, turgescence and pulsation of the jugular veins. The structure of the valve permits this ebbing movement of the blood under circumstances which might otherwise be perilous. The tricuspid has accordingly been called the *safety* valve of the heart. But the reflux seldom, if ever, announces itself by a bellows sound.

Angina pectoris.—We cannot always thus rigidly connect morbid changes with definite signs. Disorders arise of which the symptoms are more cognizable and constant than the seat. We assign a name to the peculiar assemblage of symptoms, and make it thenceforth a distinct object of our study; tracing the symptoms as well as we can up to their organic causes and conditions. Now the complaint called *angina pectoris* is one of this kind. It is, moreover, a very curious and interesting disorder; and I shall devote the remainder of the present hour to its consideration.

This disease was first accurately described, in this country at least, by the celebrated Dr. Heberden, the author of the Commentaries. It had been adverted to by many writers before, but obscurely: and Dr. Heberden's observations were quite original. The description that he has given of the complaint, in the second volume of the Transactions of the College of Physicians, is very accurate and striking. He calls it a *disorder of the breast;* and observes that "the seat of it, and the sense of strangling and anxiety with which it is attended, may make it not improperly be called *angina pectoris.*"

Symptoms.—"Those who are afflicted with it are seized whilst they are walking, and more particularly when they walk soon after eating, with a painful and most disagreeable sensation in the breast, which seems as if it would take their life away, if it were to increase or to continue. The moment they stand still, all this uneasiness vanishes. In all other respects the patients are, at the beginning of this disorder, perfectly well; and in particular have no shortness of breath, from which it is totally different." Such is the brief description of the malady, given by Dr. Heberden. You will observe, that the distress occurs in paroxysms; and the patient at first, has intervals of apparent health: and even when the disease is more advanced, he has periods of comparative ease between fits of suffering. The paroxysms are especially liable to come on when the patient is walking, and, above all, when he is ascending,—going up hill. He is then seized, all at once, with a painful sensation, which seems to be, in many cases, indescribable, but which is always referred to the situation of the heart. Sometimes the sensation is spoken of as being a spasm, as giving the patient a notion of constriction; but it also carries with it the impression, that any continuance of the exertion, the stirring another step, would be fatal. Yet the patient is not out of breath. It is not dyspnœa that oppresses him; for he can, and generally does, breathe freely and easily. He lays hold of any neighbouring object for support. His face is pale and haggard; and you would suppose, from his appearance, that he was actually at the point of death. But in the early stages of the disease, the pang soon subsides, the distress is over, and the patient is entirely himself again. However, after the lapse of some time, generally of some months, the anguish does not so instantaneously cease upon standing still; nor does it always require some bodily exertion to bring it on. It will occur when the patient is quiet, even in bed. He feels as if the action of the heart was arrested: and he is obliged to rise up, every night perhaps, for many weeks together. In exquisite cases it will be brought on by causes of any kind that slightly accelerate the circulation: coughing, straining at stool, mental emotion.

The pain, which is at first referred to the region of the heart, frequently extends, accompanied by a sort of numbness, from that part to the left shoulder, and down the left arm; stopping short, in a curious manner, and from some inexplicable cause, either just about the insertion of the deltoid muscle, or at the elbow, or at the wrist. Sometimes, however, it runs down to the very extremities of the fingers; particularly of the last two fingers, following mainly the course of the ulnar nerve. And occasionally similar pains affect the right side and arm. There is (I say) no dyspnœa in the genuine form of the disease; although you will find it stated by some modern writers, of good repute, that the paroxysm is accompanied with difficulty of breathing. In the instances that I have seen, and they have been few, the patient was able slowly and fully to inspire and expire, even when the fit was on him. The truth I believe to be, that other affections, more akin to

asthma, have been confounded with angina pectoris; and this confusion has led to the belief, that it is not altogether so dangerous a complaint as used to be thought: but in its genuine shape it is undoubtedly a very fatal disorder. Dr. Forbes, by a diligent search among authors, has collected some statistical facts respecting it, which are worth remembering. Thus, out of eighty-eight cases, eight only, or one in eleven, occurred in females. The ages in eighty-four of these eighty-eight cases are recorded; and of the eighty-four, seventy-two were above fifty years; and twelve, or one-seventh of the whole, under fifty years. It is a disease, therefore, for the most part, of advanced life: and this alone would afford a strong presumption of its dependence upon some organic change. Again, the event of the disease was recorded in respect to sixty-four of the patients. Of these forty-nine died, almost all of them suddenly; while fifteen recovered, or were relieved. And among the forty-nine fatal cases, there were only two of women.

Seat.—That the seat of the disorder is the heart, and that it consists in some structural change, can scarcely be doubted. Yet some pathologists are disposed to consider it a merely neuralgic affection, "commencing, for the most part, in the pneumogastric nerve, and spreading in different directions, as other nerves become involved." But this doctrine is scarcely consistent, in my judgment, with the facts—First, that the paroxysm is excited by such causes as are "especially calculated to disturb the natural action of the heart, bodily exertion and mental emotion;" and, Secondly, that the disease is so very frequently and so suddenly *fatal.* This is not at all the character of mere neuralgic diseases in general. And when we add to these facts the further fact, viz., that, in a vast majority of instances, organic disease of the heart, or of the great blood-vessels, has been discovered after death, I think we shall be obliged to admit, that the symptoms are often (for I will not say always) dependent upon cardiac disease. One theory explains the "breast-pang," by supposing that the blood, whenever its movement is accelerated by exercise or otherwise, arrives in the heart faster than it can be transmitted onwards; and accumulating in its cavities, painfully distends them. It is not improbable that the paroxysm may be sometimes so produced. The great Dr. Jenner took a most ingenious view of the matter, which was made public and further enforced by Dr. Parry. He had found, in examining the bodies of some who had died of well-marked angina pectoris, that the coronary arteries of the heart were ossified; converted into bony canals, and constricted in their calibre. He thence concluded that the paroxysms result from the circumstance, that when some increase of the muscular contraction of the heart happens to be called for, the increased supply of blood, rendered necessary by the additional exertion, is not capable of being furnished by the diseased nutrient arteries of the organ; that the heart comes to a stand, because its muscular tissue is not duly injected with arterial blood: and the phenomena of the paroxysm agree remarkably well with that theory. He calls the disease accordingly "*syncope* anginosa." And this simple and beautiful theory was for some time admitted as the true one. However, later investigations have abundantly shown that angina pectoris may occur in a decided form, without there being any ossification or other disease of the arteries; and, on the other hand, that the coronary arteries may be ossified, and yet no angina pectoris be the result.

I may here again avail myself of the researches of Dr. Forbes, and give you a numerical account of the organic changes in the heart that have been found associated with this disease. The total number of instances collected by him, in which the body was examined after death, was forty-five. Of this number, there was disease found in the liver only, in two instances: organic disease of the heart, or great vessels, in forty-three. Dr. Forbes, indeed, makes the last number thirty-nine, instead of forty-three, excluding four cases in which nothing morbid was found in or about the heart, except an excessive coating of fat. This Dr. Fothergill considered the essence of the disease; and certainly a heart cannot be said to be in a healthy condition which is thus loaded with adipous matter. The fat is generally deposited at the expense of the muscular substance, which is apt in such cases to be thin, pale, and soft; atrophied, in short. Taking, however, the table as it is given by Dr. Forbes, the thirty-nine cases, in which there was no disease except in the heart and great vessels, were thus distributed:—In ten of the cases there was organic disease in the heart alone: in three, organic disease of the aorta alone. In one instance only was the disease confined to the coronary arteries; but there was ossification, or cartilaginous thick-

ening of the coronary arteries, combined with other disease, in sixteen instances. Again, there was ossification, or other disease of the *valves* of the heart, in sixteen cases also. There was disease of the aorta (ossification, or dilatation, or both), in twenty-four cases; and in twelve cases there was preternatural *softness* of the heart.

My friend, Dr. Latham, lately gave me this sketch of a case of angina pectoris, which had fallen under his own observation. It is remarkable for the shortness of its course. A gentleman about fifty years old, was recovering from the influenza, of which nothing remained but a slight cough, that troubled him at night. It was to relieve this that Dr. Latham was consulted. The gentleman looked perfectly well. After Dr. Latham had prescribed for this little ailment, the patient begged to see him the next day to talk over with him (he said) a very strange affection he had. Accordingly he then described a paroxysm of angina pectoris in terms that could not be mistaken; dwelling especially upon the præcordial pain, the sensation down the left arm, the sense of approaching dissolution, and then the perfect recovery. This gentleman had, during the previous summer, performed a walking tour through Switzerland, and returned home in excellent health. The first notice of his angina was not more than a month ago, when he was walking up Hampstead Hill. It was then that he had his first paroxysm. In the short period which had elapsed, the attacks had rapidly increased, in severity and frequency: occurring now every two or three days, or every day, or several times a day, with or without an exciting cause. Dr. Latham made a careful examination of the chest, and found the respiration perfect, the heart free from all unnatural murmurs, and its beats rhythmical. The only thing that particularly attracted his notice was the exceeding feebleness of its impulse. In the afternoon of the next day Dr. Latham visited him again, when he described a paroxysm he had suffered in the course of the morning, of much greater severity than any that he had hitherto experienced. Dr. Latham saw enough to convince him that his patient's existence was very precarious: and as he had previously been a stranger to him, he inquired about his friends, and took down the address of a brother, intending to call and apprise him of what he feared. On reaching his own home, two hours afterwards, a messenger met him, announcing that his patient had fallen into another paroxysm, soon after he left the house, and was dead. The body was carefully examined by a thorough anatomist, Mr. Stanley. There was no disease of the aorta, or of the heart generally; but the coronary arteries resembled tubes of coral, being completely ossified as far as they could be traced.

The patient may even expire in the first or second paroxysm. This happened, I believe, in the case of the late lamented Master of Rugby school.

Prognosis.—You will perceive, from what has been said, that the prognosis of this singular and formidable affection is extremely unfavourable. The cases are very rare in which no disease of the heart has been detected: and the organic changes that have been found are remediless, and, for the most part, progressive: and, in point of fact, the greater number of those who have laboured under the disease have died suddenly.

Treatment.—It follows, also, as another corollary from the facts now brought before you, that there are very few cases in which we can dare to contemplate a cure. Our measures must be preventive when the paroxysms are absent: and our object will be to shorten the fit when it is present and protracted.

Now the preventive measures are simple and obvious. The patient must be cautioned to avoid the exciting causes of the paroxysm; walking up hill; or against the wind, which has also been known to produce it. Whatever is likely to hurry the circulation, and, therefore, among the rest, all mental emotion and anxiety, should be guarded against as much as possible. John Hunter died of angina pectoris: and the fatal seizure was brought on by a fit of anger. Care should be taken also to obtain and preserve a healthy state of the digestive organs. It is observable of this, as I mentioned before it is observable of other cardiac diseases, that they are often attended and aggravated by flatulence of the stomach and bowels. Persons labouring under a paroxysm of angina often experience great and sudden relief upon getting rid of a quantity of gas, by which the stomach had been distended. The flatulence acts, no doubt, by pressing the diaphragm upwards, and so diminishing the dimensions of the thorax, and impeding the play of the heart. It is upon the same principle that we must explain the fact, that the paroxysms are particularly apt to

come on if the patient walks *soon after a meal:* also that they occur in the night, when he is in a horizontal position, and are relieved by his getting out of bed; that is, by his assuming the vertical posture, and taking off the pressure of the abdominal viscera from the diaphragm.

In the paroxysm itself, bleeding has been fairly tried; but, as I think might have been foreseen, it has seldom been attended with any benefit, and sometimes it appears to have done harm. The affection has a nearer relation to syncope, and often to syncope by asthenia, than to any thing else. That is the way in which the patients die: and consequently, cordials, stimulants, and antispasmodics, are found to be of service. Dr. Elliotson thinks prussic acid is the best thing you can administer. Dr. Davies has more faith in belladonna plasters than in most other things. Dr. Copland advises stimulant liniments externally; and warm carminative or aperient medicines, as the circumstances may require, internally. The general condition of the sufferer will suggest, I believe, the proper treatment. Not that it will suggest any particular drug, but it will teach you the main principle on which you are to proceed. If auscultation reveal any of those morbid states of the heart which were noticed in the beginning of the lecture, the means which I pointed out as suitable for remedying *them* may be put in force.

LECTURE LXI.

PERICARDITIS: ITS FREQUENT CONNECTION WITH ACUTE ARTICULAR RHEUMATISM. RHEUMATIC CARDITIS. ANATOMICAL CHARACTERS OF ACUTE INFLAMMATION OF THE PERICARDIUM; OF THE ENDOCARDIUM. GENERAL SYMPTOMS. AUSCULTATORY SIGNS. RELATIONS OF CARDITIS WITH ARTICULAR RHEUMATISM.

I YESTERDAY considered, cursorily indeed, but as fully as the limits of these lectures will permit, the effects of hypertrophy, and of dilatation with and without hypertrophy, of the several chambers of the heart: and the means we possess of obviating or alleviating those effects. I passed in review also the chronic changes to which the *endocardium*, especially in those parts where it covers the valves, and the tendinous rings that give support to the valves, is liable; and most of the circumstances which give origin to such changes: and I spoke of that singular and perilous affection to which the name of *angina pectoris* has been applied. I proceed this afternoon to the diseased conditions of the *pericardium*. This membrane is often the seat of acute inflammation; and the consequence of such inflammation is sometimes, though rarely, the speedy extinction of life. But in nineteen cases out of twenty, the disorder proves fatal at a remote period; destroying the subject of it more slowly indeed, but almost as surely. Pericarditis is therefore, and has always been regarded as, a very interesting disease; and the more so, that it is in many instances a very *insidious* disease also.

Causes.—Acute pericarditis is liable to arise, like all other internal inflammations, after exposure to cold; or when no exciting cause is to be discovered. But for one such case of what may be called spontaneous acute inflammation of that membrane, you will meet with a dozen, or more, in which it occurs in connection with a disease that we have not yet had before us—acute rheumatism; inflammation of a specific character, affecting the structures that lie around the joints, or enter into their composition—the *fibrous* tissues. I shall therefore consider acute pericarditis with reference to its occurrence in rheumatism; for in so doing, I shall embrace all the practical points which belong to it under any form. But I must tell you that when *peri*carditis happens, in the course of an attack of rheumatism, so also, to the best of my belief, in almost every instance, does *endo*carditis. For this reason I shall include, in the account I am desirous to give you of *rheumatic carditis*, both these inflammations: inflammation of the investing membrane, and inflammation of the lining membrane of the heart. I mentioned in the last lecture, that, in respect to the

latter, to endocarditis, there were some peculiarities noticeable which I should reserve for the present occasion.

Anatomical characters.—The pericardium is one of the serous membranes; so also may the endocardium be considered. But the pericardium is also a shut sac; and the primary effects of inflammation upon it are the same, *mutatis mutandis*, as upon the shut sac so near it, the pleura. The second series of effects is however much more formidable. Adhesion of the pleura does not necessarily abbreviate the natural term of the patient's life; adhesion of the pericardium almost always does: and effusion into the cavity that contains the lung is far less serious than effusion into the bag that surrounds the heart. In the one set of organs the mischief may be great, but it is *final;* in the other, it leads, with unfailing certainty, sooner or later, to worse changes, which at length prove incompatible with the further continuance of life.

You will understand, then, without the necessity of my going again into much detail, that the pericardium, under acute inflammation, may undergo the same changes, which, on a former occasion, we saw that the pleura might undergo. Coagulable lymph may be poured forth from the entire membrane, and abolish the cavity by gluing the whole of the pericardium to the heart: or serous fluid may be effused, distending the bag of the pericardium, and keeping its smooth surfaces more or less asunder: or both serum and lymph may be effused together; or fibrin, in some shape or another, may be *deposited*, for aught I know, from the homogeneous fluid which is thrown out by the inflamed membrane in the first instance; and the result of this mixed effusion may here also, as in the case of the pleura, be the *partial* adhesion of the membrane to the heart.

But in the majority of instances the inflammation spreads over the whole membrane, as it is apt to do in serous membranes generally; and one of these two things happens: either there is a large quantity of liquid effusion, which is not reabsorbed; and then generally the patient dies in a few days: or there is not much liquid effusion, or the liquid part is absorbed, and the pericardium becomes permanently agglutinated to the heart, and *apparent* recovery takes place.

In the cases that have proved fatal at an early period, when the inflamed membrane has been unadherent, it has been found to contain serous fluid; sometimes clear, oftener turbid, frequently tinged with blood: and it has been seen to be covered with a coating of the fibrinous or albuminous part of the blood; what we call plastic or coagulable lymph. The deposited lymph assumes a variety of forms in different cases; but in every case that I have seen, the prevailing character of the unattached surface has been that of *roughness;* and this is a circumstance of some importance, as we shall presently see. The lymph is not arranged in smooth layers; but it is rugged, villous, or cellular. According to the fancy of different observers, it has been thought to resemble lace-work, or a sponge, or a honey-comb, or some kinds of coral, or the interior of a calf's stomach. Sometimes it bristles with a multitude of small, short, pointed papillæ: less frequently it is softer and shaggy; always it is rough and uneven. Dr. Hope, following Laennec, states that the surface looks sometimes like that which would be produced by suddenly separating two flat pieces of wood, between which a thin layer of butter had been compressed. To my own eye, the appearance presented by the membrane, in its recent condition, has been more like the rough side of the pieces of tripe which you see in butcher's shops than anything else.

When, on the other hand, the patient dies, as he sometimes will do, soon after the whole of the membrane has become adherent, you will find the medium of adhesion to consist of lymph, in which a number of bloody points or streaks are visible: but still the connecting substance is soft, and the agglutinated membranes can readily be torn apart.

Such is the state of things on the *outside* of the heart in such cases. But what do we find *within?* Why, here also, in *all* cases *probably*, certainly in by far the majority of cases, we discover evident traces of active inflammation; and we discover them chiefly on the valvular apparatus. There does not appear to be such a tendency in endocarditis to diffuse itself over the whole membrane. Occasionally that naturally transparent portion of it which covers the muscular fibres is rendered whitish opaque; and occasionally some of the deposits, that are common on the valves, encroach also somewhat beyond them, and even stud, here and there, the interior of one or more of the chambers of the heart, and

especially of the left auricle. But the valves or the fibrous rings from which they spring, are the parts first and chiefly implicated, especially the aortic valves, and the mitral valve: not uncommonly the tricuspid valve also; and sometimes even the semilunar valves of the pulmonary artery. Inflammation thus affecting both the external and internal membranes of the heart, in acute rheumatism, I would call *rheumatic carditis*.

The inflamed valves undergo two kinds of change, distinct from each other. They become thicker than natural; they lose their transparency and pliancy, and are puckered. These changes depend upon the deposit of lymph *beneath* the membrane; between the membrane and the fibrous substance it covers. Sometimes they are folded down, and glued, as it were, to the opposite surface. This must be by coagulable lymph deposited on the *outer* side of the membrane. But more frequently than all, they present more or fewer of those wart-like excrescences, or fleshy granulations, which I spoke of in the last lecture, and which are of course *above* the membrane. Sometimes these vegetations are scattered irregularly over the convex surface of the valve, or in its immediate neighbourhood; but much oftener they have a more definite and curious distribution; an arrangement which I have never seen noticed by any author, but which it has been my lot so many times to observe, that it has led me to remark an anatomical peculiarity with which it is connected; and this piece of minuter anatomy I have looked for in vain in books, and have in vain sought information about it from all my anatomical acquaintances. They none of them have seemed to be aware of it, though they acknowledged that the fact was so when I pointed it out to them. Recently, indeed, I have been told that the peculiarity of structure to which I allude is somewhere adverted to by Morgagni.

It becomes necessary, therefore, that I should describe this discovery of mine (if it deserves so grand a name) to you, as I have, for several years past, been in the habit of showing it to the pupils of the hospital in the dead-house. It derives its chief interest from the light it throws upon the morbid appearances to be spoken of presently. You will find, then, if you examine closely the semilunar valves of the aorta, or of the pulmonary artery, that in each valve there may be distinguished two parts; one thicker, the other thinner. The thicker part lies next the base of the valve; the thinner next its edge. And the valve does not become thin by degrees, but the difference is marked by a manifest line of separation between the thicker and thinner portions; and this is not a straight, nor even one sweeping curved line, but it is a double curve. It consists of two semicircular lines, running each from the centre of the edge of the valve, from the sesamoid body there situated, to either extremity of the edge, where the edge joins the side of the aorta. So that there are two segments, of a crescentic shape, thinner and less opaque than the remaining part of the valve, and lying near its free margin. This peculiarity of structure is uniformly present. It is less distinctly visible in the valves of the pulmonary artery than in those of the aorta; and it is much less apparent in some individuals than in others; but it is always to be seen when it is looked for.

The anatomical account of this arrangement is not far to seek. The cardiac valves consist of a loose duplicature of the delicate endocardium, between the folds of which is received a thin prolongation of fibrous tissue, from the tendinous rings surrounding or constituting the several orifices that are furnished with a valvular apparatus. In the semilunar valves this fibrous substance does not interpose itself between the entire space of the folded membrane. It reaches the free edge of each valve at three points only; namely, at the centre, where it forms the *corpus aurantii*, and at the two extremities. Between these points it stops short, and has a definite limit and outline; a scolloped edge: and so leaves two crescentic portions of the valve formed merely by the doubled endocardium. The crescentic margins are thin and transparent: the remaining shield-shaped portion of the valve is more or less thick, firm, and opaque.

And the physiological reason of this arrangement is also apparent enough; though I failed to perceive it until it was pointed out to me by Mr. Thurnam. Each valve, when opened out, is convex towards the ventricle. The three valves do not merely meet by their edges. Their common purpose would be but insecurely consulted if such were the case. They meet and bend up, and come broadly into contact with each other. Each valve during the diastole has its right and left crescentic portion applied respectively to the corresponding portion of its right and left fellow valves. The thin segments are

pressed mutually together, and lie *dos à dos*, as dancers say; while their edges look in the direction of the vessel. All this you may convince yourselves of by injecting the aorta of an ox with wax, and picking out the wax when it is cold.

Now the curious fact which first led me to remark this natural structure is, that the minuter vegetations, which form upon the aortic valves, in acute rheumatic carditis, most commonly arrange themselves in a row, like a string of beads, along the line of union between the scolloped edge of the thicker scutiform portion of the valve, and the inner convex margin of the two thinner crescentic portions. Sometimes they follow that double festoon very exactly and completely: sometimes the continuity of the line is broken, and the excrescences straggle from it a little; but still the general tendency to adhere to it is evident. No one that I know of has publicly noticed this fact; yet that it is a fact, a good many persons, who have been for some time about the Middlesex Hospital, are perfectly aware. The truth is, that death seldom happens early in these cases; and perhaps the valves have not always been carefully examined when opportunity did offer. My friend, Dr. Latham, had been watching for such a case in vain for some years. Recently, however, two of his hospital patients died in the first attack of rheumatic carditis; and he tells me that, looking with great interest for the morbid appearances within the heart, he found them such as I have been stating. I have chanced to see six or eight such cases.

The arrangement just described is the most common one, so far as the aortic valves are concerned: but sometimes even there, and generally upon the mitral and the tricuspid valves, the little wart-like excrescences have a different position; jagging the free edge of the valve with numerous fine serræ, like the teeth of a small saw; or being disposed, just within its border, in one continuous line.

After what has been said, you will readily detect the physical cause of this curious distribution of the wart-like excrescences. The membrane suffers *acute* inflammation. Soft lymph exudes from, or is deposited upon it: and as fast as it is formed it is pressed aside, by the repeated concourse of the opposed surfaces, from the crescentric portions of each valve; and heaped up along the boundary lines of contact: just as a thin layer of butter on a board would be displaced, and heaped up in a little curvilinear ridge, by the pressure of one's thumb. The double festoon, and the little marginal teeth, are obviously both formed in this way.

If my verbal description has been insufficient to make all this clear to your apprehension, the drawings before you speak, I hope, in plainer language.

These, then, are the appearances commonly seen within and without the heart, when the patient does not long survive the first attack of rheumatic carditis. When death takes place at a later period, you find more than this. You find the consequences which flow from these primary lesions, operating as mechanical causes of further change: hypertrophy and dilatation in their various degrees and combinations.

You will please to bear the primary changes in mind; for they satisfactorily account for the *physical signs* of pericarditis and of endocarditis which are displayed in these cases, and which I shall describe and explain after I have shortly inquired into the *general symptoms*.

General symptoms.—The symptoms, then, of pericarditis, as set down by authors, and such as I have myself frequently noticed, are the following. There is often, very early in the disease, a singularity of manner, a peculiar expression of countenance, difficult to describe, yet strikingly manifest to the observer; a strangeness of deportment mixed somehow with an aspect of distress. To this are frequently added, palpitation; a sense of oppression at the epigastrium; a catch in the breathing; a dry cough; inability or unwillingness on the part of the patient to lie on his left side; pain in the situation of the heart, increased by a full inspiration, by pressure upon or between the corresponding ribs, and more particularly increased by pressure upwards against the diaphragm by means of the fingers thrust beneath the cartilages of the false ribs; stiffness and pain in and about the left shoulder, and extending thence down the left arm, and stopping short perhaps at the elbow or wrist. This last circumstance, however, the pain shooting down the arm, is more common in *chronic* affections of the heart. And I have yet another symptom to mention, and a very important one; and that is delirium, sometimes quiet, but often wild and furious delirium, not dependent upon any disease of the encephalon.

Of course there are also the febrile symptoms which accompany the acute rheumatism; or if the pericarditis occur independently of acute rheumatism, there will be fever symptomatic of the local inflammation.

Now each of these symptoms I have repeatedly observed; but they seldom all concur in the same case. If they did, there would not be much difficulty in the diagnosis: nor would the cardiac disease be so often overlooked as it is. The diagnosis of pericarditis has been confessedly uncertain and obscure. Not unfrequently, nearly all the symptoms I have been enumerating are wanting; or so indistinctly marked as to attract no attention. It is therefore an important matter to ascertain what help we may derive, in these equivocal cases, from auscultation.

Auscultatory signs.—In truth, the help we sometimes get is peculiarly valuable and satisfactory. There are certain morbid sounds to be heard when the heart is beginning to labour under rheumatic carditis.

The morbid sounds which may reach the ear applied in such cases to the surface of the chest are two: very distinct the one from the other, and very distinguishable; depending upon different causes, and denoting diversities of operation and of site in the morbid processes going on within. But they are not both heard in all cases.

One of these sounds I have been accustomed to call a *to and fro* sound. It conveys to the ear the notion of the rubbing of two rough surfaces, backwards and forwards upon each other. It seems near to the ear; and therefore near to the surface of the patient's body. Like all the other morbid sounds heard within the chest, it is capable of much variety in tone and degree. Sometimes it very closely resembles the noise made by a saw in cutting through a board. Sometimes it is more like that occasioned by the action of a file, or of a rasp, or of a nutmeg-grater. But its essential character is that of *alternate rubbing;* it is a *to and fro* sound. This very peculiar sound I had noticed and described, and explained, before I was aware that it had attracted the attention of any other persons. Others, however, had remarked it, and had correctly interpreted its meaning. I claim no credit therefore for the discovery of what I think a very important symptom: but I claim for the symptom itself that additional weight which accrues to it, from its having been originally perceived by different observers, independently of each other. The physician who, in this country, without my being aware of it, had noted and published some cases in which this phenomenon occurred, is Dr. W. Stokes, of Dublin. There is a good deal said about it by Bouillaud also; and he too appears to have discovered the sound, without any previous knowledge of its having been noticed by others. I have heard the *to and fro* sound now in upwards of a score cases. In a few of these it never ceased except with life. The patients died during the primary attack, and the *to and fro* sound remained as long as the heart continued to beat. In all the other cases, the *to and fro* sound was audible for a few days only, and then ceased entirely and for ever; the patients recovering more or less completely.

The other of the two morbid sounds, is the ordinary bellows sound, with which you are already familiar. In the cases in question it is a single sound; a deep-seated rush, or whiz, accompanying the systole of the heart. It usually continues long; often for life.

These two sounds, the superficial *to and fro* sound and the deep-seated *bellows* sound, may sometimes be heard, by a careful listener, to exist together. Sometimes the bellows sound begins to be distinguished when the rubbing sound ceases; appears to supervene upon it, or to take its place; perhaps it then first becomes audible, simply because it was previously drowned in the louder superficial sound. Sometimes there is no *to and fro* sound, but only the deep blowing noise; or (what in many cases is extremely probable, nay, what I may venture to say is certain) the *to and fro* sound has *come* and *gone* unnoticed—unlistened to.

Now of these sounds, which I repeat are perfectly distinct, and capable of being easily discriminated the one from the other, the first mentioned, viz. the *to and fro* sound, is always indicative of inflammation of the *external* membrane; the other, the bellows sound, is always, as I believe, in these cases, indicative of inflammation of the *internal* membrane of the heart. You will bear in remembrance, that I am speaking of these sounds as they somewhat suddenly occur for the first time, and especially as they occur in *rheumatic* carditis at its *first* accession.

Those of you who have seen the thorax opened in an animal whose heart still continued to palpitate, may have observed, as I have done, that the pericardium lies closely in contact with the heart, but that a considerable extent of slipping motion between them goes on at every successive act of systole and diastole. They glide over each other evenly and without noise; but this is only while the surfaces are smooth and healthy. When they are already made rough by inflammation and the deposition of lymph, which lymph always, as I have shown you, *is* rough in such cases, then the attrition will be no longer noiseless: it will give rise, in the alternate movements of the organ, to the harsh and superficial *to and fro* sound. But why does that sound, when once it has occurred, at length cease; and why, having once ceased, does it never by any accident, when the inflammation has been universal, recur? Clearly because the pericardium has become adherent to the heart: after which there can be no motion of the one membrane over the other, and therefore no sound indicative of such motion.

That this is the true explanation of the occurrence, and of the permanent cessation, of the *to and fro* sound, I am now (March, 1837) convinced. It was a matter of inference with me for some time. A few of the patients died during the primary attack. By much the majority recovered. I do not mean got perfectly well as they had been before; but they regained a great share of their usual health, perhaps *thought* themselves *well*, and left the hospital where they had been under treatment. Now of those who died, the pericardia were non-adherent. The opposite surfaces of the membranes were rough, and like tripe: and the *to and fro* sound never ceased in these persons. Such cases are always soon fatal. But, in the others, did adhesion take place? I make no doubt of it. Within the last twelve months I have had demonstrative proof of it in two instances. One of these occurred in a hospital patient, whose case has been published in the *Medical Gazette*.* He was a painter, nineteen years old; and he became my patient last May, with acute rheumatism and carditis. From the 26th of May to the 13th of June, a *to and fro* sound was distinctly audible, as well as a bellows sound which had preceded it. After that date, the bellows sound continued, but the rubbing sound was no longer to be heard. The patient improved; and was about to be discharged from the hospital: when, on the 29th of June, sixteen days after the sound of friction had finally ceased, he suddenly dropped down dead in the garden of the hospital.

Here I had concluded that the pericardium was adherent; though I had not expected to have so soon the opportunity of verifying my opinion. And accordingly, except over a small portion of the posterior part of the right ventricle, the union was complete at all points. The agglutination was evidently the work of recent disease. The medium of adhesion was of considerable thickness; and consisted of coagulable lymph and coagulated half-organized blood. The pericardium was stripped off, as I have seen a poulterer skin a rabbit, and with about the same ease. This was a very interesting case to me, for it was the first in which I had had the privilege of examining the heart after having witnessed the peculiar succession of phenomena that I have been describing.

But since that time, I have met with another such a case in private practice. The particulars of it are sufficiently important to warrant my relating them.†

In the month of October, 1836, I was taken by Dr. Sweatman to see a patient of his; whom I found sitting up in bed, pale, with sharp features, breathing shortly and laboriously. His legs were anasarcous, and his belly was tense and fluctuating.

I learned that he had been for years given up to intemperance in drinking, and to indolent and low habits. He told me, that the wind troubled him, shooting up through the whole of the left side of his chest. On further inquiry I found that he meant that he had much pain there. There was loud wheezing over the upper lobes of both lungs; both sides of the thorax were dull on percussion at their lower part: and on the right side no breathing whatever was audible below. These latter symptoms were indicative of dropsical effusion into the pleura also. The jugular veins were swollen and tortuous on both sides of the neck. On applying my ear to the præcordial region I at once heard a very loud and distinct *to and fro* sound. This was equally manifest when he held his breath. Dr. Sweatman, who was not so much accustomed as I have been to listen to the sounds of

* Vol. xviii. p. 701. † *Medical Gazette*, vol. xxi. p. 544.

the heart in disease, recognized instantly the peculiar character of this sound. I ventured to express my certain conviction, that the patient was labouring under recent and acute pericarditis. I added, that he had also, hydrothorax; and that, whatever chronic changes might have taken place in his heart previously to his present illness, dilatation of the right cavities constituted at least one of them.

He had been attacked by his present urgent symptoms three days before I saw him, viz., on the 8th of October. On that day, in all probability, the inflammation of the pericardium commenced.

This was his history. In the spring of the year, having, from indolence, kept the house for months before, he crosssd from the Isle of Man to Liverpool, and was sick, and suffered a good deal during the passage. After landing he had a mile or more to walk. His companions outstripped him, but were called back to him, and found him very pale, breathing with difficulty, and unable for a time to proceed. He attributed all his subsequent complaints to that exertion: and he had been ailing, though not confined to his room, till the 8th of October. I did not venture, in his condition, to open a vein: but leeches were applied to the præcordia, and he took diuretics.

I did not see him again till the 15th of October, four days after my first visit. The rubbing sound was still there, though less loud, less harsh, and less extensive. The leeches had given him much relief. His pulse was very small. A blister was now applied. On the 20th I saw him for the third and last time alive. The rubbing sound was quite gone. This Dr. Sweatman also satisfied himself of. There was a dull systolic bellows sound in its place. The pulse was scarcely perceptible: but he continued apparently improving, making a vast quantity of urine, while the dropsical swellings fast diminished, till the 31st; when, after talking a short time oddly, and in a peculiarly loud voice, he sat up to take some medicine; and having done so, reclined his head against the nurse, and expired. Mr. Shaw assisted in the subsequent examination of the body. I omit giving an account of the condition of the lungs and pleura, which was what we had anticipated; and confine myself to the state of the heart. That organ was large. The pericardium was adherent universally by means of lymph, mottled with blood: and it was easily separated, so that the adhesion must have been recent; as was proved indeed by the symptoms. The right cavities of the heart were very large; and the aorta was diseased.

The existence of the *to and fro* sound in these cases no one can doubt who has once listened for it when present: and the facts respecting it, which have been established beyond the reach of controversy, are these:—1st, That when it occurs *de novo*, it always surely denotes acute inflammation of the pericardium. I say *de novo*, because (as I stated in the last lecture) a *bellows sound* may accompany *each movement* of the heart, in consequence of *internal* disease of some standing; and this double, sawing, bellows sound might possibly be confounded with the alternating noises produced by the attrition of the opposite surfaces of the inflamed pericardium. 2ndly, The *to and fro*, or rubbing sound, is never of long duration, but soon terminates in one of two ways. Either the patient dies in a short time, the sound continuing to the last; and then the pericardium is found coated with rough lymph, but throughout the far greater part of its extent, or altogether, *un*adherent: or he even seems to himself and to others, to recover his perfect health. In these cases, the sound ceases, never to return, while the condition of the patient improves; or the sound ceases from a physical impossibility of its continuance, viz., from adhesion of the pericardium over the whole, or the greater part, of the surface of the heart. And in this category of apparent but unreal recoveries, I cannot doubt that many of Bouillaud's cases of "*pericarditis terminating in health*" ought to be included.

It follows as a necessary consequence from these facts that acute pericarditis, so far advanced as to occasion the pathognomonic rubbing sound, does not admit of a perfect cure: and that its best event is the adhesion of the membrane, and the obliteration of its cavity.

And even then, I say, the change is not final. Mere adhesion of the pericardium does so embarrass the movements of the heart as to produce at length, sometimes rapidly, sometimes slowly, hypertrophy, and other alterations which have already been considered. It is held, too, and I believe justly, though I am not so sure of this as of some of the other points I have been dwelling upon, that the inflammation which begins in the membrane, sometimes dips into the muscular substance of the heart, weakens its elasticity and cohesion, and so leads ultimately to dilatation of its cavities.

I need not occupy much of your time in speaking of the other morbid sound that is audible in these cases, the bellows sound: which sometimes may be heard before the to and fro sound commences; which I have heard *through* the *to and fro* sound; and which often remains after the superficial rubbing sound has ceased. This depends, no doubt, upon those alterations in the valves of the heart which take place, from inflammation, at the same time with the alterations of the pericardium. And when it is met with in such cases, it may be set down as very strong evidence of the existence of endocarditis.

I am anxious that you should take an interest in the disease which I have been speaking of at so much length this evening, and that you should keep it in mind in your future practice; for I am sure that it is a fertile, but often unsuspected source of chronic disease of the muscular substance of the heart, and of its consequences; asthma, dropsy, sudden death. The number of patients that come into the hospitals of London affected with acute rheumatism is annually very large: and I do not think that I am exaggerating when I say that nearly one half of them have the heart or its membranes implicated. The cardiac affection may easily be overlooked both by the patient and the physician. The recovery may appear to be perfect. But after some time, palpitation begins to be occasionally felt; and, by degrees, other symptoms, marking disease of the heart, declare themselves; but their origin is unsuspected or forgotten. You will be surprised, if you search back into the past history of all the patients who apply to you having disease of the heart, especially among the lower classes of society,—you will be surprised to find how many of them will tell you that they have, at some time or other of their lives, been laid up with rheumatic fever.

It is no part of my purpose to treat at present of that specific disease of the joints to which we give the name of acute rheumatism: but I may as well complete what I have to say of carditis as it occurs in connection with that disease; and then I shall not need to repeat myself when I come to rheumatism itself.

Head symptoms.—In the first place, then, I would say a word more respecting the nervous disturbance which is apt to supervene in such cases, and to mask the real disease, and to mislead the unwary practitioner. Patients labouring under rheumatic carditis very frequently become affected with delirium, or violent mania, or stupor and coma, or convulsions, or all of these in succession: and you might suppose that they were labouring under inflammation of the brain, or of its membranes. Such cases are in fact spoken of as cases of *metastasis* to the brain. It may sometimes be so, nay, I know that it sometimes *is* so; but not often. Again and again, when death has occurred, and the delirium had been extreme, no traces of disease have been discoverable within the skull, while marks of violent and intense inflammation were visible in the pericardium. I presume that the acute cardiac affection interferes somehow with that regulated supply of blood to the head, which is necessary for the due performance of the cerebral functions. But whatever the explanation, recollect the fact; and whenever, in acute rheumatism, you find your patient flighty and wandering, or more distinctly delirious, examine carefully the condition of his *heart*.

As this is really a point of great importance, and as you will not find much information respecting it in books, I shall take leave to quote, here, some part of a clinical lecture delivered by myself at the Middlesex Hospital, in the year 1835, and printed in the 16th volume of the *Medical Gazette*. My subsequent experience has been quite in conformity with what I then stated.

"The functions of the brain not unfrequently become disordered in rheumatic fever: and disordered in such a manner and degree as would lead, and has led, many to believe in the presence of active inflammation of that organ, or of its enveloping tissues. Yet this affection of the brain is not, I believe, inflammation, but some secondary affection of the circulation therein; resulting from disturbance at the central organ of the circulation, capable of producing a corresponding derangement in the cerebral functions. I can best explain what I mean by reciting a few examples.

"The first case of this kind that I ever saw or heard of, occurred in St. Bartholomew's Hospital, many years ago. I took notes of it at the time, and will read you the substance of them. Charlotte Rankin, aged 17, was admitted there on the 12th of August, 1824, under the care of Dr. Roberts, with acute rheumatism of the joints. Her illness had come

on suddenly a week before, after unusual exposure to cold and wet. The pain and swelling had shifted much from joint to joint. She had been bled, on account of pain in her left side, two days before admission. On the 14th, she complained of much difficulty of breathing, and of pain when even slight pressure was made upon the chest. These symptoms were entirely removed by a blister. On the 16th, she was observed to be odd in her manner—peevish, querulous, restless, without sleep, and desirous of getting out of bed. Her pulse was then 100. On the 20th the pulse had risen to 120; it was quite regular. She said she felt no pain, except the soreness occasioned by the blister. She slept very little. On the 21st, the pulse was 128. Some jactitation of the left arm was now observed, which, she said, had never happened before. No sleep. On the 22d, about nine in the evening, she became furiously maniacal, and it was necessary to confine her by a strait-waistcoat. She continued in that state for upwards of four hours, and then died.

"Twelve hours afterwards the body was examined. The brain was found quite healthy: its vessels seemed, indeed, somewhat fuller of blood than is usual, but there was no effusion, nor any other vestige of inflammation.

"The pericardium was glued to the heart, in several places, by recent adhesions; and it was universally coated, where not adherent, by a layer of rough, reticulated lymph, remarkably harsh to the touch.

"Now here the most prominent symptoms were such as we are accustomed to refer with tolerable confidence, to inflammation of the membranes of the brain; whereas, in fact, the inflammation was strictly confined to the heart. If no examination of the body had been made, the case might have been quoted with much show of reason, as a well-marked example of metastasis to the brain. It *was* so considered, before the brain was inspected.

"There had, indeed, been symptoms which indicated, and that not obscurely, the cardiac disease. At that time, however, I did not know how frequently carditis is combined with acute rheumatism. Auscultation had not yet come much into fashion in this country; at any rate, I knew little or nothing of its use; and I had supposed (and it had been supposed by others who witnessed the case) that the chest symptoms resulted from rheumatism of the intercostal muscles.

"Another instance, in which the course of the symptoms was somewhat different, yet equally calculated to mislead, you have lately seen in this hospital.

"William Wilkins, a post-boy, twenty-eight years old, was admitted on the 25th of last November.

"He complained of pain in most of the large joints, shifting from one joint to another There was no visible redness or swelling, but he had much fever. The pain was greatest at night. He had profuse perspirations, during which the pain was not mitigated.

"He had been ill eight weeks; and at first his joints (according to the statements of his friends) were both swelled and red. He appeared to be recovering at one time, but relapsed. For three or four days previously to his admission he had coughed a little, and spoken of pain at the pit of his stomach. He lay more comfortably on the right than on the left side, but this was habitually the case. He had never had acute rheumatism before.

"He rambled a good deal during the night of the 26th, and on the 27th he began to refuse to take his medicine, appeared confused and stupid, and answered questions tardily and imperfectly. He was bathed in perspiration, which had the strong acid smell so common in cases of acute rheumatism.

"During the next ten days he remained in a singular state of quiet delirium, rejecting medicine and food, saying he had had enough; getting out of bed, especially in the night, and declaring that he was going home. When questions were put to him, his lips moved, and his limbs began to stir and fidget, as though he were about to answer; yet he said nothing. He understood what was said to him, and put out his tongue when desired so to do; imperfectly, however, and with slowness and apparent difficulty. His bowels were costive, and he passed his stools, when purgatives acted, in the bed. His pulse was small and frequent; and when his wrist was taken hold of, that the artery might be felt, he always resisted, and forcibly contracted his arm.

"Then for three or four days he appeared to improve; his countenance became more

clear and lively; but he still showed the same restlessness, and maintained the same dogged silence when spoken to, and obstinately refused to swallow medicine. He was somewhat cunning, too, for he would take pills into his mouth, and then, when he thought he was not observed, chew and spit them out again.

"His pulse became at last very frequent, and his strength diminished rapidly. He died on the 18th December, and the body was examined on the following day.

"The cerebral veins were gorged with dark blood, and there was a considerable quantity of serous fluid beneath the arachnoid, and in the lateral ventricles.

"The pericardium was free from disease; but upon the mitral valve, near its edge, there was a perfect row of small, slender, bead-like warts.

"A few weeks ago I was consulted in a case of a similar nature, which occurred in the practice of a gentleman who was formerly a pupil here, and who was fully aware both of the frequent occurrence of carditis in acute rheumatism, and of the anomalous symptoms with which it is sometimes attended. The patient was a young man, twenty-four years of age.

"On the 22d of December he was seized with pain and swelling of several of the larger joints, and with fever. The attack was ascribed to exposure to cold the day before; he had previously enjoyed perfect health. The inflammation shifted rapidly from one joint to another. He was confined to bed for six days: then, feeling better, he got up, changed his room, and presently underwent a relapse. Mr. Elwin tells me that, after that time, he was never comfortable about this patient; his countenance was pale, and his aspect unpromising; his pulse frequent; and more than once he complained of slight pain in the epigastrium, increased by a full inspiration. This was removed by a mustard poultice. No morbid sound was detected upon a careful examination of the præcordial region by the ear. He remained low-spirited, but slowly mending, till the 3d January, when in the evening, without any notice or obvious cause, he began to be restless and delirious.

"On visiting him the next morning, Mr. Elwin found him with an anxious countenance; a frequent and irregular pulse, which occasionally intermitted; his mind wandering; the action of his heart strong, and attended towards the sternum with a loud bellows sound. The next day his breathing was difficult, and 'catching;' the pulse 120, hard and wiry. At that time I had the opportunity of seeing him. He was lying in a sort of stupor, yet not unconscious, for he put out his tongue at my request, and answered pertinently one or two questions, after they had been frequently repeated. He had the air of a person obstinately determined to say as little as possible. He became more distinctly delirious towards evening; and the next day his pulse and breathing were both so frequent (148, and 78, in the minute, respectively), that he was thought to be dying. A distinct bellows sound was audible near the left mamma. This state continued, with slight fluctuations, till the 8th, when his condition appeared somewhat more hopeful. He was calm, had no dyspnœa, and conversed more readily,—saying sometimes that he felt as if he were 'dead;' sometimes that he was 'burnt up.' He complained, for the first time, of pain in the right temple; his gums were slightly under the influence of mercury; his pulse scarcely exceeded 100; the bellows sound was very manifest.

"On the 9th he again became, first restless, and then violently and wildly delirious, screaming out, refusing to take medicine, or to open his mouth when it was offered; yet he evidently knew what was said to him. During the night general convulsions came on in occasional spasms, of a tetanic character: in the intervals between them he lay in a state of coma. He survived in this condition till the 12th.

"I was present at the inspection of the body ten hours after death. Some of his family insisted on being in the room with us; but we were able to make an accurate examination of the head, and of the heart.

"The veins of the brain seemed somewhat fuller of blood than is common. The arachnoid was slightly elevated by a clear serous fluid collected in the pia mater. There was but a small quantity of a similar fluid in the lateral ventricles. The lungs appeared quite healthy.

"There was no fluid in the pericardium. Its surface was everywhere exceedingly vascular, but it presented no appearance of lymph, except where it adhered to the posterior side of the heart, over a space of about two inches and a half in length, and upwards of

an inch in breadth. The lymph which formed the medium of connection was firm, but evidently of recent formation: and a very slight degree of force sufficed to separate the adhering membranes. The heart was rather small, and the left ventricle had a singular wrinkled appearance externally. Towards the edge of the mitral valve there was a profuse crop of little wart-like vegetations, of the size of millet-seeds; and numerous red lines converged towards them from the base of the valve. The aortic valves all presented curious festoons of similar excrescences, larger, however, and more prominent, than those upon the mitral valve.

"In the beginning of the year 1832, a girl, nineteen years old, Frances Kirk by name, was a patient of mine in the hospital, with acute rheumatism of the joints, and carditis, manifested by many of the most usual symptoms—by pain in the situation of the heart, dyspnœa, great frequency of pulse, and a distinct bellows-sound. She lived two months from the commencement of the cardiac disease. During that period she was at times wildly delirious—at times stupid, taciturn, and almost idiotic—and at times quiet and rational. The brain in that case was found perfectly healthy, except a slight serous effusion beneath the arachnoid. The pericardium was everywhere adherent to the heart. By some mismanagement the opportunity of inspecting the inner membrane of the heart was lost.

"In each of the three last-mentioned cases there was more or less serous fluid found in the meshes of the pia mater, and in the lateral ventricles. You may ask, perhaps, whether this effusion was not good evidence of previous inflammation there? whether it did not show that the metastasis, which I have spoken of as seldom happening, really did happen in these very cases?

"I apprehend not; and for the following reasons.—In one only of these cases was the amount of the serous accumulation at all considerable. There was no *other* trace of inflammatory action in any one of them; no redness, nor pus, nor lymph; none of the *unequivocal products* of inflammation. What quantity of serous effusion beneath the arachnoid, or in the ventricles of the brain, is requisite to establish its morbid origin—within what limits such effusion may be considered natural—whether it may not be ascribed wholly or in part, to mechanical transudation after death; these are questions which have not yet been definitively settled among pathologists. For my own part, whenever I see the veins of the pia mater full of blood, I *expect* to find serum between that membrane and the arachnoid. How much of it may have been poured out before death, and how much afterwards, it would be difficut to estimate. In each of the cases before us there was evidence, not to be mistaken, of cardiac inflammation. Now that acute inflammation, fixing itself upon some portion of the heart, should embarrass its action, and modify the condition of the circulation through the cerebral blood-vessels, is not only conceivable, but highly probable. Any retardation of the venous circulation in the head—any engorgement or congestion of that system of vessels—would be likely, if we may reason from the analogy of other parts, to produce effusion. I have seen, in the brain of a criminal who had been hanged while in a state of perfect health, as much serum collected in the same parts as we found in the patients whose cases I have been relating. It is possible that, in them, the disorder of the sensorial functions depended upon simple disturbance of the cerebral circulation; it is possible that the same disorder depended upon the serous effusion; and it is possible, and (I think) probable, that it depended in part upon both these causes. It is very certain that similar symptoms have occurred in similar cases, when there was no appreciable effusion; and, apparently, from mere derangement of the natural circulation of the blood in its vessels. On the other hand, we know that an equal, or a greater amount of effusion, has often been observed, when no such cerebral symptoms had manifested themselves. I conceive, therefore, that the symptoms referable to the brain, and the quantity of serum found effused there (whether these bear to each other the relation of cause and consequence, or not,) are both to be regarded as secondary effects of the cardiac disease; that they denoted no inflammatory condition of the brain, or of its membranes, but were the common result of that inflammation of the heart, concerning the existence of which the inspection of the bodies left us no room to doubt.

"That this view of the matter is correct, is the more probable because (as I just now stated) the same symptoms have been known to accompany carditis, although no serous

effusion was met with in the head. There was none in the case of the girl Rankin; none in a case related by Dr. Davis; none in a remarkable case detailed by Dr. Latham; none in a striking example of a similar kind which fell under the observation of that accurate and most accomplished pathologist, Andral. I shall take the liberty of citing these two instances.

"'One of the children of Christ's Hospital,' says Dr. Latham, 'had, in the opinion of all who saw him, the severest inflammation of the brain. The attack was sudden, with great heat and frequency of pulse. He had delirium and convulsions, and pointed to his forehead as the seat of his pain. In three days he died, and, upon dissection, not a vestige of disease was found within the cranium; but the heart was exclusively the seat of the disease, and no other part of the body discovered the slightest morbid appearance. The disease of the heart was not confined to its investing membrane. It was the most intense inflammation pervading the pericardium and the muscular substance.'

"Andral's case, which is referred to by Dr. Latham, occurs in his *Clinique Medicale.*

"A woman, twenty-six years old, was brought to La Charité, in a state of delirium, and no account could be obtained of her previous condition. The delirium was remarkable for the obstinate taciturnity which attended it. When questioned, the patient turned a fixed gaze upon the person who spoke to her, but made no reply. Her face was pale; her pulse small and frequent. During the two following days the head was frequently drawn backwards, the trunk was shaken at intervals by convulsive movements, and she had subsultus tendinum; but she now spoke, and appeared to comprehend what was said to her, but talked incoherently. The pulse was very frequent, and intermitting. On the fourth day the delirium ceased; she complained of nothing but great debility. The muscles of the face were almost continually agitated by convulsive twitchings, and the arms from time to time presented a sort of tetanic stiffness. On the fifth day the delirium returned; the patient then fell into a state of coma, and died the next morning.

"Neither the brain, nor the spinal marrow, nor their membranes, presented any appreciable morbid appearances. The pericardium was lined by coagulable lymph, and its opposite surfaces were connected, in some places, by recent bands of adhesion. It contained also some ounces of a greenish flaky serum. No other trace of disease was discoverable.

"Now if you are not made aware beforehand of this strange course of the symptoms arising, sometimes, out of rheumatic carditis, you will be apt to overlook the cardiac affection, and to direct your remedial measures wide of the mark. In a second instance mentioned in Dr. Latham's essay, 'the whole force of the treatment was directed to the head, from a beilef that the brain was inflamed. Upon dissection the brain and its coverings were found in a perfectly healthy and natural state; and the pericardium, towards which during life there was no symptom to direct the slightest suspicion of disease, discovered the unequivocal marks of recent and acute inflammation.' Dr. Davis also, in reference to a case published by him so early as 1808, has the following remark:—'The restlessness in the case of Miss H. C. was also attended with delirium, a symptom not previously noticed as belonging to pericarditis by any writer whom I have consulted. It was so prominent a feature of the disease under which this young lady laboured, as to divert the attention of her medical attendants from its actual seat.'

"This occurrence, in the course of rheumatic carditis, of cerebral symptoms calculated to perplex and obscure the true nature of the disease, is probably not so rare as has been supposed. In less than three years three instances of it have fallen under my own notice; and I have been informed by a medical man residing in the neighbourhood, that a friend of his, who has a very large general practice among the middle and lower classes, attended within the last year or two not less than twenty cases of acute rheumatism, in which a metastasis, or an extension of the inflammation, appeared to take place to the brain.

"In all the detailed cases of this kind that I have met with, and in those which I have myself watched, there were certain general points of similarity, which you will do well to bear in mind. In all of them the pulse was extremely rapid; the delirium, though violent and active at intervals, was characterized for the most part by a singular, and, as it seemed, perverse taciturnity: even when the patient was evidently able to speak, and understood the questions that were put to him, he maintained a sullen silence. In most of these

patients, also, not long before the fatal event, a brief interval of amendment took place, and encouraged some hope of recovery. In many of them various convulsive movements were observed; and in two of the cases the head symptoms, and probably the heart disease also, supervened after a *relapse* of the rheumatism of the joints."

Relations of carditis with articular rheumatism.—It is a curious circumstance that rheumatic carditis is sometimes the first step in the whole disease; the cardiac symptoms will sometimes, I mean, precede those of the joints; even by two or three days. For example. A lad was brought to the hospital with acute articular rheumatism, and with unequivocal symptoms, which I need not detail, of carditis. He gave the following distinct history of his illness:—He had been on a visit into the country several days before, and there, after having felt poorly for nearly a week, with a sensation of "sinking within him," he ate largely of oysters, and drank more porter than he was accustomed to. On the same day he was seized with pain in the left side of the chest, and violent beating of the heart. The attack was probably a severe one, for he applied to a medical man, who immediately bled him. In the course of the ensuing night he began, for the first time in his life, to feel some stiffness beneath and about his knees, but he was able to walk about the next day. On the evening of the second day the joints became so painful and swollen, that he could not leave his bed, and then the pain of the side and the palpitation diminished. This boy has several times since returned to the hospital with acute rheumatism, and on each occasion presented manifest indications of some permanent affection of the heart,—slight, probably, in amount, but aggravated upon every return of inflammation of the joints.

I have met with one or two other instances in which the cardiac disease appeared to have preceded the arthritic; but none so well made out as that I have just related. Dr. Duncan, in the *Edinburgh Medical and Surgical Journal* for 1816, gives a case of "inflammation of the heart" which *began* with symptoms of *pectoral inflammation*, succeeded *the next day* by rheumatic affections of the joints.

One law respecting the connection between the cardiac and the arthritic symptoms may be stated with confidence, namely, that the *younger* the patient is who suffers acute rheumatism (and I have seen it so early as the third or fourth year) the more likely will he be to have rheumatic carditis. The chance of the combination appears to diminish, after puberty, as life advances. I have known only two persons pass through acute rheumatism with an untouched heart prior to the age of puberty; and in these two I am by no means certain that the articular disease was genuine rheumatism. In each of them, the large joints became painful, and swelled, for a day or two only, towards the close of scarlet fever;—a circumstance not, I believe, unusual. I was dreadfully apprehensive of carditis, but it did not occur.

I have observed, also, that when a patient has come under my care who has had *repeated* attacks of acute rheumatism, in him I have generally found reason to believe that some organic affection of the heart was present. Probably the disposition to such repetitions of the disease, so remarkable in some individuals, may be kept up by the cardiac complication.

With respect to the period of the actual attack, and the circumstances under which the extension of the disease to the heart occurs, no fixed law has been observed. Sometimes the cardiac affection declares itself as the inflammation of the joints declines. Quite as often, however, they proceed together, and are aggravated or mitigated simultaneously. On this point my own experience nearly agrees with that of Dr. Latham, who says:—"It (the cardiac affection) is incident to all the degrees and all the stages, and all the forms (?) of acute rheumatism. It is not more to be looked for when the disease is severe than when it is mild; more at its beginning than during its progress and decline; more when it is shifting and inconstant in its seat, than when it is fixed and abiding."

There are some other symptoms that I must not omit to mention as occurring in some cases of pericarditis. In one of the fatal instances which fell under my own notice, there was a very strong purring tremor felt by the hand placed upon the region of the heart. This is not a constant, nor even a frequent, symptom; but it has a certain degree of corroborative value when it does occur.

When the fluid products of the inflammation predominate, when there is much serum poured out, the symptoms, as well as the danger, will be different from those which are

remarked when there is not so much serous liquid. If the pericardium be distended, percussion will furnish a dull sound over an unusually large space; much beyond the natural limits of the præcordial region: and you may often measure the amount of the effusion, and its daily increase or decrease, very accurately in this manner. But the general symptoms will vary also. The pulse will be feebler, and more disposed to falter, and to become irregular, in proportion as the liquid effusion is large; and at the same time the patient will frequently be fixed in one position, and unwilling or afraid to change it, lest that small exertion should further excite the action of his heart, and hurry his respiration. He will lie, perhaps, always upon one side; or he will remain immovable on his back, with his head elevated; or he will sit up continually, with his body leaning forwards; and he will not dare to alter his posture. But when the solid products of the inflammation predominate; when there is coagulable lymph, and but little serum; when the pericardium, instead of being distended, becomes attached to the heart; then the pulse will retain that force and regularity with which the disease commenced, the dull sound yielded to percussion will not transgress the præcordial limits, and the patient will not in general experience any absolute necessity of accommodating his body to one constrained position.

Of a merely adherent pericardium there are no diagnostic signs to which, so far as I know, we can trust, either auscultatory or general. None, I mean, presented by the body at the time. If we are accurately acquainted indeed with the history of the patient's disease, and if we know that, at any time, a *to and fro* sound existed, which *to and fro* sound soon ceased, and has never recurred; then our conclusion that the pericardium is adherent will scarcely be open to any source of fallacy.

When the opposite surfaces of the membrane have been once united, they never separate again; the adhesion remains for life. But the lymph interposed between them, if the inflammation be not renewed, becomes less and less thick; until at length, in some cases, a mere layer of firm, but thin, cellular tissue is left, through which the heart is visible.

But when inflammation has stiffened the valves of the heart, or studded them with little wart-like masses of fibrin, how far do these morbid states admit of perfect recovery? It is not so easy to say. I am not aware of any facts which would forbid altogether the hope that here, as in iritis, the reabsorption or removal of the lymph may be total, and the restoration of the parts complete. On the contrary, the *comparative* infrequency of wart-like excrescences in the slowly fatal cases of rheumatic carditis leads to the opinion that such deposits may disappear as readily and entirely from the valves of the heart as from the iris.

Since I lectured upon this subject last year, two examples of rheumatic carditis have occurred among my hospital patients, differing in some remarkable points from any that I had ever seen. A brief description of them will complete my personal experience of this terrible disease.

The histories of the two cases are curiously similar. The patients were young women; their ages respectively twenty-one and twenty-two. They were admitted during the same week; one a day after the other. Both were suffering under a first attack of rheumatism: both had also acute pleurisy, with effusion into the chest; and both died; one of them three weeks, the other a month, after her admission. In both cases there were symptoms referable to the heart; pain, and unnatural sounds: but in neither case was there any friction-sound; nor were any traces of pericarditis discovered after death. But the inflammation had fallen, partially, upon the aortic valves; whence it had extended (so I imagine) to the muscular substance. I show you the morbid appearances represented in these drawings, made by Mr. Lonsdale at the time: and, better still, I show you the parts themselves, which are preserved in our museum.

The whole of one of the aortic valves was, in each case, a mass of ragged ulceration; and the adjacent portions of the two other valves were, in a slighter degree, implicated in the mischief. What remained of the tattered valve was covered with rough irregular shreds of lymph, or vegetations. In one of the cases, the ulcerating process had penetrated through the valve, and into the muscular substance beyond, and had eaten a hole completely through the septum. A portion of lymph protruded just below the valves of the pulmonary artery through the channel of communication thus formed between the left and right sides of the heart. In the other case, an abscess as large as a hazel-nut was

found in the muscular substance of the septum, immediately opposite the disorganized valve.

Till I met with these cases, I was not aware that this destructive incrustation of the cardiac valves with wart-like excrescences was ever the result of *acute* inflammation. Chronic changes of that kind are not uncommon. Suppuration in the heart is very rare. In these two instances, the cardiac affection was complicated with acute pleurisy. I should have mentioned before, that the pleura very often participates in the inflammation when pericarditis occurs. You will not wonder at this if you consider the close vicinity, and the similarity in texture, of these two serous membranes.

I must defer what I have to say respecting the treatment of acute pericarditis and endocarditis, to the next lecture.

LECTURE LXII.

TREATMENT OF ACUTE PERICARDITIS, AND ENDOCARDITIS: BLOOD-LETTING; MERCURY; BLISTERS. CHRONIC AND PARTIAL INFLAMMATION OF THE PERICARDIUM. DISEASE OF THE AORTA. THORACIC ANEURISMS; THEIR VARIOUS SITUATIONS, AND SYMPTOMS: PLAN OF TREATMENT.

I TRUST that I made distinctly apparent, in the last lecture, the great danger which belongs to every case of acute inflammation of the pericardium. First, there is the danger of *speedy death*. If the inflammation goes to the extent of effusion, and the collection of serous fluid be large, and the pericardium be distended by it, the action of the heart is so much oppressed by the liquid surrounding it, that it falters and flutters, and at length stops, and goes on no more. Secondly, there is the danger that (the pericardium having become adherent) other structural changes will, soon or slowly, develop themselves; and first render life burdensome and full of suffering; and then consign the patient to an earlier grave than, but for the rheumatism, might have awaited him.

Treatment.—Now what can we do to prevent, or to diminish, these evils? I once thought that if we caught the inflammation at its very commencement, we might calculate upon a perfect cure, by first bleeding the patient freely, and by, secondly, putting him as speedily as possible under the specific influence of mercury. I am sorry to be obliged to say, that the more I see of this formidable malady, the more reason I find for fearing that it is seldom within the possibility of thorough repair. Bring the inflammation to a stop, you perhaps may; or nature will do it for you; and you may greatly assist the natural powers in effecting this. But that alone can be called a cure, which either leaves the structure of the part affected in its original integrity; or, at any rate, leaves no spring or source of further changes for the worse: and such complete recovery as this I seldom dare to hope for in cases of acute and general pericarditis.

There can be no use in deceiving ourselves in this matter; but we may very easily deceive ourselves. In a large proportion of cases, whether they be treated well, or ill, or not treated at all, the patients will *seem* to recover. But I say that the recovery is so far unreal, that it involves the germ of future destruction. If any of you have read Bouillaud's heavy, yet instructive, work on diseases of the heart, you will know that he boasts of the success of his treatment in acute pericarditis. He declares that by the bold use of the lancet he *extinguishes* the inflammation; jugulates (as he calls it) or slaughters the disease at its birth; and restores the patient to the full condition of health, or to the state in which he was before the disease came on. You must hereafter judge of this question for yourselves; but it is my duty to caution you against crediting these statements. Not that I would insinuate a doubt of M. Bouillaud's veracity; but I believe that he has been deceived by false recoveries; and I would not have you beguiled, by his representations, into the indiscriminate adoption of that "enlightened hardiness" which he endeavours to inculcate.

But if we look closely at his statements, we do not find, after all, any such wonderful success. Of 18 patients, 6 died: a very large proportion, 1, viz., in 3. To be sure, with some ingenuity he makes the proportion to be 1 in 7. For three of the fatal cases occurred, he says, before he took to his heroic plan of blood-letting; and excluding these 3, he has 15 cases, and only 3 deaths; or 1 in 5. But one of these three proved fatal from the supervention of tetanus; therefore setting that also aside, there will be 14 cases of the disease and 2 deaths. Now, I have not, hitherto, been able to look over my books, but I am quite certain that the mortality in the Middlesex Hospital has been nothing like so great as this—the *immediate* mortality, of course, I mean—either among my patients, or among those of my colleagues; and I know that, until within the last twelve months, Dr. Latham had not lost a single case of rheumatic pericarditis in the course of the first attack of that disease for several preceding years.

But what I most doubt about, is the *true* recovery of Bouillaud's surviving patients. I say such patients do *apparently* get well. In some of them, indeed, a bellows-sound remains, sufficiently indicative of the damage that the organ has sustained: and I have already told you that *any* amount of change, however small, which alters the healthy proportion of the cavities and their outlets, or which interferes with the natural play of the heart, is a seed from which further changes will at length be found to grow. But patients will get so far well that you can detect nothing wrong about them. Follow them, however, in their subsequent lives; and you will learn that many of them very soon begin to find that they are incapable of doing or enduring all that they could do or endure before their illness: and if this does not soon happen, it does at last. The disease of the heart (if the patient be not cut off by some other malady) becomes at length obvious: and when he dies, the source of the ultimate changes is commonly to be detected. There is an adherent pericardium; or there is disease of the valves: of which no other account can be given than that it had continued to exist since the primary symptoms of carditis ceased; and that it had caused all the rest—the hypertrophy, to wit, and the dilatation.

The remarks that I have now been making bear upon the question, to what amount blood-letting should be carried in acute pericarditis. If the general symptoms teach you that it exists, and yet no sound of attrition is heard, you may, in that case, if in any, hope to arrest the inflammation, and to achieve a complete cure, by early and copious bleeding. But if the *to and fro* sound has been audible, I do not think the consequences of the inflammation can be so abolished. I believe that the best event which can then happen is adhesion. We must, however, in many cases, take blood by the lancet from the arm, or by leeches from the præcordial region; yet not in the lavish manner recommended by Bouillaud. I know that this treatment has been fairly tried in this country, and has failed. And I think (but this I only offer as an opinion) that there is a peculiar risk in frequently bleeding to syncope in this affection. There is, almost always, endocarditis (in the rheumatic cases at least) coincident with the pericarditis; and there is a readiness or tendency towards a deposition of the fibrin of the blood, in the shape of minute vegetations, upon the inflamed valves: and it is probable that this tendency may be favoured by a retarded movement of the blood over them; and still more so by its temporary stagnation. In the experiments on the ass, referred to before, the circulation (kept up by artificial breathing) became languid and sluggish, and vegetations were deposited upon those valves which had been irritated by the wire. Hence there is, I think, a danger in bleeding to such an extent in these cases, as to bring the heart's action to a pause *in deliquium*. Bleed, therefore, (if you deem it requisite to bleed at all,) till some effect upon the pulse has been accomplished, and then stop: and renew the venesection, or refrain from it, according to circumstances. But you may freely cup the præcordial region, or cover it repeatedly with leeches: and to this mode of abstracting blood, from the neighbourhood of the inflamed part, my own experience would assign the preference. In fact I seldom open a vein in such cases.

At the same time, you will endeavour to get the gums tender with mercury. And it is most unfortunate that, in this disease, the system frequently resists, with great obstinacy, the influence of that mineral. Sometimes, do what you will, you fail to attain your object. A knowledge of this fact may render you less scrupulous than you would otherwise be in the use of the remedy. Not only should calomel be given in frequently

repeated doses, guarded, if need be, by opium: but mercurial inunction should also be had recourse to, in these perilous cases, from the first. When the gums do begin to rise, there will always be, according to my experience, a manifest subsidence of the distress, and mitigation of the symptoms. There will be less pain, less palpitation, less dyspnœa. The mouth should be *kept* sore for some time together: for supposing the membranes to adhere, it does not follow that the inflammatory process should thereupon cease.

When you learn, from symptoms and signs already described, that the pericardium is distended by liquid effusion, I would advise you to lay a large blister over the præcordia. The diminution, or complete disappearance of the liquid, under this treatment, is often rapid and striking.

Even when all the symptoms have vanished, previous disappointments have taught me not to be sanguine as to the permanency of the recovery. I believe that months, and years even, may elapse before the secondary effects of the mischief left behind by the inflammation begin to be palpable. But in many instances, they show themselves very early. Others have noticed all this; especially Dr. Latham, who truly remarks, that "in acute pericarditis there is no medium between complete cure and certain death." He deemed, at the time when the lectures from which I quote were given, that the early and vigorous use of mercury would be equal to the complete cure. I think I may venture to say that he has since seen reason to *doubt* on that point. I have several times already expressed my own belief, that where the *to and fro* sound manifests itself, that is, where the inflammation has gone so far as the effusion of coagulable lymph, if the patient does not die outright, he survives at the expense of an adherent pericardium; and he survives only for a time. *Hærit lateri lethalis arundo.* But I am also of opinion, that by the cautious employment of the lancet and of leeches, and by the early and unshrinking use of mercury, the mischief may be greatly limited, and the consecutive changes staved off to a distant period. The final catastrophe arrives much more slowly where there is mere adhesion of the pericardium, than where that condition is complicated with permanent disease of the valves within the heart. I need scarcely say that other, minor expedients are to be put in force; strict abstinence, I mean, perfect quiet, and an occasional purgative to clear out the alimentary canal.

When the pericardium has once become adherent, if (as is very likely) the patient again suffers acute rheumatism, he may again have heart symptoms. Not, of course, the *to and fro* sound; but pain, palpitation, and dyspnœa. Now it is of some practical importance to be aware that this renewal of morbid action does not require that active treatment which the primary inflammation demanded. The effect of such renewal will be to augment the existing mischief; but the morbid process is much less vigorous, and much more easily subdued. It will generally yield to the repeated application of leeches, or of blisters, over the situation of the heart, and to the moderate exhibition of mercury.

Chronic partial inflammation of the pericardium.—Although acute inflammation in this, as in other serous membranes, shows generally a strong disposition to spread all over the affected surface; yet does the pericardium seem readily susceptible of slight and partial inflammation. You will very frequently indeed see, upon laying open the pericardium, a white spot, as big as the finger-nail, upon the surface of the heart. I have examined these spots very often; and I believe they almost always consist of a thin flake of lymph lying sometimes beneath, but oftener upon, the membrane. They may, in fact, be peeled off sometimes, and the subjacent membrane left smooth and sound. On one occasion, I met with a long ribbon of lymph passing from the centre of one of these white spots, to connect itself with the loose bag of the pericardium. I conclude, therefore, that these spots are really the result of a very limited inflammatory process: but under what conditions they arise, or whether during their formation they furnish any symptoms, I do not know.

Such is the view which I had long taken, and taught, of these white spots upon the surface of the heart; and I am glad to have it confirmed by the observation of M. Paget, who has recently adduced (in the twenty-third volume of the *Medico-Chirurgical Transactions*) conclusive evidence, both of their frequency, and of their inflammatory origin.

Disease of the aorta.—So much, then, for the heart itself, and its membranes. There still remain to be considered the morbid conditions of the great vessels that spring from it,

and lie in the thorax, and especially of the aorta; those morbid conditions, I mean, which declare themselves by symptoms, and which become the object of medical treatment.

The aorta is very frequently indeed found diseased, and its disease, as I have already explained, is a common cause of organic changes in the left ventricle of the heart. You will find that its inner membrane, instead of being smooth, and of a uniform yellowish white colour, is rendered very uneven by a great number of yellow opaque projections, of cartilaginous consistence, lying immediately beneath the membrane. And in a more advanced stage of the same diseased condition, you may perceive that some of these projecting little masses consist of irregular scales of bone, having sharp edges; and sometimes these plates of ossific matter are quite bare; the inner membrane is gone, and the exposed bone is washed by the current of blood.

Aneurism.—Now the necessary effect of these changes is to diminish and destroy the natural elasticity of the vessel; and as there is a perpetually recurring strain upon it, by the blood sent out from the heart, the vessel dilates, becomes larger than it should be. This, if you please, you may call aneurism; but a simpler name is dilatation. In other cases, the enlargement is not general, but partial. A pouch is formed on one side of the artery, and this pouch may be very small or very large. It appears to result from the giving way, the rupture in short, or the ulceration, of the inner and middle coats of the artery, and then the blood, passing through the broken part, presses against the cellular coat of the vessel, and distends it into a sort of bag. There have been curious discussions as to what should be called true aneurism, and what should be called false aneurism, discussions upon which I have neither time nor taste for entering. It is enough for all practical purposes to state, that the artery sometimes dilates only, sometimes throws out a pouch. I know that you have received, or will receive, from my colleague, Mr. Arnott, all the information that is requisite concerning the modes in which aneurism may arise. These are matters of the highest interest in surgery, for surgery can cure an aneurism; an achievement which is but seldom within the skill of physic.

I have known two cases in which the diseased artery all at once cracked across—its inner and middle coats, I mean—and death very rapidly ensued. In one of these instances, the crack extended round a considerable part of the circumference of the aorta. It looked exactly like a clean cut made by a sharp knife. The blood, in this example, dissected its way (if I may so say) between the middle and external coat of the aorta, and got at last into the pericardium, and coagulated round the heart in a uniform layer: so that a *bag* of coagulated blood was enclosed in the bag of the pericardium. Of course, nothing can be done for such cases as these.

Most commonly the aneurismal tumour goes on enlarging: and often it becomes lined, and sometimes it is nearly filled up, by layers of coagulated blood, which form in its interior. At length the tumour bursts, and the patient perishes.

Aneurisms of the thoracic aorta are met with chiefly in the earlier portions of that vessel, in its ascending part, and in its arch. There seem to be two reasons for this. One is that the diseased state of the coats of the artery (to which the rupture and subsequent aneurismal pouch, or the dilatation, as the case may be, are owing,) is more common, and more advanced generally in that part of the aorta; and another reason is, that the momentum of the blood, as it is forcibly propelled from the left ventricle, is sustained chiefly by the same part.

Mere disease or dilatation of the commencing aorta, affords, as I formerly endeavoured to explain, a physical impediment to the due emptying of the left ventricle. It is a common cause, therefore, of hypertrophy and dilatation of that ventricle; and consequently, the *signs* of hypertrophy and dilatation of the left ventricle of the heart will at length result from disease and enlargement of the aorta near its mouth.

When aneurismal pouches form, as they often do, at the very entrance of the aorta, or in the coronary arteries, they often defy detection. I, at least, know of no sign of their existence upon which a physician can rely, or which can lead him even to suspect such a state of matters. But all at once the patient drops down dead: and upon searching for the cause of this sudden extinction of life, you find the pericardium distended with blood, and the source of that blood you find to be the ruptured aneurismal pouch, so near the

root of the aorta, as to project *within the pericardium*. In the preparation which I hold in my hand, an unbroken aneurism actually bulges into the *right ventricle* of the heart.

When the aneurismal tumour occupies a portion of the ascending aorta a little more distant from the heart, or is formed at the arch itself, it sometimes attains a large size, and the symptoms of its existence are derived from the effects its enlargement produces on the surrounding textures; and these effects are apt, for a while, to be obscure and equivocal, until an external pulsating swelling makes its appearance, or a sudden gush of arterial blood through the mouth discloses the true nature of the malady.

In these cases we *infer* the existence of aneurism sometimes from peculiar symptoms. Aneurism at the arch of the aorta may come to press upon the trachea, and impede the breathing; or by its effect upon the recurrent nerves it may cause a very accurate mimicry of laryngitis. The operation of tracheotomy, as I told you before, has more than once been performed, to relieve the supposed inflamed condition of the larynx, while the sole disease was aneurism at the arch of the aorta. Such mistakes are always discreditable; and the lesson they furnish should not be lost upon us. Whenever we find that a wheezing dyspnœa has gradually arisen, which no apparent affection of the air-passages satisfactorily accounts for, and the patient has a sense of pulsation within the thorax, we may suspect that an aneurism is at the bottom of these symptoms.

The effect of aneurismal enlargements of the artery to cause *absorption* of the neighbouring tissues, upon which the tumour presses, is very curious. You know that even the solid bone is removed, worn away as it were, before an advancing aneurism. Hence it not unfrequently happens that the trachea, or some of the larger bronchi, are at first flattened, and then give way; the aneurism breaks into the air-passages; and the patient, overwhelmed by a torrent of blood into and from his lungs, perishes in a few seconds. Or the tumour may contract adhesions with the pulmonary tissue, and destroy it to a certain extent, and *so* cause mortal hæmoptysis. But such cases are not always *fatal at once*.

Again, according to its situation and extent, an aneurism of the thoracic aorta may press upon the œsophagus, and cause the ordinary symptoms of stricture of that tube. Hence cardiac disease, and pulsation within the chest, accompanied at length by the signs of a constricted œsophagus, form strong presumptive indications of the existence of an aneurism; and in such cases, the œsophagus at last ulcerates through, and then copious and fatal hæmorrhage ensues. Hæmatemesis it may be called, though the blood is vomited not from the stomach, but from the gullet. A patient in the Middlesex Hospital, with symptoms of stricture of the œsophagus, one day brought up from the throat a red mass, which, at the moment, was supposed to be a bit of meat that he had been trying to swallow. It really was part of the clot from an aneurism; and it was speedily followed by a stream of red blood, and by death.

Again, aneurism of the thoracic aorta does frequently obstruct, by its juxta-position and pressure, the vena cava superior; nay, it may even obliterate that vessel, of which I have seen two instances. To one of these cases I alluded before, as a most curious example of dropsy. It illustrated exceedingly well, the effect of venous obstruction in causing serous effusion. The man was a patient of Dr. Hawkins. He presented a most extraordinary spectacle. His face, neck, and arms, were tumid and anasarcous to an enormous degree; while there was not the least trace of swelling or œdema anywhere below the ribs. He looked as if his upper half had been stuffed; and except that it was distressing, his appearance was extremely comical. His countenance was livid; his eyes seemed starting from their sockets, and even the cellular tissue beneath the conjunctiva was œdematous. The integuments of his neck and chest were quite brawny; and his arms were so swollen that they projected from his sides. The surface of the thorax in front was embossed by numerous veins, which were turgid with blood; and here and there patches of ecchymosis were visible. You may form some notion of the degree of mechanical congestion that existed, when I tell you, that upon the scarificator being applied, after a cupping-glass was taken off, upwards of twenty ounces of blood escaped in two minutes. The epigastric veins were visible and tortuous, and a free communication by anastomosis existed between these veins ascending from the inguinal region, and the mammary veins. There was a bellows-sound, which increased in loudness and harshness, from the root of the

aorta to the top of the sternum. The patient soon died; and a large aneurism of the aorta was laid open by lifting up the sternum, to which the artery had adhered, and into which, indeed, it had eaten a little. Not far above the right auricle, the vena cava was totally impervious; its sides having been gradually pressed together, as the tumour grew. The other case, of the same kind, which occurred in one of my own patients, I shall have occasion to refer to hereafter.

But aneurism of the thoracic aorta may exercise its pressure in another quarter, and wear away the bones of the vertebræ, and cause pain in the back, and ultimately palsy of the parts below that portion of the spinal cord; so that pain in the back, with pulsation, may justly awaken suspicion of aneurism making its way backwards. I remember hearing Dr. Farre describe a case of this kind, to inculcate the necessity of paying attention to the *sensations* of a patient. A man came to him for advice, having been told by another physician that there was nothing the matter with him—that he was fanciful. But when an adult person makes a constant complaint of certain morbid feelings in a part, the probability is that he *has* something the matter, and we must investigate the case with what helps we can get. In the instance in question, there were two signs of disease, and two only; a white tongue, and pain in the back. The whiteness of the tongue soon disappeared under the use of some medicine addressed to the digestive organs. The pain in the back remained. Dr. Farre interrogated the patient minutely every time he visited him, till at last the man got vexed and tired, and said, pettishly, "I know that if you split me down the middle, I am sound on my right side, and diseased on my left." Very soon after, he was found dead in his bed. What he had said was perfectly true: there was an aneurism pressing on the left side of the dorsal vertebræ.

Another consequence of an enlarging thoracic aneurism, sometimes observed, is pressure upon the *thoracic duct*, causing engorgement of the absorbent vessels and glands, and inanition. In short, whatever parts the aneurism may reach, and subject to its pressure, may have their function thereby suspended or disturbed, or their structure spoiled.

One sign, which I have myself frequently verified, of aneurism of the arch of the aorta, is a difference in the force of the pulse in the two radial arteries. The pulse in the one wrist will be extremely feeble, or even disappear. This happens when the state and position of the arteria innominata, or of the left subclavian artery, become altered in consequence of the enlargement of the aorta: and sometimes the one or the other of these arteries is completely closed up. But inasmuch as a similar difference of the pulses may arise from other causes, we can only look upon this symptom as one which may help to solve an ambiguous case. To give you an example of a difference in the beating of the arteries in the two wrists from other causes, I may mention a case in which the subclavian artery was thrown forwards, and compressed by an exostosis on the first rib. The case is mentioned in Mr. Mayo's Pathology. It occurred in a patient of mine, the husband of a nurse in my family. I had a girl for some time in the hospital, in one of whose arms no artery could be found to pulsate. Why, we none of us could make out.

Very lately, a surgeon from the country came to my house, desirous (he said) to consult me about a sense of discomfort in his head; and particularly about the state of his vision. When erect, he saw things obscurely. At three yards distance he could see my face, but could not distinguish the separate features. What he thought very strange was that he could see perfectly well when in the horizontal posture.

On my proceeding to feel his pulse, he said, in a careless manner, "By-the-by, that is another thing wrong with me; I have no pulse." Nor could I detect any, in either arm. He then told me that, four or five years previously, a medical friend, intending to feel his pulse in the left wrist, could find none. He was confident pulsation had existed a short time before that. After a while, the movement of the radial artery returned, in a very slight degree, and then finally ceased. Within nine or ten months of this discovery, the right pulse, after growing less and less distinct by degrees, had vanished also. Though somewhat weak, and subject to faintness, this gentleman had not wasted; nor had the muscles of his arms lost either bulk or vigour. Their veins were full enough of blood. His hands were often cold; and he felt altogether worse during cold weather.

Failing to detect any pulsation in the brachial and subclavian arteries, I next felt for the carotids; but I could perceive no beating in the track of their course. I had placed

my finger, for a few seconds only, in front of the left sterno-mastoid muscle—when I saw that his head drooped, his cheeks became white, and he was on the brink of fainting. But he recovered immediately. Then I made similar pressure, for a moment, on the right side of the neck, and the same phenomena were instantly repeated, with the addition of convulsive jerking movements of the head and arms. He rallied again directly upon my removing my finger, and was scarcely aware of what had happened. For a second or two he had been unconscious. His femoral arteries throbbed as usual.

I next examined his chest. There was no external irregularity or want of symmetry. Percussion gave a clear resonant sound everywhere in front. The heart was heard, beating with frequency, but without any bruit, over the greater part of the thorax. Its impulse in the præcordial region, below the nipple, was feeble: but a strong jarring impulse was communicated to the ear when the stethoscope was applied to the upper part of the sternum.

The patient complained of pains affecting his shoulders, clavicles, and the back of his neck; and of slight difficulty of swallowing.

From the intelligent physician who had attended this gentleman in the country I learned the instructive fact that, twenty months before, a loud rasping bruit had been audible, without impulse, at that part of the sternum where he, as well as I, now found no bruit at all, and a very considerable impulse.

I cannot doubt that in this painfully interesting case there is aneurismal disease of the aorta, interfering with and lessening, but not absolutely excluding, the stream of blood through the arteries which spring from its arch.

In the numerous specimens upon the table, you will find ample evidence and illustration of almost every one of the effects which I have described as apt to result from the pressure of thoracic aneurisms of the aorta. But similar effects would ensue from the same degree of pressure, however caused; and other morbid tumours, cancerous tumours in particular, are not uncommon within the thorax. Hence these same effects, considered as symptoms, are in themselves of equivocal import. If they occur in conjunction with signs of disordered circulation, or of a diseased heart, we may reasonably conjecture that they are produced by an aneurism. But we can seldom be quite sure of this, until the advancing aneurism comes near the surface, and causes an external prominence or tumour which pulsates visibly, or of which the pulsations are perceptible by the touch. And even then it may require some care and tact, to avoid mistaking an enlarged gland or a malignant growth, lying over a sound artery, and receiving an impulse from it, or communicating to it some unnatural sound, for the diseased vessel itself.

The pulsating tumour, if the aneurism has formed in the ascending aorta, makes its appearance, usually, on the right side of the sternum. If the aneurism is situated in the forepart of the arch, it produces a bulging at the sternal extremities of the upper ribs on that side. When it springs from the summit of the arch, the tumour rises above the sternum, and the sternal ends of the clavicles; and when the disease occupies the descending portion of the thoracic aorta, it will sometimes destroy the ribs and the bodies of the vertebræ, and push forward the lower part of the left scapula: or it may show itself in front, beneath the left clavicle.

When such a tumour presents itself, and is attended with a steady, heaving pulsation, synchronous with the systole of the heart, the doubt and obscurity which may have previously hung over the nature of the patient's disorder is cleared away. A little attention to all the circumstances of the case, will generally suffice to determine its true character.

There are, however, some errors prevalent respecting these pulsating tumours, which errors I shall glance at in passing. In the first place, the pulsation of the tumour is frequently, most frequently, attended with a rough bellows-sound; and some persons rely upon this as distinctive of the nature of the pulsating tumour. They hold that this harsh bellows-sound is always discernible in an aneurismal tumour, and that when such a sound cannot be heard, the tumour is not an aneurism. But this is a mistake. There have been in the Middlesex Hospital within the last six months (1837), two instances of pulsating tumours in the fore part of the thorax, unattended with any bellows-sound; yet they were both ascertained, after death, to be aneurismal tumours. One of the aneurisms is before you. In the case of the country surgeon, a bellows-sound was present at a cer-

tain period of the disease, and absent at a more advanced period. I may say the same of the purring thrill. It is a common, but by no means a necessary attendant upon thoracic aneurisms. Again, much stress was laid by Laennec, upon the circumstance of the aneurismal pulsation being single, being unattended by any second sound. But this is not a true rule if taken universally. In the instance which furnished this very preparation, the sounds were double, just like those of the heart. The second sound heard *is*, no doubt, the second sound of the heart, conveyed from the place of the aortic valves, where it originates, along the course of the vessel, to the aneurism; which often indeed lies in contact with the heart, and could scarcely fail to have the diastolic sound propagated through it. It is a fact not so easily explained, yet it certainly is a fact, that a double sound *may* be audible in aneurisms very distant from the heart. Dr. Davies states, that he never heard a second sound in abdominal aneurisms; yet I presume that, under favourable circumstances, the sound of the closing of the flood-gates at the root of the aorta, may be heard far along its channel. I can account in no other way for the second sound, heard by myself and by many others, in a *popliteal* aneurism. I mentioned before, a patient whom I saw in St. Bartholomew's Hospital, and in whom an exceedingly loud diastolic sound, like the sharp whining note of a dog, was audible by the ear placed upon his arm, over the brachial, and even over the radial arteries.

There are some very judicious remarks made by Dr. Hope upon the sounds that are apt to be heard in these pulsating aneurismal tumours to the right of the sternum; showing how they may be distinguished from the natural sounds of the heart itself, conveyed to that spot through some dense conducting medium. He observes (and all that I have seen coincides with this observation), that the first of the aneurismal sounds, when there are two, the sound that coincides with the pulse, is always louder than the natural systolic sound of the heart, and generally louder than any of the morbid systolic sounds; and that instead of increasing in intensity, as the stethoscope is moved gradually towards the præcordial region (as it ought to do, if it were the conducted sound of the heart itself), it diminishes in loudness, until it is gradually lost in the actual systole of the heart. Whereas the second sound heard over the tumour does augment as we get nearer the heart, for it is, in truth, the diastolic sound of the heart, and therefore is more audible as we approach the point where it is generated. The sounds of aneurisms of the aorta are usually audible in the back also; and if a very loud bellows-sound be heard there, where the natural sounds, if heard at all, are always much abated, that circumstance furnishes strong grounds for suspecting the presence of an aneurism, or of some great change in the aorta.

To give you some notion of the course which aneurism of the thoracic aorta may run, I will describe another instance of that disease which occurred under my own observation. The subject of it supplied the preparation to which I last referred.

He was a stout, healthy-looking man, forty years old, a private coachman. He became my patient in the hospital on the 8th of September, 1836. He complained of pain and tenderness around and above the right mamma. The pain was increased by a full inspiration; and when lying on the opposite side, he felt as though he was tied in the painful part.

He had been ill a month only. His illness commenced with severe rigors, and fever, and sudden pain in the side, for which he was bled three times with much relief. He attributed the attack to having lain, upon his right side, in a damp bed.

There was scarcely any projection at the spot where the pain and tenderness were experienced. By careful examination several times repeated, I satisfied myself upon the following points.

On the right side of the thorax, no vesicular breathing could be heard; and the whole was dull on percussion. On the left side percussion gave a hollow sound, and the respiratory murmur was clear and strong. In the tender spot, an inch and a half above the mamma on the right side, a strong pulsation could be felt, and two sounds were distinctly audible, the first of them keeping time with the pulse at the wrist. But there was no bellows-sound. M. Sanson, the celebrated French surgeon, was then in London, and went round with me one day, and examined this patient; and he expressed his opinion that it was not a case of aneurism, because there was no *whiz* or bellows-sound to be heard. Of course his examination was a cursory one, and I merely mention this circum-

stance to show you what importance is attributed to the presence or absence of a *bruit de soufflet* in such cases. M. Sanson suggested that the heart might be displaced, and pushed over to the right side. However, it was clear to me that this could not be the case, because the breathing was deficient, not on the left, but on the right side; and, above all, because the apex of the heart could be both seen and felt beating in its proper situation, in the præcordial region on the left side. Also on the left side, percussion made on the edge of the ribs gave a tympanitic sound, indicating the place of the stomach; on the right a dull sound, pointing out the situation of the liver; so that it was not a case of transposition of the viscera, such as had been found, not long before, in one of my patients. In the course of the disease, a slight bellows-sound did become perceptible over the right mamma, when the patient sat up; but even then, the natural sounds of the heart, without any morbid quality, could be heard in the natural position of that organ. Œgophony was audible at the back part of the right side of the chest.

This patient had repeated attacks of pain, dyspnœa, restlessness, and inability to lie down; and these attacks were always most sensibly mitigated by the application of leeches to the diseased part. By the 11th of October he was so comfortable that he wished to go out; and he went to his master's in Connaught Place.

Two days afterwards, he sent to beg that I would go and see him there. He had brought up, on the preceding evening, during a paroxysm of coughing, about a pint of bright red blood; and he had continued to cough, and to expectorate small quantities of blood. I had him again brought to the hospital on the 14th of October, where he remained, apparently much the same as before he went out. But on the 19th he suddenly expired. The whole duration of his illness had been nine or ten weeks.

We found the heart natural in size and in appearance; the pericardium healthy, and containing no more than the usual quantity of serum. All the cavities were natural in their dimensions, and in the thickness of their walls; and all the valves healthy, excepting one white spot on the mitral valve, which could not have interfered with its motions.

The aorta at its origin was also natural in size; but it began to dilate just before it escaped from the pericardium, and the dilatation continued to the giving off of the left subclavian, where the vessel resumed its proper capacity. The arteries arising from the aorta did not partake of the dilatation; but the sac overlapped and adhered to the external surface of the innominata, for about a quarter of an inch from its origin. This explained a symptom I omitted to mention, viz., that the right radial artery beat much more feebly than the left.

The pouch formed by the aneurism adhered in front, for the space of two inches, to the inner surface of the third rib; and close to the edge of this adhesion there was a small irregular aperture about two lines in diameter, by which the interior of the pouch communicated with the right pleural cavity. Nearly a pint of loosely coagulated blood was found in that cavity, together with a greater quantity of serous fluid than could have belonged to the coagulum. Just above the adhesion to the rib, the pouch adhered to the substance of the lung, over a space about an inch square; and here the parietes of the artery seemed wholly wanting. This doubtless had been the channel of the copious hæmoptysis a week before his death: and it is interesting to observe that the opening of the aneurism into the lung was not immediately fatal. The artery was much diseased, in the usual manner. The right lung was nearly all of it "carnified" by the compression it had undergone.

If this case had not terminated as it did, no doubt the aneurism would have made its way outwards through the ribs, as happened in the very remarkable specimen before you; in which you see that the sternum and five of the ribs have disappeared before the pressure of an aneurism in the ascending portion of the aorta. Sometimes, the tumours that form in this manner, project and attain the size of the head of a full-grown fœtus before they burst.

Treatment.—What can we do in these melancholy cases? Not much. Certain points of practice are so obvious that it is almost superfluous to mention them. I mean the observance of quiet, and the religious avoidance of every thing likely to excite or quicken the circulation: bodily exertion, therefore; straining of all kinds; mental emotion; stimulating food and drink. These are not only likely to aggravate the existing mischief, but prove often the immediate cause of the rupture of the aneurism, and of sudden death.

I mentioned, in describing the morbid anatomy of aneurism, that when the diseased vessel began sensibly to dilate, and more especially when it was protruded into a sac or pouch, the blood began to coagulate upon the diseased membrane. And it continues to do so, from time to time, in successive layers, so that upon dividing the aneurismal sac, you will see concentric laminæ of firmly coagulated blood. This is clearly a strengthening of the weak place—a reparatory and compensating process analogous to others which we have already had occasion to notice. And our object, here as in other cases, must be, not to interfere with the natural attempts towards repair, but to assist and promote them, if we can.

This principle has long been distinctly recognized in the treatment of aneurisms that are incapable of relief by surgical means. But it is much to be doubted whether the principle, so sound in itself, has been judiciously followed out. You have probably heard, or will hear, a good deal of Valsalva's and Albertini's mode of treating aneurisms. It was simply that of bleeding the patient repeatedly, and keeping him upon as low a diet as was barely enough to prevent his perishing of inanition. The object of this plan of treatment was to facilitate the coagulation of the blood by diminishing its force and velocity, in the hope that at length such a solid barrier might be built up and organized, as might, in some sort, furnish a new wall to the artery in the dilapidated part. When this object had had the best chance of being accomplished; when the patient had been so reduced as to be scarcely able from weakness to raise his hand from the bed, to which he was strictly confined; then Valsalva increased his quantity of nourishment by degrees, until the necessary strength was restored.

Now, I quite agree with Dr. Copland in thinking that this practice may be carried, and has been carried, to a hurtful extent. He says that he has seen cases "in which aneurismal tumours had existed for some time without any increase, so long as the patient avoided any marked vascular excitement, and continued his accustomed diet; but when repeated depletions, and vegetable or low diet were adopted, great augmentation of the tumour, and fatal results, soon followed."

In truth, we shall perceive reason to expect that this would be the case when we consider, first, that the starving system, and the frequent abstraction of blood, diminish the quantity of fibrin in that fluid, rendering it more watery, and less disposed to coagulate; and, secondly, that what is called reaction—or a violent palpitating action of the heart—is very apt to follow repeated losses of blood; and this forcible action of the heart must tend rather to sweep away the existing coagula, than to cause an additional deposit.

A more reasonable and hopeful plan of management, therefore, would, in my opinion, be one which should keep the action of the heart gentle and moderate, and the motion of the blood as slow and languid as possible, without impoverishing that vital fluid. We should husband the materials of repair, and promote the deposit of them where they are wanted. A nutritious but unstimulating diet; perfect repose of mind and body; and a due regulation of the natural functions; with the abstraction of so much blood only as may be necessary to alleviate pain, or to subdue *excessive* arterial action, or to unload vessels which are manifestly oppressed by their contents;—these, I humbly conceive, constitute the most rational means of furthering the endeavours of nature towards a cure. Few cures, indeed, can be hoped for in any way. Yet life may be prolonged in these cases, by great care; and the extension of existence even for a month or two, or a week, or a day, may sometimes be an acquisition of the greatest moment.

I have little to say concerning particular drugs. Digitalis may, perhaps, be sometimes of use; and the acetate of lead is well spoken of by those who have tried it. I have not had sufficient experience of either of these remedies in the treatment of aneurism, to enable me to state any thing to you confidently, in respect to their value.

LECTURE LXIII.

DISEASES OF THE VEINS. PHLEBITIS; ADHESIVE, AND SUPPURATIVE: CONSECUTIVE SCATTERED ABSCESSES. TREATMENT OF INFLAMMATION OF VEINS. EFFECTS OF THE GRADUAL OBSTRUCTION OF LARGE VENOUS TRUNKS.

YESTERDAY I concluded what I had to say, as a physician, respecting diseases of the *arteries:* and this seems as fitting a time as any for taking a final notice of some of the morbid conditions of the *veins*—especially of their inflammation. Already, more than once, brief reference has been made to this important subject: important whether we consider the large amount of mortal disease which it comprehends, or its wide and intimate relation with general pathology. I should have done better if I had given you, in an earlier part of the course, a more complete and connected account of *phlebitis*, and its consequences. It is this malady which gives to many fatal injuries, and to many, nay to most, of the fatal operations of surgery, their mortal character: it is of surpassing importance, therefore, to the surgeon. The same malady lies at the bottom of the deadliest cases of puerperal fever: it is consequently of the deepest interest to the accoucheur. It occurs also, not seldom, in the practice of the physician, appalling him by its insidious, its rapid, and too frequently its resistless course. Moreover, its pathology, which has been successfully investigated only within these few years, furnishes a key to that of other morbid conditions of great moment.

The first effect of inflammation of a vein is to impede, or arrest, the passing blood, which, coagulating upon the inflamed surface, adheres to it. In some instances the inflamed coat of the vessel is merely (as Mr. Hunter said) furred over: in others its channel is completely dammed up. The obliteration of a small vein in this manner can seldom have any serious consequence; but much suffering and distress, and even death itself, may result from the sudden and continued obstruction of one of the large venous conduits. For example, the painful disorder, called *Phlegmasia dolens*, is caused by a stoppage of the blood in the *femoral* vein. A similar arrest of its current in the *sinuses* of the brain, is a mortal change.

This adhesive form of phlebitis is a local disease. Whatever ill effects it may produce are purely mechanical; and depend upon the closure of the canal. If the organ mechanically affected by it be not a vital organ;—if the system can await the development of a collateral venous circulation;—all, at length, may end well. Sometimes, indeed, as the inflammation gradually subsides, the coagulum is softened and partly reabsorbed, the blood drills for itself a fresh passage through the centre of the plug, and the circulation is restored in its accustomed channels.

This is, fortunately, the commoner form of phlebitis: but sometimes the inflammation advances beyond the adhesive, and into the suppurative stage. Even then the disease may remain a local one. The adhesive process may bound and isolate the suppurative in both directions: and an abscess in the part is the usual result.

But if the suppurating surface of the vein be not so shut off, and pus mingle and circulate with the blood, the disorder is no longer merely local. The contaminated blood is conveyed to distant parts, and the whole system tainted. The malady has become general, and of the most formidable character.

It had long been noticed, as a matter of fact, that collections of pus were not uncommon in various parts of the body, when death had followed mechanical injuries, or great surgical operations. Abscesses in the liver, in particular, were known to be associated

with mortal fractures of the skull. Very fanciful reasons were assigned for this coincidence. By degrees it was ascertained that these scattered collections of matter—occurring most commonly in the lungs and liver, but not unfrequently in or near the joints also, in the serous cavities, among the muscles, in the brain, in the eye, and elsewhere—were connected with the introduction of some vitiating secretion, and especially of pus into the current of the venous blood.

And this step having been gained, fresh speculations arose, concerning the manner in which the internal collections of pus were forced. In the viscera they were usually small, well-defined, surrounded by the healthy tissue of the organ, and several in number. Some maintained, that the pus, in substance, was carried to the parts in which it was found, and there simply deposited. Others were of opinion that the tainted blood created in the system a general tendency to inflammation, which was developed in many places simultaneously. Neither of these suppositions was quite true, neither of them quite false. The pus discovered in the serous cavities was accompanied by unequivocal traces of inflammation in those parts. This alone rendered it probable that the smaller purulent collections were not merely dropped there by the blood in its course, (a thing very difficult to conceive,) but were the products of actual inflammation, excited somehow in those very spots. And it is now believed that these abscesses *of*, as well as *in*, a part, proceed from suppurative inflammation, provoked by the presence of particles of pus, brought thither with the circulating blood.

I told you before, that foreign substances, entering the blood, and failing to pass out of it again through the natural emunctories of the body, are liable to be stopped when they arrive at the first network of capillary vessels that lies in their course. Now the blood, circulating in the veins, reaches (much of it at least) in each of its circuits, two such great networks, the hepatic and the pulmonary. Through the pulmonary network all the blood must pass, through the hepatic, some of it; and it is there, in the capillary tissue of these organs, that particles of pus, and other material substances, foreign to the blood, and incapable of elimination with the customary excretions, are apt to stick, or be entangled, and to excite inflammation. Some of them, however, in general, pass on, and arriving at the left side of the heart, are transmitted, with the arterial blood, to various parts of the body, there to exercise a similar deleterious influence.

Such was, and is, the theory: and it has been tested and confirmed by direct experiment. Inasmuch as the conveyance of the *pus* cannot be traced by the eye, nor the manner of its being collected into an abscess demonstrated, except by inference, Cruveilhier introduced *quicksilver* into the veins of animals; a metal which is liquid and divisible into very minute particles, and which exerts no chemical agency upon the vital fluid. When the mercury was inserted into the veins which concur to form the vena portæ, the whole, or the geater part of it, was arrested in the liver. In that organ, the animal being killed a certain time after the introduction of the metal, small, roundish, red spots were always discoverable, which passed gradually into little abscesses surrounded by a halo of inflammatory redness; and in the centre of each red spot, and of each abscess, lay a minute globule of mercury. A few similar points of suppuration were usually to be seen in the lungs also. But when the quicksilver was put into the blood in its direct course towards the vena cava, then it was in the lungs that these points were either exclusively detected, or at any rate most numerous.

You must, I think, perceive how strictly these experiments bear upon the rational humoralism acknowledged at the present day. If pus, and mercury, may thus be distributed to particular organs, and thus excite circumscribed inflammation, so doubtless may other extraneous impurities—introduced by the poison of what is called good living, by the respiration of foul air, and in various other ways—reach and settle in different parts of the body (the liver, the lungs, the kidneys, the joints) and there produce, if not inflammation and pus, yet such changes at least as spoil the texture of the organ, and pervert its healthy office. That cancer is propagated in this way we have heretofore seen reason to believe. In all probability the deposition and increase of tubercles fall under the same law.

Suppurative phlebitis—with all its horrible effects—is liable to arise, not only after severe but also after slight injuries; from the trivial as well as the grand exploits of surgery; nay,

spontaneously, as it were, without any local hurt, under the agency of natural causes, such as exposure to cold. And the part in which the phlebitis occurs has some influence, as you will now understand, in determining the principal seat of these scattered abscesses. When they succeed amputation of a limb, or fracture of the skull, or the interference of surgery with varicose veins, or (as they may) even the simple operation of phlebotomy, they are likely to be most numerous in the lungs. But they are more conspicuous to hasty observation in the liver than in the lungs; and that is why hepatic abscess was supposed to have some special connection with injuries of the head. Morgagni, however, long ago pointed out the fact, that other parts also were affected in those cases. Again, we may expect to find these disseminated abscesses chiefly in the liver, when suppurative plebitis occurs in any of the tributary veins of the vena portæ: when it supervenes, therefore, upon operations on the bladder or on the intestines—operations for the removal of stone, for the release of hernia, for healing fistula in ano, for the cure of piles.

It is, however, very common for the poison to pervade the whole body, and for abscesses to form in various other situations, as well as in the lungs and liver. I once saw a young woman die, in the Middlesex Hospital, from phlebitis, with large abscesses in many parts, and especially in the joints, after the simple excision, with scissors, of some small spongy irritable growths about the orifice of her urethra.

Two or three instances of suppurative phlebitis, unconnected with any known hurt, and originating apparently in exposure to cold, have fallen under my own observation: but I prefer giving you the following short case, with the details of which I have been favoured by Dr. Malden, of Worcester.

Miss ———, a teacher in a Ladies' School, was attacked, after exposure to wet and cold, with acute pain, heat, and redness, in the front of the left forearm. Mr. Cole, an eminent surgeon, of Bewdley, by whom she was at first attended, discovered inflammation following the course of the cutaneous veins. Upon its subsidence the veins were left like hard cords. Soon after the right arm was affected in a similar way: and next, both the lower extremities, which became anasarcous. All this was attended with paroxysms, simulating those of tertian ague; exhausting sweats, diarrhœa, and a frequent feeble pulse. At the end of a month, deep-seated fluctuation was detected in the right thigh, three inches below Poupart's ligament. The abscess gradually approached the surface, and was opened, and more than three pints of very fœtid pus were discharged. The wound never closed, and she sank, exhausted, a month after it was made.

There was no pain, premonitory or attendant, connected with this formation of matter.

The abscess was traced, after death, upwards, behind the muscles of the pelvis, as far as the sacro-iliac symphysis, where the bones were extensively carious.

Many of the superficial veins, both of the upper and the lower extremities, were found to be completely obliterated by adhesive inflammation, or sealed up by coagula of blood.

This spreading and morbiferous inflammation of veins is sometimes so remarkably prevalent, as to partake of the character of an epidemic disorder: and this, its occasional prevalence, appears to be owing to some peculiar condition of the atmosphere—or rather to some predisposition of the human body, engendered by the operation of influences which are probably atmospheric. During such periods prudent men refrain, if they can, from the performance of surgical operations.

The view which I have now set before you of suppurative phlebitis, and of its distant effects, involves some apparent difficulties, and some curious questions. A short consideration of these may serve to throw a clearer light upon the main subject.

First, then, how is it—if indeed the disseminated abscesses result from the introduction of pus into the blood—how is it that we do not meet with them oftener? Pus is absorbed, in numberless instances, without the occurrence of any such formidable consequences. We see great abscesses disappear spontaneously, and yet no other smaller scattered abscesses ensue. Does not this fact invalidate the theory of the cause and formation of such distant points of suppuration? No. It seems that, for their production, pus as such, pus in substance, pus in the mass, must be received into the veins, and circulate with the blood. The pus which is taken up by ordinary absorption, is altered, probably, by that

process, before it reaches the blood: at any rate it has not the same mischievous and fatal effect.

You may here inquire in what manner pus gets into the circulation, in consequence of an amputation? Is it not absorbed from the suppurating stump? I conceive not. Supposing the amputating knife to cross and sink into an existing abscess, and to divide a vein—then, indeed, pus might be sucked into the vein, and the usual consequences follow. But the veins that lead to, or rather from, a stump, become blocked up, and impervious, from adhesive inflammation, or from mere coagulation and adhesion of the blood in them, before the stump has had time to suppurate. How then does the pus ever find admission? No doubt it is a product, in this case also, of phlebitis. The interior of a vein inflames, and goes on to suppuration; and the pus which it pours forth mingles, as pus, with the circulating stream.

Indeed these scattered abscesses appear to originate always in phlebitis. Such is the opinion of my colleague, Mr. Arnott, who has contributed a valuable paper on this subject to *the Medico-Chirurgical Transactions*. Such is also the opinion of M. Cruveilhier. It has been objected that, in some fatal cases of this kind, no phlebitis could be detected: that the principal veins have been diligently traced, yet no vestige of suppuration, nor even of adhesive inflammation, has been visible. To make this objection valid, *all* the veins throughout the body should be scrutinized; and that has seldom, I fancy, been done. I have known several instances, in which most of the larger trunks were searched in vain, till at length a short tract of one of them, an inch or two perhaps, was found, bearing marks of having been inflamed. You must not conclude, therefore, against the antecedent existence of phlebitis, until you have examined every vein in the soft parts of the body: no, nor even then. You must go deeper than the more obvious veins. There is good reason for believing that the *bones* and their veins are often the seat of the primary mischief, the fountain from which the pus, which thus renders the blood a poison, first proceeds: the veins, for example, of the diplöe of the skull, when scattered abscesses ensue upon injuries of the head; the veins of the bones of the extremities after unsuccessful operations. There is yet another explanatory supposition applicable to some cases. All local traces of the primary inflammation may vanish before death, while the vital powers are being undermined, and about to sink under its secondary effects.

Cruveilhier repeatedly performed the following experiment, and always with similar results. He introduced crude mercury within the hollow shaft of the thigh bone of a living dog. When the quantity was considerable, death occurred in a few days, and the metal was found strewed thickly through the lungs, each globule occupying a capillary branch of the pulmonary artery, and surrounded by a small sphere of inflammatory redness. When the quantity was minute, the animals lived longer, and little *abscesses*, enclosing each a particle of mercury, were then discovered in the same organs. The mercury he supposes to have found a direct entrance into the blood, in these cases, from the cancellous portion of the bone: and through the same channel it seems to be that pus often enters the circulation. You may remember my relating some fearful examples of scattered abscesses, supervening upon chronic disease of the bones of the ear. Cruveilhier states that having been present at the examination of the body of one who had sunk after amputation of the leg, and whose lungs were full of little abscesses, he sought, without success, for some inflamed vein: but upon dividing the tibia and fibula, he found the spongy extremities of these bones infiltered with pus. Here, beyond question, had been the source of the visceral mischief.

The local phenomena, when a superficial vein of some magnitude is inflamed, are pain and tenderness in the course of the vessel, which, in the adhesive variety of the complaint, is soon converted into a tangible, hard, and sensitive cord. Whether the vein be near the surface or deep seated, there is usually more or less œdema of the cellular tissue of the part. Phlebitis of this kind has been sometimes confounded, I believe, with inflammation of the lymphatic absorbent vessels. You distinguish the latter by the slenderness of the painful cord; by its position, which is still more superficial than that of a subcutaneous vein; by the number of little knots which diversify its course; and by the streaks and patches of bright inflammatory redness which appear along the same track.

In the suppurative form of phlebitis the general symptoms take the lead. The forma-

tion of pus in separate and often distant parts is rapid, and frequently unannounced by any local pain. When however the joints, or parts near the joints, are the seat of suppuration, much soreness is complained of, and the malady is liable to be mistaken for rheumatism; and when the serous cavities are implicated, the pain is sometimes severe. Suppurative phlebitis is commonly attended in its progress with repeated shiverings, and with profuse sweats, and occasionally with copious and very unnatural discharges from the bowels. These last have been noticed in animals soon after the introduction of pus, or of putrid matters, into their veins. Nature seems to attempt to eliminate the poison in this way: and where the quantity of pus so introduced has been small, the attempt is now and then successful. But in general there is a continual supply of the noxious substance, and the system is irrecoverably infected. Typhoid symptoms occur in most cases, but not in all. Very constantly there is great agitation, and a marked disturbance of the nervous system.

I have called this purulent infection of the blood a formidable disorder: in truth it is almost always a fatal disorder. Yet that it is not inevitably mortal I know by a case which has recently occurred in Mr. Arnott's practice at the Hospital, and which he permits me to mention. He had occasion to amputate the forearm of a man whose hand had been crushed by machinery. Two or three days after the operation, the patient's pulse quickened, and he had a severe rigor. These two circumstances led Mr. Arnott to apprehend the supervention of phlebitis; and accordingly one of the large, superficial, visible veins of the forearm became swollen, hard, and tender. Leeches were applied along its course; and the parts were kept covered with the water dressing. In no long time an abscess formed in the other arm; next, a large one in the back, from which twenty ounces of pus were evacuated; then one beneath the glutei muscles of the buttock, on both sides—each of these two contained about sixteen ounces. In short, dating between the beginning of October and the middle of December, no less than seven collections of matter presented themselves in various places. The last of them was in a very unusual part, beneath the man's tongue, in the ordinary situation of ranula, for which, indeed, it was at first mistaken. In every instance the pus was let out as soon as possible, and the main feature in the general treatment was the administration of opiates, and of wine, with a liberal allowance of good beef tea in the earlier stages, and of meat afterwards. This man recovered; and was seen in the month of May following, in perfect health. The case is extremely interesting. It shows, I say, that suppurative phlebitis, even when it disseminates consecutive inflammation and suppuration throughout the body, is not absolutely and hopelessly fatal. Whether abscesses, from this cause, distributed in the lungs or liver, are capable of repair, I cannot tell you. Under the treatment employed, the inflammation of the vein in the arm gradually subsided. All outward evidence at least of its existence, all induration even, disappeared; and presumptively all inward evidence too. So that had this patient sunk, late in the course of his disorder, under the multiplied secondary abscesses, his venous system might probably have been searched in vain for any remaining traces of phlebitis: and yet we know that at one time he *had* phlebitis, more severe and extensive than belongs to the natural and kindly healing of every stump.

The treatment found most suitable in phlebitis has just been briefly indicated; local depletion when the inflamed vein is accessible; regulation of the bowels; strong animal broths and wine to support the strength. Our object is in the first place to subdue and resolve the inflammation; or at any rate to prevent its passing beyond the adhesive stage. To this end, the vein being obvious and superficial, we apply leeches, cold lotions, or fomentations. During the progress of the malady, especially when suppurative phlebitis is prevalent, it would be unsafe to cut into a large vein, lest by that slight violence we establish a fresh local phlebitis. Indeed after the suppurative form has once been set up, general blood-letting does no good; but, on the contrary, impairs the power of the system at large to struggle against the disease.

The *obliteration* of a large vein, whether by adhesive phlebitis or in any other way, is perilous in proportion to its magnitude, and to the rapidity with which its complete occlusion has been effected. The gradual stoppage of even the largest—the primary venous trunks, the venæ cavæ—admits of some degree of compensation. In one instance of this kind, which I briefly described yesterday, and which I myself witnessed, the *superior* cava

was flattened, and its channel completely effaced, by the pressure of an aneurismal tumour: in another, which I mentioned formerly, on Mr. Kiernan's authority, an immense varix of the superficial veins of the abdomen supplied to the returning blood the passage denied to it, in its natural course, by the partial obliteration of the *inferior* cava. To impress upon your recollection the ordinary phenomena that result from these grave derangements in the hydraulic machinery of the body, I will state here, from my hospital case-book, the outlines of two additional examples of a similar character.

James Buck, aged thirty-three, was admitted on the 6th of March, 1838. The appearance of this man was very remarkable. His countenance was swollen and livid; his eyeballs projected; his lips, the end of his nose, and the rims of his ears, were of a deep purple colour. It was manifest that the blood did not freely descend from the head. Further evidence of this became apparent when the trunk of his body was uncovered. The throat was very full and tumid, like that of a goitrous person, yet the swelling was not owing to enlargement of the thyreöid gland, nor to œdema; but felt firm and fleshy. The jugulars were distended; and the whole surface of the thorax in front, with that of the shoulders, and of part of the abdomen, was thickly overspread with a network of prominent veins. The external mammary veins were seen to communicate freely with veins proceeding from the neck on each side, with the veins of both the upper extremities, and with the epigastric veins from beneath. Here and there were patches of minute purple varicose branches, crowded closely together.

He told us that whenever he stooped down, to tie his shoe-strings for instance, he became giddy, his head swelled, and his face and ears grew black: that he was very nervous, easily flurried, and dreamed much, thinking that he was flying in the air, falling down precipices, and the like. He had not noticed any swelling of the face or throat until three weeks previously; and he had never, he said, had a day's illness before. He knew of no cause for the complaint; had been making no extraordinary bodily effort; had never suffered rheumatic fever. He professed, also, temperate habits, but he had been a soldier, and afterwards a pugilist, and his wife informed me that he had led an irregular life.

The evidence, I say, was strong, of some obstruction to the return of the blood through the superior cava. Now such obstruction is most commonly produced by intrathoracic tumours—sometimes by carcinomatous; much oftener by aneurismal tumours. There were no circumstances to make it likely that malignant growths existed; but there were circumstances which corroborated my first suspicion that the symptoms were dependent upon aneurism of the aorta, or of one of its primary branches.

There was indeed no external prominence, no pulsative or other swelling, no aneurismal whiz, to guide us to this diagnosis. Upon careful and repeated auscultation of the chest, the murmur of respiration was found to be in some parts feeble and unequal. This might consist with the presence of any kind of tumour. The heart's action was heard, and felt, strong and heaving, in the proper place, beneath the left nipple. To the right of the sternum and near the middle of its upper portion, one's ear was distinctly jarred at each systole of the heart, though with less force than in the præcordial region. But in the space intermediate between these two spots, no such jarring sensation was perceptible, although the heart could be heard, beating with a slight bellows-sound. Moreover, the right radial artery was considerably weaker and smaller than the left. This showed that the innominata was interested in the disease. The symptoms, taken together, left no doubt on my mind that there was an aneurismal pouch beneath the sternum, where the jar was experienced. I have gone somewhat into particulars to show you how confidently sometimes, by close observation, you may pronounce upon the condition of parts which you can neither see nor touch.

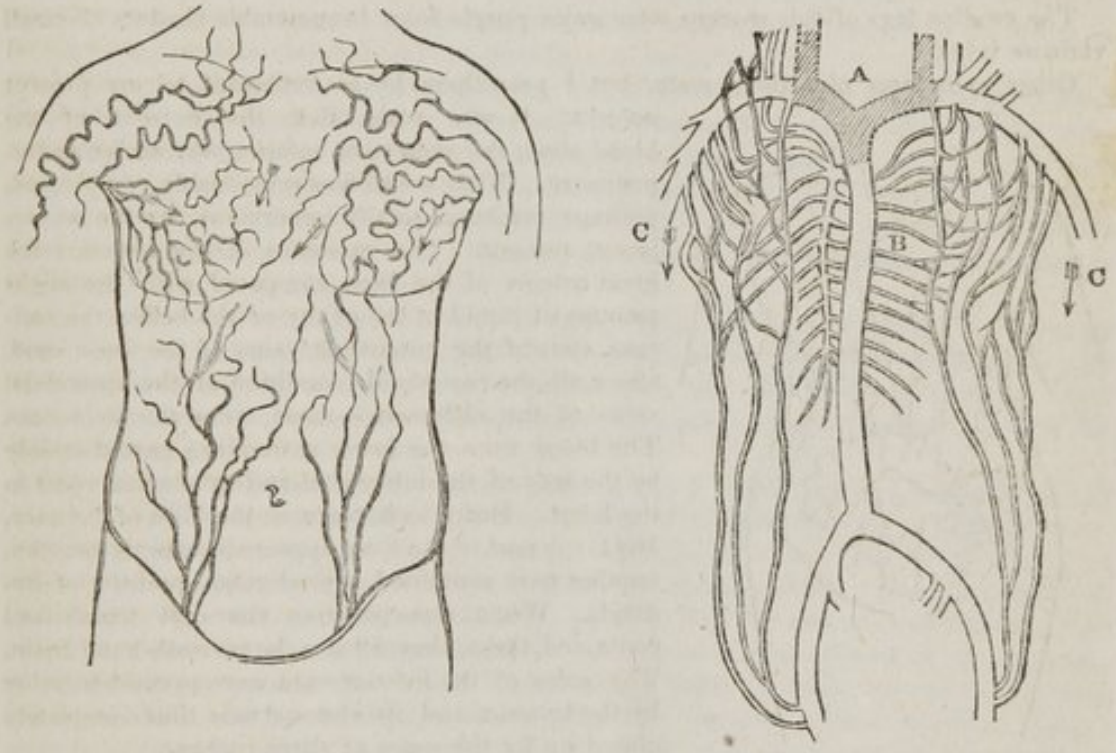
All that could be reasonably hoped for from medicine, was postponement of the evil day. To relieve the oppressed blood-vessels by taking away part of their contents, by freely purging the patient, and by setting his kidneys to work—this was what was to be attempted; and this was done. He was repeatedly cupped, and always with most sensible relief to his feelings, the blood flowing copiously. Purgatives and diuretics also acted well; and so much was the man benefited by these measures, that *twice* he left the ward, and became an out-patient.

About the middle of June a new symptom arose—severe pain extending from the

right collar-bone across the shoulder. He died on the 10th of July. Three or four days before his death, he had rigors and extreme dyspnœa, complained of pain over the whole thorax, and declared that his "heart seemed on fire." These symptoms were caused by the supervention of pericarditis, which proved speedily fatal. A thin layer of recent, reticulated lymph was found covering a considerable extent of the surface of the heart.

The body was examined by Mr. Shaw, after injection of the veins, and of the thoracic duct.

There was a large aneurism of the arteria innominata; of which I omit all particular description, my present object being to draw your attention to the state of the veins. The two great trunks that, coming from either side, unite to form the vena cava superior, were closed up, as well as the corresponding portion of the cava itself, which was lost and confounded in the walls of the aneurismal sack. The subclavian veins were pervious up to the point where they joined the internal jugulars, but no further. The preparation of these parts, which is before you, and the rough diagrams which I here exhibit, will aid your comprehension of the mode whereby the blood, descending from the head, found its way at length, through many circuitous channels, to the heart. The larger deep-seated compensating veins were not greatly magnified, but the number of the smaller branches was much augmented. The *vena azygos*, for example, was very little above its usual size; yet it was apparently provided with a greater number of considerable branches than are commonly observed under natural circumstances.



A Obstructed veins.
B Seat of the right auricle.
C Thoracic, pectoral, and mammary veins conveying the blood in a contrary course to its usual direction, and anastomosing with the intercostal and epigastric veins.

As the veins into which the trunks of the absorbent vessels discharge their contents were obliterated, it became interesting to examine the state of these vessels, and of the lymphatic glands. But the condensation and confusion of all the parts around the tumour rendered it difficult to trace the thoracic ducts. The glands were remarkably large, of a purple colour, and gorged with bloody serum. The fulness of the neck, noticed during life, was occasioned by this turgescence of the *glandulæ concatenatæ*. Large glands were seen studding the walls of the aneurism, and adhering to the great vessels connected with it: *i.e.*, in situations where, under ordinary circumstances, such glands, from their minuteness, can scarcely be detected at all.

With this case, contrast the following:—

Harriet Baldwin, thirty-three years old, was admitted, December 29, 1840, anasarcous as high as the hips, and with an enlarged abdomen. The swelling had begun, she said, a fortnight before.

She complained of cough, and of expectoration, which was sometimes tinged with blood. She could not lie down in bed for dyspnœa. Her urine was scanty and dark coloured.

She told us she had dropsy, quite as bad, five years ago, of which she was cured in St. Bartholomew's Hospital.

All this we learned in the admission-room. The next day, when she was in bed, we learned a good deal more.

The large abdomen did not owe much, if any, of its bulk to ascites. It contained a palpable tumour, filling the right hypochondrium, and extending thence far beyond the umbilicus to the left, and into the right groin. This tumour, from its situation, and from the continuous dulness elicited by percussion from the right mamma downwards over its whole extent, was evidently formed by the liver, much enlarged, and out of its place.

But besides the tumour, the abdomen presented on its surface a very singular appearance. Two zig-zag lines of varicose veins ran up its middle, near the right edge of the linea alba. These, which were evidently the epigastric veins, inosculated above with the mammary. Large, but straighter veins, wandered over the front of the thorax on both sides.

The swollen legs of this woman were quite purple from innumerable clusters of small varicose veins.

Other symptoms also there were, but I pass them by as irrelevant to my present subject. It was plain that the current of the blood along the vena cava inferior was suffering impediment. That vessel was presumably compressed, perhaps rendered totally impervious, by the superjacent tumour. The existence of the tumour; the great œdema of the legs, compared with the slight amount of liquid in the cavity of the belly; the varicose state of the cutaneous veins of the legs; and, above all, the remarkable condition of the superficial veins of the abdomen:—these were the evidences. The blood from the lower extremities passed mainly by the way of the intercostal and subclavian veins to the heart. Death took place on the 19th of January, 1841. A part of the liver appeared perfectly healthy, another part contained a prodigious quantity of hydatids. When removed from the cyst which had contained them, they filled a large wash-hand basin. The sides of the inferior cava were pressed together by the tumour, and its channel was thus completely closed up for the space of three inches.

In each of these two cases, the closure of the great venous trunk was gradually effected, as the compressing tumour augmented; and time was afforded for the development of collateral supplementary channels. In both cases the superficial veins of the thorax and abdomen contributed largely to supply the growing needs of the system: but the stream of returning blood ran oppositely in the two cases; from above downwards in the first, from below upwards in the second. The direction in which the blood in the veins is moving can, of course, be always readily ascertained; and this might furnish a test, were other tokens wanting, whereby to determine whether the obstruction lay in the superior or in the inferior cava. And there is another circumstance worthy of remark, and of which the same use might be made. In the first case, the dilated veins of the thorax were tortuous, those of the abdomen direct. In the second this was reversed; the epigastric veins were singularly sinuous, the mammary veins were straight. In other words, those veins were, in each instance, contorted and winding, in which the actual course of the blood was retro-

grade. The vessels were bent and twisted as the current forced its backward way against the opposing but ineffectual barrier of the valves.

It is impossible, I think, to find more clear evidence than these interesting cases exhibit, of the power inherent in the animal body of rectifying, to a certain extent, its own accidental derangements. You cannot, under such circumstances, overlook the existence, or mistake the tendency, of a *vis medicatrix naturæ*.

LECTURE LXIV.

ASTHMA: ITS NATURE; COMPLICATIONS; EXCITING CAUSES; AND TREATMENT. DISEASES OF THE ŒSOPHAGUS: INFLAMMATION; STRICTURE; SPASM; DILATATION.

I MUST not leave the subject of thoracic diseases without saying a word or two respecting *asthma:* a complaint which might have been properly arranged among the *nervous spasmodic diseases*, in a former part of the course. But I purposely deferred speaking of it, because, though in many instances purely spasmodic, and independent of any discoverable faulty structure, it is still more often connected with organic diseases of the heart, or of the lungs; which diseases had not then been described.

I scarcely need caution you against the vulgar error of calling all kinds of difficult breathing by the name of asthma. You will be constantly meeting with persons who, labouring under some permanent embarrassment of the respiration, tell you they are asthmatic. They conceive that asthma is simply an inconvenient, and not at all a dangerous affection; and they please themselves with the notion—consumptive patients and their friends do this continually—that they are *merely* asthmatic. Asthma *is* dyspnœa, but dyspnœa is not *necessarily* asthma.

Symptoms.—Asthma may be defined as being—great difficulty of breathing; occurring in paroxysms; accompanied by a loud wheezing sound of respiration; going off, after some hours, with more or less mucous expectoration; and unattended with fever. And these paroxysms of dyspnœa are believed to depend upon a spasmodic constriction of the bronchial tubes.

To go rather more into detail: the phenomena which constitute and characterize a fit of asthma, are somewhat as follows:—The patient, if he have previously suffered under the disease, has usually some well-understood warnings that an attack is hanging over him. Loss of appetite; frequently much flatulence and eructation; languor, irritability, drowsiness, oppression, chilliness; and he goes to bed ill and uncomfortable. The dyspnœa comes on generally after midnight, about two or three o'clock in the morning; often during sleep; and the patient wakes with a sense of tightness and constriction about the chest, and an inability, as it seems to him, freely to expand it. He is obliged at once to rise up; and he sits, leaning forwards, with his knees drawn up, his elbows on his knees, and his head supported by his hands, labouring for his breath, and making such a loud wheezing noise as to be audible at a considerable distance. He experiences a strong desire or necessity for fresh air; opens the door of his room and goes out upon the staircase, or flies to an open window, even in very cold weather; and remains there, with his head out, sometimes for hours. That he can do so with impunity is a strong presumptive proof that it is the nervous system which is principally affected in these cases. His extremities at the same time are usually cold, and his countenance is distressed and haggard: while the trunk of his body may be wet with perspiration. Sometimes the face is a little flushed and turgid; but more commonly it is somewhat pale, and shrunk. The pulse is often small, feeble, and even irregular; and in many instances there is much palpitation of the heart. At other times the pulse remains undisturbed. If urine be passed, as it frequently is, at the beginning of a fit of asthma, it is copious and watery, pale, and without smell, like the urine of hysterical women. The bowels are also sometimes re-

laxed, with "something (as Dr. Forbes observes) of the impatient hurry and imperfection of spasmodic action." There may be some propensity to coughing, but the patient can hardly achieve a cough; and is so occupied with his breathing, that he can speak in an interrupted manner only, with difficulty and uneasiness. He has not, however, in general, any misgivings about the event of the attack, but looks forward with hope to the expected termination of the paroxysm.

"These symptoms often continue for many hours together; and particularly from midnight till morning is far advanced. Then, commonly, a remission takes place by degrees. The breathing becomes less laborious, and more full: so that the person can speak or cough with more ease. And if, as is usually the case, the cough brings up some mucus, the remission becomes immediately more considerable, and he falls into a much-wished-for sleep."

Paroxysms of this kind will often continue to recur for many nights in succession: remitting at length in their severity; and ceasing for a period, altogether.

During the intervals between these paroxysms, in the day-time, the patient *may be* perfectly well; but he seldom *is* so: though so great is the difference between his condition during the remissions, and in the paroxysms, that he declares, and perhaps fancies, that he is quite well. You will mostly find, however, that he is short-winded; that he does not utter many words of a sentence before he pauses to take breath; that slight bodily exertion hurries his respiration; and that he is not easy in a horizontal posture, with his head low.

Although the dyspnœa is thus intermittent, or remittent, you are not to suppose that the paroxysms recur with the regularity of those of ague. The interval is of uncertain duration; and the circumstances of the paroxysm differ in different instances. I may remark also, that when the paroxysm ceases with little or no expectoration, the case is said to be one of *dry* asthma: when the expectoration is copious, it is *humid*, or *humoral* asthma.

Pathology.—Now this, I say, is looked upon as being essentially a spasmodic affection. Upon what grounds?

Why, in the first place, the patients have a *sensation* of constriction in the chest. An old gentleman whom I saw lately, and who is subject to fits of asthma, made use of the term *cramp* when he described what he felt about the thorax; and his attacks were always accompanied or succeeded by *actual* cramp of the muscles of the calves of his legs. This is no uncommon circumstance, this coexistence of decided spasm in other parts; and it throws some light upon the nature of the disorder. Again, the rapidity with which the dyspnœa comes on, and the suddenness with which it often abates, resemble the caprice of spasm. The supervention of extreme, sometimes enormous flatulence, and the secretion of hysterical urine, mark also the nervous character of the symptoms. So likewise do the *juvantia* and the *lædentia*, as I shall presently explain further; the affection being suddenly produced by certain causes of irritation, and even by mental feelings—suddenly relieved, sometimes, by medicines which are reckoned antispasmodic. If we add to these considerations the fact that patients dead of asthma have often, on being examined, presented no vestige whatever of disease, either in the lungs or in the heart, we obtain very strong presumptive evidence, that the phenomena attending a fit of asthma are often the result of pure spasm.

But if this be so, what are the muscles thus fixed in spasmodic contraction?

You are doubtless aware that the air-tubes are encircled with a series of little fibres, or bundles of fibres. I have more than once shown you these, exaggerated by hypertrophy, in the larger bronchi. They have been traced, by Reissessen, in tubes of a very small diameter. Laennec states, that he had distinguished them in bronchial ramifications less than one line across. Now, supposing these circular fibres to be muscular, it becomes at once, and *à priori*, likely that they, no less than other muscles, should be liable to spasm. And the phenomena of asthma prove, to my mind, that they are so. Analogy would say that the fibres, thus disposed, are slender muscles, similar to those which surround the intestines and the urinary bladder: and the microscope, scrutinizing their minute texture and appearance, asserts that they are actually muscles, of the unstriped kind; like other involuntary muscles subserving the organic life. This fact—which I know has been doubted—I state upon the authority of Professor Todd and Mr. Bowman; both known to

you all, as faithful and expert observers. But a test, less fallible than the microscope, has practically settled the question. Dr. Williams has recently demonstrated, by a set of ingenious and satisfactory experiments, that the lungs and air-tubes are actually *contractile* to a very considerable degree, under electrical, chemical, and mechanical stimuli. The contractions take place steadily and deliberately; and are followed, as soon as the stimulus is withdrawn, by an equally gradual relaxation. This is very like tonic spasm. The contractions were rendered apparent by means of a bent glass tube, containing coloured liquid, and adapted to the windpipe of an animal just deprived of life. The column of liquid in the glass tube would of course be readily moveable by any contraction of the lungs and air-tubes, causing pressure of the included air against it. In one of the experiments, "on passing a galvanic current from the margin of the lungs to the insertion of the tube in the trachea, the fluid rose quickly, but gradually, nearly two inches; sunk speedily on breaking the contact; again rose upon completing it; but fell slowly when the current was continued for some seconds;" *i. e.*, when the irritability of the tissues was temporarily exhausted. Temporarily, I say, for on waiting two or three minutes between each application of the galvanism, the liquid was raised again and again for upwards of an hour; till, in fact, the organic life was extinct. Is not all this exceedingly like the behaviour of parts acknowledged to be muscular, under similar influences?

The phenomena were not occasioned by any general shrinking of all the pulmonary tissues. For when the lungs were cut across by sharp scissors, at right angles to the air-tubes, and the open sections of these tubes were galvanized, they were *seen* to contract to one half of their former diameter; and even to become smaller than that. The contraction was the most distinct in the middle-sized tubes, being about the bigness of a straw: but it was sensible enough in the trachea, which was sometimes so far reduced in dimensions, that the ends of its cartilaginous rings came together.

A foreign experimenter, M. Valentin, carries us a step nearer to the full solution of this interesting question. He found that the rings of the trachea could be made visibly and distinctly to contract, *by irritating the par vagum*.

Upon the whole, we may safely conclude that asthma is one of the spasmodic disorders of the excito-motory system of nerves. I believe, moreover, that, as in most other disorders of the same class, the spasm may be of centric, or of eccentric origin. In the eccentric form the par vagum is doubtless the afferent nerve; and the impression it conveys to the medulla oblongata is reflected, through associated motor nerves, upon the bronchial muscles. The centric variety results from a similar impression originating in the nervous centres: which respond, mysteriously, to certain feelings of the mind.

I have never had a favourable opportunity, since I became aware of the value of auscultation, of listening to the sounds of the breathing during a paroxysm of pure spasmodic asthma. But they who have enjoyed such opportunities declare that no respiratory murmur, or very little indeed, can be heard. And an attentive inspection of the outside of the chest shows, that amidst all the tugging and heaving for breath, the expansion of the thorax is very limited. The patient cannot open his lungs, as it were: and what air does get in, has a difficult and narrow passage, as the wheezing noise demonstrates. Laennec affirms, that if the patient, after holding his breath nearly as long as he can, attempts a quiet and gentle inspiration, the spasm may often be overcome as if by surprise; and, for a few seconds, the entrance of the air into the cells may be heard in a clear and even puerile sound. If this be true, it is a strong additional proof that the obstruction to the admission of air was really owing to a tonic contraction of the little muscular fibres of the bronchi and their ramifications.

The hereditary nature of asthma is perfectly consistent with the same theory. It is one of the maladies which are distinctly transmitted—the disposition to them, I mean—from parents to children. And like other spasmodic disorders, it facilitates its own return. When it has once occurred, it seldom fails to happen again and again.

But though I believe, for the reasons I have now mentioned, that asthma, in the restricted sense of that term, is purely a spasmodic affection; yet I know also that it is very frequently indeed combined with organic alterations within the thorax. These changes of structure are to be regarded as so many strongly predisposing causes. They induce a readiness to take on spasmodic action: and some of them are perhaps aggravated, or even

produced, by the fits of asthma, upon which they afterwards react injuriously. Judging from my own experience, I should say that genuine uncomplicated spasmodic asthma was *rare*.

Complications.—The organic diseases with which spasmodic asthma is often found connected, are principally emphysema of the lungs, and structural changes in the heart and great blood-vessels. It is extremely probable that the first step towards the production of the spasm, consists in some altered condition of the *circulation* through the lungs. The chilliness of the surface, and the sensation of want of air, make it likely that the blood accumulates in the lungs at those times: that there is congestion of the membrane, as well as spasm of the circular fibres. And it would seem that, in the humoral asthma, the congestion is relieved by a copious secretion of mucus; and that, with the congestion, the spasm also subsides and disappears. You will observe that very generally the paroxysms come on during the first sleep: at which time, as Dr. Alison has suggested, "the blood is perhaps in fullest quantity, its movements slow, and its congestion in internal parts easiest, because it is least solicited to the organs of sense or locomotion." But there seems to be another reason for this remarkable circumstance. Respiration is mainly an automatic act; yet it also obeys the will. During sleep this moderating influence of the will is suspended. Those changes of posture, and those voluntary alterations in the rate of breathing, which are wanted to balance and correct the commencing derangement of the pulmonary circulation, and which are prompted at once during the waking state, do not occur: until at length the derangement reaches that pitch at which it provokes spasmodic contraction, and rouses the sufferer.

This same congestion, leading to spasm, sometimes passes into a slight form of inflammation; and we have symptoms of bronchitis. And these symptoms may remain manifest even during the intervals of the paroxysms. It appears probable also that the vesicular emphysema of the lungs, which so often accompanies asthma, is increased by the paroxysms, even if it be not, in all instances, originally induced by them. In either case, there will be some abiding dyspnœa between the fits.

Many of these asthmatic patients have just healthy lung enough to breathe with, in tolerable ease and comfort, under ordinary circumstances; and dyspnœa is brought on whenever even a slight additional demand upon the respiration any how arises. Hence, as I stated before, flatulent distension of the intestines, undue repletion of the stomach by an excessive meal, the recumbent posture, all of which cause pressure against the under surface of the diaphragm, may suffice to bring on the fit. Hence also, probably, in part, its frequent occurrence in the night-time.

Liability.—Asthma is a disorder which is incident to both sexes, but it is much more common in men than in women. It is incident to all ages also; but it belongs more to adolescence, and to the middle portion of life than to its extremes. It is not, I think, a common disease prior to the age of puberty: yet instances of it do occur at an earlier period than that. I have lately seen a boy of eight or nine, who has had several well-marked attacks of pure asthma. Nor does it often *begin* to show itself in old age. Sometimes, after plaguing the subject of it for several years, it leaves him altogether. The chronic dyspnœa, with occasional irregular exacerbations, which is so frequent a disorder among old people, and which always depends upon organic disease, is not to be confounded with true asthma. It is said that asthmatic persons are exempt from phthisis· and I understand that one physician in this town, who announces that consumption is curable, maintains the doctrine of the incompatibility of phthisis and asthma; and endeavours to bring about the latter, that he may protect his clients from the former. It *may* be that persons affected with genuine asthma seldom become the victims of pulmonary consumption: but I am sure the rule is not a universal one. One of my earliest friends had from time to time, while we were schoolfellows, and long afterwards, the most exquisite fits of spasmodic asthma. At length, when he was between thirty and forty years old, they wholly ceased: whereupon he greatly congratulated himself. But they only yielded before a worse disease. He began, in a few months, to spit blood: and in a few months more he died of well-marked phthisis. Our lamented principal, the late Mr. Rose, afforded another sad example of the same sequence. And I have known two or three families in which one individual was subject to asthma, while others were scrofulous and phthisical.

Causes.—The exciting causes of the asthmatic paroxysm are manifold; and some of them curious. They seem to be reducible to two classes. 1. Particular states of the atmosphere, which irritate or offend the mucous surface of the air passages; or rather, some of the fibrillæ of the par vagum. 2. Certain subtle influences which affect in a peculiar manner the nervous system. All the known exciting causes of catarrh are therefore likely to bring on attacks of asthma in the predisposed. But there is a singular caprice in asthmatic patients in this respect. Some persons, subject to the disorder, are unable to breathe in the thick smoky air of London; require a high and clear situation; and respire easiest in "the difficult keen air of the mountain top." Others can nowhere breathe so comfortably as in low moist places: in some of the streets by the water side, in the city, for instance. The friend to whom I lately alluded lived at Newmarket; a most exposed and bleak spot. But if he left it, and attempted to sleep in a strange place, he never was certain that he should not be assailed by his well-known enemy. So that there were towns in which, after experiencing the effects of their atmosphere, he dared not sleep; and there were others in which he knew he might go to bed in security. It would have been difficult, I believe, to point out any essential difference between some of those localities. His lungs, however, formed an infallible eudiometer. Another college acquaintance of mine, much tormented by asthma, is equally sensible to these inscrutable influences. Two inns in Cambridge are named respectively the Red Lion, and the Eagle. He can sleep in one of them, and not in the other. Nay he is thus variously affected within much narrower limits. He assures me that, when in Paris, he never escapes a fit of asthma if he attempts to sleep in the back part of Meurice's Hotel, and never suffers if he sleeps in a front room. Dover street suits him; Clarges Street does not. He cannot rest in Manchester Square. This he attributes to its being built upon piles. Whether it really has such a foundation I do not know. And agencies still more slight and subtle are enough to set the springs of these seizures in motion. The mere absence of light, for instance. Laennec speaks of a man who invariably was roused from his sleep by a paroxysm of asthma, if his lamp was extinguished; or if his chamber door was shut. The consciousness that the customary preventive remedy was not at hand has, apparently, brought on a fit.

There are many persons who never fail to become asthmatic if they inhale certain effluvia. Particles of ipecacuan floating in the atmosphere, or (what is perhaps the same thing) its mere odour, are insupportable to many. They are thrown into a paroxysm of dyspnœa if they enter a laboratory where that drug is under preparation. I think I mentioned before a certain laboratory man at St. Bartholomew's Hospital who possessed this peculiar and inopportune susceptibility: he was obliged to fly the place whenever ipecacuan was about. Most persons, probably, who have had much experience in druggists' shops, are acquainted with similar examples: so that the influence of ipecacuan in exciting fits of difficult breathing, resembling asthma, is undoubted, and common to *many* constitutions. We might as well speak of ipecacuan asthma, as of hay asthma, which is a precisely analogous affection. Dr. Marshall Hall calls attention to the familiar but interesting fact, that the same drug, ipecacuan, acting upon the *gastric* branches of the par vagum, excites the reflex spasmodic act of *vomiting*.

Treatment.—I have said, that the relief afforded by antispasmodic remedies affords presumptive evidence of the spasmodic nature of these attacks. If asthma supervene upon manifest bronchitis, or if there be any signs of congestion about the head, it may be prudent to abstract blood: but this measure will not in general be requisite; and when not requisite, it should be avoided: for whatever tends to debilitate the patient, or to lower his vital powers, tends, at the same time, to augment his susceptibility to the exciting causes of the disease. The dyspnœa may frequently be moderated or altogether assuaged by some form of narcotic. Now opium is the narcotic to which we most trust for the mitigation of spasm in general: and opium *is* of vast service in paroxysms of asthma. But there is also another of the vegetable narcotic substances which has obtained an especial repute for its effect in quieting the difficult breathing in these cases; and that is *stramonium*. This herb, the Datura stramonium, and another species of the same genus, the Datura ferox, had long been employed in India as a remedy for asthma. And when it was introduced into this country, about the beginning of the present century, it was cried up as a

specific; and everybody who called himself asthmatic began to smoke stramonium: for that is the way in which it has been chiefly employed. The leaves and stalks are cut and put into a pipe, and smoked like tobacco. The smoke descends, of course, into the lungs: and when the saliva is swallowed, the remedy is introduced into the system in that way also.

Stramonium thus used, sometimes fails altogether: sometimes calms the paroxysm like a charm. The late Dr. Babington told me of a patient of his who had been grievously harassed for a series of years, by asthma, but who declared to him, after he had made a fair trial of stramonium, that he no longer "cared a fig" for his asthma, which he could always stop in a moment. So a Mr. Sills, in a collection of communications relative to the Datura stramonium, published in London in 1811, states, that he had been a great sufferer from asthma: that the fits usually continued, with short interruptions, from thirty-six hours to three days and nights successively; during which time, he had often, in the seeming agonies of death, given himself over, and even wished for that termination of his miseries. But having at length discovered the virtues of stramonium, he uses this strong language:—"In truth, the asthma is destroyed. I never experienced any ill effects whatever from the use of the remedy; and I would rather be without life than without stramonium."

This, then, is an expedient which it will always be well to suggest, for relieving the urgent distress of the paroxysm of asthma. But most patients subject to that complaint, try it of their own accord. We have still to learn why it is so efficacious in some cases, and so entirely useless, or even hurtful, in others. This probably depends something on the presence or absence of organic disease in the lungs or heart: but more accurate observations are wanted on this point.

Some of the animals upon whose lungs Dr. Williams performed his experiments, had been killed by poison. In two instances stramonium was the poison employed; and it is interesting to know that scarcely any contraction of the air-tubes could be produced by the galvanic apparatus. The trachea, at the same time, was lax. It is requisite to notice the condition of the windpipe in these experiments; for the same want of contractility would be *apparent*, supposing the parts to be already in a state of tonic spasm, from the operation of the poison. This seems to have been the case when conium was used: the fluid in the glass tube indicated scarcely any compression of the air contained in the lungs; but then the windpipe was so far contracted, that the extremities of its rings met. There was but little contractility, and a lax trachea, after death by belladonna: and after death by the meconate of morphia. The contractility was slight when life had been destroyed by strychnia; but the condition of the trachea is not reported.

Experiments of this kind appear to be well worthy of careful repetition.

I have found a mixture of opium and sulphuric ether of great service in tranquillizing the breathing in asthma. And in one case, which was under my care for some time, I made comparative observations respecting opium and stramonium. Both gave much relief; but stramonium the most, and the most certainly, *if it were applied in time*. This patient was in the habit of being roused from sleep by the supervention of the paroxysm: and if he had the means of lighting his pipe *instantly*, he could stave the fit off. But when once it had attained its full intensity, he was *unable* to smoke. Under such circumstances, he would swallow the morphia and ether; and the effect of this depended also in a great measure upon the period at which it was taken. It would stop a commencing paroxysm; but had little influence over one that was fully formed.

It has been suggested, as one clue towards determining the particular kind of case to which the stramonium is applicable, that it succeeds if it produces *expectoration;* and not otherwise. But I doubt about this. The relief is sometimes too sudden to admit of its being so explained. Dr. Forbes quotes the following passage of a letter from an old and intelligent asthmatic to himself. "Smoking tobacco or stramonium is sure to give relief, if it produces expectoration; and it will generally do so if, the moment I awake (*i. e.*, in the incipient paroxysm), I begin to smoke, and continue to do so for three or four hours. Smoking, I am able to say, after fifteen years' practice, and suffering as much as mortal can suffer and not die, is the best remedy for asthma *if it can be relieved by expectoration*. I have been in the hands of all the doctors of the place for fifteen years; and still I say, *smoke*."

The lobelia inflata has of late been much lauded for its beneficial operation upon dyspnœa of all kinds, and upon asthma in particular. I believe its virtues have been overrated. It sometimes, like stramonium, has an almost magical effect; but frequently it fails to do the smallest good; and I know that sober practitioners who have employed it more than I have, have thought that it may occasion dangerous symptoms. Of my own knowledge, I have nothing worth communicating to you of this drug.

Strong coffee is a common domestic remedy for asthma. The friend and schoolfellow already mentioned used to take it in considerable quantities, and, as he assured me, with very great benefit. It is a safe, and simple, and grateful remedy, and has numerous testimonies from medical practitioners in its favour. But it is much less sure than the stronger narcotics.

Ipecacuanha, which is so frequently the *cause*, has also been recommended for the *cure* of asthma; and a host of drugs besides, with which I have no intention to weary you. Among the rest, the application of galvanism was once in great vogue. In the only patient who ever tried it under my own eye,—and he insisted upon being galvanized when his fits were quite absent,—the galvanism brought one on immediately.

If we can shorten or mitigate the paroxysms we do our patient a most essential service, and spare him a great deal of suffering. And during the intervals between the fits, we must endeavour to prevent their recurrence.

For this purpose, I can only just hint at the principle on which we should go. In the simple form of the complaint, when it is apparently uncomplicated with any organic disease, we must caution the patient against whatever has a tendency to disturb the general health. He must be temperate in all things; he must pay attention to the regulation of his digestive organs; he must discover what kind of situation suits him best; and avoid those which experience has shown to disagree with him. And if any one remedial measure be likely to fortify him against his malady, I believe that measure will oftenest be found in the employment of the shower-bath, in the way which I formerly recommended.

If the asthma occurs in connection with any obvious pulmonary or cardiac disease, we must, in addition to the means I have now been adverting to, apply ourselves to the mitigation of such superadded disease. And in respect to this I have nothing more to offer.

Diseases of the œsophagus.—I go next to the morbid conditions of the *œsophagus*, so far as they concern the physician; and these morbid conditions are not many. The œsophagus lies partly in the chest, and partly in the belly, and therefore may very properly close the subject of thoracic diseases, and introduce those of the abdomen.

The œsophagus is less liable to disease than any other part perhaps of the alimentary canal. It differs in structure somewhat (as you know) from all other parts. Its mucous membrane is provided with a thick epithelium, which extends a little beyond the cardiac orifice of the stomach. Beneath lies a dense web of cellular tissue, and two layers of muscular fibres; the one layer being disposed circularly around the tube—the other longitudinally, in the direction of its axis. In some cases there are pouches found in the sides of the œsophagus, formed apparently by a kind of hernia of the mucous membrane, between the separated fibres of the muscular coat. These are not common, however, and scarcely worth mentioning, except that their existence has been supposed to have a possible connection with a curious phenomenon, peculiar to some persons; the power, viz., of *ruminating;* the power of bringing into the mouth again, by a voluntary effort, food which has been for some time swallowed, as cows, and the rest of the *ruminantia* do. There are but few individuals of the human species who possess this faculty; there are but few who have appendices to their œsophagus. Whether the phenomenon in question belongs to these last few has never been determined; but as the possible connection of the two circumstances has been suggested, it is well for you to be aware of it, that you may refute or verify the notion in case you ever have the fortune to examine the dead body of a person who had the power of ruminating.

The covering of cuticle protects the œsophagus from the injurious influence of matters passing over it, which might otherwise be hurtful. The morbid state for which we are most often consulted is stricture, *actual* or *spasmodic*. I believe that the œsophagus is very little subject to inflammation, except from mechanical violence or chemical injury.

I have seen a few cases, however, in which I inferred a spontaneous inflammatory condition of the tube, from the symptoms complained of; which were a sense of heat and pricking exactly in the course of the œsophagus, and felt between the shoulders, and precisely in the part (the patients said) where a potato, swallowed too hot, gives pain while it is descending into the stomach. With these symptoms there was some degree of dysphagia, not explained by any thing visible in the throat or pharynx; and some degree of fever. In all the instances of this kind that I have met with, the symptoms have yielded in a few days to abstinence, purgatives, and the application of leeches along the track of the œsophagus.

This part of the alimentary canal often suffers severe injury from the deglutition of certain poisons, especially the corrosive poisons; the strong mineral acids, for example; or the caustic alkalies. We have, in the museum of the College, some very interesting specimens of the effects of these destructive substances. Sometimes, when the quantity of the poison has been small, and its transit rapid, the cuticular lining alone of the gullet is destroyed. It is shrivelled up, broken into fragments, abraded. At other times, the subjacent textures are affected, and ulceration takes place, which at length heals, and leaves a permanent, and generally progressive constriction of the œsophagus: and sometimes the whole of the internal membranes slough away, and are discharged in one continuous tube, from the mouth; and yet the patient survives for some time. My colleague, Dr. Wilson, had a case of that kind. The patient, a young woman, swallowed about a table-spoonful of oil of vitriol. A week afterwards, she brought up, during a paroxysm of choking cough, a complete cast of the gullet, with ragged ends; or rather the gullet itself. Some of the muscular fibres of the œsophagus were plainly visible on the outside of this tubular slough, in its recent state. She lived eleven months afterwards, swallowing all that time with difficulty and pain, and subsisting on slops and soft food. Yet at one period she certainly gained flesh. After her death, the channel, as it remained after the injury, was taken out and examined. They are both before you;—the original slough, and the ultimate gullet. The latter was formed by a surface which consisted of an irregular cicatrix. The tube was contracted considerably in the lower two-thirds of its course.

Stricture.—When patients have suffered inflammation and ulceration of the œsophagus from these causes, and do not perish at the time, they are very liable indeed to have their existence abridged by the occurrence of stricture of the gullet, which goes on slowly increasing, until no food can pass it, and then, of course, the patient dies of starvation. I show you here an œsophagus taken from a man whose case I had an opportunity of observing from the beginning. He was under the care of Dr. Macmichael, in the Middlesex Hospital. He was brought there in November, 1830, having swallowed, half an hour before, a solution of the impure carbonate of potass, which had been made for the purpose of cleaning paint, and which he had mistaken for beer. Not more than a table-spoonful passed the fauces, and probably none of the poison reached the stomach. He suffered severely, and was in considerable peril for several days, in consequence of inflammation of the fauces and epiglottis; but this gradully subsided, and he went out apparently well. From what I knew of the result of such cases, I ventured, however, to predict that this man would, sooner or later, come back with stricture of the œsophagus. He had always pointed out a spot about half way down the sternum, where he said the oil of tartar had caused him extreme pain, at the very first, and below which he had not felt it.

Accordingly, I was not surprised to see the poor fellow at the hospital in February, 1834, attending as an out-patient. He came there, he told me, because in eating some soup, he had accidentally swallowed, without chewing it, a piece of carrot, which lodged in its way down, and which it became necessary to push onwards into the stomach by means of a probang. Morsels of food had stuck in the same spot before; and it was the very spot where he felt the effects of the caustic at the time of the accident. He looked tolerably stout and healthy, but said that, since swallowing the potass, he had never been the man he was before.

He continued to make his appearance, from time to time, at the hospital, with similar symptoms, till the 5th of last December (1836) when he was brought there insensible, and evidently dying. We could obtain no satisfactory account of his recent symptoms. He had the mark of a blister, however, on his left side; and upon closely examining him

it was plain that that side was full of fluid. It was perfectly motionless in respiration; it was palpably larger than the right side; it yielded everywhere a dull sound on percussion; and no vesicular breathing whatever could be heard there by the ear. The respiration on the right side was puerile; and the beating of his heart, with a systolic bellows sound, was audible on the right of the sternum.

Although I was certain that the left pleura was full of liquid of some kind, I did not have the thorax punctured: because, in the first place, he was manifestly *in articulo mortis*, and I thought that his death, which was certain, might be attributed to the operation; and secondly, because he was not dying of *suffocation*. His breathing was not laborious or much distressed; but he was dying of coma, and his extremities were already cold, and his pulse was fluttering. I conjectured that an ulcer of the œsophagus had made its way into the pleura, and caused inflammmation there. But my conjecture was wrong.

I will mention the main particulars of the examination of the dead body, because the case was, in several respects, an interesting one.

There was a considerable quantity of serous fluid in the meshes of the pia mater, beneath the arachnoid; and there was some liquid of the same kind in the lateral ventricles. No other diseased condition could be detected in the brain. The effusion was sufficient, supposing it to have come on suddenly, to explain the coma.

I had the ribs sawed away on the right side, leaving their cartilages attached to the sternum; and then we saw plainly that the heart and mediastinum were thrust over, about four inches by measurement, beyond the mesial line on the right side. The body was on its back. It was easy to perceive how a pleura thus full of fluid must oppress the lung of the other side, especially when assisted by the force of gravity. The left cavity was distended by a grayish coloured and most offensive fluid, of the consistence of gruel; the pleura pulmonalis was covered by a layer of coagulable lymph; and the lung was flattened against the vertebral column. We could not discover any communication between the cavity of the pleura and the œsophagus or air-tubes.

About the middle part of the œsophagus there was a distinct stricture occupying about half an inch of the tube. Through this portion it was impossible to push one's little finger; which elsewhere found a loose and ready passage.

In this case, the man did not die of the stricture; but he would have done so, had not another disease carried him off. I do not know why the constriction, after it has once taken place, should go on continually increasing; yet it seems to be so. In his *Surgical Observations* Sir Charles Bell mentions three cases like that just described. In one of them, where soap lees had been the substance swallowed, death took place by starvation from stricture of the gullet, twenty years afterwards; and Sir C. Bell had no doubt that the stricture originated in the chemical injury inflicted by the soap lees.

When the symptoms of stricture come on in these cases, physic can do almost nothing. Surgeons pass bougies into the gullet, and attempt to dilate the strictured portion, or to prevent any further narrowing. But this expedient is usually of temporary benefit only; and the patient dies at last of inanition. His miserable existence may perhaps be protracted a little, by injecting nutritive enemata into the rectum. Sometimes the œsophagus ulcerates through, and a communication is formed between it and the neighbouring parts.

Spasm.—But the œsophagus, like the urethra, and like the bronchial tubes,—like every canal, indeed, in the living body, that is surrounded by circular muscular fibres,—is liable to temporary constriction and closure, by the spasmodic action of its own muscles; and this affection is, of course, a far less formidable one than the last.

Patients who are subject to spasmodic stricture of the œsophagus experience occasionally, in some point or other of that tube, a sensation as if there were a knot; or sometimes a feeling as if some solid substance was ascending from the stomach towards the pharynx. If they happen to be then engaged in eating, the morsels of food, after mastication, readily pass the pharynx: but, at a certain distance down the gullet, they stop, and occasion pain which is felt between the shoulders, or distinctly in the passage itself. Great anxiety and distress accompany this stoppage: and the food is often ejected by a reversed action of the œsophagus.

The symptoms, in fact, are identical with those which result from permanent stricture of the gullet, except that *they* are *not* permanent. When the stricture is organic and abiding, the symptoms occur during or after every meal. When it is simply spasmodic, they come and go, capriciously, we often cannot conjecture why or wherefore; after the fashion of other spasmodic ailments.

Spasmodic stricture may be independent of any disease of structure in any part of the body; but it is of some importance to be aware that it also may be symptomatic of very serious organic changes. Mr. Mayo relates the case of a young man "who had difficulty of swallowing; he could get down liquid food only; and that not without an effort. A bougie being introduced, some resistance was found at the upper opening of the œsophagus, but it yielded: the resistance was spasmodic, and depended upon neighbouring irritation caused by ulceration in the interior of the larynx. The use of the bougie for a few days, with appropriate remedies to the larynx, removed the dysphagia."

The purely spasmodic cases occur principally in persons of a movable constitution; in young women whose uterine functions are deranged, and who are liable to hysteria. The remedies for hysteria will prove remedies for the spasm of the œsophagus also. And whatever is calculated to excite ordinary hysterical symptoms, whatever tends to render the system weak and irritable, will tend to aggravate the œsophageal stricture. I alluded to such cases in a former lecture. I give you another, related by Sir Benjamin Brodie. A lady consulted him, unable to swallow the smallest morsel of solid food; and swallowing liquids not without great difficulty. The symptoms had been coming on upwards of three years. A full-sized œsophagus bougie being introduced, entered the stomach without meeting the slightest impediment. This lady's face was pale and bleached: her feet were œdematous. She had long been labouring under internal piles, from which repeated discharges of blood had taken place. Under the use of remedies which relieved the piles and the bleeding, the difficulty of swallowing went away.

Dilatation.—It is a singular, and it might, if more frequent, be a puzzling circumstance, that very nearly the same symptoms which occur when the œsophagus is permanently or temporarily constricted, happen also sometimes under a totally opposite condition of that tube; I mean its dilatation into a large, inelastic, inert bag. One remarkable example of this I witnessed, in a woman whom I attended in conjunction with Mr. Mayo, some years ago, in the Middlesex Hospital. The case has been fully described by Mr. Mayo, in the third volume of the *Medical Gazette;* and more briefly in his *Outlines of Pathology.* She was thirty-three years old. She was brought to the hospital in a state of extreme feebleness and emaciation. They who brought her said that for the preceding month she appeared to keep down nothing. What she took as food seemed to her to stop in the gullet; and, after a few minutes, it returned. A large œsophagus bougie passed readily into the stomach. She could swallow liquids more easily than solid food. When she took a small quantity, it did not feel to her as if it reached the stomach; and in three or four minutes it was invariably rejected. The vomiting was not preceded by nausea, although in its progress it had the appearance of ordinary retching. She craved for food and drink, and seemed literally starving. The complaint had begun ten years before, during her pregnancy, and had gradually got worse. The belly was so shrunk that the umbilicus was not more than an inch distant from the spine. There was no enlargement nor hardness about the stomach; no particular tenderness on pressure of the epigastrium; nor any uneasiness there. She died, utterly attenuated, sixteen days after her admission.

The stomach was found small, and contracted at its middle to the breadth of an inch and a half. The upper part of the duodenum was but half the ordinary size of the ileum. The œsophagus I show you, turned inside out.

It is enlarged to an extraordinary degree of dilatation, as you perceive. It was healthy, and of its natural size, at and near each extremity. Intermediately the lining tunic was thickened and opaque, with numerous depressions in it. The muscular fibres, which appeared to have multiplied with the expansion of the canal, were of their natural colour and thickness.

Here is another preparation: a dilated œsophagus with cancerous degeneration of the cardiac orifice of the stomach. I do not know its history; but the mechanism of such dilatation is intelligible enough. The food, unable to pass out of the gullet into the

stomach—or passing slowly and uncertainly—the tube behind it is habitually distended, and loses at length its proper contractility. I saw last summer, in consultation with Mr. Mayo, an old gentleman, of seventy, who for two years, had experienced difficulty in getting food into his stomach. He would eat a few mouthfuls very well; and, then, of a sudden, the next mouthful, after passing the pharynx, would stop just short of the stomach; and a sensation of swelling would arise in the lower and middle part of the œsophagus; and presently up the mouthful would come again. Sometimes, by waiting quietly a little while, the morsel would go on; sometimes he could wash it forwards by a gulp or two of drink; but if once the food got fairly into the stomach he had no further trouble with it. This gentleman had no discoverable disease of the heart or lungs. He gradually grew worse. At last he began to vomit grumous matters, resembling coffee-grounds, and soon died. He was at some little distance from London at the time, and the body was not (I believe) examined. I have no doubt that he had malignant disease of the cardia; and I think it probable that his œsophagus was dilated. I had a female patient about two years ago in the hospital with very similar symptoms; and her stomach was found to be full of cancerous disorganization. The state of her gullet is not recorded. We are apt, in such cases, to satisfy ourselves with ascertaining the gastric disease, without carefully examining that part of the alimentary canal which lies above.

For maladies like these medicine has no cure. Opiates may give comfort, and promote the euthanasia: and that is all.

LECTURE LXV.

Diseases of the Abdomen: sometimes difficult to identify. Method of investigating these diseases; by the eye, the hand, the ear. Inflammation of the peritoneum: its symptoms; and causes. Puerperal Peritonitis. Peritonitis from Perforation.

I am about to consider the *diseases of the abdomen*. The organs contained in this cavity of the body are not vital organs in the same sense in which the brain, the heart, and the lungs, are vital. That is to say, the *functions* of the abdominal viscera will bear to be suspended for some considerable time, without the extinguishment of life. But these parts are subject to numerous diseases, some of which are apt to be quickly fatal, and others carry with them a vast amount, and very severe kinds, of suffering.

Diagnosis.—The parietes of the fore part of the belly being soft and flexible, you might naturally suppose that the physical morbid conditions of the organs they cover would submit themselves to an easy diagnosis; that the sense of touch, exercised through these yielding walls, would detect alterations of bulk, of form, or of place, in the subjacent viscera, with much facility and exactness. But the truth is, that since the discovery of the method of auscultation, the diseases of the abdomen are much more hard to discriminate than the diseases of the thorax. The reason of this is to be found in the number and complexity of the parts contained in the abdomen; the loose manner in which some of them are packed; and the consequent readiness with which they pass out of their proper and natural situations. It is necessary that I should say a few words, but I shall not detain you long, respecting the *mode of examining* the abdomen, with the purpose of investigating its diseases.

Mode of investigating.—In the description of symptoms, we are often obliged to speak of particular portions of the abdomen: and it will be of future convenience to us if we make ourselves acquainted, at starting, with such a superficial map, marking out the topography of the belly, as I exhibited to you some time since, in reference to the chest. Draw a horizontal line round the body, touching the extremity of the ensiform cartilage; and this will form the superior boundary of the abdomen, thus roughly defined for prac-

tical purposes. Draw another such line round the body, horizontally, touching the lower edge of the last false ribs: and a third touching the crest of each ilium. We then have three horizontal *zones* formed. These must be further divided by vertical lines: one on each side from the anterior spinous process of the ilium perpendicularly upwards. Each zone will thus be subdivided into three regions. The middle region of the upper zone is the *epigastric* region: on either side are the *hypochondria*. The middle region of the middle zone is the *umbilical* region; the *iliac* regions or the *flanks* lie to the right and left of it. The *hypogastric* region is the middle region of the lowermost zone; and the *inguinal* regions are contiguous to it. This is all the division which is necessary.

Now independently of the *general* signs of diseases that have their seat in the abdomen, we are greatly assisted in many cases by the *physical* signs. I shall take a very brief survey of the modes by which these physical signs are collected. They are derived from the exercise of the three senses of sight, of touch, and of hearing.

The sense of sight supplies, occasionally, very valuable information; and in all serious and equivocal cases we must not dispense with its use. We are not, indeed, to make an ocular inspection of the *naked* abdomen *unnecessarily:* and I hold it superfluous to admonish you that when we *do* avail ourselves of that mode of investigation, especially in the case of females, we are bound to do so with the most careful attention not to offend the patient's delicacy. We may sometimes ascertain all that is required concerning the *movements*, *size*, and *shape* of the abdomen, without removing the under garments.

This rule applies, indeed, to all parts of the body that are ordinarily covered by the dress.

I was lately consulted by a lady, who told me she had, on the rear of her person, a painful boil. She thought any physician ought to be competent to prescribe for a boil, without wanting to see it. But she seemed very ill, and her sister told me that the boil had lasted a fortnight, and was a very large one; so that I was obliged to press for an inspection. And I found—a boil sure enough, but of that gigantic and formidable species which we call *carbuncle*.

M. Rostan relates a case still more in point. Going round the wards of his hospital, he came to an old woman, who was complaining of severe pain in the abdomen, towards the left iliac region. Her face was flushed, her skin hot, her pulse strong and frequent, her tongue dry; and she was very thirsty. The abdominal pain was exasperated by pressure, and by the movements of the patient. Upon these data, Rostan founded his diagnosis. He concluded that the case was one of acute abdominal inflammation; and he prescribed accordingly; and with befitting energy. One of the pupils, however, lingered behind him: and having removed the woman's chemise, in order to examine the seat of the pain, he discovered that all the symptoms proceeded in reality from a very harmless, though troublesome, disorder, *herpes zoster;* what is vulgarly called *the shingles*.

Vestis adempta est,
Quâ positâ, nudo patuit cum corpore crimen.

In the second place, we gather very important intelligence by the sense of *touch*. We learn the existence and the size of *tumours;* we approximate to a knowledge of their nature, whether it be solid or fluid; we determine whether they are movable or fixed, painful or indolent, pulsating or not. We ascertain whether the surface be hot or cold. In order to make palpation most effectual, the patient should be placed in the most favourable posture for its performance; *i. e.*, he should lie on his back, with his head a little raised, and his knees up. In this position, the abdominal muscles are relaxed and unstrung: and the patient is to be cautioned not to do any thing which may make them tense. Sometimes, in spite of this caution, and in spite, probably, of the patient's endeavours to obey it, the recti muscles remain so tightly contracted as to prevent any satisfactory examination of the parts beneath them. The very occurrence of this instinctive striving against the pressure of our hand may be taken as a ground of suspicion that those parts are not in a healthy state. We must take care, when the muscles are thus obstinately rigid, not to mistake the swelling central portions of the recti, or their well-defined edges, for tumours, or for indications of an enlarged stomach or liver. By a peculiar management of the palpation, we often satisfy ourselves at once of the presence of liquid in the

cavity of the peritoneum, or in a cyst: we obtain that sensation which we call *fluctuation*.

The exploration by the sense of touch is very much aided—often confirmed, sometimes corrected—by that which addresses itself to the sense of hearing. Sometimes we listen to the natural sounds through a stethoscope; and we may thus decide the important question, whether a pulsating tumour be or be not an aneurism; or the question, sometimes scarcely less important, whether a different kind of tumour encloses another living being or not. But, for the most part, our information respecting the maladies of the abdomen, collected by the sense of hearing, is obtained by listening to sounds which we ourselves produce; in one word, by *percussion:* and mediate percussion, percussion performed through the finger as a ready pleximeter, is *particularly* applicable to the disorders of the abdomen. By this expedient we can tell whereabouts the intestines lie; whether the parts beneath the place percussed be hollow and filled with air, or solid; or, though naturally hollow, distended with liquid. By making the patient change his posture, we are enabled often, through the aid of percussion, to trace fluid effusions hither and thither, when they have changed their relative situation, by reason of the force of gravity; and then we know that they occupy the cavity of the peritoneum. All these points I pass over cursorily, because I must advert to them again when speaking of particular diseases. And I shall proceed, on that account, without further delay, to the consideration of those special diseases.

Consulting your convenience, and my own, rather than any scientific order, I shall take, in succession, the several parts and organs contained in the cavity of the belly, and inquire separately into their diseases; inflammatory, organic, and functional. And I begin with the *peritoneum:* the great serous sac which lines and constitutes the cavity of the abdomen, and in which most of its viscera are wholly or partially folded.

Peritonitis.—Like the serous membranes in general, the peritoneum is very *ready* to take on inflammation, upon the operation of certain exciting causes. Acute inflammation, beginning in one spot, is almost sure to transfer itself to any other spot that happens to lie in contact with the first; and is very apt to extend itself rapidly to the whole membrane. The inflammation tends to the effusion of serum, and of coagulable lymph; it is of the adhesive kind: and its effects are those of distending the peritoneal cavity with fluid—or of gluing its opposite surfaces together so as to obliterate that cavity—or of forming *partial* attachments. In all these respects, the analogy between inflammation of the peritoneum and inflammation of the serous membranes of the thorax—the pleura, and the pericardium—is perfect: and, therefore, these are points which I shall not dwell upon, except where specific differences arise, from original diversities of structure or function in the parts affected. I may observe at once, that the morbid conditions which are apt to remain *after* peritonitis, are sometimes, like those which follow pericarditis, *inceptive of further disease;* sometimes, like those of the pleuræ, *final*, and limited to their *immediate* influence upon the health and comfort of the individual: or even *protective* against some worse evil.

Symptoms.—Acute inflammation of the peritoneum is characterized by pain in the abdomen, increased on pressure, and attended with fever. But as these symptoms are common to almost all the inflammatory conditions of the parts contained in the abdomen, we must look for more distinctive circumstances. Cullen defines the disease in this manner: "Pyrexia: dolor abdominis, corpore erecto auctus, absque propriis aliaram phlegmasiarum abdominalium signis." He concludes that it is the peritoneum simply that is inflamed, when the specific symptoms that indicate inflammation of particular organs are wanting. It is not inflammation of the liver, for there is no pain of the right hypochondrium in particular, increased by lying on either side: no pain of shoulder, no jaundice, no vomiting perhaps: neither is it inflammation of the bowels or stomach, for there is no disturbed function of the alimentary canal to denote such inflammation.

The pain, Cullen says, is increased if the patient sits up. He might have added, that it is increased also by drawing a long breath, by coughing, sneezing, or straining, and by pressure made with the hand upon the belly. All these circumstances resolve themselves into the same obvious principle; viz., that of pressure aggravating the pain of an inflamed membrane. The erect posture throws the weight of the viscera upon the peritoneum, and

tends to stretch parts of it. The pain occasioned by pressure is often excessive; the patient cannot even bear the weight of the bed-clothes. Though the pain is, *at first*, sometimes confined to particular spots, yet it generally soon extends over the whole abdomen; and this is a circumstance of some importance as respects the diagnosis. But *before* the inflammation has become universal, while it is yet restricted to particular spots, the pain is often much increased by pressure made on *other* parts of the abdomen. In truth, in a shut sac of that kind you cannot compress any one part without exercising pressure indirectly upon every other part. The patient cannot sit up, nor, usually, lie on his side; but remains always upon his back: in which position you will perceive that the pressure made by the viscera upon the peritoneum is a *minimum:* is the least possible. He draws up his legs too. And he lies *still:* for *movements* cause pressure, and therefore pain. The descent of the diaphragm in inspiration presses also upon the membrane; and the patient not only complains of the pain thus produced, but in order to avoid it gets into a way of breathing by means of his ribs only. So that upon *inspection* of the abdomen, it is perceived that, instead of rising and sinking alternately in respiration, it remains motionless. The phenomenon of *thoracic* respiration is a symptom of peritonitis. The breathing is necessarily shallow in these cases, and less air is admitted at each movement of respiration, and therefore the number of those movements is increased: the breathing is quick as well as shallow: there are perhaps forty, or even sixty respirations executed in a minute, instead of eighteen or twenty. When we find a person lying only on his back, with his knees up, breathing in this manner, and complaining of tenderness of the belly on pressure, and feverish withal, we may be tolerably sure (unless that person be an hysterical girl) that the peritoneum is inflamed, whatever else may be the matter.

The pain in peritonitis is generally sharp, cutting, or pricking in its character. And independently of any pressure made from without, or caused by any change of posture, this pain is apt to be much aggravated at intervals. This, when the inflammation is general, is sometimes owing to the passage of flatus along the bowel, partially distending it, and stretching the inflamed membrane; so that here, also, it is really *pressure* which augments the pain.

When you explore the abdomen by pressure, take care not to make the examination *unnecessarily* a source of pain. Press first gently, with the open flat hand; and keep your eyes on the patient's face at the same time. You will perceive by the expression of his features, whether you are hurting him; even before he takes to verbal complaining.

Acute peritonitis generally sets in with well-marked symptoms: sharp rigors, and high fever, with a hard and strong pulse, which very soon becomes frequent, and often becomes feeble, and is sometimes small from the very first. After the disease has continued for a certain time, it is attended with tension and swelling of the belly. The tension and swelling are tympanitic in the earlier stages. You learn this with certainty by mediate percussion. As the disease advances, the enlargement is sometimes occasioned, in part at least, by the effusion of serum: of the presence of which infallible indications may be obtained by the joint employment of the finger and the ear; by palpation and auscultation; and by noticing the difference, as to the results of percussion, caused by alterations of posture.

When the disease is advancing towards a fatal termination, the abdomen often becomes greatly distended; the pulse is exceedingly frequent and feeble; the countenance (which in all the stages of the disorder is expressive of anxiety) becomes pinched and ghastly; cold sweats ensue; and the patient dies at length by asthenia: death beginning at the heart. The mind is often clear to the very last.

Such is the ordinary course of peritonitis. But other symptoms, which I have not mentioned, do sometimes accompany it; arising out of the peculiar circumstances of different cases. Thus sickness and vomiting occur very frequently: and these symptoms are supposed to denote that the peritoneal covering of the stomach is especially implicated: but I question whether this is always a correct inference. When strangury happens, which is not uncommon, that part of the membrane which is reflected over a portion of the bladder is probably involved in the mischief. Inflammation of that part of the peritoneum which lies in the immediate vicinity of the kidneys, may cause, Dr. Abercrombie thinks, suppression of urine.

Causes.—Peritonitis is apt to arise under the influence of cold, like other internal inflammations: especially when cold combined with moisture is applied, under certain conditions, to the surface of the body. It is occasionally produced by mechanical injuries inflicted upon the abdomen. It often prevails epidemically, and produces great mortality, among parturient women: and there is ground for believing that this form of the disorder is propagable, and often propagated, by contagion. Besides this, a very terrible kind of peritonitis is a frequent result of the extravasation of the contents of the alimentary canal, or of urine, or of bile, into the cavity of the membrane; through apertures that are sometimes made by external violence, but more often are the consequences of the progress of previously-existing disease.

I shall make a few observations in respect to one or two of these points, and but a few.

Puerperal peritonitis.—That awful disorder, *puerperal fever*, is more frequently accompanied with *inflammation of the peritoneum*, than with any other inflammation. This variety of peritonitis necessarily engages the attention of the accoucheur; and it doubtless is more fully considered in the lectures of the Professor of Midwifery than I propose to consider it. Indeed, if you would understand puerperal fever as a whole; its shifting aspects, its single source, and its appropriate management; you must study Dr. Ferguson's masterly and conclusive essay on that subject.

Of forty-four fatal cases of well-marked puerperal fever which fell under the observation of Dr. Robert Lee, and in which the bodies were carefully examined, the *peritoneum and uterine appendages* were found inflamed in thirty-two: *i. e.*, in eight cases out of every eleven. The inflammation commences, no doubt, in the uterine portion of the membrane, and spreads thence over the larger part of its surface. Now this peritoneal inflammation, occurring in women after childbirth, may be accidental and sporadic; or it may prevail in a district epidemically. And a most dreadful and deadly affection it may then become. In either case, the peritonitis may commence a few days, or even a few hours, after parturition. The pain generally begins low in the abdomen, in the situation of the uterus; which may be felt through the abdominal parietes, and is tender on pressure: but soon a universal swelling takes place, and the womb can no longer be distinguished. Cases of this kind arise sometimes, apparently, from cold; and exhibit no peculiar feature. It is just what we might expect when a female, in a state of weakness and irritability, happens to be exposed to the exciting causes of inflammation. In such a state, a less degree of the exciting cause would be sufficient to produce the inflammation: and probably a less degree of inflammation may prove fatal.

But when peritonitis is frequent among women after child-birth in a particular neighbourhood, or in a lying-in hospital, it is marked by greater depression of the vital powers, and runs a more irregular course. The nervous system suffers, the sensorium is apt to become affected, and the complaint assumes rather the character of continued fever than of simple inflammation of the peritoneum. And no wonder; since this variety of peritonitis forms part of a disease which, like continued fever, is a *general* disease, and results from contamination of the blood. This Dr. Ferguson has clearly established. The contamination may originate in the body of the patient herself: the noxious material being supplied by putrid coagula, or portions of placenta, remaining in the uterus; or some of the products of inflammation may enter the blood-vessels, and constitute the poison. And this it may be difficult, or impossible, to prevent. But, on the other hand, the contamination may arise in the way of *contagion:* this horrible malady may be communicated from one lying-in woman to another by the intervention of a *third person;* and doubtless it is so carried and propagated, in many instances, by midwives and accoucheurs. Now this source of the disorder may be obviated; and therefore it is of the utmost importance that it should be clearly recognized, in order that it may be carefully provided against.

You must know, however, that great differences of opinion have existed, and, I believe, still exist, in respect to the contagious quality of certain forms of puerperal peritonitis; just as great differences exist as to the contagiousness of continued fever, of cholera, of the plague. There are parties who regard the whole notion of contagion as a mere bugbear: and there are others who embrace in their allegations of contagion many more diseases than can be *proved* to be so caused. The same strong assertions are made, the same kind of eagerness is displayed (the same party-spirit, I had almost said) as mark the strife

of ordinary politics. It is our serious duty, however, to inquire what is the truth in this matter: for the safety of individuals, and the happiness of whole families, may often hang upon our opinions. I must trouble you, therefore, with a few facts that bear closely upon the subject.

We possess some valuable and highly instructive accounts of epidemics of the kind I am alluding to. One by Dr. Gordon, on the epidemic peritonitis after child-birth, which took place at Aberdeen, in the years 1789, 1790. Another by Mr. Hay, on that which happened at Leeds from 1809 to 1812. And a third by the late Dr. Armstrong, on that which was observed in Sunderland and its neighbourhood in 1813. Dr. Robert Lee has also collected some very interesting facts in reference to the spreading of the peritonitis by contagion. Dr. Gordon had, he affirms, unquestionable proof that the cause of the disease was a specific contagion, and that it did not arise from any noxious constitution of the atmosphere. The disease seized such women only as were visited or delivered by a practitioner, or taken care of by a nurse, who had previously attended patients affected with the same disorder. And Dr. Armstrong observed that forty out of the forty-three cases that happened in Sunderland, occurred in the practice of one surgeon and his assistant.

From among other histories—all tending to the same conclusion—brought together by Dr. Lee, I take the following:—

"On the 16th of March, 1831, a medical practitioner, who resides in a populous parish on the outskirts of London, examined the body of a woman who had died a few days after delivery, from inflammation of the peritoneal coat of the uterus. On the morning of the 17th of March (*i. e.* the *next* morning), he was called to attend a private patient in labour, who was safely delivered on the same day. On the 19th, she was attacked with severe rigors, great disturbance of the cerebral functions, rapid feeble pulse, with acute pain of the hypogastrium, and a peculiar sallow colour of the whole surface of the body. She died on the fourth day after the attack, on the 22d of March; and, between that period and the 6th of April, the same practitioner attended two other patients, both of whom were attacked by the same disease in a malignant form, and fell victims to it. On the 30th of March he bled a young woman who had pleurisy: the wound became inflamed after a few days; erysipelatous redness, and swelling, extended from it up the arm; and in four or five days that patient died of phlebitis."

Mr. Roberton, of Manchester, states the following facts, in a paper in the *Medical Gazette:*—From the 3d of December, 1830, to the 4th of January, 1831, a midwife attended thirty patients for a public charity. Sixteen of these were attacked with puerperal fever, and they all ultimately died. In the same month, 380 women were delivered by other midwives for that institution; but none of the 380 suffered in the smallest degree. All the sixteen had inflammation of the peritoneal surface of the uterus. So, also, Dr. Robert Lee tells us that, in the last two weeks of September, 1827, five cases came under his observation. All the patients had been attended in labour by the same midwife: and no example of a febrile or inflammatory disease of a serious nature occurred during that period among the other patients of the same dispensary, who had been attended by the other midwives belonging to the institution.

Statements of this kind—and they could be largely multiplied—furnish irresistible evidence, that the peritonitis, which prevails epidemically among lying-in women, is of a specific nature, and communicable from one person to another. It is observed, also, to reign as an epidemic especially in *lying-in hospitals*, and that it recurs at *irregular intervals*, sometimes leaving them quite exempt from its ravages for *years* together.

Indeed, I believe that these cases of puerperal fever occurring in succession to the same practitioner, are examples of something more than ordinary contagion, operating through the medium of a tainted atmosphere. I believe them to be instances of direct inoculation. Recollect, that the hand of the accoucheur is brought, almost of necessity, into frequent contact with the uterine fluids of the newly-made mother. Recollect,—those among you who have examined the interior of the dead body with your own hands,—recollect with what tenacity the smell, which is thus contracted, clings to the fingers in spite even of repeated washings; and, whilst this odour remains, there must remain also the matter that produces it. Recollect how minute a quantity of an animal poison may be

sufficient to corrupt the whole mass of blood, and fill the body with loathsome and fatal disease. Illustrations will occur to you in the inoculated small-pox, in hydrophobia, in the viper-bite, in the scratches and punctures of the dissecting-room. Recollect the raw and abraded state of the parts concerned in parturition; the interior of the uterus forming a large wound, and presenting, as Dr. Ferguson observes, an exact analogy to the surface of a stump after amputation; the more external soft parts bruised and sore. Bear in mind the remarkable fact, that this contagion does not affect other persons, but only lying-in women. Recollecting these facts, you will see too much likelihood in the dreadful suspicion, that the hand which is relied upon for succour in the painful and perilous hour of child-birth, and which is intended to secure the safety of both mother and child, but especially of the mother, may literally become the innocent cause of her destruction; innocent no longer, however, if, after warning and knowledge of the risk, suitable means are not used to avert a catastrophe so shocking.

I need scarcely point to the practical lesson which these facts inculcate. Whenever puerperal fever is rife, or when a practitioner has attended any one instance of it, he should use most diligent ablution; he should even wash his hands with some disinfecting fluid, a weak solution of chlorine for instance: he should avoid going in the same dress to any other of his midwifery patients: in short, he should take all those precautions which, when the danger is understood, common sense will suggest, against his clothes or his body becoming a vehicle of contagion and death between one patient and another. And this is a duty so solemn and binding, that I have thought it right to bring it distinctly before you.

In these days of ready invention, a glove, I think, might be devised, which should be impervious to fluids, and yet so thin and pliant as not to interfere materially with the delicate sense of touch required in these manipulations. One such glove, if such shall ever be fabricated and adopted, might well be sacrificed to the safety of the mother, in every labour. Should these precautions all prove insufficient, the practitioner is bound, in honour and conscience, to abandon, for a season, his vocation.

Peritonitis from perforation.—That variety of peritonitis which results from perforation of the stomach or intestines, and the effusion of their contents into the cavity of the belly, is full of interest. The inflammation is violent in degree; universal (generally) in extent; and almost always fatal. The attack is characterized by its *suddenness*. All at once intense pain arises in some region of the abdomen, which soon becomes tender in every part. The pain is incapable of removal, and generally even of mitigation, by medicine, and death takes place in a short time. These are the general features of such cases. Occasionally, the symptoms follow some different order. Thus, I have seen a case in which no pain was complained of, and the source of the inflammation was not suspected until the dead body was examined. This was a case of *fever;* and it had been attended with much stupor, which was probably the reason that no indication of suffering was made by the patient. Occasionally, but that is uncommon, the pain intermits. For the most part, however, it resists all treatment, and ceases only with life, or a short time before life is terminated.

Most of the instances of this kind of peritonitis that I have witnessed have resulted from perforation of the ileum, in the progress of continued fever. You are already aware, that the glandulæ agminatæ, which are found only in that bowel, and the glandulæ solitariæ, which are scattered over nearly the whole inner surface of the alimentary tube, are very liable, in continued fever, to inflammation, sloughing, and ulceration: and sometimes the ulcers *go through:* the contents of the gut are poured into the cavity of the serous membrane, and intense inflammation is lighted up. I purposely abstain from going into any particulars respecting these ulcers. We are no further concerned with them at present than as they furnish the channel by which the cause of the peritoneal disease is introduced. Once, and once only, as I stated before, have I known perforation occur from the extension of scrofulous ulceration of the same glands in *phthisis*. In general, in that disease, the ulcer runs a much slower course. As it approaches the peritoneum, circumscribed chronic inflammation is set up in that membrane; lymph is thrown out; and the bowel becomes adherent to some other portion of the canal, or to some of the other viscera of the abdomen. In this way the perforation is prevented; or, should it take place, the

escape of the contents of the bowel into the peritoneal sac is prevented. Occasionally, when two portions of the tube thus adhere together, a communication is formed between *them*, and the contents of the intestine either reach an advanced point of their stated journey by a short cut; or are carried back again perhaps to a spot which they had already passed.

A very curious instance of this latter event has been described by Dr. Abercrombie. A man, fifty-six years old, who had shown no signs of serious illness, but had laboured under impaired appetite, languor, and occasional pain in the abdomen, for two or three weeks, was suddenly seized, while out walking, with vomiting; and he observed that what he brought up was stercoraceous; and this occurred again and again, at variable intervals; the matter vomited being distinctly fæcal, and sometimes so solid that he was obliged to swallow warm water to soften it, that it might he expelled from the stomach more easily. He never vomited his *food;* and no tumour, or other sign of organic disease, could be detected by external examination. He lived about three months, and died at last of exhaustion: and then it was discovered that the stomach and the transverse arch of the colon were adherent to each other, and that a ragged aperture of communication between them existed at the place of adhesion.

It is a curious fact, that the vermiform appendage of the cæcum is not unfrequently the seat of a penetrating ulcer. I have traced little groups of glands in that slender tube: and I have known perforation to happen from the specific ulceration of continued fever; and from the accidental ulceration caused by a cherry-stone lodged there.

Sometimes it is the *stomach* that is perforated, either by a common or by a specific ulcer; and the symptoms are exactly the same as when the *bowel* gives way. Sudden, unremitting pain; tenderness, and tympanitic distension of the abdomen; and early death.

Perforating ulcers of the stomach are of various kinds. It is not uncommon to find one small roundish hole, with edges as smooth and clean as if a piece of the stomach had been cut out by a punch, and without any surrounding hardness, or other mark of disease. Occasionally the orifice is more irregular, and occupies the centre of a thickened and indurated patch of the mucous membrane. Several instances of this sort of perforation have occurred under my own eye; two within the last fourteen months (1839). Almost all the patients have been young women, plump, and in good condition; who, up to the moment of the fatal seizure, either seemed to enjoy perfect health, or, at most, had complained of slight and vague feelings of dyspepsia. Ulcers of the stomach are, however, mostly chronic; and the diseased viscus is commonly fenced about and protected by adhesion to the neighbouring parts, before its coats are completely penetrated by the ulcer. You are probably aware that this happened in the body of Napoleon Bonaparte. He died of cancer of the stomach. That organ was strongly adherent to the concave surface of the left lobe of the liver, which formed a part of the wall of the stomach; and this adhesion, no doubt, prolonged his life.

Usually the actual perforation, in cases such as we are now considering, takes place merely from the natural progress of the ulcer; but sometimes it would appear that the thin membrane which remains is broken by some accidental force applied to it. Thus the symptoms have immediately followed the act of vomiting, brought on by an emetic. Bouillaud relates an instance in which the perforation happened while the patient was straining at stool; and it is conceivable enough, that rough pressure of the abdomen might complete the rupture, when the ulcer had already eaten through all the coats of the bowel except its peritoneal coat.

Less commonly than this, in my experience at least, the peritonitis is set up by the escape of urine from the *urinary bladder*, through the extension of an ulcer, or from a forcible rupture of that bag by a blow or a fall, when it was distended with urine. Rupture of the *gall-bladder*, whether by violence or from ulceration, has the same results; so also has, in general, the rupture of the *uterus*, which sometimes takes place during the efforts of parturition. *Abscess of the liver*, bursting into the peritoneum, is another occasional source of severe and fatal inflammation of that membrane. Acute and general peritonitis sometimes arises, also, in consequence of penetration from without; *i. e.*, it succeeds the puncture made by the trocar in the operation of tapping the belly; and these cases, too, are almost all of them mortal: chiefly, I presume, because, in nine instances out of ten, they occur in an unhealthy and debilitated subject.

I formerly offered you some observations respecting a form of hysteria which very closely *mimics* peritonitis, and would most certainly deceive a medical man who was not on his guard against it. We judge by the age and sex of the patient somewhat; by the presence of hysteria in other forms, or of the hysteric diathesis; by the *excessive* tenderness of the abdomen, or rather of its surface; by the coexistence of the same exquisite sensibility in other parts; and by the incongruity and shifting character of the symptoms. The pulse and the tongue will perhaps be natural, while the abdominal irritation is at its height. Forewarned, you will seldom find much difficulty in establishing the diagnosis. Of the signs by which peritonitis may be distinguished from enteritis, I shall speak when I come to the latter disease.

LECTURE LXVI.

TREATMENT OF ACUTE PERITONITIS; BLEEDING, MERCURY, OPIUM. CHRONIC PERITONITIS: GRANULAR PERITONEUM. ASCITES; OVARIAN DROPSY; DIAGNOSIS OF THESE DISEASES. OTHER FORMS OF ABDOMINAL DROPSY.

Prognosis.—Acute peritonitis, in its simple form, is always dangerous, yet frequently a manageable disease. When it is complicated with other and earlier organic mischief, and especially when it has been excited by the entrance of foreign matters into the cavity of the belly, it is all but hopeless under any treatment.

Treatment.—In speaking of the mode of cure, I have again to repeat, *mutatis mutandis*, the grand remedies for inflammation, and particularly for the adhesive inflammation proper to serous membranes: *blood-letting; mercury.*

It is of the greatest importance in this, as in many other cases of inflammation, that the blood-letting should be performed *early*. You must not be deterred from bleeding by the mere smallness of the pulse: a quality which I have frequently shown you to be characteristic of acute inflammation within the abdomen; and which, in the disease now in question, is often present from the very beginning. If the pulse be wiry and hard, we disregard, in these cases, its smallness. Not uncommonly it is rendered more full, as well as softer, by venesection; and this, when it happens, gives assurance of the propriety of that measure.

Topical blood-letting is of much efficacy—of greater efficacy perhaps than in most other forms of abdominal inflammation. Cupping is out of the question, from the tender state of the abdomen. But in adults, after a full bleeding from the arm, such as has produced some sensible impression upon the circulation, or brought the patient to the verge of syncope, the surface of the belly should be *covered* with leeches. From twenty to forty may be applied at once: and sometimes this will make any further loss of blood unnecessary. But in severe cases, you may expect to find that repetitions of at least the local bleeding will be requisite.

After the leeches have fallen off, a light poultice may be laid over the abdomen; or it may be assiduously fomented with flannels wrung out of hot water. These means will encourage the bleeding from the leech-bites; and are generally found to afford great comfort to the feelings of the patient. *Cold* applications have been recommended by some practitioners of high authority. Dr. Sutton injected cold enemata, and applied cloths, made wet with cold evaporating lotions, to the abdomen, with good effect: and Dr. Abercrombie has since reported favourably of the same kind of treatment. However, I should think this a more precarious plan than the opposite: and I have always observed so much relief to be given by warm epithems that I have never had the inclination, or the courage, to employ cold.

It is extremely desirable, in these cases, to obtain as speedily as possible the specific influence of mercury upon the system; by calomel and opium, or by inunction. It can-

not be necessary that I should again go over in detail the means of following out this indication; but it is an indication which we must diligently pursue.

The treatment of *puerperal* peritonitis is much more uncertain and difficult: for this reason—that it springs out of an antecedent morbid condition more deeply seated, more generally diffused, and less accessible to remedies, than itself. Whenever inflammation arises here and there in the body in consequence of a vitiated state of the blood, we have not only the inflammation itself to deal with, but its physical cause also, which may still be in uncontrolled operation.

Dr. Ferguson justly remarks that "inflammation being made up of vascular and of nervous action, of the afflux of blood to a part, and of pain, it is not irrational to act on both the elements of the malady at the same time, or in periods shortly consecutive of each other." "The abdominal pain that occurs in puerperal fever, is accompanied by two very different states of constitution: one in which little or no depletion is borne, another in which relief is obtained only by very large evacuations of blood." Between these two there is every conceivable gradation. "In no malady are a cautious boldness, and a sagacious adaptation of remedy to constitutional power, more imperatively demanded."

"If large bleeding be determined on, it must, to be beneficial, be resorted to within the first twenty-four hours from the attack. In the second stage of the disease it often produces, rapidly, a fatal result."

In ambiguous cases he gives ten grains of Dover's powder, and covers the whole of the abdomen with a large linseed-meal poultice, sufficiently thick to retain warmth for four hours. At the end of that time, if the symptoms are alleviated, "ten grains more of the Dover's powder, and a fresh poultice, should be prescribed. If within four hours from this second medication, the practitioner is not satisfied that the malady is yielding, he must at once resort to depletion."

Of all the means we possess of arresting this malady, bleeding, general or topical, is, in Dr. Ferguson's experience, by far the most extensively applicable. "But," he says, "while I admit this, I am equally certain that *large* bleeding has not been borne in this complaint, generally speaking, during the last twelve years."

Another most important truth enunciated by our Professor is, that "epidemic puerperal fever has, invariably, the character common to the ordinary fevers raging with it: if the latter require depletion, the presumption is that the former will also."

Undoubtedly the very same species of febrile disease is variously affected by a given remedy in different places; and during different periods in the same place. There is such a thing as an epidemic state of the human constitution, gradually produced by a gradual fluctuation in the influences whereby communities of men are surrounded and impressed. The fevers that were cured in London ten or fifteen years ago by copious blood-letting, would now be rendered by that measure, carried to a like extent, irretrievably mortal. There is scarcely a more important object of study to the practical physician than this different capacity, exhibited by the average of constitutions at different times and seasons, of bearing active depletion. "Nihil mihi prius est," says the wary Sydenham, "quam quando novæ febres grassari incipiant, cunctari paulisper, et ad magna præsertim remedia non nisi suspenso pede, ac tardius procedere; diligenter interim illarum ingenium atque morem observare, quibus itidem præsidiorum generibus ægri juventur vel lædantur, ut quam primum his repudiatis, illis utamur." The exciting cause of the fever remains the same; the system upon which it operates undergoes from time to time great changes: which are brought to light partly by the altered phenomena of the disease, partly and chiefly by the effects of remedial measures.

Unless you bear these differences in mind, you will be perplexed and disheartened by the discrepant accounts given by competent and faithful observers, respecting the success of different or even opposite plans of treatment, in the same complaint.

Most writers whose works I am acquainted with, recommend *purgatives* as highly serviceable in peritonitis. I do not think the good which they are calculated to do as antiphlogistic remedies can at all be put in competition with the harm that I am persuaded they may produce, by increasing the peristaltic action of the intestines, and so causing additional friction and tension of the inflamed membrane. I believe that in all cases of well-marked and pure peritonitis, when the inflammation is limited to the serous membrane, it

is far better and safer to restrain than to solicit the internal movements of the alimentary tube. In a pamphlet published several years ago by Mr. Bates, of Sudbury, some striking instances are recorded of recovery from severe peritonitis, under large and frequent doses of opium, and a rigid adherence to the horizontal posture, until all pain had subsided. The patients were not allowed to raise themselves, on any account, into a sitting position: and the opium was administered sometimes by the mouth, sometimes by the rectum. These cases, related in an unpretending manner by a practical observer, made a strong impression on my mind when I read them. To simple inflammation of the peritoneum, to those perilous forms of peritonitis which occur in women after delivery, and to those still more terrible cases that follow perforation of the serous membrane, this principle of keeping the intestines at rest, is alike applicable. I stated a little while ago, that the last-mentioned cases are all but hopeless. The *all but* I inserted on the strength of some most interesting facts published by Dr. Wm. Stokes, in the second number of the *Dublin Journal of Medical and Chemical Science*. He truly remarks, that in most of these accidents the powers of life sink so rapidly that bleeding, either local or general, cannot be attempted. Neither can we employ mercury internally, for fear of exciting the peristaltic action of the bowels, which action would tend to tear asunder recent adhesions, to keep the communication between the mucous and serous surfaces open, and to cause a fresh ingress of fæcal or other extraneous matters into the sac. Yet in a few instances we find that the patients live for several days, and that a process of organization commences in the effused lymph. It seems that some years before Dr. Stokes wrote this paper, he had witnessed the admirable effects of opium in low forms of peritonitis, as administered by Dr. Graves; who thus saved two individuals in whom that disease followed paracentesis, without abstracting a drop of blood. I cannot refrain from quoting to you the particulars of one instance, in which the efficacy of the opiate treatment was conspicuous. The well-known symptoms of perforation of the intestines had existed for two days; the patient was apparently sinking, "his countenance was collapsed, anxious, and expressive of dreadful suffering; the extremities were cold, and the pulse hardly perceptible. The exhibition of sixty drops, in the twenty-four hours, of the preparation called the *black drop* was followed by the most signal improvement. The pulse regained fulness and softness, the extremities became warm, and the countenance had lost the Hippocratic expression. The patient could bear pressure on the abdomen, which, the day before, was exquisitely painful. The same treatment was continued for twenty-four hours longer; and by the end of that time every symptom of abdominal inflammation had completely subsided. The belly felt natural, there was no tenderness, the pulse was good, and the patient declared himself well." At this period of the case, Dr. Stokes omitted the opium, and gave the mildest possible saline laxative, as there had been no stool for forty-eight hours. Four evacuations took place, followed by the immediate return of the symptoms of peritonitis, under which the patient rapidly sunk.

"The intestines were everywhere agglutinated together, and adherent to the parietal peritoneum, except in the left iliac fossa, where a quantity of yellow puriform matter was collected. On detaching the caput coli from the peritoneum lining the right iliac fossa, a small perforation of the gut was discovered, by the escape of the contents of the intestines in a jet," &c. &c.

This example puts in a very strong light the *good* effects of *opium;* the *dangerous* effects of *purgatives;* and the *mode* in which recovery from these frightful accidents *may* sometimes be brought about.

Dr. Stokes gives another instance in which the patient *did* recover; after taking 105 grains of opium, besides what was administered in injections: and he alludes to a third case, in which the employment of opium was successful, when peritonitis had supervened upon the bursting of an hepatic abscess into the cavity of the abdomen.

Now I would earnestly recommend you to consider the expediency of applying the same principle of treatment, as an auxiliary, when the peritonitis does *not* grow out of previous organic disease: in all cases, in short, of *mere peritonitis*. The opium is not to supersede the bleeding, nor the mercury; it is not incompatible with either of those remedies; and it may, I believe, be most advantageously adopted in conjunction with them both.

I shall relate one example, which has lately occurred to me, of the successful use of

opium in simple, but severe, peritonitis. Several of you saw this patient. His case is published in the appendix to Dr. Ferguson's volume.

H. Middlehurst, a tailor, seventeen years old, was admitted into the Middlesex Hospital on the 17th of September: looking very ill, and complaining of pain in the epigastrium, with extreme tenderness over the whole abdomen, which was full and tense. He had been ill several days; had shivered in the outset; and had vomited frequently, up to the period of his admission. His bowels were confined; his tongue was dry and white.

Twelve leeches were placed upon his abdomen, and calomel, in five-grain doses, was given two or three times at intervals of four hours. An enema of warm water was injected, and retained. In the evening sixteen more leeches were applied, and a drachm of mercurial ointment was rubbed into his arm.

I first saw him on the 18th. His countenance was then pinched and anxious, and he lay moaning with pain; his knees being drawn up towards his belly, which was tense, and exquisitely sensible to pressure. He complained of nausea and retching, but had not vomited since his admission. His tongue was thickly coated; his pulse small, sharp, 108 in number. No permanent relief had been obtained from the leeches.

I directed immediate venesection; but not more than four ounces of blood could be got from the arm. Thirty fresh leeches were therefore put upon the abdomen, and afterwards a warm poultice to receive the blood from their bites. Three grains of calomel, and three of blue pill, were ordered to be given every four hours.

The last leeches mitigated the pain; but it returned in the evening with increased severity, and he vomited the pills. He appeared to be in great agony. In this state the apothecary gave him twelve grains of calomel, and five grains of opium, in one dose. Soon after this he fell asleep; and slept during a greater part of the night. Next morning his countenance had lost, in a great degree, its expression of anxiety; his belly was less tender, but still tense; and his tongue cleaner. No stool.

Capiat pilulæ Saponis cum Opio gr. v. 8vâ. quâq. horâ.

On the 26th the bowels were freely open, the dejections dark and watery; the abdomen was less tender. Pulse 114. He continued to take a grain of opium thrice daily till the 3d of October: the bowels being every day moved; the pulse and tongue gradually improving; and the abdomen painless even under firm pressure. On the 3d, as the bowels had not acted for the last two days, I discontinued the opium. On the 5th diarrhœa set in, with some renewed tenderness of the belly; and the pinched and anxious countenance returned. He had then an opiate enema; and resumed the opiate pills as before. Under this treatment he at length got quite well; and left the hospital on the 30th.

Chronic peritonitis is sometimes merely the sequel of that acute form of inflammation of the peritoneum, which I have just been describing. Plastic lymph is effused, and becomes organized; serous fluid is poured out, and is not absorbed again; the products of the original inflammation remain; a low degree of inflammatory action perhaps remains also, or is re-excited by slight causes; the mischief augments; and the patient is slowly conducted to the grave.

There is, however, another, not at all uncommon, and equally formidable source of chronic peritonitis; the presence, I mean, of a multitude of little granules, lying within or immediately beneath the membrane, or occupying, in countless numbers, those folds of the peritoneum which compose the omentum. These granules occur principally, if not exclusively, in scrofulous persons. Louis, indeed, who considers them to be *tubercles*, affirms that they are never met with in the peritoneum, without being met with also—and usually in a more advanced state and greater abundance—in the lungs; but this rule is not universally true. I have seen more than one instance of well-marked granular disease of the serous membrane of the abdomen, without a single tubercle in the pulmonary tissues. Still the observation of Louis holds good in a vast majority of cases: and when we have symptoms of chronic peritonitis, which were not preceded by those of acute inflammation of the membrane, and when we perceive at the same time indications of phthisis, or of any other unequivocal form of scrofula, we shall seldom be wrong in connecting the chronic peritonitis with the presence of these miliary granulations. Whether they are truly scrofulous tubercles, or whether, as some suppose, they are *sui generis*, or, again, simply minute

spherules of coagulable lymph, I do not undertake to determine. I have been in the habit of regarding them as the cause, and not as the consequence, of the inflammation with which they are found associated.

The *symptoms* of chronic inflammation of the peritoneum are more obscure, in general, than those of the acute disease. And when the disorder is primitive, not the relics I mean of more active inflammation, it often begins, and steals on, in a very insidious manner. The patient complains of abdominal pains: sometimes slight, amounting to scarcely more than uneasiness, but abiding; sometimes occasional only. Usually there is a sensation of fulness and tension in the belly, although its bulk may not be sensibly altered. Sometimes there is a sense of pricking felt. Dr. Pemberton remarks, that you may detect a sort of deep-seated tension; that the skin and muscles lie loosely on the peritoneum, which gives to the hand a sensation as of a tight bandage underneath, over which the integuments appear to slide. The uneasiness, or the pain, is augmented by pressure; or perhaps is felt only when pressure is made. Sometimes the functions of the intestinal canal are disturbed: there is loss of appetite; nausea and vomiting; an irregular state of the bowels, and unnatural evacuations from them. Sometimes, on the contrary, the digestive organs perform their office in a tolerably healthy manner. These differences depend apparently upon the circumstance of the inflammation visiting, or sparing, the peritoneal covering of the stomach and bowels; and of the parts concerned in the secretion of bile. Sooner or later, in most cases, the abdomen enlarges, and fluctuation is felt. All along there is some fever, more or less distinctly marked; with progressive emaciation and debility. The face is pale and sallow, and wears an expression of languor.

Granular peritoneum.—Very much the same set of symptoms are apt to result from scrofulous disease and enlargement of the mesenteric glands; and consecutive slow inflammation of the peritoneal membrane.

Accordingly, after death, we often find those glands swelled, and red, and hard; sometimes forming very large tumours: or we discover the whole surface of the membrane thickly bestrewed with innumerable small, round, grayish or white granules; sometimes it is covered, here and there, or everywhere, with false membranes: the intestines are frequently agglutinated into one mass; or they are adherent to each other, or to the other parts of the peritoneum, in places only. The omentum is generally thick, red, and fleshy, as if its component parts had been matted together; and there is more or less fluid, commonly turbid and flaky, in so much of the cavity as happens to be left.

These are very unpromising forms of disease; and it is seldom that we can do more than mitigate the most distressing of the symptoms; or retard, perhaps, the march of the disorder. Leeches to the abdomen, in moderate numbers, and frequently repeated, and followed by soft warm poultices. Blisters, when the pain is not severe, and the tenderness less. Attention to the state of the bowels, which should be regulated by mild laxatives rather than by drastic purges. A nourishing, but unstimulant diet. These are the measures to which we must look for benefit. It has been thought that frictions upon the belly, with ointments containing iodine, have done good: so that it will be well to make trial of such. But do what we may, in nine cases out of ten, our best directed efforts will be disappointed.

Ascites.—When there is much fluid collected in the abdominal cavity in these cases, they take their character from this predominant symptom, and are called cases of *ascites*. But this is only one form of ascites—that form which results from chronic inflammation of the peritoneal membrane. I shall pass, however, by an easy transition to the other forms of dropsy of the belly.

There is another form of ascites, not very common, which approaches in its character to inflammation, and which is therefore called *active* ascites. I mean that we sometimes see persons, who were previously in good health, become rapidly ascitic, after exposure to cold and wet, and rapidly recover again under the remedies that are used to subdue inflammation. Perhaps it may be said that these *are* cases of inflammation; and it may be so. But they want many of the ordinary symptoms of peritonitis; and if inflammation *be* present, it has no worse effect than the effusion of serum, which, under depletion or

mercury, is speedily taken up again. I should rather conceive, however, that these cases are to be included in that category of dropsical effusions which I spoke of formerly, as resulting from the detention or absorption, in the first place, of an undue quantity of watery fluid into the blood, and its subsequent discharge, by a kind of secretion, either into shut cavities, or through some one of the natural vents of the body. The balance of the circulation between the skin and the internal surfaces appears to be destroyed on these occasions, by the operation of external cold upon the tegumentary membranes.

But by far the greater number of cases of ascites are cases of passive dropsy which arise slowly from a mechanical obstacle to the free return of the venous blood towards the heart.

Ascites occurs, as you know, in general dropsy, with anasarca of the universal cellular tissue; and this general effusion of fluid depends, in almost every case, either upon a peculiar renal disorder, or upon organic disease of the viscera of the thorax; of the lungs, or of the heart, or of both; and, above all, upon such disorder as is attended with dilatation of the right chambers of the heart.

But I exclude this form of ascites, wherein the dropsy of the belly is only a portion of more general disease of the same kind, and limit myself at present to that kind of passivve ascites which is unattended with dropsy elsewhere; or which at any rate precedes the occurrence of serous accumulation in other parts.

The symptom which first leads us to suspect ascites, is the progressive enlargement of the abdomen. But the abdomen may grow gradually large and prominent when there is no disease whatever: in pregnancy, for example; or in mere obesity. It is necessary therefore to search for more definite signs of peritoneal dropsy.

Diagnosis.—In order to make an *accurate* diagnosis of ascites, we must know what are the morbid conditions with which it is most liable to be confounded. Solid tumours and simple corpulency are readily enough distinguished. But there are certain kinds of *encysted* dropsy of the abdomen, of which the recognition is not so easy and obvious. Of these what is called *ovarian dropsy* is the chief. In some of its symptoms this complaint closely resembles ascites; in some it differs from it widely. So also the treatment of the two disorders is alike in some respects; dissimilar in others. For these reasons, and because I am more solicitous to be practical than to be methodical, I shall consider these two maladies together; turning first to the one, and then to the other, and marking, as I go on, the various points of similitude, and contrast, which they mutually offer.

Recollect that ascites signifies the accumulation of serous liquid in the bag of the peritoneum; whereas ovarian dropsy consists in the collection of fluid in one or more cells within the ovary: or in a serous cyst connected with the uterine appendages.

One source of distinction between the two is furnished by the condition of the abdomen during their early stages.

In ascites the enlargement is uniform and symmetrical, in reference to the two sides of the body. When the patient lies on her back the flanks bulge outwards, or swag over, from the weight and lateral pressure of the augmenting fluid. This increased *breadth* of the trunk is not observable in the case of an ovarian tumour; nor, I may add, in pregnancy.

When we are able to trace the early history of ovarian dropsy, we find, in most instances, that the abdominal tumour was first perceived on one side; in one or the other of the iliac fossæ, or somewhere between the ribs and the ilium. But when the enlargement of the abdomen is great, the distinction between ascites and encysted dropsy, drawn from the shape of the swelling, ceases. The ovarian tumour distends the abdomen, if not uniformly, yet nearly or quite as much on side as on the other.

The next thing we do, when the *visible* bulk and shape of the abdomen have suggested a suspicion of ascites, is to employ the sense of *touch*.

Examination by *pressure* will sometimes suffice to assure us that there is fluid in the peritoneum. If you press suddenly with the tips of your fingers, in a direction perpendicular to the surface, you will often become aware of a sensation which it is difficult to describe in words, yet which is quite decisive, and not to be mistaken; a sensation of the displacement of liquid, and of the impinging of your fingers upon some solid substance below. So that by this manœuvre, you frequently detect, not merely the presence of the

liquid, but an enlarged liver, or spleen, or (it may be) an ovarian or other tumour; even when simple palpation, or handling in the ordinary way, would not enable you to ascertain these enlargements.

Again, *percussion* of the abdomen is fertile of information in these cases. First, by the sense of fluctuation which it causes when liquid is collected within. The left hand being laid flat against one side of the tumid abdomen, if a slight blow be struck with the fingers of the right upon the opposite side, the impulse is conveyed by a wave of the liquid to the open flat hand, which feels a little shock that is perfectly distinctive. The larger the amount of the accumulated liquid, and the thinner and tighter the walls within which it is confined, the more sensible and decided is this fluctuation. Even when the quantity is small, not exceeding a few ounces, a little practice and management will enable you to detect it. Percuss with one finger the most depending part of the cavity, and apply at the same time a finger of the other hand, very near the part struck; and if liquid be there, you will perceive a limited yet distinct fluctuation. In the same way the presence of liquid in a small cyst may sometimes be ascertained. Much more when the cyst is large. And the cyst, in ovarian dropsy, is often very large; and the liquid it contains is often thin and aqueous; and then fluctuation may be quite as perfect and perceptible as ever it is in ascites.

Hence mere fluctuation is not a discriminating symptom between ascites and ovarian dropsy.

But, secondly, percussion is full of instruction in the *sounds* it elicits. The sense of *hearing* will generally supply what the sense of touch may leave wanting.

In true ascites the relative place of the liquid and of the intestines is determined by the posture of the patient. The bowels, which always contain some gas, float to the upper part of the liquid, and there give out (when the finger, as a pleximeter, is applied to the corresponding surface, and struck) their peculiar resonance. Mediate percussion will thus follow the gravitating fluid, and discover always a dull sound in the lowermost and a hollow sound in the upper part of the abdomen.

But it is not so in ovarian dropsy. The cyst, in a diseased and enlarging ovary, rises in front of the intestines, which, being tied down by the mesentery, cannot embrace the tumour so as to reach its anterior aspect, but are in fact pressed back by it towards the spine. Hence, if there be any resonance produced by percussion, it is in one, or the other, or in both, of the flanks; and the umbilical region yields a dull sound whatever the position of the patient may be. The same is true of the enlarging womb in pregnancy.

This simple expedient, then, is quite decisive. In ascites the epigastric or umbilical region is tympanitic on percussion; in ovarian dropsy it is dull. To be quite sure it is well to make the patient assume different postures in succession.* If the person affected with ascites turns upon her side, the uppermost flank will become resonant; the umbilical region dull: whereas in ovarian dropsy, the sounds remain severally where they were under every change of position. In ascites, with a little care, you may ascertain the exact level at which the contained liquid stands, and measure its rise or fall from day to day.

This mode of diagnosis is scarcely open, under ordinary circumstances, to fallacy or exception. Yet there are two or three possible conditions in which it may fail; and these it is right that I should briefly mention.

1. The distension, in true ascites, may be *so* great, that the mesentery shall not be broad enough to allow the buoyant intestines to reach the surface, when the patient is supine. This impediment to the efficacy of the proposed test I have met with in practice. A woman came under my charge in the hospital with ascites. Fluctuation of the belly was unequivocal. While she lay on her back, the umbilical and epigastric regions were resonant when percussed; the flanks were dull. When she turned upon either side, the other side, previously dull, gave the hollow sound; the umbilical and epigastric regions, previously resonant, gave the dull flat sound. Under the treatment employed, the accumulated liquid was removed, and she left the hospital.

Some time afterwards, as I was going round the wards, I recognized the same woman among the patients recently admitted by my colleague, Dr. Hawkins. The ascites had returned. The abdomen, enormously distended, projected upwards, as she lay on her

back, to an excessive height. I found that fluctuation was very distinct, as before: but every part of the belly yielded a dull sound when struck by the fingers. At length this patient died: and it was seen, after death, that there was nothing to prevent the rising of the intestines. They had floated, at the utmost tether of the mesentery, as high as they could, without reaching the surface of the prominent belly.

2. Another occasional source of fallacy I have just now hinted at. The intestines may be tied down, and so prevented from ascending by their specific lightness, to the upper part of the surrounding liquid. And this may happen, either in consequence of the adhesion of the various coils of the intestines to each other, and to the parts behind them; which is not an uncommon occurrence:—or the intestines, though unadherent, may be swathed, as it were, and bandaged down, by a thickened and diseased omentum. This also I have myself seen. A man died in the hospital, who had manifest ascites. Yet his whole abdomen, though not so much distended as to hinder the intestines, had they been free to rise, from reaching its walls, sounded dull on percussion. Inspection of the body explained this circumstance. When the peritoneum was opened by an incision carried through the forepart of the abdomen, a quantity of serous liquid gushed out. The floor of the cavity which it had occupied was smooth and level; and was found, on further examination, to be formed by a thick cake of omentum, strapped tightly over the subjacent intestines. Of course, the same diseased condition may occur in the female.

3. On the other hand, I have once known an ovarian cyst to exist, when the umbilical region was tympanitic under percussion. The case furnished just that kind of exception which serves to prove a rule. This was also a hospital patient. Her *history* was the history of ovarian dropsy. Some time previously she had discovered a small tumour in one of the iliac regions. It increased without much disturbance of her general health, until it became very inconvenient from its bulk. She was then tapped in one of the Borough hospitals: and she stated distinctly that it was not a clear watery fluid that was evacuated; but a glutinous, mixed, and grumous matter: such as belongs to ovarian disease. No doubt could be entertained that the enlargement of the abdomen resulted from disease of that kind. Yet the umbilical region, when percussed, always rendered a hollow sound. Upon the death of the patient the mystery was solved. Air hissed forth from the opening made by the scalpel through the abdominal parietes: and the source of it being traced, an ovarian cyst, of considerable magnitude, was found adhering to the peritoneum in front of the belly, and containing no liquid, but some yellowish shreds only; the remains, apparently, of some smaller included cysts. This ovarian bag had been filled with air, and had given occasion to the equivocal sounds.

These sources of possible mistake or obscurity very seldom occur; and the physical diagnosis, as I have now pointed it out, is very sure and valuable. So completely physical, indeed, are these tests, that we recognize ascites by them as readily and certainly in the dead, as in the living body.

Other points of distinction may frequently be derived from the history and progress of the two disorders.

The equable enlargement of the abdomen, on both sides, in ascites; and its unequal prominence on one side in the earlier stages of ovarian disease, I have already mentioned.

Again, it is observable, that in true ascites, there are almost always manifest indications of constitutional suffering and disturbance; a sallow complexion; debility; emaciation. The morbid accumulation results (as we shall see) from disease in some organ, of which the functions cannot be impaired without injury to the whole system.

Ovarian dropsy, on the other hand, may last long, and be extreme in degree, while the general health is scarcely affected. The very bulk and weight of the swelling produce, indeed, much inconvenience and discomfort; but, in other respects, the patient often remains in good health. This appears to be owing to the circumstance that the ovary is not directly necessary to the life or well being of the individual, but is merely subservient, for a limited time, to the purpose of reproduction.

Among the symptoms that are common to ascites and ovarian dropsy in their advanced stages, are all those which are occasioned by weight and pressure: viz. shortness of breath, from the resistance opposed to the descent of the diaphragm; anasarca of the legs and thighs, from pressure upon the inferior cava and its branches; and a peculiarity of carriage

and gait, like those of a woman big with child, and depending upon the same cause, the necessity of throwing the head and shoulders backwards, to balance the weight of the distended abdomen in front.

It is necessary to caution you against mistaking a distended bladder for dropsy of the abdomen. An old Frenchman was brought into the Middlesex Hospital, afflicted (so his friends said) with dropsy. He had been treated for that complaint. The abdomen was large, and dull under percussion from the pubes to above the umbilicus. In the hypogastric region I could detect an obscure sense of fluctuation. I noticed a strong smell of urine about this patient. Being interrogated, he said that he had formerly had some "stoppage," but that he now passed plenty of water; that it even ran from him. It was obvious that his bladder was enormously distended, unable to contract upon its contents, and overflowing. With some difficulty a catheter was introduced, and some quarts, I forget the exact quantity, of turbid and stinking urine were drawn off. The patient sunk at length, and the bladder was found to be much diseased. I have known similar mistakes occur in private practice. You will not think the caution I am now giving you superfluous, when I tell you, on the authority of Sir Everard Home, that John Hunter once actually tapped a distended bladder, in the belief that the disorder was ascites.

Encysted dropsy, in the abdomen, is not always *ovarian* dropsy. *Omental* dropsy is described: the omental cavity alone being unfolded, and full of liquid. This I have never seen. Cysts containing a considerable quantity of a clear thin liquid, and connected with the *liver*, are common. Probably these are in all cases (they certainly are in many) the effects of the growth of hydatids. Dropsy of the *fallopian tubes:* dropsy of the *uterus:* large serous cysts in the *kidney:* constitute other forms of abdominal encysted dropsy. Such states must be discovered by their own particular circumstances. None of them are very common.

LECTURE LXVII.

PATHOLOGY OF CHRONIC ASCITES: OF OVARIAN DROPSY. TREATMENT OF THESE TWO DISORDERS. INTERNAL REMEDIES: EXTIRPATION OF THE OVARIAN SAC: PARACENTESIS ABDOMINIS.

IN my last lecture I pointed out the means we possess of distinguishing ascites from ovarian dropsy. Continuing the parallel between these two disorders, I have still to consider their pathology; and to prescribe their treatment.

Pathology.—I mentioned that chronic ascites is sometimes the sequel of acute inflammation of the peritoneum. In such cases, the abdomen is usually uneasy, and tender under pressure; or at any rate, more sensible than common: and I believe more *hot* also than common. Whereas when ascites is passive as well as chronic, you may make the requisite examination without causing any distress to your patient. There is no pain produced by palpitation, by pressure, or by percussion. Even when the dropsy has resulted from bygone inflammation, it does occasionally, though rarely, happen, that no other trace of such inflammation is discoverable in the *living* patient. The absorbing function of the membrane having, however, been spoiled, the collected liquid remains. Such a condition, I believe, I have witnessed. The history of sudden and sharp pain, and tenderness of the abdomen, with fever, immediately before the dropsical swelling took place, made it probable that it was the consequence of inflammatory effusion. But the fever had entirely subsided; no tenderness was left; no large veins were visible on the surface of the belly, denoting internal obstruction; and the general health was good. The patient had no other dropsy.

The main exciting cause, however, of true and uncombined ascites, when no inflamma-

tion is, or has been, at work, is some impediment to the venous circulation in the abdomen. Whereabouts, and of what kind, is this impediment? That is the question which, in each particular instance, we ask ourselves.

The old doctrine respecting the causes of ascites, vaguely referred the collection of liquid to *obstruction;* and to *organic diseases* of the abdominal viscera; and above all, to *hepatic* disease. But as we are now better instructed, and know that organic diseases produce the dropsy, ultimately, by retarding the flow of blood through the system of the vena portæ, we see that the truth was only half perceived by the ancient pathologists. We can now understand why some organic diseases of the abdomen lead to dropsy of the peritoneum, and others (even of the same viscus) do not. And we have no difficulty in comprehending why, of all the abdominal viscera, the *liver* is the one whose diseases are the most frequently connected with ascites: that gland being traversed by the converging branches of the venous trunk, through which passes by far the greater part of the serosity absorbed from the surface of the vast membrane that inwraps most of the abdominal organs, and lines the cavity containing them. It is plain that an accumulation of serum in the peritoneal sac may arise from a mechanical obstruction in the trunk of the vena portæ, or in some of the principal branches that unite to form that vein; or from certain diseases of the liver itself. But we know that disease of the liver is of very common occurrence, and oftentimes very obvious, while there is no ascites. And a further question arises—With what kinds of disease of the liver is hepatic ascites most apt to be associated?

In truth, there is one special form of liver-disease which, though not the sole, is the grand cause, of passive and simple ascites. It has long been noticed that mere enlargement is not the most common condition of the liver met with in hepatic dropsy; but rather the small, hard, contracted viscus. Mere increase in the size of the organ may interfere but little with the portal circulation; whereas a shrinking and diminution of its bulk must needs do so. In point of fact, that particular state of the liver which the French have termed *cirrhose*, and which is familiar to morbid anatomists in this country as the *hobnail* liver, is the great source of passive ascites.

The true character of this remarkable condition of the liver is of modern discovery. The credit of correcting the erroneous opinions which had been entertained respecting it is due, I believe, to Mr. Kiernan. The change undergone by the organ has also been clearly described by Dr. Carswell; whose delineations of its physical appearance are now before you. The change results from chronic inflammation, and chronic thickening (miscalled hypertrophy), of Glisson's capsule. Since Mr. Kiernan's admirable exposition of the minute anatomy of the liver has been given to the world, few can be ignorant that the cellular tissue, termed the capsule of Glisson, accompanies the portal vein, the hepatic artery, and the biliary ducts; and forms a sheath around these vessels in their course through the liver: while the hepatic vein and its branches are lodged in the proper substance of the gland without any such investing membrane. It follows that a general thickening of this tissue must produce a general pressure upon the portal veins, and hinder the return of the venous blood from the intestines. Hence, as in analogous cases, congestion of the capillaries, arrested absorption, mechanical transudation of serous liquid. The pressure affects also the nutrient vessel, the artery of the liver; so that, in most instances, there is atrophy and shrinking of the organ. And sometimes, but not always, from pressure upon the biliary vessels, there is jaundice also. By degrees, the cellular tissue itself begins to shrink; and the spaces in which it ramifies on the surface of the liver are pulled inwards; the lobules appear to be prominent, and the surface becomes irregular and knobby, and studded with little roundish eminences like the heads of nails. The constricted lobules are very conspicuous in the cut surface also of the liver.

In the living body the presence of this hepatic disease is, for the most part, a matter of inference only. It is rendered probable by its ascertained frequency in connection with ascites, and by the absence of any other obvious cause for the dropsy. But sometimes the irregular surface can be felt through the walls of the abdomen.

The nature of this morbid change affords a reason for the intractable and unpromising character of ascites in general. The obstructed blood seeks indeed new channels; but the compensation they furnish is rarely sufficient. The superficial veins become obvious, numerous, large; and wander with many inosculations over the surface of the belly.

Large veins, significant of the same compensating effort, have been met with also in the adhesions which previous inflammation had left between the liver and the diaphragm.

Among the causes to which the thickening of the capsule of Glisson may be ascribed, habitual intemperance holds the chief place.

But this condition of Glisson's capsule, though it is the principal, and by far the most frequent, is not the only cause of obstruction to the current of the blood in the portal vessels, and of consequent ascites. In those specific forms of liver disease in which separate tumours are scattered through its substance, one of these tumours may be so placed as to press upon the trunk of the vein. So, obviously, may abdominal tumours of any kind; enlarged mesenteric glands; cancer of the pylorus; cancer of the head of the pancreas; and the like.

Ascites is found to be not unfrequently associated with disease and enlargement of the *spleen* also; but in most instances of this kind, the enlargement of the spleen and the peritoneal dropsy are not connected as cause and effect; but are both consequences of portal obstruction.

When, after death preceded by ascites, the cavity of the abdomen is laid open, its contents present a bleached and sodden appearance. It has been made a question whether this be the result of the long-continued immersion of the living tissues in the accumulated water; or of their short maceration after death. The question has no practical importance.

Dropsy of the ovary, in its ordinary form, consists (I believe) in disease and enlargement of one, or more, of the Graafian vesicles; or of the ova which they inclose.

The actual condition of the dropsical ovary is subject to much variety. Sometimes there is but one cyst; and this may be no bigger than a pea; or it may be large enough to contain many gallons. Its walls may be as thin and flexible as those of the healthy urinary bladder; or they may be firm, and half an inch or more in thickness. It may spring from a small pedicle, and lie free and unattached in the cavity of the peritoneum; or it may adhere, partially or at all points, to the contiguous parts; or it may be tied down by bands of coagulable lymph. Its inner surface may be smooth and even, or knobby and irregular. Lastly, the fluid contained in the cyst may be thin, or consistent; limpid, or glutinous; opaque, or transparent; and of various tints; so that, in different cases, it may be colourless, green, purple, red; and more or less resemble in appearance pure water, white of egg, jelly, glue, birdlime, or treacle. Most commonly, however, when the cyst is single, its contents are thin and aqueous.

Again, the dropsical ovary may be multilocular, composed of many cysts, which are usually distinct, but which sometimes communicate together; and these cysts, in the same ovary, while they vary much in size, may differ also from each other in any or all of the particulars just enumerated as being incidental to a solitary cyst.

Commonly one of the cysts is much more capacious than the rest; and some part of its inner surface is frequently embossed, as it were, by the projecting outline of a group of small nodules, which seem to lie within the parietes of the larger cyst, but which, in truth, are cysts of similar origin with it, but of more stunted growth.

The external surface also of the multilocular ovary is generally lobulated; and its inequalities may often be discovered by a careful examination of the abdomen in the living subject.

Sometimes the tumour is solid throughout; in which case the term dropsy is altogether misapplied.

These differences are not without occasional importance, in reference to some points in the treatment of the disease.

The *progress* of ovarian dropsy is no less wanting in uniformity. Sometimes it is very rapid; sometimes it is very slow. It may destroy life in a few months; it may continue, a mere burden, with scarcely any fatal tendency, for many years. Not unfrequently, after a period of active increase in the tumour, the morbid process, without any obvious cause, suddenly stops: and the pause may be final; or, after an uncertain interval, the disease may resume its former activity.

Under all circumstances the malady is a serious one: for its possible grievances are many;

and its issue is precarious and unpromising. Although, in some cases, the general health for a long time is but slightly or not at all impaired, in others the disease runs a short course; the tumour increasing rapidly and proving ultimately fatal by its bulk and pressure; or embittering and abridging the unhappy patient's existence by some accident of growth or position. Even when of no vast magnitude, it may be so situated as to impede or prevent the expulsion of the fæces from the bowel, of the urine from the bladder, or of the fœtus from the gravid uterus.

The single cysts, having thin parietes, and containing a serous liquid, are not always produced by disease and distension of a Graafian vesicle; for they sometimes have no connection with the ovary, but spring from some other part of the uterine appendages.

Neither, perhaps, can it be *demonstrated* that the complaint originated within the Graafian vesicles, when it exists in its more complicated form; when the cysts are many, and their contents various. But the shape of the cysts, which are more or less spherical, their number, their isolation in most cases, and the diversity in the matters by which they are filled, render this view of their origin at least a probable one.

This form of the disorder has been considered as belonging to the category of *malignant* diseases; but, in my judgment, without sufficient reason. It is true that the tumour does sometimes involve one or more of those morbid conditions, which have been denominated scirrhus, fungus hæmatodes, cerebriform disease, or melanosis, and which all, or nearly all, appear to be varying results of the same morbid process, and to be referable to the genus *carcinoma:* but whenever this is observed to be the case, other structures also are found to be infested with analogous changes. The so-called malignant disease occupies the ovary in common with other parts; and this is one of its most constant characters, namely, that proceeding from some vice in the constitution, or disseminated from some local germ, it pervades different organs of the body at the same time, or in succession; whereas in by far the majority of instances of ovarian dropsy, these peculiar products are met with neither in the diseased gland, nor in any other place. It has already been remarked that many women, labouring under ovarian dropsy, enjoy nevertheless in all other respects very good health, even for many years. The victims of malignant disease are not so fortunate. *They* either are soon cut off, or, if they linger, they seldom fail to exhibit, in their complexion and general condition, notable indications of the mischief which is in progress, and gradually undermining the powers of life.

If it be admitted, as a reasonable conjecture, that the Graafian vesicles, or the ova they contain, are the seat of the primary changes, we may push our speculations a little further. These ova are destined, under the peculiar stimulus of impregnation, to build up the fabric of the body in all its parts and qualities. And we may suppose that, in consequence of some unnatural and morbid stimulus, perverted and erring action may be set up, and strange products result. It is not uncommon to find fat, hair, cholesterine, teeth, and other bones, in the diseased ovaria, even of virgins.

This view of the matter is strengthened by the fact, that dropsy of the *ovary* has not been known to *commence* before the age of puberty; nor often after the capability of child-bearing had ceased; but only, or chiefly, during that period in which the organ, if healthy, is susceptible of its proper and temporary function. Virgins, and barren and fruitful wives, are alike subject to the disease; but in what relative proportions, statistical inquiry has not yet (so far as I know) determined. Where it accompanies, it may also account for, sterility.

The catamenia during the progress of the malady sometimes appear with more or less of regularity and quantity; sometimes are entirely suspended. This function is so often interrupted, under other circumstances, that its derangements shed but little light upon cases that are otherwise obscure. When the discharge continues to recur, we may presume that one, at least, of the ovaries is in a tolerably healthy state: when both are sensibly diseased, the catamenia may be expected to be wanting.

The *treatment* of these two forms of abdominal dropsy must, up to a certain point, at which the operation of tapping becomes expedient, be considered separately. Of both it may be said, that their cure is seldom accomplished; yet, for reasons already assigned, ascites has, upon the whole, a more certain progress towards the destruction of life than ovarian disease; while, perhaps, it is oftener cured.

Treatment of passive ascites.—In passive ascites, when the distension of the peritoneum has crept on without pain, fever, or other marks of inflammatory action, our first and best hope of evacuating the collected liquid will rest upon diuretics. Hepatic ascites and renal disease may be sometimes found in conjunction, but according to my experience, they seldom are so: and except that both may probably owe their occasional origin to habits of intemperance, there appears no reason why they should be. Diuretics may be administered, therefore, without scruple. The hydragogue purgatives are to be employed, also, when diuretics fail to act, or to reduce the swelling; and when the disease is not already complicated with diarrhœa. And inferring with more or less certainty the existence of hepatic disease—sometimes from palpation of the enlarged or altered liver, sometimes from the coincidence of jaundice, but most of all from the result of accumulated experience respecting the morbid anatomy of such cases—we give the patient the chance of the remedial influence of mercury. The disorder being chronic, the introduction of that drug should be gradual. The iodide of potassium is held by some physicians to be especially serviceable in hepatic ascites. Compounds of mercury and iodine may be applied, by inunction, to the surface of the abdomen, and to the right hypochondrium in particular. In Germany, the muriate of ammonia is in much repute as a therapeutic agent. This diuretic salt, though seldom administered internally in this country, is believed by some practical men who have employed it, to exercise the same beneficial influence upon the functions of the liver, as is commonly attributed to preparations of mercury; while it is less productive of distress or inconvenience. My own experience upon this point is too limited to justify me in expressing any confident opinion about it; but in some recent instances I certainly have noticed a remarkable improvement in the character of the biliary excretion, after the daily exhibition of sal-ammoniac combined with the extract of taraxacum.

You will generally be obliged to try, in their turn, all the diuretics within your reach, and frequently to no purpose. Our efforts to remove by medicine the accumulated liquid, or to cure the morbid condition on which the accumulation depends, are too often made in vain. The distension of the peritoneum continues to augment; the distress arising therefrom becomes urgent and extreme; and at length, to afford temporary ease to the patient, and in the faint hope also of giving him permanent relief, we resort to the mechanical expedient of *paracentesis.*

Treatment of ovarian dropsy.—When we have the opportunity of treating *ovarian dropsy* from its commencement, we sometimes find that the enlarging ovary is painful, or tender. This is an indication for *antiphlogistic* measures. But from such remedies, or from any remedies, little more than temporary relief is to be expected. My position as physician to a hospital, has brought under my notice several cases of ovarian swelling, at a very early period of its development; when all that could be detected by careful examination of the abdomen was a small tumour, not larger, perhaps, than an egg, and occupying the situation of the ovary; to which the attention of the patient had been drawn by some pain or uneasy feeling in that part. I have treated such cases assiduously, with the remedies of chronic inflammation, frequent topical bleedings, and the use of mercury till the gums were affected; with the remedies of ordinary dropsy, diuretics and drastic purgatives; and with remedies accounted specific, the liquor potassæ, the various preparations of iodine; and I must honestly confess to you that I am unable to reckon one single instance of success. Yet these are the measures that we are bound to try. They have succeeded—as we are assured by competent and credible witnesses: they may therefore succeed again. The amount of my own experience, however, tends to the persuasion that medicine has, in general, very small influence over the progress of this disorder. The cases that do well, do well we scarcely know how or why; the cases that prove fatal run their course in spite of us.

Sometimes, as has been stated, these ovarian tumours reach a certain magnitude, and then (wherefore we cannot tell) enlarge no more; but remain a mere inconvenience and deformity, for many years. Occasionally they burst spontaneously, or in consequence of some accidental violence, and cause fatal peritonitis; or empty themselves harmlessly (adhesion having previously taken place) through some channel of communication with the bowels, or with the bladder; or externally through the parietes of the abdomen.

Tumours, supposed to be ovarian, do sometimes disappear entirely. It may, however, be doubted whether all, or even many, of the enlargements which have had this fortunate issue, were really connected with the ovary. One source of mistake I have myself more than once encountered, and I believe it to be not uncommon. A brief statement of the circumstances under which I first observed the fallacious symptom, will show you at once what I mean. Some years ago I was sent for by a lady, who for many days had been labouring under an ordinary attack of continued fever. While examining the abdomen by pressure, I discovered, on the right side, between the ilium and umbilicus, a round, hard, painless tumour, as big as a swan's egg. The patient was aware of it; and thought it had existed for some time. At the next visit it was gone. In the interim, very abundant discharges from the bowels had followed the administration of purgative medicine. The tumour had obviously been formed by the accumulation of fæcal matters in the cœcum.

Similar collections take place, less frequently, on the left side, just above the sigmoid flexure.

The parts concerned in this disorder are not essential to life, or to the enjoyment of health. On some of the lower animals, the operation of spaying is as customary in the one sex, and is performed with as little risk, as that of castration in the other. The ovaries have in several instances been extracted from the living human body, without any ill consequences. These facts, and the intractable character of the disease, have naturally suggested the expediency of extirpating the tumour in cases of ovarian dropsy.

But although the ovary, when healthy, or when not much enlarged, may be removed without much difficulty or hazard, the operation becomes always perilous, and often impracticable, when the altered gland has attained any considerable magnitude. Yet these are the very cases for which the remedy is needed. A large ovarian tumour is usually multilocular, with firm parietes, and thick, internal septa; and is therefore capable of collapsing much when punctured. To extirpate such a tumour, the abdomen must be (as it frequently has been) laid open from the sternum to the pubes. Most commonly, also, a large ovarian swelling is adherent to the contiguous parts; a circumstance which either makes the proposed removal of the tumour impossible, or, if the connections admit of being broken down, augments in a fearful manner the jeopardy of the patient. It is not surprising, therefore, that the results of experience have been so discouraging as almost to prohibit such attempts in future. The operation has, indeed, in some authentic cases, been quite successful. In other instances, the surgeon, after exposing the adherent mass, has been fain to replace the effused bowels, and to sow up the abdomen, as speedily as possible; and the patient, having suffered all this in vain, has been fortunate enough to escape with life. In others, the adhering tumour has been separated, and the object achieved; and the woman has lived thereafter for some hours. Three times, at least (one of the cases is recorded by Mr. Lizars, another by Mr. King, the third fell under the cognizance of Dr. Richard Bright), the abdominal muscles and the peritoneum have been slit open for the purpose of extracting a diseased ovary,—when no disease existed.

But of late, a modification of the process of excision—whereby it is adapted to certain forms or conditions of the disease—has been proposed and practised; and holds out somewhat more of promise.

It consists in making, not a long, but a small incision through the walls of the abdomen, and through the peritoneum, so as to bring the surface of the diseased ovary into view. The cyst is then secured, by means of a tenaculum, or of a ligature, from receding inwards, and punctured, and its contents are suffered to escape. When the sac has emptied itself, it is withdrawn through the external orifice by gentle traction, until its stalk, or place of attachment to the broad ligament, comes near the wound. A thread is tied round this stalk, the cyst is cut off, the uterine appendages are put back into the cavity of the abdomen, and the lips of the wound are brought together.

Although this method does not appear to have been actually *performed* till very recently, it had been *suggested* as long ago, at least, as the time of Dr. William Hunter, who, in a paper on the disease, has these remarks. "If it be proposed, indeed, to make such a wound in the belly as will admit only two fingers or so, and then to tap the bag, and draw it out, so as to bring its root or peduncle close to the wound of the belly, that

the surgeon may cut it without introducing his hand, surely in a case otherwise so desperate, it might be advisable to do it, could we beforehand know that the circumstances would admit of that treatment."

In these few words, Dr. William Hunter not only describes the mode of excision, but alludes to circumstances that may render it inadmissible, as well as to the uncertainty that may arise about the existence of those circumstances. What the circumstances are is sufficiently obvious.

However, this suggestion has been carried into successful practice by Mr. West, Mr. King, and others. To an interesting paper on the subject, published by Mr. Gorham, in the *Lancet*, is appended a summary account of ten cases; by which account it appears, that five of the ten patients were cured by the operation; two recovered from the attempt to extract, which was unsuccessful; two died very soon after the operation, and evidently in consequence of it. The remaining patient, whose case has already been adverted to, recovered also from the incision; but there was no diseased ovary to amputate: so fallacious, sometimes, is the diagnosis.

This mode of operating merits careful consideration, and further trial. Its recommendations are—

1. That the first steps—the incision and the puncture—are the same in kind as the first steps in ordinary paracentesis, and not much more severe or dangerous; the only difference being that the incision requires to be somewhat larger in the one case than in the other.

2. That, when successful, it affords a complete and permanent cure, which can hardly be hoped for from any other plan.

The objections to its general use are—

1. That the single sacs, with thin and flexible walls, do not, commonly, reach a very large size. When small, or of moderate dimensions, they produce so little distress or inconvenience, that an operation, which must always be uncertain and tentative, ought not to be recommended.

2. That the multilocular tumours, with solid walls and partitions, can scarcely be so drawn through the opening made into the abdomen.

3. And above all, that adhesion of the tumour to the neighbouring parts would interfere with and prevent the success of the operation.

In most, however, of the five cases of cure referred to by Mr. Gorham, the sacs thus removed were large. From one of them 12 pints of fluid were let out; from another, 27 pints; from a third, upwards of 20; and from a fourth, 24 pints.

It is difficult to guess beforehand whether the tumour be attached to the parts surrounding it or no. If it be readily movable by the fingers applied to the surface of the belly, so as to admit of being pushed hither and thither without pain or distress to the patient, it is probably unadherent. In one of the same five cases, the patient being in labour, Mr. West found that the tumour lay between the uterus and the brim of the pelvis. By gentle pressure, *per vaginam*, it was made to recede into the cavity of the abdomen. This afforded a presumption that it was free from adhesion; accordingly the sac was afterwards easily excised. The tumour is attached anteriorly to the peritoneum lining the front of the abdomen more frequently than to any other part. This Mr. Gorham attributes to partial inflammation produced by the puncture in the operation of tapping; hence the presumption that the cyst is loose is, *cæteris paribus*, the stronger, when paracentesis has never been performed.

If this mode of excision should be thought advisable, it ought to be attempted while the tumour is yet of moderate bulk, before the peritoneum has been much stretched, and while the chance of adhesion is the least.

To the similar operation of tapping, the contrary precept applies. Neither in ascites, nor in ovarian dropsy, should paracentesis be resorted to, until it seems absolutely indispensable. To this rule there are, in my opinion, very few exceptions.

The operation itself, though commonly esteemed a trivial one, is not without its dangers. The instances are not few in which it has been followed by fatal peritonitis, excited either by the mere passage of the lancet or trocar through a previously unhealthy mem-

brane, or (in the case of ovarian dropsy) by the escape of some portion of the contents of the cyst into the cavity of the abdomen. Formerly, the rapid evacuation of a large quantity of liquid from the belly was often attended by terrifying effects; fainting, convulsions, almost instant death. This made the ancient physicians afraid of the operation: and when they could no longer avoid it, they let the accumulated fluid out by little and little, and at short intervals.

The cause of these alarming symptoms is now well understood, and easily obviated. They were owing, doubtless, to the sudden removal of the pressure to which the viscera and large blood-vessels had been for some time submitted and accustomed. For this explanation of the fact we are indebted to the sagacity of our celebrated countryman, Dr. Mead, who was the first to suggest that external compression should be substituted, in lieu of the tension taken off by the operation. The complete success of that expedient fully justified his ingenious opinion; we now drain the cavity of its liquid contents without scruple or delay. A sheet, or broad roller, is thrown round the patient's body, and tightened as the fluid escapes, so as to maintain an equable pressure, which is continued for a while, and at length gradually withdrawn.

Other casualties occasionally happen; the trocar has sometimes pierced the intestine. In one instance which I myself witnessed, clear serum issued for some time through the canula, but at length pure blood; not less than a pint. The patient sunk; and no opportunity was given to investigate the cause of the bleeding. In another strange but well-authenticated case, the almost incredible quantity, twenty-six pints, of blood flowed out at the orifice made by the trocar, and afterwards separated into clot and serum. To the wonder of those who saw the incident, this patient recovered from the tapping; and the source of the hæmorrhage is still a matter of conjecture.

And apart from these mischances—which, after all, are not of frequent occurrence—you must bear in mind that paracentesis can seldom be contemplated as a mode of cure, but simply of temporary relief from distress. A few instances have happened where the liquid has been drawn off, and has not again collected: but such cases are very few. So, also, according to my experience, are those, much talked of by authors, in which the kidneys resume their activity upon the removal of the dropsical fluid. Ordinarily, the liquid re-accumulates, often with more rapidity than before; and again, and again, the pain and the hazard of the operation must be repeated: wherefore, in my judgment, paracentesis in abdominal dropsy ought seldom to be performed, unless the quantity of liquid is so great as to occasion painful distension; or causes great distress of breathing by its upward pressure against the diaphragm; or gives rise to some positive suffering or urgent inconvenience, which the evacuation of the water may be expected to remedy.

Acupuncture of the dropsical abdomen has of late been recommended; and cures, thus effected, have been announced. It is said, or supposed, that the inclosed liquid, oozing gradually into the cellular tissue of the integuments of the abdomen, is thence removed, gradually, by absorption. Of this method of treatment I have no practical knowledge.

It is seldom that tapping is many times performed upon the same person, when the complaint is mere passive ascites. The dropsy returns indeed, and again the operation is required: meanwhile, in most cases, the health and strength rapidly deteriorate, and the patient sinks.

The same speedy declension and early death occur sometimes in ovarian dropsy also; yet the operation commonly bears to be repeated more often than in ascites, without serious detriment to the general health. Sometimes the liquid re-accumulates in the cyst very quickly, sometimes slowly; in a very few instances not at all. I have had under my own care a patient who had been tapped for this disease thirty-eight or thirty-nine times. Extraordinary examples of a similar kind are on record; one or two I may mention as specimens.

Dr. Mead narrates the case of a lady, who, "for the information of posterity, ordered by her will that the following English inscription should be engraved on her monument, in Bunhill Fields:—

Here lies Dame Mary Page,
Relict of Sir Gregory Page, Bart.
She departed this life, March 4, 1728,
In the 56th year of her age.
In 67 months she was tapped 66 times:
Had taken away 240 gallons of water,
without ever repining at her case,
or ever fearing the operation.

Among authenticated instances, the most remarkable that I have met with is detailed in the *Philosophical Transactions* for 1784, by Mr. Martineau, who was at that time surgeon to the Norfolk and Norwich Hospital. An abstract of the case is given in the printed catalogue of the Hunterian Museum, where the cyst is preserved: it belonged to the left ovary of Sarah Kippus, a widow, fifty-five years old. "The complaint began after a miscarriage, at the age of twenty-seven. From the year 1757, to August, 1783, when she died, she had been tapped eighty times, and had, in all, had taken from her 6631 pints of fluid, or upwards of thirteen hogsheads. 108 pints was the largest quantity ever taken away at any one time. But after death, Mr. Martineau could not make the sac contain more than fifty pints."

Upon the whole, it may be stated of this operation, as applied to ovarian dropsy,

1. That when it is essential to the comfort and continued existence of the patient, it brings sensible relief to her distress, and often materially prolongs her life.

But, 2. That when it is performed under less pressing circumstances, it tends to shorten the patient's days. Dr. Bright is of opinion that the number is small of those who survive the first tapping more than four years. A respectable woman having very large ovarian dropsy, entered the Middlesex Hospital, under my care, for the express purpose of being tapped. The tumour incommoded her by its bulk and weight, but in no other way; and she had carried it for thirteen years. I felt that I should not be justified in sanctioning the operation in such a case. The patient was made to understand that the performance of it would not be altogether free from immediate danger; and that if she went through it safely, the swelling would return, and the same kind of remedy again become equally necessary. She was instructed how to suspend the heavy overhanging abdomen by a sling passing over her shoulders. There appeared no reason why she should not continue in good health for another period of thirteen years.

I am aware of another instance, in which a woman, similarly burdened, but otherwise in comfortable health, has lived, not without enjoying life, between twenty and thirty years. Had she been tapped when the mere enlargement might have seemed to justify the operation, she would probably have been for twenty years in her grave.

LECTURE LXVIII.

ACUTE GASTRITIS; SYMPTOMS; ANATOMICAL CHARACTERS; TREATMENT. CHRONIC INFLAMMATION OF THE STOMACH; THICKENING OF THE MUCOUS MEMBRANE; ULCERATION; SYMPTOMS AND TREATMENT OF THE DISORDER. CANCER OF THE STOMACH.

ACUTE inflammation, when it affects the peritoneum, usually spreads with rapidity over the whole surface of the membrane. This is characteristic of inflammation of the serous membranes generally. But it is not so with the other tissues that compose the alimentary canal. Inflammation of the mucous membrane may be, and often is, very limited in extent: and the different portions of the intestinal tube, as they differ in function, so also they differ somewhat in their diseases, and still more in the symptoms by which those diseases are revealed. Not being fettered by any artificial system of arrangement, I shall take the course which promises to be practically most useful, and consider separately the

maladies of the several parts of the alimentary canal in the abdomen, extending my remarks occasionally to the whole of the tube, when speaking of disorders that are common to all portions of it.

Let me, then, in the first place, draw your attention to the organic diseases, and the morbid conditions, of the *stomach*.

It is remarkable, all things considered, how seldom the stomach is affected with *acute inflammation*. Scarcely ever do we find either the organ as a whole, or any one of its tissues separately, the subject of *spontaneous* acute inflammation. What is described in books as *gastritis*, means inflammation of the *mucous membrane* of the stomach: and almost all that we know, for certain, of this disease, we derive from observation of the effects of strongly irritant substances upon that membrane. Idiopathic gastritis, in an acute form, I never saw. Acute gastritis, from the contact of corrosive or acrid poisons, I have frequently seen: and a highly interesting affection it then becomes. This is a subject that cannot be thoroughly discussed in this course of lectures: neither may it be altogether omitted.

Symptoms.—When an irritant poison has been received into the stomach and excites inflammation there—or when acute inflammation arises from any cause—the symptoms which mark that inflammation are pain, usually of a burning character, in the epigastrium; with frequent vomiting, especially upon the entrance of any thing into the stomach; and often with hiccup, and with tension of the upper part of the abdomen. To these local symptoms are added fever of a low type; and a small and weak pulse. At first, indeed, the pulse, although small, is generally sharp and hard; but it soon becomes thready and feeble. The muscular power undergoes a corresponding depression; the patient is pale and faint, with collapsed features, cold extremities, and a damp skin.

In all this we see a strong tendency to death by *asthenia*. It is clear that the subdued state of the circulation is dependent upon the inflammation, for it is often relieved by the remedies of inflammation. In acute gastritis, as well as in peritonitis, you will find that the pulse expands, becomes more distinct and full, under early bleeding: sometimes even while the blood is flowing.

Upon this remarkable sympathy between the heart and the stomach I have frequently had occasion to insist. You are aware that a smart blow upon the epigastrium may put a sudden stop to the movements of the heart, and induce mortal syncope; without leaving any local trace of its operation. On the other hand, a person in a state of extreme exhaustion and faintness, will sometimes revive at once, upon swallowing into the stomach an ounce or two of brandy, and recover his pulse and colour much too speedily, to allow of our ascribing these effects to the absorption of the alcohol into the blood. Dr. Alison suggests that the depression of the circulation may be attributable to the peculiar sickening pain which accompanies inflammation or sudden injury of the stomach. It appears, however, more probable (as has been intimated to me by an obliging correspondent) that the remarkable sympathy in question is governed by the nerves of organic life. The great solar plexus of the ganglionic system lies upon the spinal column immediately behind the stomach. The heart is largely supplied with nervous filaments from the same system. Hence we might almost expect that any sudden stimulus applied to this important plexus would excite, and that any sudden depressing influence would subdue, the natural action of the heart. Upon the same principle may be explained the facts that deadly faintness and nausea are apt to result from injury of the testes; which are also abundantly endowed with influence from the nerves of organic life. Be this as it may, it is important for you to know that the mode of dying in these cases is precisely what Bichat describes as *death beginning at the heart*.

The pain that accompanies gastritis is augmented by pressure upon the epigastrium. It is increased also by the full descent of the diaphragm, and the *breathing* is consequently short and constrained. In the most exquisite cases of gastritis, produced by chemical or mechanical irritants applied to the interior of the stomach, the inflammation probably reaches and involves, more or less, the peritoneum. The patients speak of the pain as a pricking and burning sensation; it is attended with great anxiety and restlessness. The sufferer is tormented with extreme thirst, while all that he drinks, even cold water, is almost instantly rejected by vomiting.

Hiccup does not always accompany acute gastritis. It sometimes occurs early; but more generally it comes on late in the disease, when the patient is sunk and much debilitated.

The bowels, in this complaint, are sometimes bound: sometimes, on the contrary—especially when the inflammation has been caused by corrosive poison—dysenteric diarrhœa ensues, with much griping and tenesmus.

Such, then, are the symptoms that indicate the existence of acute gastritis; but you ought to be aware that they occur in varying combinations, and with different degrees of severity; and consequently that the course of the disease is not uniformly the same in all cases. When the symptoms are the most violent, and the progress of the complaint is the most rapid, the peritoneal coat of the stomach is usually, I believe, more or less implicated.

Intense inflammation of the stomach may be expected to be rapid in its progress. It may destroy life within twenty-four, or even twelve hours. When it is fatal, it generally is so within a few days; and death takes place by fainting; with a remission of the pain, sometimes very sudden, and sometimes occurring only just before dissolution. But as idiopathic gastritis is rare, *fatal* idiopathic gastritis is, of course, still more so. Louis states, that during six years' experience at La Charité, in which period he noted the details of 6,000 cases of disease, and of 500 dissections, he did not meet with a single instance of fatal idiopathic gastritis. The subject derives almost all its importance, therefore, from its connection with poisoning; and the many interesting points of inquiry which arise out of that connection will be brought before you by the Professor of Forensic Medicine. This consideration is a great satisfaction and relief to me; because I find that the limits of my own course will not permit me to go into any detail in this matter.

Anatomical characters.—The morbid appearances to be looked for after death by acute gastritis, are redness of the mucous membrane, softening, sloughing, and even (after the action of strongly corrosive poisons) perforation of all the coats of the stomach.

I wish particularly to caution you against being misled by mere *redness* of the interior of the stomach; or of the inner surface of the alimentary canal in general; or of *any* mucous membrane; and indeed I may add, of any *serous* membrane also. Redness and inflammation have been made, too often, convertible terms. Persons finding the inner surface of the stomach red, have hastily concluded, from that circumstance, that suspicions of poisoning, which had arisen, were well founded. We are indebted to Dr. Yelloly, in the first instance, and to M. Billard and some other Frenchmen, in the second, for correcting this error—an error which not only was of importance in questions of imputed poisoning, but has run through and vitiated almost the whole of pathology, both lately and heretofore. Mistaking mere redness for evidence of inflammation, Cullen divided gastritis into two species—one of which he called gastritis *erythematica;* and he inferred from the observation of cases in which redness of the membrane had been met with after death, that this peculiar kind of inflammation of the mucous coat of the stomach might take place, without fever, pain, or vomiting, or any other symptom indicative of gastritis: whereas it is almost certain that, in the cases to which he refers, there really was no inflammation at all. So also Morgagni, puzzled by intestinal vascularity, was disposed to attribute the absence of pain, in what he believed to have been inflammation of the bowels, to a paralytic affection which blunted the sensibility of the parts; and Haller conceived, from so constantly meeting with this vascularity in *his* inspections of the body, that inflammation of the bowels was almost always present in fevers of all kinds; and was frequent in every other complaint. And the same doctrine has been strenuously inculcated of late years, as I dare say you know, by Broussais, in France, and adopted by a vast host of his disciples. Finding the lining membrane of the stomach and intestines red and vascular in most of the bodies of patients who had died of fever, Broussais concluded that fever depends, in all cases, upon inflammation of the gastro-enteric mucous membrane. You will perceive that this doctrine must exercise a vast influence upon the *practice* of those who entertain it. If inflammation constitute an essential part of any disorder, it follows that the *remedies* of inflammation will be adapted to that disorder; and thus, even so slight a mistake as that may appear to be, against which I am now cautioning you, of regarding every surface which is red as being inflamed also, may lead to very mischievous views in respect to treatment.

The redness that is *in*dependent of inflammation may be of various kinds; but the principal cause of it is venous congestion. "The appearances of vascular fulness (says Dr. Yelloly) in the villous coat of the stomach, whether florid or dark coloured, in distinct vessels, or in extravasations of different sizes, are not to be regarded as unequivocal marks of disease; inasmuch as they occur in every variety of degree and character, under every circumstance of previous indisposition, and in situations where the most healthy aspect of the organ may be expected." To the truth of this statement I can bear witness, having at one time of my life carefully examined, with a view to this matter, a great number of stomachs in succession, in the dead house of a large hospital. "The vascularity (according to Dr. Yelloly) is entirely *venous*, and depends on a power capable of being exercised on the artery itself at the close of life, which carries on the blood to the veins, after the further supply of fresh blood from the heart is stopped. The branched or stellated form of vessels, under which the vascularity usually appears, is capable of being *imitated*, either by injecting the veins with fine injection, or by forcing back with the finger, or the back of a scalpel, the blood from the larger branches of veins into the smaller." "And this vascularity soon becomes diffused redness, by transudation of the blood through the coats of the containing vessels, just as happens with the bile in the gall-bladder."

Redness, from mere repletion of the smaller veins, is usually extensive and undefined; except that, being influenced by the force of gravity, it settles into the most depending parts of the organ, which are either its exclusive seat, or at any rate are of a deeper colour than the parts more elevated. It is attended with an empty state of the arteries, and with a full state of the larger veins. Hence the condition of the venous and arterial trunks, and especially of the vena portæ, should, in doubtful cases, be ascertained before the main blood-vessels are laid open and drained of their contents.

The redness that belongs to inflammation is generally circumscribed, and of limited extent; it occupies indiscriminately the upper or the lower side of the tube (for these remarks apply alike to the stomach and to the intestines); it is attended with some fulness of the corresponding arterial trunks; and it may or may not be coincident with comparative emptiness of the venous system within the abdomen. Much will depend, in this respect, upon the *mode of dying*, as I have fully explained to you on a former occasion.

You will please to remember, then, in all your future investigations into morbid anatomy, that it is generally difficult, and often impossible, to determine, from the aspect of the vessels of a dead part, from its redness, that inflammation had been present in that part during life, unless the unequivocal products or effects of inflammatory action are present also.

A much more certain evidence of inflammation of the mucous membrane of the stomach and intestines, is its *softening*. This can be attributed to nothing else, except it be to decomposition; and it is well known that this membrane is slow and late in passing into the state of putrefaction after death. To certain questions respecting perforations of the stomach I shall by and by return.

Causes.—I say that gastritis is most commonly the effect of poisons applied to the mucous surface of the stomach; but I must include under that head certain substances, which, to most people, are not poisonous or injurious at all, and which only become so to some persons under particular circumstances. Thus, large draughts of cold water, taken when the body is hot, and rapidly parting with its heat, and especially large draughts of cold *sour* liquors, as cider or stale beer, are apt to give rise to acute gastritis. Another occasional cause of gastritis is the ingestion of very large quantities of food at one time, especially during convalescence from any serious disorder. It is an exceedingly curious fact, too, but one which I merely mention without dwelling upon, that certain poisons introduced into the body through some other channel, will cause inflammation of the mucous membrane of the stomach, with which they have *not* been in contact. Corrosive sublimate, and arsenic, excite inflammation, with ulceration or sloughing of the mucous membrane, even when they are merely rubbed, in a certain quantity, upon the skin; or when they are inserted into the rectum.

Treatment.—The treatment of acute gastritis is simple. The chief nicety respects the employment of blood-letting. Early in the disease, if the pain be severe, you must *try* the effect of venesection, notwithstanding the smallness and feebleness of the pulse. How

much blood you are to abstract cannot be told beforehand. Take away a small tea-cupful, keeping, meanwhile, your finger on the wrist. If the beat of the artery does not grow weaker, and still more if it becomes fuller and stronger, go on with the bleeding, and take another cupful, and another, according to the circumstances of the case and to the effects produced. Apply leeches to the epigastrium, and cover the bleeding bites with a soft, light poultice. Keep the patient as strictly as you can, in the horizontal posture: in other words, see that the depressing influence of the disease upon the action of the heart is not aided and augmented by the position of the body. If cold water is retained, that is the best medicine which you can give by the mouth; purgatives so administered, would be almost sure to be rejected; and if not rejected, they would be likely to increase the existing inflammation of the organ. Enemata are, however, extremely useful: of warm water, if the bowels are not much confined; of purgative materials if they are. After the intestines have been thus cleared—or when they are loose and irritable—opiate injections (thirty or forty drops of laudanum, with three or four ounces of starch or gruel) do much good. They often have a tranquillizing effect upon the irritable stomach, and check the vomiting. These measures are to be pursued until the inflammation has subsided.

When any *corrosive* substance has been swallowed, I scarcely need say that pains should be taken to remove it as speedily as possible from the stomach; or to administer such remedies as are known to be capable of decomposing the poison, or of affording a specific antidote to it. Not that the stomach-pump should be employed in such cases, as it too often is. These, however, are points that must be fully treated of in the lectures on forensic medicine, and therefore I shall dwell upon them no longer here.

Chronic inflammation of the stomach is probably a very common disorder. It does not put life in immediate jeopardy; and it is often recovered from. It deranges, however, the functions, and perverts the feelings of the stomach: it gives rise to the manifold and multiform symptoms of *dyspepsia*. But dyspepsia, with its manifold and multiform symptoms, may be, and often is, entirely independent of inflammation. You see, then, why the effects of chronic gastritis are various; and why the symptoms that are supposed to denote its presence are apt to be obscure, uncertain, and equivocal. I intend, before I quit the subject of the stomach, to investigate the principal circumstances that mark its functional disorders, and to describe the means which we sometimes find effectual for their relief. I shall therefore restrict myself at present to a few points which seem to have been fairly ascertained respecting chronic gastritis.

Anatomical characters.—We know that chronic inflammation had been going on in the stomach when, after death, we see that its coats are thickened; or when we perceive that a portion of one or more of them has been removed by ulceration.

It is not at all uncommon to find the mucous membrane of the stomach, over a larger or smaller space, thick, granular, uneven, and of an unnatural colour. Gray, or slate-coloured, it often is. This slate colour is much dwelt upon by the French writers, as being a sure and unequivocal impress of chronic inflammation. The colour proceeds, I believe, from the operation of the gastric acids upon the blood, which, under habitual congestion or slow inflammation, is detained in the vessels of the altered part. The ulcers that result from chronic inflammatory action are usually small, varying from the size of a split pea to that of a shilling; sometimes with no surrounding vascularity or thickening at all, but looking exactly as though a piece of the mucous membrane had been struck out by a stamp; sometimes with rounded and elevated *edges* only; and sometimes they occupy *patches* of thickening and induration of the parietes of the stomach. There may be one solitary ulcer; or a few; or many. It is seldom, however, that they are numerous.

Ulcerative disease of the stomach may prove fatal in various ways. The ulcer may penetrate as far as the peritoneum, and excite inflammation of that membrane, whereby the stomach becomes adherent to the neighbouring parts. In these cases, prior or subsequently to adhesion, death may at length ensue, from gradual exhaustion and protracted suffering.

If an ulcer happen to lie over the track of a large blood-vessel in the stomach, it may eat its way into that vessel, and give rise to fatal hæmorrhage.

Or the ulcer may perforate the walls of the stomach, without any previous adhesion, and suffer the food, or the secretions of the stomach, to pass into the peritoneal cavity, where intense inflammation is lighted up, and the patient soon perishes.

Or the ulcers may *heal*. Of this we are certain, because we sometimes find cicatrices marking the spots which the ulcers had occupied.

The *symptoms* of chronic gastritis are pain or uneasiness in the epigastrium, increased by pressure; increased also on the introduction of food, or perhaps felt only while digestion is in progress; flatulence and eructation; vomiting of mucus, and of the meals; loss of sleep; languor and debility.

Not that even these symptoms are constant in all cases. Sometimes there is no actual pain, but a sense of heat or of acidity. The vomiting too, in the outset, is occasional only; and is then attributable, often, to some error or imprudence in respect to diet. And sometimes the complaint may run nearly its whole course, up to one of the fatal terminations just now mentioned, with scarcely any other sign of its existence than uneasiness after meals, which subsides in two or three hours entirely. With the local symptoms I have been describing there are often conjoined some acceleration and hardness of the pulse: more or less thirst: a dry skin: scanty and deep-coloured urine: a red tongue; red especially at its tips and edges; patchy and fissured perhaps; or smooth and glossy, like a slice of raw meat. The throat also is frequently tender; and the pharynx and palate unnaturally vascular.

Treatment.—In attempting to relieve or cure chronic gastritis we have to guard against applying any thing to the inflamed surface which may be *likely* to add to the existing mischief, or which has been found, upon trial, to give pain. We employ at the same time the ordinary remedies of chronic inflammation. Leeches may be put upon the epigastrium, and repeated day by day, or every other day, in small numbers, so long as there is much tenderness on pressure. When the tenderness is less, counter-irritation is very useful: repeated blisters; or friction with the tartar emetic ointment over the region of the stomach. But, after all, the main dependence must be placed in the due regulation of the food, which should be mild and unstimulating in quality, and sparing in quantity. The well known farinaceous substances which figure in the bill of fare of a sick chamber: arrow-root; sago; tapioca; gruel: milk also; and jellies. And even these bland articles of nourishment must be given in moderation, so as never to distend the stomach or stretch its coats by their bulk, or to overtask its power of digestion. It is difficult, in such cases, to give precise rules for the management of the diet, which must be left to the common sense of the practitioner.

With respect to internal medicines, they must be such as appear to be called for by particular symptoms. Of these, which are in fact the symptoms of dyspepsia, I postpone the further consideration. Appropriate means must be used for the regulation of the bowels: mild laxatives by the mouth; emollient or purgative enemata by the rectum.

When I say that a good deal must be left, in these cases of chronic disease of the stomach, to the judgment and good sense of the practitioner, I am forcibly reminded of a most striking and instructive case, beautifully told by the celebrated Dr. William Hunter, in the sixth volume of the *Medical Observations and Inquiries*. The perusal of that history has afforded me hints upon which I have often acted with great advantage to my patients, and with some credit to myself. As I doubt whether many of you would find immediate opportunity or leisure for referring to the narrative, and as I should spoil it by attempting to give an abstract of it, I am tempted to read it here in Dr. Hunter's own words.

"Many years ago (he says) a gentleman came to me from the eastern part of the city with his son, about eight or nine years old, to ask my advice for him. The complaint was great pain in the stomach, frequent and violent vomitings, great weakness, and wasting of flesh. I think I hardly ever saw a human creature more emaciated, or with a look more expressive of being near the end of all the miseries of life. The disorder was of some months standing, and from the beginning to that time had been daily growing more desperate. He was at school when first taken ill, and concealed his disorder for some time: but growing much worse he was compelled to complain, and was brought home to be more carefully attended. From his sickly look, his total loss of appetite, besides what

he said of the pain which he suffered, but especially from his vomiting up almost every thing which he swallowed, it was evident that his disorder was very serious.

"Three of the most eminent physicians of that time attended him in succession; and tried a variety of medicines without the least good effect. They had all, as the father told me, after sufficient trial, given the patient up, having nothing further to propose. The last prescription was a pill of solid opium; for in the fluid state, though at first the opiate had staid some time upon his stomach, and brought a temporary relief, it failed at length, and like food, drink, and every medicine which had been given, was presently brought up again by vomiting. The opiate *pill* was therefore given in hopes that it would elude the expulsive efforts of the stomach. It did so for a time; but after a little use, *that* likewise brought on vomiting. Then it was that his physician was consulted for the last time, who said that he had nothing further to propose.

"Though at first the boy professed that he could assign no cause for his complaint, being strictly interrogated by his father, if he had ever swallowed any thing that could hurt his stomach, or received any injury by a blow, or otherwise, he confessed that the usher in the school had grasped him by the waistcoat at the pit of the stomach, in a peevish fit, and shaken him rudely, for not having come up to the usher's expectation in a school exercise. That though it was not very painful at the time, the disorder came on soon after. This account disposed the father to suspect that the rude grasp and shake had hurt the stomach. With that idea he brought him to me as an anatomist, that an accurate examination might if possible discover the cause or nature of the disorder.

"He was stripped before the fire, and examined with attention in various situations and postures; but no fulness, hardness, or tumour whatever could be discovered; on the contrary, he appeared everywhere like a skeleton covered with a mere skin; and the abdomen was as flat, or rather as much drawn inwards, as if it had not contained half the usual quantity of bowels.

"Having received all the information that I could expect, and reflected some little time upon the case, I wished to speak with the father alone, in another room; and to give my patient some employment as well as refreshment, asked him to take a little milk in the mean time. But his father begged that taking any thing into his stomach might be put off till he got home, because he was certain that it would make him sick; 'just before we set out (said he) I gave him a little milk; but he was sick, and brought it all up in the coach, before we had got many paces from the house.'

"In the adjacent room I said to the father, This case, sir, appears to me so desperate, that I could not tell you my thoughts before your son. I think it most probable, no doubt, that he will sink under it; I believe that no human sagacity or experience could pretend to ascertain the cause of his complaint: and without supposing a particular or specific cause, there is hardly anything to be *aimed at* in the way of cure. Yet, dreadful as this language must be to your ear, I think you are not to be without hope. As we do not know the cause, it may happen to be of a temporary nature, and may of itself take a favourable turn; we see such wonderful changes every day, in cases that appear the most desperate, and especially in young people. In them the resources of nature are astonishing.

"Then he asked me if I could communicate any rules or directions, for giving him a better chance of getting that cure from nature, which he saw he must despair of from art.

"I told him that there were two things which I would recommend. The first was not so important indeed, yet I thought it might be useful, and certainly could do no harm. It was to have his son well rubbed, for half an hour together, with warm oil and a warm hand, before a fire, over and all around his stomach, every morning and evening. The oil, perhaps, would do little more than make the friction harmless, as well as easy; and the friction would both soothe pain, and be a healthful exercise to a weak body.

"The second thing that I had to propose, I imagined to be of the utmost consequence. It was something which I had particularly attended to in the disorders of the stomach, especially vomitings. It was, carefully to avoid offending a very weak stomach, either with the quantity, or quality, of what is taken down; and yet to get enough retained for supporting life. I need not tell you, sir, said I, that your son cannot live long, without taking *some* nourishment; he must be supported to allow of any chance in his favour.

You think that for some time he has kept nothing of what he swallowed; but a small part must have remained, else he could not have lived till now. Do you not think, then, that it would have been better for him if he had only taken the very small quantity which remained with him, and was converted to nourishment? It would have answered the end of supporting life as well, and perhaps have saved him such constant distress of being sick, and of vomiting. The nourishment which he takes should not only be in very small quantity at a time, but in quality the most inoffensive to a weak stomach that can be found. Milk is that kind of nourishment. It is what Providence has contrived for supporting animals in the most tender stage of life. Take your son home, and as soon as he has rested a little, give him *one* spoonful of milk. If he keeps it some time, without sickness or vomiting, repeat the meal, and so on. If he vomits it, after a little rest, try him with a smaller quantity, viz., with a dessert, or even a teaspoonful. If he can but bear the smallest quantity, you will be sure of being able to give him nourishment. Let it be the sole business of one person to feed him. If you succeed in the beginning, persevere with great caution, and proceed very gradually to a greater quantity, and to *other* fluid food, especially to what his own fancy may invite him; such as smooth gruel, or panada, milk boiled with a little flour of wheat or rice; thin chocolate and milk; any broth without fat, or with a little jelly or rice or barley in it, &c. &c.

"We then went in to our patient again; and that he might be encouraged with hope, and act his part with resolution, I repeated the directions with an air of being confident of success. The plan was simple, and perfectly understood. They left me.

"I heard nothing of the case till, I believe, between two and three months after. His father came to me with a most joyful countenance, and with kind expressions of gratitude told me, that the plan had been pursued with scrupulous exactness, and with astonishing success; that his son had never vomited since I had seen him; that he was daily gaining flesh, and strength, and colour, and spirits, and now grown very importunate to have more substantial food. I recommended a change to be made by degrees. He recovered completely; and many years ago he was a healthy and a very strong young man."

Carcinoma of the stomach.—The stomach is very frequently the seat of specific malignant disease; of cancer, in its various forms and denominations. The fatal nature of this complaint; the obscurity in which it is sometimes wrapped; the possibility of overlooking it altogether, or or confounding it with disease of a more innocent character, combine to invest it with peculiar interest.

Carcinoma of the stomach has sometimes no symptoms at all, or none which the most sagacious practitioner would refer to the organ affected. Not long since I saw, in consultation, an elderly clergyman, who complained of pains in his back, which were brought on or aggravated by certain movements of the body. His bowels were costive; and purgatives always relieved his pains. He was passing lithic acid gravel. The pains were felt in or near the renal region. Several years before he had suffered in a similar manner; and had then been cured by being cupped in the loins. What was the matter here? Was it lumbago? Was there a calculus in one of his kidneys? These were the best guesses that I could make. The eminent physician whom I met, and a surgeon of no less eminence, who had seen the patient previously, had not been able to attain any more exact diagnosis. Upon this gentleman's death, which occurred not long afterwards, his disorder was discovered to have been cancer of the stomach. Excepting slight sickness a day or two before he died, there had been no symptom to direct attention to that part.

A young woman came into the Middlesex Hospital, under one of my colleagues, with a pulsating tumour in her epigastrium. It was thought, at first, to be an aneurism, and the case attracted, on that account, a good deal of notice. But the tumour subsided very much after free purgation. This led some to suppose that it was formed by accumulated fæces in the transverse colon. There was no sickness; nor indeed any one symptom referable to the stomach. She died. The tumour was cancerous; and in the stomach. Lying in front of the abdominal aorta, it had been lifted by its pulsations.

Cases to the same effect are related by Dr. Seymour, in the *Medico-Chirurgical Transactions;* and by M. Andral, in his *Clinique Médicale.*

But even when the stomach is the organ pointed out, by the symptoms, as the probable

seat of the malady, those symptoms fail, often, to indicate with any certainty its nature. The effects of the carcinomatous disease exhibit no uniformity. The ingestion of food is apt to produce great distress; but differently in different cases: sometimes as soon as the food is swallowed; sometimes not for an hour or two afterwards. Some cases are attended with much pain; some with none at all. One patient vomits continually; another has little or no vomiting from first to last.

Can these differences be in any way accounted for? Partly they may. By analyzing case after case, we approximate to a knowledge of their causes. But this knowledge is yet far from being complete.

One circumstance that has a considerable influence upon the symptoms, is the *situation* of the disease. In respect to this point there are certain general rules which are for the most part true. Still we can speak of them only as applicable *on the average;* they are not absolute or infallible.

The rules I mean are these:—

1st. That there is more suffering, *cæteris paribus*, when the cancerous disease is situated at, or very near, either extremity or orifice of the stomach, than when it occupies the intermediate parts; whether in the greater, or in the lesser curvature.

2nd. That when the cardia, and its immediate neighbourhood, is the part solely or principally diseased, the food and drink find a hindrance in passing into the stomach; but being once there, the distress is over. The symptoms are very like those of stricture of the œsophagus. The morsel reaches the bottom of that tube, and there causes uneasiness, till at length it is brought up again through the mouth, or passes gradually in the natural direction.

3d. That when, on the other hand, the disease is limited to the pyloric end of the stomach, the food enters that bag readily enough, and remains there for a certain time; then uneasy sensations arise, and the imperfectly digested meal is apt to be rejected by vomiting.

It is the difficulty of passing the *doorway* in these cases, that gives rise to the principal suffering; the difficulty of getting into, or the difficulty of getting out of the stomach. But when the disease is confined to the intermediate space, no such difficulty occurs; and therefore little or no pain.

You must expect, I say, to meet with individual variations from these rules. A remarkable example of such variation was presented by one of my hospital patients, in the year 1837. I have the notes of that case before me, which I will read *short*.

Simon Ailes, aged thirty-six, admitted March 14. His main complaint was of pain in the epigastrium, always present, but augmented, in frequent paroxysms, to an extreme degree of severity. At first, pressure gave him some relief. The pain was most violent an hour or two after he ate. He was troubled also with flatulence, and with sour eructations. Occasionally a clear tasteless fluid, looking like water, rose into his mouth. His bowels were costive.

At this time his countenance was natural and placid; but it gradually assumed that pinched and anxious expression, and that peculiar yellowish hue, which are so significant of organic visceral disease. He wasted fast. At length the epigastrium became tender as well as painful: but no tumour, except the left edge of the liver, could be felt there. He died on the 11th of May, about eight months from the commencement of the pain. A week before his death he vomited some dark, grumous, offensive fluid, evidently containing blood. With this exception he had no vomiting.

Many remedies were tried, which I do not specify, for none of them gave him any sensible or continued relief.

In the smaller curvature of the stomach we found a ragged, sloughy surface, as big as the palm of one's hand, and extending to within half an inch from the pylorus. A section of this diseased portion exhibited the characters of true scirrhus: a white and hard mass, nearly half an inch across at its thickest part. The mucous membrane of the duodenum was congested, and dark coloured. The rest of the intestines were healthy except the rectum, which was surrounded, towards the anus, by scirrhus and thickened cellular tissue, intermixed in laminæ. The gut itself was not affected.

The diseased stomach was removed, and examined by Mr. Kiernan, who found, upon

careful dissection, that the trunk of the gastric branch of the par vagum ran directly into, and was lost in, the scirrhous mass. This sufficiently accounted for the dreadful sufferings of the patient.

And I am here reminded that, with regard to the structural alteration itself, there are some circumstances well worth attending to.

Andral places all these organic affections of the stomach in the class of chronic gastritis. But it is clear that he is wrong: and you will perceive at once that it is of immense importance to recognize the specific disease from the mere result of common inflammation.

But though cancer is not, in any case, a mere product of common inflammation; neither is all that is called cancer really such. Specimens of morbid texture, misnamed scirrhus of the pylorus, are not uncommon in anatomical museums. I show you some from our own; not so denominated, however. The correct labelling would be *hypertrophy*. You may perceive that the cellular and the muscular tissues near the pylorus, are very much thickened. A section of the thickened parts presents an appearance somewhat like horn; and is crossed by whitish lines that run nearly parallel to each other. The morbid structure is quite definite and uniform; and very dissimilar, in that respect, to the irregular masses of scirrhus, and to the amorphous deposits of encephaloid cancer. Neither does it at all resemble that of the colloid variety of carcinoma. Changes of this kind are liable to occur in the muscular tissue of this, as of other organs, whenever a permanent obstacle is opposed to the onward progress of the contents of the hollow viscus. The impediment may have been originally produced by inflammatory thickening of the textures composing the pylorus; and then the hypertrophy may, in a certain sense, be accounted a consequence of inflammation. To that extent alone is M. Andral right. He has unquestionably pushed his theory on this subject too far. Whatever narrows the pyloric orifice leads to increased effort of the propelling muscle, and to augmentation of its bulk and power. Now cancer itself, situated at, or close upon, the pylorus, may impede the exit of the digested aliment; and then it causes a gradual hypertrophy of the muscular coat. In these cases there is a mixture of the two changes; of the cancerous growth, with the muscular hypertrophy: and this is one reason why they have been confounded together. Here are several preparations, exhibiting true cancer of the stomach. At the bottom of each bottle may be seen a sort of whitish powder, or sediment, consisting of some of the matters peculiar to cancer. This fact has been pointed out to me by Mr. Kiernan in the numerous specimens contained in his private collection. But there is no such deposit when the hypertrophy is not combined with malignant disease.

It has long been thought and asserted, that cancer of the stomach is not so apt to be attended, as cancer of other parts, with a disposition to present itself in various organs of the body at the same time, or in succession. Now I believe—and I am glad to add the weight of Mr. Kiernan's authority to my statement—that this is not really so. Cancer is a constitutional affection: or, if local and solitary in the outset, is prone to disseminate itself. The error has arisen out of that confounding of one morbid condition with another, against which I have been warning you. Instances are not at all uncommon of thickening of the cellular and mucous tissues about the pylorus, producing first a mechanical impediment to the passage of the food, then more energetic muscular efforts towards its expulsion from the stomach, and at last hypertrophy of the muscular tunic. In these cases, you do not find cancer in other organs: because, in fact, there is no cancer in the stomach.

You may say that as both forms of disease are alike fatal, it signifies nothing whether there be really cancer or not. But it is always satisfactory to clear away an apparent anomaly, and to show that it has no real existence. Besides, you know with how much anxiety the relatives of the dead inquire concerning these matters. That cancer "runs in families" is well understood even by the public: and the distinctions I have been pointing out are surely worth learning, if they do no more than enable us to comfort the minds of survivors, and to relieve them from the apprehension that they also may be doomed or likely to become the victims of cancer.

Notwithstanding what I have now been saying, it must be allowed that genuine cancer of the stomach is accompanied less frequently than some other modes of carcinomatous

disease by cancer elsewhere. In fact cancer of the stomach is most often of the scirrhous variety, which until it softens is not so readily disseminated as the others, and which is apt to prove fatal before it softens.

When primary cancer of the stomach is of the encephaloid kind,—or when, being scirrhus, it begins to grow soft,—you would expect, after what was stated respecting suppurative phlebitis, that its secondary manifestation should occur chiefly in the *liver*. And it is so: the reason being that the veins of the stomach communicate with the general system not directly, but through the hepatic capillary system.

But to return to the *symptoms* of carcinoma of the stomach. In some cases, I say, the food is rejected by vomiting; in some cases it is not. Now it has been argued that this difference depends upon the condition of the pyloric outlet; whether it be free and open, or contracted and shut. The explanation is more plausible than sound. It is not strictly consistent with facts. Vomiting of the food has been an urgent symptom, when there was no mechanical bar to its passage into the duodenum. The pylorus is a sphincter muscle, of which the natural and habitual state is that of contraction. It yields, however, in health, to the pressure of the digested aliment, which is driven forwards by the muscular fibres that surround, and compress by their action, *the pyloric end of the stomach*. If there be a mechanical impediment, that affords a sufficient reason why the food should be thrown up again. But sometimes, I repeat, the orifice is wide open, and yet the food is rejected: and it is rejected because the disease so involves the pyloric end of the stomach, that the propelling force cannot be exercised.

When there is a mechanical obstacle, the disposition to hypertrophy of the muscular coat is conservative. But in feeble and delicate persons, the baffled muscles may never acquire strength enough to overcome the impediment; and then the very opposite condition is apt to take place: the coats all become very thin; meal after meal is retained; the stomach is enormously distended, and relieves itself now and then, at distant intervals, by copious vomiting; until at last it is unequal to that effort, and the patient dies.

Sometimes the sickness and vomiting are urgent even when the stomach contains no food: and the matters rejected are of various character and appearance. They often resemble coffee-grounds, and consist, no doubt, of altered blood. Vomiting of this kind is a very pregnant sign of *organic* mischief in the stomach.

Emaciation is another ugly circumstance in these cases; and forms a strong ground of presumption that the symptoms depend upon structural disease. Yet it is not a uniform consequence, even of malignant disorganization of the stomach. Napoleon Bonaparte was very fat when he died. His omentum is described as having been "remarkably fat:" and "the fat was upwards of an inch thick upon his sternum, and one inch and a half upon his abdomen."

The existence of a palpable tumour strengthens the unfavourable diagnosis. But this is far from being a constant phenomenon. It is not even pathognomonic when it does occur. The diseased head of the pancreas has been mistaken for a thickened pylorus. The stomach is liable also to be dragged much out of its place; and then a thickened pylorus may be mistaken for something else. Sometimes the form of the stomach may be distinctly traced. In the person of a medical practitioner who died lately in this neighbourhood, the shape of the organ, its occasional peristaltic motions, and the irregular and hardened pylorus, were plainly to be felt. They might indeed almost be *seen*, in the hollow and attenuated abdomen. When a tumour is ascertained to belong to the stomach, it indicates disease of the pylorus rather than of the cardia.

It is a curious feature in these malignant diseases of the stomach, that the symptoms sometimes remit, in a remarkable manner, so as to excite a hope in the mind of the patient, and in that of his medical attendant, that the nature of the malady had been mistaken, and that recovery is about to take place. But the truce is not for long. Frightful disorganization is at length produced, ragged ulceration, perforation of the coats of the stomach, adhesion to the parts adjacent, which thus are constituted adventitious walls;—and inevitable death at last.

The treatment of this dreadful complaint can only be palliative. If there be pain, we are driven, sooner or later, to opium. Anodyne enemata have often as good an effect in relieving the pain as opium given by the mouth; and they have this advantage, that their

constipating properties are more easily obviated than when that drug is put into the stomach. Nutritive injections are proper when food taken through the natural channel is not retained.

Other palliative measures may be aimed at particular symptoms: of these I propose to speak when I come to the symptoms and remedies of *dyspepsia*.

LECTURE LXIX.

HÆMORRHAGE FROM THE STOMACH: SOMETIMES FROM A LARGE VESSEL, USUALLY BY EXHALATION. IDIOPATHIC HÆMATEMESIS. VICARIOUS HÆMATEMESIS; HÆMATEMESIS FROM GASTRIC DISEASE OR INJURY: FROM DISEASE IN OTHER ORGANS. MELÆNA. HÆMATEMESIS FROM A MORBID STATE OF THE BLOOD. GENERAL PHENOMENA OF HÆMATEMESIS. DIAGNOSIS. TREATMENT.

HÆMORRHAGE from the stomach, to which I wish next to direct your attention, is of much more frequent occurrence than acute gastritis. It is a complaint, or a symptom, that presents several points of interest and importance. I use the phrase "hæmorrhage from the stomach," rather than the single term "hæmatemesis," because that term, signifying strictly a *vomiting* of blood, does not necessarily imply hæmorrhage *from the stomach;* nor, indeed, does it always accompany such hæmorrhage, although it is one of its most common and most striking symptoms.

What I have so frequently mentioned in respect to hæmorrhages from the mucous membranes generally, viz., that the efflux of the blood is seldom owing to the rupture of a large blood-vessel, holds true in this. It can rarely happen that any vein or artery belonging to the stomach is divided or laid open by accidental injury, so as to pour out its blood. When hæmorrhage does proceed from one or more of the larger blood-vessels, the opening by which the blood escapes is commonly the result of chronic ulceration; such as I spoke of yesterday. I have not, myself, met with an instance of that kind. Andral states, that, at most, there are but five or six such cases to be found in the records of medicine. Since his work on *Pathological Anatomy* was published, another example of the same lesion has been detailed in the *Journal Hebdomadaire* for May, 1830. I show you a drawing after Dr. Carswell, representing an ulcer which laid open the coronary artery of the stomach, and caused fatal hæmorrhage. In these cases, we have, first, the symptoms of chronic gastritis; next, faintness, or actual syncope, from the sudden abstraction of a large quantity of the vital fluid; and lastly, the visible eruption of the blood itself; for vomiting seldom occurs until a large quantity of blood has accumulated in the stomach.

As this variety of gastric hæmorrhage is rare, I will briefly relate an instance of it, which occurred in the year 1831, at St. Bartholomew's Hospital. Dr. Latham, who had charge of the case, was good enough, some time ago, to give me the following history of it. The subject of the disorder was a man thirty-eight years old. He was admitted on the 19th January. His countenance was dusky, but exsanguine; his pulse 100, and weak; his tongue pale, and slightly furred. He made no complaint of pain anywhere.

He had been ailing for two years; had suffered much pain across the epigastric region; and had frequently vomited his meals. Two days before, he had been suddenly attacked with faintness and giddiness, and then vomited about two quarts of blood. He was an habitual spirit drinker.

In the afternoon of the day on which he entered the hospital, he was again seized with giddiness; and fell into a state of syncope, in which he remained for several minutes. Upon recovering he vomited a large quantity of blood, not less than three pints. The next morning, early, he brought up a like quantity, under similar circumstances; and he

passed three evacuations from the bowels, all of them *black*. He was gradually sinking during the whole of that day, the 20th. Towards the evening, he vomited about half a pint more blood. He died quietly the next morning.

When the abdomen was laid open, the stomach was seen to be distended. The intestines had, in several places, a black appearance; from the colour of their contents. The stomach contained about two pints of coagula, and of a dirty red liquid. At the upper part of its lesser arch was a small excavated ulcer, with hardened edges. In the centre of this ulcer there were visible the orifices of three or four arteries, filled with minute clots of blood.

It would seem as if gastric hæmorrhage, having this origin, were capable of being staunched by some natural process; and as if the injury done to the stomach were susceptible of repair. Andral describes a curious case, in which an open blood-vessel was found in an ulcer of the stomach; but no hæmorrhage had occurred. That mere ulcers in the stomach may heal, there can be no doubt: two or three scars of healed ulcers are represented in Dr. Carswell's drawing. The patient whose case is related in the *Journal Hebdomadaire* had vomited considerable quantities of blood for eight days in succession, five years previously to the attack which terminated his life. So that hæmatemesis from this cause is not absolutely hopeless.

But (as I have already said) hæmorrhage from the mucous membrane of the stomach, and from that of the alimentary canal generally, takes place far more commonly by *exhalation*. The evidence that the blood really does ooze from the membrane is the same as that to which I have so often had occasion to advert; and it is very satisfactory and conclusive in these cases, because we are able to scrutinize closely the whole extent of the mucous surface. This cannot so well be done in regard to the mucous membrane of *the lungs*. When death has followed immediately upon the hæmorrhage, and has indeed been its rapid effect, the membrane has been found quite entire, and of its natural consistence and texture throughout. Sometimes partially red, and pulpy, and vascular; sometimes universally so, the submucous capillary network of vessels being still gorged with blood; sometimes quite pale, the same system of vessels having been completely emptied by the last attack of hæmorrhage; and sometimes studded with minute dark points, which could be made, by slight pressure, to start from the surface, and looked like grains of black sand. This latter appearance, as I once before remarked, is very corroborative of the opinion that the blood escapes through the natural pores or channels; which it cannot enter so long as the solids and fluids of the body retain their healthy condition. These sand-like bodies *are*, doubtless, small portions of blood, which have coagulated in the exhalant orifices of the membrane, and received from them their shape.

This kind of hæmorrhage happens under various circumstances; and is attended with different degrees of danger. 1. The bleeding may be idiopathic. 2. It may be vicarious of some other habitual hæmorrhage. 3. It may depend upon disease or injury of the stomach itself. 4. It may be the consequence of disease situated elsewhere, and producing, mechanically, a plethora of the veins of the stomach. 5. It may result from a morbid condition of the blood, and form one symptom of a more general disease; as in the passive hæmorrhages of purpura and sea-scurvy. Each of these varieties requires a short notice.

1. *Idiopathic hæmatemesis.*—Hæmorrhage strictly *idiopathic*—*i. e.*, independent of any apparent change of texture, whether in the surface itself, or in any part obviously capable of influencing its blood-vessels—is as rare, I believe, from the mucous membrane of the *stomach*, as from that of the *lungs*. I have never seen, nor do I recollect to have read of, any instance of hæmatemesis analogous to the *epistaxis* which is so common in children and young persons; and which affords the most familiar example of idiopathic hæmorrhage.

2. *Vicarious hæmatemesis.*—But hæmorrhage from the stomach, occurring in connection with other constitutional hæmorrhages, or in their stead—and above all, occurring *vicariously* of menstruation—is *abundantly* common. It is the most common indeed of all the species of *hæmorrhage by deviation*. I told you, in a former lecture, that patients will sometimes menstruate for years together through the lungs; without any apparent injury to their general health. More commonly still do they menstruate through the stomach. I will mention one concise but curious example of this which I had from Dr. Latham, and which came within his own knowledge. A young woman became the subject

of hæmatemesis, recurring at monthly periods, about the age of fourteen. She had never menstruated. This continued until she married, and in due time, fell with child. Thereupon the hæmatemesis ceased. She brought forth and suckled her infant. During lactation the hæmorrhage did not recur. It came on again soon after she ceased to nurse the child; no regular menstruation by the uterus having ever happened. This was the woman's own account, and there appeared no reason to question its accuracy.

Gastric hæmorrhage of this kind, vicarious of regular menstruation, is not generally thought to have any tendency to shorten the existence of those who are afflicted with it. Cullen states broadly that this species of hæmatemesis is hardly ever a dangerous disorder: and this is true. Yet it is not so *entirely* free from peril as to preclude the necessity of *some* caution and qualification in stating the prognosis. The exhaustion from the mere loss of blood is sometimes so great as to create serious alarm for the patient's safety. And Mr. North has recorded (in the *London Medical and Physical Journal*) two instances in which suppressed menstruation was followed by repeated and at length *fatal* hæmatemesis.

In neither of these women was the health seriously deranged; nor, previously to the hæmorrhage, did there exist debility, or any other symptom calculated to excite the apprehension of danger. In fact, in both of these cases, a strongly favourable prognosis was given by experienced physicians a very short time only before the fatal event.

3. *Hæmatemesis from gastric disease or injury.*—Gastric hæmorrhage, by the way of exhalation, is often a consequence of *disease or injury of the stomach itself:* it is sometimes one of the earliest declaratory symptoms of scirrhus or cancer of that organ—occurring long prior to ulceration. Hæmatemesis attends also, very commonly, the *ultimate* stages of that fatal disease: and then it may be owing to the erosion of some vessel of notable magnitude, in the course of the process of disorganization, as in the examples already spoken of: or (what I believe is far more common) it may result from a kind of general oozing or exhalation from the ulcerating surface. Blood is often vomited soon after the reception of strongly irritant poisons into the stomach. I show you again Dr. Roupell's plate, representing the crimson surface of a portion of the stomach of a dog which had been killed shortly after the administration of a dose of alcohol. The intense congestion thus produced is doubtless *active* congestion; congestion belonging to inordinate *arterial* action. Pushed a degree further, such congestion passes into hæmorrhage.

4. *Hæmatemesis from disease in other organs.*—On the other hand, intense *passive* congestion—congestion arising from the detention of blood in the *veins* by some mechanical obstacle to its progress—is a very common source of gastric hæmorrhage. Hæmatemesis is therefore an occasional symptom of obstructive disease in the heart. Much more frequently, however, it depends upon abdominal changes. The hæmorrhage is symptomatic of disease situated not in the stomach itself, but elsewhere. And the viscera, with the diseases or morbid conditions of which, bleeding from the stomach is most often connected, are the liver and the spleen.

All this is well known: and it is easy to see, from the peculiar construction of the venous apparatus in the abdomen, how disease of one or both of these viscera may produce mechanical congestion of the submucous capillary tissue; and how that congestion may be relieved, under certain circumstances, by the effusion of serous fluid on the one or the other surface, constituting ascites or diarrhœa, as the case may be; or under *other* circumstances, not perhaps easily discriminated or well understood, by the extravasation of the collected blood itself. It would be superfluous to describe the peculiar distribution and functions of the vessels which return the main portion of the venous blood from the stomach and intestines towards the heart. It seems to me highly probable that one at least of the offices of the *spleen* is to provide a receptacle or reservoir for this blood when its free passage through the portal vessels is temporarily obstructed. It then becomes a sort of safety valve (if such an illustration be allowable), which obviates the danger that might otherwise arise to more vital parts from any great or sudden disturbance of the venous circulation. The stress of the congestion is continually felt in the submucous capillary system; and the hæmorrhage which is apt in such cases to occur from the loaded membrane, receives a simple solution upon principles almost purely mechanical; nay, the very circumstances which lead to the effusion of the blood from the *mucous* surface on the one side, rather than from the *serous* on the other, may perhaps (as I stated more at large in an early part of the course) be themselves susceptible of mechanical explanation.

Gastric hæmorrhage, symptomatic of hepatic disease, is chiefly to be looked for in those morbid conditions of the liver which imply obstruction of the portal vein and of its ramifications. We are not surprised, therefore, to find it coincident, often, with a contracted and shrunken state of that organ. The state of the spleen, on the contrary, for reasons that must be obvious to you, is uniformly, in the cases we are now considering, a state of *enlargement*. And the augmentation of bulk is not so much to be ascribed to disease inherent in its proper texture, as to distension by the mere quantity of blood which it holds. The internal structure of the spleen furnishes a credible presumption in favour of that view of one of its uses to which I just now alluded; and this structure, and this presumed function, when considered together, throw a strong light upon some of the pathological relations of the spleen which well deserve attention.

Numerous instances are on record of hæmatemesis going along with evident enlargement of the spleen; and in some of them that organ has been observed to diminish in bulk, in proportion as blood was poured out by the stomach. If I am not greatly mistaken, I have more than once seen this myself. In such cases the tumid condition of the spleen may be regarded as an evidence of venous obstruction *elsewhere;* and as depending, sometimes at least, upon disease of a less striking and prominent character in the liver, impeding the progress of the blood through the vena portæ. Of this kind would seem to have been a case related by Morgagni, wherein, after repeated attacks of hæmatemesis, under which the patient sank at last, the spleen was found to weigh four pounds, and to be gorged with dark blood; while the liver was pale and exsanguine. Frank gives the history of a patient, who had vomitings of blood, and whose spleen, taken from the body after death, weighed sixteen pounds: the ordinary weight of the spleen in a healthy adult being from eight to ten ounces. In Latour's work on *Hæmorrhage*, which is remarkable for the number of examples it contains, collected from various sources, and amounting to nearly a thousand, several instances are detailed of this combination of splenic enlargement with hæmatemesis. One of these occurred in the person of a friend of his, who had been living in a malarious district, and who had laboured for nearly two years under obstinate intermittent fever. This was followed by an immense enlargement of the spleen—a great *ague-cake*—which came to occupy almost the whole of the abdomen. Latour's experience enabled him to predict that hæmatemesis would probably supervene upon this condition of the spleen; and, accordingly, one night he was called in a hurry to his friend, and found that he had vomited an enormous quantity of clotted blood. A great deal passed away through the bowels also. The hæmorrhage recurred from time to time, till in the course of a month the spleen was so far reduced in bulk, that it could no longer be felt in the belly; and the patient lived and enjoyed good health, for twenty-five years afterwards.

It is necessary, therefore, in marking the connection which frequently subsists between hæmatemesis and enlargement of the spleen, to guard ourselves against concluding that these two circumstances hold always the relation of cause and effect. In many such cases, probably in most of them, they are simply concurrent effects of one common cause; and that cause is chiefly to be sought in such morbid conditions of the liver—or of other parts within the abdomen—as are competent to produce a considerable impediment to the free transmission of blood through the system of the vena portæ.

When gastric hæmorrhage results from hepatic obstruction, there is almost always *intestinal* hæmorrhage also. At any rate there are almost always black alvine evacuations, like tar or dark paint. This form of disease has therefore been called *melæna*. The ancients supposed that the unnatural stools consisted of black *bile*.

Hæmorrhage from the stomach, independent of *disease* in that or any other part, sometimes happens in the advanced periods of utero-gestation. Yet, though it does not result in these cases from disease, it is difficult to class it among idiopathic hæmorrhages. The want of periodical recurrence, and the absence of the hæmorrhage during the *earlier* months of pregnancy, are circumstances which sufficiently refute the old notion, that this form of hæmatemesis depends also upon the suspension of the catamenia. It is caused, no doubt, by the pressure of the gravid uterus, which impedes mechanically the venous circulation in the abdomen.

5. *Hæmatemesis from disease of the blood.*—Gastric hæmorrhage, resulting from *changes*

in the blood itself, occurs in sea-scurvy, in purpura hæmorrhagica, and in the yellow fever. Being merely a symptom in these cases, it requires no separate consideration here.

Phenomena of hæmatemesis.—When a *large* quantity of blood is poured into the stomach, whatever may have been its source, it appears to have a nauseating and emetic effect. At least the blood ejected in hæmatemesis is almost always considerable in amount. The vomiting may, for aught I know, be dependent on the mere distension of the stomach, which appears to be tolerant of the presence of the blood up to a certain point, but no further. A small quantity may, doubtless, pass all of it onwards through the pylorus, after undergoing, more or less completely, the process of digestion in the stomach; and a *portion* of the blood pursues that course in most instances. But when it is vomited, it comes up in large quantities, usually of a dark colour, and more or less coagulated. Sometimes the coagula have evidently been moulded in the stomach; and sometimes clots are thrown up, partially deprived of the colouring matter of the blood, and resembling the fibrinous polypi so often met with in the cavities of the heart. Of course the degree of the coagulation of the blood, and of its separation into serum and crassamentum, will depend upon the time that it remains in the stomach; and this again would seem to bear a proportion to the rate of its effusion.

The blood that is vomited is almost always of a dark colour; while that which is coughed up is most frequently florid and bright. Why is this? We are told that the blood which comes from the lungs is rendered florid by the admixture of atmospheric air. But this is not the whole of the matter. Neither can we say that the dark hue of the blood ejected in hæmatemesis is always, or solely, due to some morbid alteration effected in that fluid while yet circulating in its proper vessels. There is another cause, which till of late years, was much overlooked, but which frequently changes the colour and appearance of blood *after* it has been extravasated into the stomach; and that in so great a degree as sometimes to render doubtful, or to disguise altogether, the real nature of the fluid vomited. I mean the chemical agency of the gastric acid. The effect of acids in blackening the blood out of the body is well known; and it is somewhat singular that the ascertained existence of an acid secretion in the stomach, varying in quantity at different times and under different circumstances, was not sooner applied in explanation of the dark colour of the blood, and its occasional blackness, when vomited. The degree of blackness will be in proportion to the relative quantity of acid which it meets with in the stomach, and the intimacy of the admixture. Sometimes the blood is clotted and not very much altered in colour; sometimes it is grumous, brown, of a chocolate tint, or like coffee-grounds. This generally denotes the existence of *organic disease;* and the appearance of the blood is probably modified in some degree by the morbid process that leads to its effusion. There is good reason for believing that in the *black vomit* of the yellow fever, the colour of the blood undergoes alteration, even while it is yet circulating through the blood-vessels: but that the black appearance of the matter vomited is in great part owing to the chemical action of the gastric acid, may be inferred from the fact, that the fluid so discharged is always (so I am informed) intensely acid. Andral has described an effusion of black liquid into the stomach, as an example of *melanosis.* He states at the same time that an accurate analysis of the liquid showed its composition to be very nearly the same with that of the blood. May we not suspect that this inky fluid really consisted of blood that had been blackened, subsequently to its extravasation, by the acid with which it mixed in the stomach? Upon the same principle may be explained the dark-brown or almost black colour of the spots which are sometimes seen (I presume when there has been a great predominance of acid) in the substance of the mucous membrane of the stomach, or even beneath it; and which have also been set down as melanotic. They are so like, in all circumstances, except in the single particular of colour, to the crimson spots which are obviously formed by minute extravasations of blood in the same parts, that we can scarcely refer them to any other source. The slate-coloured patches, which I spoke of yesterday as being vestiges of chronic gastritis, depend likewise upon the blackening effect of the gastric acid upon the congested surface. We have the same dark colour of the effused blood, in many cases, when it is poured out in the intestines. Here, of course, its colour is not referable to the gastric juice; but it is blackened by some of the intestinal

gases: probably by the sulphuretted hydrogen, for example, or the carbonic acid that enters into their composition.

There can be no doubt that this gastric acid, when intense in strength, or copious in quantity, is capable of changing the colour of the blood, after death, even while it is contained in the submucous blood-vessels. In these cases it must be conveyed to the blood by imbibition. And the very same thing takes place when strong acids are introduced into the stomach from without. When, for instance, the sulphuric acid, or what is perhaps more to our present purpose, the vegetable oxalic acid, has been taken as a poison, it has the effect of blackening, and, as it were charring the blood, with which the membrane becomes loaded in consequence of the irritation produced by the poison. It does this when no destruction of the mucous membrane has been produced.

It is but justice to observe, that the credit of having been the first to perceive, and to explain, this cause of the blackened state of the blood, while yet remaining in its proper vessels, is due to Dr. Carswell.

Diagnosis.—When blood is ejected through the œsophagus and mouth, we have demonstrative evidence of the existence of *hæmorrhage;* and the *diagnosis* of *hæmatemesis* may appear to be so simple as to admit of neither mistake nor doubt. The diagnosis of *hæmorrhage from the stomach*, however, is really oftentimes difficult and obscure, and to be established by presumptive evidence alone.

In the first place, bleeding may take place from the mucous membrane of the stomach, and no hæmatemesis ensue, especially when the blood is poured forth in small quantities and slowly. In these cases the blood becomes visible only in the stools, where it may not be looked for, and where, if seen, it may not always be recognized, in consequence of the changes it has undergone during its passage through the intestinal canal. And even supposing that its presence is detected in the alvine evacuations, it will remain uncertain in what part of that long canal it was effused. The hæmorrhage may even be profuse, and the patient may die, without *any* escape of the blood externally. There is a case related by Frank, in which death took place from hæmorrhage of the stomach without hæmatemesis; and both the stomach and the intestines were found distended by an enormous coagulum of blood which had assumed their form.

Even when the blood is ejected by the mouth, the exercise of some care and sagacity are occasionally, though not always, required, in order to determine the part from which it was originally poured out.

Thus blood may be swallowed, and afterwards vomited: and so we may have hæmatemesis without hæmorrhage from the stomach; just as we may have hæmorrhage from the stomach without hæmatemesis. There are cases of slow bleeding from the lungs, the fauces, the mouth, or the nasal cavities, where the blood, collecting in the pharynx, provokes, from time to time, an instinctive and involuntary act of deglutition; and thus is gradually accumulated in the stomach up to that point at which the organ becomes impatient of its contents, and ejects them by vomiting. This is very apt to happen during sleep, and especially to young children: and as the blood, *when vomited*, is coagulated, and in considerable quantity, it is scarcely possible to conclude, from its mere appearance, that it has proceeded from any other source than the stomach itself. If, however, we mistake such cases, our error is likely to produce much needless alarm, and to lead us to unnecessary activity in treating them. We are assisted towards forming a right judgment (when our attention happens to be directed to this source of fallacy) partly by the general history and symptoms, and partly by an examination of the mouth, fauces, and nostrils, to ascertain whether any coagula, or other marks of hæmorrhage, are visible on the mucous membrane belonging to those parts.

But blood may be swallowed knowingly, and purposely, by impostors, and afterwards vomited. Hæmatemesis is one of the complaints which have frequently been feigned; either for the sake of avoiding some imminent punishment, or distasteful service; or with the view of exciting compassion, and of profiting by the contributions of the charitable and the credulous; or sometimes from a kind of wilful perversity, akin to insanity. In treatises on forensic medicine, you will generally find reference made to an instance of this kind recorded by Sauvages, in his *Nosology*. A young girl, who was anxious at all hazards to

escape the restraints of a convent, pretended that she was suffering from violent hæmatemesis. In fact she did, for several days in succession, vomit large quantities of blood in the presence of the physician who had been summoned to her assistance. It was afterwards discovered that on each of those days she had swallowed blood which had been secretly conveyed to her from the neighbouring shambles. A case of precisely the same kind occurred (as I was informed by a gentleman who witnessed it) in the Bristol Infirmary some years ago. A girl had been long a patient there, labouring (as was supposed) under hæmatemesis; but it was at length discovered that she was a malingerer. She was in the habit of assisting the nurses in their work; and this afforded her opportunities—of which she availed herself—of drinking the blood which had been drawn from the veins of other patients: and this blood she afterwards vomited.

And even where no fraud is attempted, nor any blood swallowed, it occasionally becomes a nice matter to determine the origin of the hæmorrhage, when blood is ejected in large quantities from the mouth: to decide, namely whether the blood has come originally from the *lungs* or from the *stomach*. In copious hæmoptysis, the blood issues from the mouth in gushes, as it does in hæmatemesis; and the reflux of the blood into the pharynx, the tickling sensation it there produces, and the cough (which we know, even when the expectoration is not of blood, frequently excites retching); these causes, acting singly, or together, occasion sometimes a convulsive contraction of the muscles of the thorax, which *looks like* the effort of vomiting: and they often indeed give rise to actual vomiting. On the other hand, in sudden and profuse hæmatemesis, the irritation caused by the blood as it passes over the upper part of the larynx, is apt to provoke a paroxysm of choking cough.

Now when I was speaking, some lectures back, of hæmoptysis, I promised that I would point out the means of distinguishing it from hæmatemesis, when I came to the consideration of the latter complaint. I have now therefore to redeem my promise.

However equivocal certain cases may be at first sight, we may generally guide ourselves to a correct decision by a careful investigation of the circumstances that *precede*, *accompany*, and *follow*, the hæmorrhage. *Vomiting* of blood is commonly preceded by a sensation of weight and uneasiness in the epigastrium; and by nausea. Hæmatemesis is also, more frequently than hæmoptysis, ushered in by paleness of the face, dimness of vision, and an approach to syncope, or even actual fainting. These symptoms are not to be regarded (I apprehend) as premonitory of the *hæmorrhage*, although they have been so considered by some; they are rather a sign that it has already taken place; and yet they *are* preliminary of the *hæmatemesis*. Occurring before the blood comes up, they cannot be ascribed to alarm at the *sight* of it. On the other hand, *hæmoptysis* is wont to be announced by dyspnœa, cough, tickling in the throat, and a sensation as if of *bubbling* within the thorax. Most commonly, too, before the expulsion of much blood from the lungs, some sputa are *coughed* up, composed more or less of that fluid. The symptoms that usually *succeed* the hæmorrhage, in either case, afford equally valuable assistance to our judgment, in cases that might otherwise be doubtful. Generally copious hæmoptysis goes on, in a succession of mouthfuls, for some time; whereas there is mostly only one access of full vomiting. At any rate, at the close of abundant pulmonary hæmorrhage, the patient manifestly *coughs* up, and expectorates, smaller quantities of blood; while we usually may observe that, a few hours after hæmatemesis has occurred, slight griping pains come on in the abdomen, and a portion of blood is got rid of from the bowels.

Other questions, often of much importance in regard to the ultimate diagnosis, when the blood is traceable with certainty to the stomach, are, whether it be idiopathic, if, indeed, it *ever* be so: whether it be supplemental of some other discharge: whether it depend on disease of the stomach itself; of one, or more, of the contiguous viscera; or of the system at large. Certainly, in a very great majority of cases, gastric hæmorrhage is symptomatic; and the nature and seat of the disease of which the bleeding is a symptom, may, in many instances, be determined without much difficulty. That which depends upon *incipient* cancer of the stomach, while it is by no means of rare occurrence, is also, (I think) more frequently than other forms of hæmorrhage from that organ, obscure. It must be obvious to you, and therefore I need not dwell upon this part of the subject, that a little attention to the symptoms and past history of the patient will usually suffice to elucidate the nature of the case, where hæmatemesis supervenes immediately upon the in-

troduction of corrosive poisons, or within a certain interval after they have been swallowed: where it depends upon the bursting of a large aneurism: where it breaks forth among other symptoms of scurvy or purpura: where it is the result of an *advanced* stage of cancer of the stomach: where it accompanies organic disease of the liver, spleen, or heart: where it occurs as a symptom of yellow fever: where it takes the place of suppressed or imperfect menstruation: or where it is occasioned by the pressure of the gravid uterus. In all these cases, there is, ordinarily, no room for mistaking the one disease for the other; or for regarding the hæmorrhage as idiopathic.

Treatment.—With respect to the *treatment* that should be adopted in cases of hæmorrhage from the stomach, it must be apparent, from what has just been said of the many different morbid conditions upon which it may depend, or with which it may be essentially connected, that remedies are, in most cases, rather to be directed against the disease of which the hæmatemesis is a symptom, than against that symptom itself. But sometimes we are obliged to treat the symptom: either because we are not certain of the exact nature of its cause; or because the condition out of which it springs is not within our reach.

Cases of *melæna* (I have told you what is meant by that term) require hard purging: and many patients recover thoroughly under that mode of treatment. You may prescribe five grains of calomel every night, and a black dose every morning, till the stools lose their pitchy colour. Do not be afraid of purging your patients in such cases. If they are curable at all, that is the way to cure them. I have pursued that plan with perfect success, even with patients whom the previous hæmorrhages had blanched, and whose pulse was feeble and irregular. You may sustain them, at the same time, by a full allowance of nourishing broths. The portal system is drained and unburdened by this active depletion. And if there be no irremediable change of texture in the liver, the recurrence of the hæmorrhage may often, by a proper regulation of the habits and diet, be obviated. The ancients had learned by observation, the efficacy of treatment of this kind; but they used a different form of medicine, and purged away the *atra bilis* with hellebore.

It is plain that for melæna, dependent on mechanical congestion, *styptic* substances would be worse than useless. They are more adapted to those cases (could we but surely distinguish them) in which the hæmorrhage proceeds from a bleeding vessel. This is indeed the mode whereby we often succeed in staunching external hæmorrhages; namely, by applying astringents to the very part. Similar means may be employed when hæmatemesis, of a purely passive character, depends upon some modification of the circulating blood. There is one remedy which is thought to have a sort of specific effect upon hæmorrhages of the gastro-intestinal canal: I mean the oil of turpentine, given in small doses; from twenty minims to half a drachm every four or six hours. I cannot say that I have had much experience of it. Of course the patient must be kept cool and quiet; whatever he drinks he should drink cold; even ice is often both grateful and effectual. If ordinary measures fail, recourse may be had to the acetate of lead; or even to the quack medicine, Ruspini's styptic. Not that I think you will often find the latter expedient successful, when more rational treatment has failed; but in obstinate and dangerous cases it ought to be tried. If, with the hæmatemesis, there be any fever, it may be proper and necessary to abstract blood from a vein, and to employ refrigerant substances as remedies: and if, with or without much fever, there be tenderness at the epigastrium, leeches, or a blister, should be applied. In cases where the catamenia desert their natural channel, and seek an outlet through the mucous membrane of the stomach, it will be well, while means are taken to discourage the hæmatemesis, as iced drinks and so forth, to endeavour to solicit the discharge in the right direction. And we often succeed in this object, by placing leeches upon the groins of these patients immediately before the period when the vicarious menstruation is expected; and by putting their feet, at the same time, into hot water; or even laying them in a warm hip bath.

LECTURE LXX.

DYSPEPSIA. PHYSIOLOGY OF DIGESTION. SYMPTOMS OF DYSPEPSIA. TREATMENT AND PREVENTION, DIETETIC AND MEDICINAL.

IT is my intention to appropriate this evening's lecture to a cursory account of *dyspepsia:* by which I mean some evident derangement in the natural process of digesting and assimilating our food; and more especially, a faulty performance of the functions of the *stomach.* Indigestion is the prevailing malady of civilized life. We are more often consulted about the disorders that belong to eating and drinking, than perhaps about any others: and I know of no medical topic concerning which there is afloat, both within and beyond the profession, so much ignorant dogmatism and quackery.

Cullen, in his definition of dyspepsia, enumerates the various symptoms, by the occurrence of more or fewer of which, that complaint is most commonly manifested "Anorexia, nausea, vomitus, inflatio, ructus, ruminatio, cardialgia, gastrodynia:—pauciora saltem vel plura horum simul concurrentia, plerumque cum alvo adstrictâ, et sine alio vel ventriculi ipsius, vel aliarum partium, morbo."

The variety in the actual presence and combinations of these symptoms is very great: and any attempt to give a perfect or complete history of dyspepsia in these lectures is quite out of the question. But I will endeavour to draw such a general outline of the disorder as may assist and direct your observation of it hereafter.

I shall first take a brief view of the *pathology* of indigestion, so far as it is understood: and to make this intelligible, it will be necessary to interweave something of the *physiology* of the subject. To these preliminary considerations, I shall add a short comment upon the several symptoms of dyspepsia, enumerated in Cullen's definition: and lastly, I shall state what I know respecting the means of curing, and preventing, this familiar disorder.

Physiology of digestion.—The conditions of healthy digestion are these: that the food should be masticated, mixed with saliva, and swallowed into the stomach: that in the stomach it should be reduced to a semi-fluid consistence, and converted into a uniform pulp, called chyme: that the chyme should be transmitted through the pylorus into the duodenum, and there mixed with the bile, the pancreatic secretion, and the intestinal mucus; in consequence, as it would seem, of which admixture, the whole is separated into two parts, viz., the chyle or the nutritive portion of the food, now in a fit state to be taken up by the lacteals which open upon the mucous surface of the intestines, and to be carried by them into the blood; and the excrementitious portion, which at length is conveyed out of the body.

The food is dissolved and transformed, in the stomach, by the chemical agency of the *gastric juice.* This is a secretion peculiar to the stomach. All that need be stated of it here—almost all indeed that is known—is, first, that it oozes forth in minute drops from the mucous surface: but only when food (or some solid substance), is present in the stomach; and secondly, that it is always *acid.* It appears to owe its solvent power to a special principle, which chemists have named *pepsin.*

The food, having arrived in the stomach, is moved about, by a sort of churning or revolving movement, and mixed with the gastric juice, and gradually changed into chyme: which also is acid. Finally, the chyme is propelled by degrees into the duodenum by the pressure of the transverse band of muscular fibres which embraces the pyloric extremity

of the stomach. The time in which the whole operation is completed varies from two to four or five hours.

Liquids introduced into the stomach disappear much more speedily; either by direct absorption, or through the pylorus.

All this we know, not from mere speculation on the anatomy and usages of the organ, but from actual observation. An American physician had, for several months in succession, the curious privilege of looking, whenever he pleased, into a healthy human stomach, and of watching its condition, its movements, and its contents, during the process of digestion. A young Canadian had a portion of the skin, muscles, and ribs, of the left side of the body blown away in a gun-shot wound, which laid open the stomach also. He recovered from this frightful injury with a permanent aperture in the side, communicating directly with the stomach. Through this loop-hole Dr. Beaumont was allowed to introduce various articles of food; and to withdraw from time to time the gastric secretions; and the aliment, in the different stages of its digestion. He has published a very interesting account of these experiments, which have set at rest some points in the physiology of the stomach that were previously uncertain. I shall embody his deductions in what I have further to say on the subject.

In order that digestion may be perfect and easy, it is requisite that the food be in a state of minute division. This object is attained by *mastication*. A weak dyspeptic stomach acts slowly, or not at all, on solid lumps and tough masses of food. The delayed morsels undergo spontaneous changes, promoted by the mere warmth and moisture of the stomach: gases are extricated: acids are formed: perhaps the half-digested mass is at length expelled by vomiting; or it passes undissolved into the duodenum, and becomes a source of irritation and disturbance during the whole of its journey through the intestines. Here then we have one common cause of dyspepsia; and an easy and obvious preventive. Dyspeptic persons should not eat in a hurry, as busy men, and studious and solitary men, are apt to eat. They are to be cautioned against *bolting* their food: it must be well ground in the mill that nature has provided for that purpose. I am not at all sure that the increased longevity of modern generations is not, in some degree, attributable to the capability of chewing their food which the skill of the dentist prolongs to persons far advanced in life.

There are certain things upon which the gastric juice has no power. The green colouring matter of certain vegetables; the husks of seeds; the rinds of many fruits. You may perhaps have observed that dried currants, and the pips of apples, swallowed entire, reappear, unchanged, among the egesta. Whatever passes the stomach untouched by the gastric liquor, passes undissolved through the whole of the alimentary canal; provoking disorder sometimes in its transit; forming sometimes a nucleus for intestinal concretions. These substances are therefore unfit for a weak stomach. When the digestive powers are active, and the bowels slow, they may perhaps occasionally be even useful. Thus brown bread—*i. e.*, the indigestible bran, or tegument of the kernel of wheat—stimulates the peristaltic motions of the intestines, and averts, in certain persons, the necessity of more direct purgatives. Unbruised mustard seed, once so much in vogue, owed much of whatever virtue it possessed to this principle. But if these intractable substances fail to excite the proper action of the bowels, they are apt to accumulate, and to lay the foundation of serious disease.

Indigestible matters, to which the pylorus refuses a passage, may remain in the stomach, and disturb its functions, for days, or even sometimes for weeks, together. If we could ascertain their presence, an emetic would be the remedy. And sooner or later vomiting is set up, and the offending substance expelled. I lately saw a mass of hard curd—a small cream cheese in respect to consistence—which was thrown up after several days of severe gastric pain and disorder. The relief was immediate and complete. The patient had been taking large quantities of cream with his tea and coffee. In another person, a similar fit of indigestion terminated in the ejection of a mass of snuff. This is no unusual source of derangement of the stomach among those who use lavishly that nasty luxury.

The essential change which the chyme undergoes after leaving the stomach, appears to consist in its separation into two parts: namely into chyle, which is taken up by the lac-

teals; and into excrement, which is discharged from the body. Any undissolved portions of the food become attached to this last part. We do not know exactly what is the function or agency of the pancreatic liquor; but with regard to the bile our knowledge is somewhat more definite. The acid developed in the stomach combines in the duodenum with the alkali of the bile, and is more or less neutralized. Dr. Prout conjectures that in a healthy state of the organs it is entirely neutralized. Bile is, moreover, the natural stimulus of the intestines; when its secretion is stopped, or its passage into the duodenum prevented, digestion and assimilation may go on, but the bowels are usually sluggish. The hepatic secretion has doubtless other important uses; but with these we are not at present concerned. It is pretty evident that the state of the biliary functions can have no direct influence in the production of mere dyspepsia. When the constituents of the bile are imperfectly eliminated from the blood, various parts of the body may suffer detriment. And when the functions of the stomach and the functions of the liver are both disordered, it *may be* that the former organ sympathizes indirectly with the morbid state of the latter: or it *may be* that one and the same cause operates in producing the derangement of both organs.

Symptoms.—Let us now review the symptoms of dyspepsia which are mentioned in Cullen's definition. The first of these is *anorexia:* want of the natural appetite. Sometimes this is almost the only symptom observable. The patient is warned, by loss of appetite, not to take too much food; he refrains instinctively from certain kinds of food; or he feels perhaps absolute repugnance and disgust at the very thought of eating. Various have been the speculations respecting the immediate cause of hunger. It has been ascribed to the action of the gastric juice upon the surface of the empty stomach. But during health the gastric juice is never present in an empty stomach. Neither can the appetite depend upon contraction of the muscular fibres of the stomach; for the empty stomach, during health, is always contracted upon itself. No doubt the sensation of hunger, like all other sensations, arises from some particular condition of the *nerves* of the part. It returns periodically, acknowledging in this respect the influence of habit. It is sensibly affected by agencies which operate upon and through the nervous system. The receipt of a piece of bad news will destroy, in a moment, the keenest appetite.

Sometimes there is no anorexia. The appetite may even be morbidly craving and ravenous; or capricious and uncertain.

When defect of appetite is the only symptom, it may be remedied, often, by the employment of bitters, or of the mineral acids, taken twice or thrice daily, for some time together. It would be out of place for me to speak in detail of particular medicines of this kind: it is enough if I indicate quina, columbo, gentian, quassia; the dilute sulphuric and nitric acids; or a mixture of the nitric and muriatic.

Nausea—vomitus.—These are, in some instances, the most distressing results and signs of the dyspepsia. Sometimes nausea comes on soon after the food is swallowed. Sometimes there is no nausea; but after the lapse of a certain period, an hour or two generally, the food is rejected by vomiting. The matters thus thrown up are most frequently sour. Not seldom they are mixed also with bile, especially if the retching has been violent, or long continued; and then the patient is apt to ascribe the whole of his complaint to "an overflow of bile," although in fact the secretions of the liver have nothing whatever to do with it; the appearance of bile in the fluids ejected from the stomach proceeding from an inverted action of the duodenum. The effort of vomiting, however induced, will, if often repeated, be attended with the expulsion of yellow bile. I have more than once referred you, for an illustration of this fact, to the phenomena of sea-sickness. The fallacy I now point out has been one cause of the notion that is prevalent among patients, and the public—and not unfrequently perhaps among practitioners—that dyspepsia very commonly depends upon a disordered state of the biliary organs

The vomiting which occurs in dyspepsia is often connected with a morbid irritability of the stomach· and it is sometimes a very troublesome symptom to treat. The carbonic acid has certainly a marked effect in allaying it, in many cases. We give it, as you know, in the effervescing saline draught, made with the carbonate of potash, or soda, and lemon-juice. Sometimes the mineral acids answer better. Sometimes, on the other hand, alkalies —the liquor potassæ, for example—or lime-water, are more effectual. In these latter cases

we may presume that there is a morbid acidity of the stomach. Small doses of opium are occasionally successful when other means fails. Opiates thrown into the rectum—opium plasters to the epigastrium—blisters to the same part: these are measures which you will sometimes have to try one after another. There are two special remedies which have been greatly extolled for their virtue in abating sickness: the hydrocyanic acid is one of them; creasote is the other. The hydrocyanic acid I have found exceedingly useful in obstinate cases. It may be given alone—or mixed with the effervescing draught—or combined with a few grains of the sesquicarbonate of soda. The creasote has disappointed me oftener than it has answered my hopes from it. Yet it has a decided influence in checking some forms of nausea; and it is the more likely to succeed, in proportion as the condition of the stomach is remote from inflammation.

But after all, the grand principle on which to treat chronic vomiting—not dependent upon disease in other parts, as the head, the kidney, or the uterus—is that laid down by Dr. William Hunter; of reducing the *quantity* of food to that amount, whatever it may be, which the stomach is able and willing to retain, and of making its *quality* as bland and nutritious as possible. The most satisfactory case which I have had to treat upon this principle occurred some years ago, in the person of one of my hospital patients. She was brought out of Kent by her father. She had been under the care of several medical men, one of whom had been a pupil at the hospital, and recommended her as a proper patient for admission there. Her age was sixteen. She and her father both agreed in the same story; viz., that she constantly vomited her meals; the food generally coming up again immediately after it was swallowed, and never remaining longer in her stomach than ten minutes. The vomiting was described as being easy; and was neither preceded nor accompanied by nausea.

She had been ill for four years: ever since a severe attack of scarlet fever. At first she vomited her meals now and then—three or four times a week—but the vomiting gradually became more and more frequent; and at the time of her admission she had vomited after *every* meal, for three months in succession. She had grown considerably in the four years; and was tolerably plump; and looked healthy; and the catamenia had begun to appear, though scantily, in the same period: but they had been altogether suspended for a year.

It was clear that a good deal of her food must have remained: and, bearing William Hunter's case in mind, I directed that she should have a very small quantity of roast meat for dinner, and a coffee cup of milk occasionally during the day; and no other food. I prescribed also some pills, consisting of aloes and soap, to act moderately on the bowels. I expected to have been obliged still further to limit her food: but she never vomited again, from that time. This distressful and protracted disorder, after long and fruitless treatment previously, yielded thus at once and easily to very simple means.

Inflatio—ructus. Flatulence, and belching. The gas that produces these symptoms is sometimes extricated from undigested food detained in the stomach, and in a state of fermentation: sometimes secreted, apparently, by the stomach itself; for the flatulence comes on when the stomach is empty of food. It is apt to arise, in dyspeptic persons, if a meal happens to be delayed beyond the accustomed hour. Patients complain grievously of these symptoms, and accuse the "wind in their stomach" as being at once the essence and the cause of all their complaints. They ask for medicines to get rid of the wind; and its escape may indeed be promoted by warm aromatics, and carminatives, as they are called: the relief thus afforded to the distended stomach being so sudden, and for the time so complete, that the sufferer ascribes to the medicine *vim carminis*, the power of a charm. One of the most effectual and popular of these carminatives is peppermint water. A due regulation of the periods for taking food will often suffice to obviate the flatulence that belongs to emptiness. That which follows eating may, in many cases, be prevented, by swallowing, immediately before the meal, five or six grains of the extract of rhubarb, with or without a grain of cayenne pepper. If the ascending wind brings into the throat and mouth a portion of the solid contents of the stomach, the patient is said to *ruminate*. The regurgitated matters are often intensely acid; and then an alkali may remedy the flatulence; a teaspoonful of *sal volatile*, for example.

Pain.—Indigestion is, in many instances, attended with scarcely any *pain;* while in

others the pain is very tormenting. Cullen speaks of it under the terms *cardialgia;* and *gastrodynia.* Cardialgia is that less violent and more permanent uneasiness which in popular language is called *heart-burn.* Gastrodynia is that more severe, and usually more transient pain, which is commonly denominated *spasm* or *cramp* of the stomach.

Dr. Abercrombie has some useful practical observations in respect to pain of the stomach. He speaks of it as occurring under four different forms; and I am able to bear witness to the reality of the distinctions that he has drawn. In the first place, some persons suffer pain, occasionally, when the stomach is empty, even when there is no flatulence; and they are comforted and relieved by taking food. It is reasonable to suppose that this kind of pain depends upon some degree of acrimony of the fluids of the stomach itself. It is often removed at once by alkalies, or absorbent medicines. A teaspoonful of the aromatic spirit of ammonia, or a tablespoonful of the liquid magnesia, in a wineglass of camphor julep, will still the whole uneasiness sometimes in a moment, as if by magic.

A second form of pain in the stomach is when it occurs *immediately* after taking food, and continues during the whole process of digestion, or until vomiting ensues, which gives instant ease. In such cases we have reason to suspect the existence of chronic inflammation, or of some undue sensibility of the mucous membrane of the stomach. The suitable remedies are such as I spoke of in the last lecture. I might have mentioned a form of medicine which Dr. James Johnson has found especially serviceable against this morbid sensibility; I mean the nitrate of silver, in small doses.

In a third species of painful disorder of the stomach the pain does not begin till from two to four hours after a meal, but continues for several hours. This is a very common form of complaint. Dr. Abercrombie is of opinion that the pain is seated in the duodenum, and connected with inflammatory action, or with morbid sensitiveness of the mucous lining of that bowel. He says it is frequently accompanied by pain and tenderness of the right hypochondrium; and that the liver is often blamed when it really is not in fault. This last remark I well believe; but I am not so easily persuaded that the pain is duodenal. I believe it depends upon acidity in the primæ viæ. It has been ascertained by several chemists, that the acid which is present in the gastrice juice is the muriatic. Dr. Prout holds that the source of this muriatic acid is the common salt which exists in the blood, and that the decomposition of this salt is owing to the immediate agency of some modification of electricity; and he conceives that the principal digestive organs represent a kind of galvanic apparatus, of which the mucous membrane of the stomach and intestinal canal may be considered as the acid or positive pole, while the hepatic system is the alkaline or negative pole. However this may be, it is certain that the muriatic acid contained in the stomach is often in excess: other acids are also found there—the acetic, and more especially the lactic; and when the food, now converted into chyme, passes into the duodenum, the remaining superfluous acid teases the stomach. I think this explanation of the cause of the pain is a more probable one than Dr. Abercrombie's, because you may generally mitigate or remove the pain by introducing an alkali into the stomach, whereby the acid is neutralized: even the swallowing a cup of warm tea, by which the acid is diluted or washed away, often stops the pain. And I have, in numerous instances, succeeded in *preventing* the recurrence of this pain by directing the patient to take a small quantity of alkali, in some aromatic water, immediately after his dinner. According to Dr. Abercrombie's theory the pain ought not to be so immediately allayed by these remedies; and, since the food is gradually propelled into the duodenum as it is digested, the pain should begin, I think (supposing him right), earlier than it does. Dr. Abercrombie has found nothing of more general utility in these cases than the sulphate of iron, combined with one grain of aloes, and five grains of aromatic powder, taken three times a day. He praises lime-water also, and small opiates, and a combination of bismuth and rhubarb. What its *modus operandi* is I know not, but I am sure that bismuth is a very effectual remedy for some kinds of gastric distress.

Cases now and then occur in which this pain, succeeding a meal, and the deposit of lithates in the urine, would seem to indicate the propriety of an alkaline treatment, but which really are more benefited by the mineral acids. How to distinguish these exceptional cases, unless by trial, I have not yet learned. Any detected admixture of phosphates might perhaps furnish the requisite clue.

Pain in the stomach occurs in a fourth form, coming on at uncertain intervals in most violent paroxysms, and properly called *gastrodynia*. It is often accompanied by a sensation of distension, much anxiety, and extreme restlessness. In females it is frequently combined with hysterical symptoms. This form Dr. Abercrombie supposes to depend upon over-distension of the stomach; and it may be so; certainly great quantities of air are sometimes extricated; and the pain is not confined to the stomach, but shoots through to the back and between the shoulders. I suspect that the pain is sometimes neuralgic. It is often very intractable; occasionally it yields to carminatives. Dr. Abercrombie states that he has observed the most effectual relief in such cases to have been obtained from exciting a brisk action of the bowels by means of a strong purgative enema. He makes this practical remark, which is worth attending to. From the facility with which such affections often yield to the remedy just mentioned, it appears not improbable that the pain may be sometimes situated in the arch of the colon. Wherever its seat may be, I know that it is frequently removed by a mustard poultice laid upon the epigastrium. Opium also is of eminent use in many of these cases; and bismuth; and cordials: but I have seen more rapid and decided relief afforded by the prussic acid than by any thing else; and the cure so wrought is often permanent. It does not bring ease in all cases, nor is it a medicine that is any particular favourite of mine, yet its good effect is in some instances so striking, that if this were its only virtue I should esteem the hydrocyanic acid a most valuable remedy.

You will meet sometimes with what is called *spasm* of the stomach (and I suppose it is such) in gouty people; who are then said to have gout in the stomach. The pain comes on in sudden and severe paroxysms; and is removable in general by laudanum and stimulants, brandy, for example; or by the mustard poultice. On these cases, however, we look with jealousy and apprehension. In some instances the attack is really inflammatory, and would then be aggravated by a stimulant treatment.

There is yet another modification of uneasiness and disorder of the stomach, of which the distinguishing characteristic is the vomiting, or rather the eructation of a thin watery liquid, sometimes sourish, but usually insipid and tasteless, and often described by the patients themselves as being cold. This is what Cullen calls *pyrosis*, the *water-brash*. It is a disorder much more frequent in the lower ranks of society than in others. It is particularly common in Scotland, and is there ascribed to the large employment of farinaceous substances as food, and especially of oatmeal. But it is said to be still more common in Lapland. Dr. Cullen, who saw a great deal of it, says that its paroxysms "usually come on in the morning and forenoon, when the stomach is empty. The first symptom of it is a pain at the pit of the stomach, with a sense of constriction, as if the stomach was drawn towards the back. The pain is increased by raising the body into an erect posture, and therefore the body is bended forward. The pain is often very severe; and after continuing for some time, it brings on an eructation of a thin watery fluid in considerable quantity." Such is Cullen's description of pyrosis. He states that the complaint often occurs without other evidence of dyspepsia; but this is not consistent with the experience of subsequent observers. It is a symptom sometimes of organic disease of the stomach. In one remarkable case of pyrosis which I saw, and in which not less than three pints of this thin tasteless liquid was brought up every day, the stomach, after death, was found to all appearance healthy; but it had been pressed upon by an enormous liver. I mention these facts that you may not suppose pyrosis to be always, as Cullen has described it, a substantive and idiopathic malady.

When pyrosis is not caused by organic disease in the stomach or in the liver, it will yield in general to opium, and especially to opium in combination with astringents. The *pulvis kino compositus* of the Pharmacopœia is an admirable remedy for it. But we often have to contend with this difficulty, that the bowels, in cases of pyrosis, are generally confined, and that the opium tends to aggravate this unnatural condition; so that it becomes necessary to administer some aperient daily while the kino and opium are given; castor oil, or confection of senna, or cathartic extract.

Costiveness is, in fact, a very frequent concomitant of dyspepsia, as Cullen's definition affirms (plerumque cum alvo adstrictâ). And this sluggish state of the bowels often aggravates, if it does not produce, the dyspepsia. At any rate the defective powers and

uneasy sensations of the stomach are rectified, in many instances, by measures which promote the regular and healthy evacuation of the intestines. Without professing to go into detail in this matter, I may state that, in our remedial attempts, we should imitate, as much as we can, the processes of nature. The mixed contents of the small intestines furnish the natural stimulus of their peristaltic movements; and the excrement excites the larger bowels. When this natural stimulus is insufficient, the want may be supplied by some substance which is involved in the food, and accompanies it in its progress—as the bran of brown bread, already mentioned, or a few grains of rhubarb or of aloes swallowed immediately before dinner. In adjusting the proper quantity of the drug the patient must assist the physician. It should be just so much as suffices to effect what nature neglects to do, and no more. With some persons an aperient pill acts more comfortably and opportunely if taken at bed time. Sometimes diarrhœa is associated with indigestion. This is usually connected with an excess of acid in the *primæ viæ*. The principle of treatment in such cases is obvious.

Sympathies.—There are innumerable sympathies of distant parts with a dyspeptic stomach, in respect to which I can do little more than barely enumerate a few. Thus indigestion is often accompanied by pain in the head, with some confusion of thought: or at all events, with a loss of mental energy and alertness. Together with a violent headache there is frequently nausea and vomiting; and the complaint is popularly known by the name of the *sick-headache;* or, in the fashionable jargon of the day, as a *bilious* headache. I must refer you to a paper of Dr. Fothergill's in the sixth volume of the *Medical Observations and Inquiries*, for a very good account of this troublesome complication.

I shall hereafter have a good deal to say respecting certain morbid conditions of the urine, which take their rise sometimes from faulty digestion in the stomach, sometimes from faulty assimilation of the digested aliment in the more advanced stages of the process of nutrition. These conditions of the urine, indicating grave derangements of the whole system, furnish the characters and the names of several distinct maladies.

I told you, in a former lecture, that certain affections of the viscera of the thorax are liable to be produced by mere indigestion. Palpitations of the heart, irregularities of the pulse, fits of asthma, are no uncommon accompaniments of a disordered stomach. This is partly to be ascribed to that reflex sympathy between the parts concerned, which I have so frequently mentioned; partly to the effect of flatulence, which, by resisting the descent of the diaphragm, impedes the free working of the lungs and of the heart. I told you, at the same time, that these symptoms torment many persons with the belief that they spring from organic disease. This notion is particularly apt to infect the minds of medical students. I suppose most teachers in our profession partake of that sort of experience which Dr. James Gregory, of Edinburgh, used to speak of in his lectures. He said that scarcely a winter passed over in which several of his pupils did not apply to him on account of palpitations supposed by them to depend upon structural disease of the heart: and in no single instance were their apprehensions well founded. They were all cases of mere dyspepsia and hypochondriasis.

Complications.—Dyspepsia is often connected with phthisis, with leucorrhœa, with amenorrhœa and chlorosis: and some persons imagine that these diseases are *caused* by the dyspepsia. Indigestion may lead indirectly to the development of consumption, by producing debility; but the truer view of the matter seems to be that the dyspepsia is a consequence, rather than an exciting cause, of these complaints. When, for instance, leucorrhœa is cured by topical astringents, as it often may be, the indigestion is often cured too.

One of the worst occasional concomitants of dyspepsia is that peculiar state of the mind to which I just now alluded under the term *hypochondriasis*. This is, in truth, a species of insanity: but it is so often connected with disorder of the digestive organs, that Cullen, whose descriptions of disease are admirably clear and true, however faulty many of his theories may be, defines hypochondriasis to be "*Dyspepsia*—cum languore, mœstitiâ, et metu, ex causis non æquis." In the following short paragraph he completes the picture. "In certain persons there is a state of mind distinguished by the concurrence of the following circumstances. A languor, listlessness, or want of resolution and activity with respect to all undertakings: a disposition to seriousness, sadness, and timidity: as to all

future events, an apprehension of the worst or most unhappy state of them: and therefore, often upon slight grounds, an apprehension of great evil. Such persons are particularly attentive to the state of their own health, to every the smallest change of feeling in their bodies: and from any unusual feeling perhaps of the slightest kind, they apprehend great danger, and even death itself. In respect to all these feelings and apprehensions, there is commonly the most obstinate belief and persuasion."

Now when the attention of the hypochondriac is thus morbidly fixed upon the states and sensations of his digestive organs (as it is very apt to be) the patient becomes a plague to his physicians as well as to himself.

Treatment.—There are a few simple rules which ought always to be kept in mind in our *treatment* of dyspepsia; although we can seldom enforce them, as they ought to be enforced, upon our patients. What patients want, in general, is some medicine that will relieve them from their discomfort and uneasy feelings, and allow them, at the same time, to go on in the indulgence of those habits which have generated the discomfort. And such remedies have not yet been discovered.

One great and indispensable principle in the treatment of indigestion, is that of restricting the *quantity* of food taken at any one time. The gastric juice is probably secreted in a tolerably uniform quantity. The muscular contractions of the stomach must needs be impaired or impeded by much distension of that organ. For both these reasons the quantity of food introduced into the stomach should be kept within the limits of its capacity and powers. The great good which the late Mr. Abernethy unquestionably did to a host of dyspeptic patients, was owing much more, I am persuaded, to the rules of diet, and the restrictions as to quantity, which he laid down, than to his eternal blue pill.

Again, as Dr. Abercrombie has well remarked, and as Dr. Beaumont actually saw, various articles of food are soluble in the stomach with various degrees of readiness. Therefore, when the digestion is liable to be easily impaired, it is of great importance, not only to refrain from those substances which are known to be soluble with difficulty, but also to avoid mixing together in the stomach different substances which are of different degrees of solubility. Hence there are two reasons why it is salutary to dine off one dish. 1st, Because we avoid the injurious admixture just adverted to; and 2ndly, because we escape that appetite and desire to eat too large a *quantity*, which is provoked by new and various flavours.

And another very important principle, greatly insisted on by Mr. Abernethy, is, that the stomach should have *time* to perform one task before another is imposed upon it. He always made his patients (at least he always strongly exhorted them) to interpose not less than six hours between one meal and another. Allowing from three to five hours for the digestion of a meal, and one hour over for the stomach to rest in, Mr. Abernethy's rule seems as much founded in reason as it is justified by experience. But we preach in vain on these topics. Mr. Abernethy was in the habit of saying that no person could be persuaded to pay due attention to his digestive organs, till death, or the dread of death, was staring him in the face. I have now in my mind a family consisting of a mother and three grown-up daughters, who are continually ailing and valetudinary. They profess to have great respect for my professional advice: yet I never can induce them to think that their plan of eating is a bad one. They are not early risers. They get to breakfast about half after ten or eleven. At two they think it absolutely necessary to eat luncheon, which consists of a mutton chop or some hashed meat, with vegetables. At six they dine: and at eight they drink tea: and then they eat no more till the next breakfast. And this is just a picture of the habits of scores of families. They huddle all their food into the stomach, at four periods, within seven or eight hours; and leave it idle for sixteen or seventeen.

Dyspeptic patients are very importunate to know *what* they may eat, and (more so still) what they may drink. It is of course impossible to lay down any general rules which will suit every case. The stomach has its idiosyncrasies. I remember seeing a publication, some years ago, one section of which had this startling title, "Cases of Poisoning by a Mutton Chop." Dr. Prout knew a person who could not eat mutton in any form. He was thought to be whimsical, and mutton was frequently served up to him

under some other guise, without his knowing it: but it invariably caused violent vomiting and diarrhœa. Yet, for the average of stomachs, mutton is probably the most digestible of all meats. And for the average of stomachs some useful general directions may doubtless be given.

In fevers and inflammatory disorders, experience has taught us to forbid or limit the use of flesh meat on account of its stimulating qualities. And when the stomach itself is affected with chronic inflammation, or with morbid sensibility, a diet restricted to farinaceous substances, and milk, is sometimes attended with the happiest consequences. I suspect that a false analogy has led some into the mistake of supposing that animal food ought to be refrained from, or taken in a scanty proportion, in merely dyspeptic complaints. Animal food is easier of digestion, in the human stomach, than vegetable food. It is nearer, in its composition, to the textures into which it is to be incorporated by assimilation. There is less of "conversion" requisite. Indeed we may look upon the appropriation of vegetable matter, by granivorous and graminivorous animals, as one stage of the process by which such vegetable matter is prepared for the sustentation of carnivorous animals: even as one great end of vegetable life seems to be that of generating or concocting matter for the nutrition of the former class, out of inorganic materials, not fitted for that purpose. A more elaborate digesting apparatus is provided for the vegetable eaters. Man, indeed, is omnivorous. But his organs of digestion are more like those of the carnivorous than of the granivorous race. And it is notorious that vegetable food, when the stomach is weak, is followed by more flatulence, that is, is digested with more tardiness and difficulty, than animal food. Nevertheless, a mixture of the two, of well-roasted or boiled flesh or fowl, with a moderate portion of thoroughly cooked vegetables, is better suited, in my opinion, for a feeble stomach, than a rigid adhesion to either kind of aliment singly. Each of the four great classes of alimentary principles specified by Dr. Prout should be represented; the aqueous, the saccharine, the albuminous, and the oleaginous. All meats that have been hardened by culinary art, or by condiments, should be avoided by him, who, as the vulgar express it, has "a bad digester;" all cured meats I mean—ham, tongue, sausages, and so forth. Mutton is thought to be more readily digestible than beef. Pork, its lean part at least, is much less so than either. All raw vegetables also must be eschewed; salads, cucumbers, pickles. But if we press our prohibitions much more strictly than this, we incur the risk of fixing the patient's attention too curiously upon his diet, and upon the sensations of his stomach; and of rendering him hypochondriacally alive to the miserable subject of his feeding.

Again, you will be continually asked whether you recommend malt liquor or wine, wine or brandy and water, white wine or port, sherry or madeira. Now it would be very easy to propound some positive rules in this matter, but it would not be so easy to vindicate them. Some allowance must be made, no doubt, for custom. I believe, however, that most dyspeptic persons would be better without any of these drinks. But it is very difficult to persuade them that the habitual use of strong liquors in small quantities can have any injurious effects. "It is not easy (says Cullen) to engage men to break in upon established habits, or to renounce the pursuits of pleasure; and particularly to persuade men that those practices are truly hurtful, which they have often practised with seeming impunity." They are too ready to believe that it is unsafe to abandon their accustomed indulgence. A friend of mine, who visited, some years since, many of the American prisons, tells me that the health of even the most inveterate spirit-drinkers improves, instead of suffering, upon the sudden and total abstinence from spirits, which the regulations of those prisons enforce. There certainly are cases in which the digestion seems to be helped by a moderate quantity of wine, or beer, or spirits: yet no one can say beforehand—at least I cannot—which of them is to be preferred. Upon these points patients should interrogate their own sensations and experience, instead of seeking the oracular counsel of a physician. Drinks which are followed by evident disturbance and discomfort are manifestly unfit. And even when a favourable effect, for the time, appears to be produced, there is always a risk of ultimate detriment to the powers of the stomach from this habitual excitement.

There are states of mind, and habits of life, which, having no direct relation to the organs of digestion, yet exercise a material influence over their functions. Mental dis-

tress; mental solicitude; mental toil; over-much study; want of exercise: these are all prolific sources of dyspepsia. Sedentary habits, when their injurious effects are known, may be altered: excessive intellectual labour may be abandoned: but it is seldom that we can minister to a wounded or an anxious spirit. Our task is hardest of all when the patient's anxiety relates to his own complaints: when he is morbidly engrossed by his bodily feelings, and despondent about his recovery. The management of the mind of a hypochondriac is peculiarly nice and difficult. It will not do to treat him as if his ailments were imaginary. He disbelieves you, contemns your judgment, and deserts you: to be fleeced perhaps by some villainous quack. You must hear what he has to say; show an interest in his case; and prescribe for him: assuring him that you understand his malady, that it is curable, and that he will be cured provided he follows your directions. If you can succeed in gaining his confidence, and in persuading him of this, the battle is half won. To tell such a person, however, not to think of his grievances would be worse than useless. The very effort to drive a subject from our thoughts fixes it there the more surely. But you must endeavour to turn his attention to other things; and to awaken in him some new interest. Prescribe *change:* change of air; change of place and of scenery; change of society. Get him to *travel* in search of health; and the chances are in favour of his finding it. A tour, in fine weather, and through a pleasant country, combines almost all the ingredients which are, separately even, desirable: the withdrawal of the mind from its ordinary pursuits and cares; the diversion of the attention from one's self, by new and varied objects; exercise, carried on in the open air; a holiday from intellectual toil. Six weeks among the mountains of Switzerland, or upon the rivers of Germany, will often do more towards restoring a dyspeptic hypochondriac than a twelvemonth's regimen and physicking at home.

With these disjointed hints, gentlemen, I must request you to be satisfied in respect to the principles upon which dyspepsia—and the hypochondriasis, which is in general so closely linked with dyspepsia—are to be managed. A full discussion of these subjects in detail would furnish matter for several lectures.

LECTURE LXXI.

ENTERITIS: ITS SYMPTOMS; CAUSES; TREATMENT. MECHANICAL OCCLUSION OF THE INTESTINAL TUBE. COLIC. COLICA PICTONUM: ITS SYMPTOMS, COMPLICATIONS, TREATMENT, AND PREVENTION.

SOME of the diseased states of the intestinal canal, while they differ a good deal in their essential nature, have yet many characters in common. Colic; ileus; enteritis; mechanical obstruction of the tube. It will be convenient, therefore, to consider these disorders in succession, and, in some degree, in connection with each other.

In *colic* we have pain of the abdomen; pain of a twisting or wringing kind, occupying generally the umbilical region; vomiting; and costive bowels. Similar pains are apt to occur in diarrhœa; but they are transitory, and are then termed *gripings*, or more learnedly *tormina*. When they are violent, and more permanent, and, above all, when attended with constipation, they constitute colic.

You have not forgotten the symptoms of *peritonitis*. They are, briefly, pain in the abdomen, increased on pressure; and fever.

Symptoms.—Now if, to the symptoms of colic, you add the symptoms of peritonitis, you have the symptoms of *enteritis:* by which word I desire to express the disease that is commonly called *inflammation of the bowels*. The term has lately been extended so as to signify any and every form of inflammation which any portion or tissue of the intestinal canal within the belly may suffer; but I use it in the old-fashioned meaning. Cullen makes two species of enteritis. One of these consists in inflammation of the mucous

membrane of the intestinal tube: he calls it enteritis *erythematica*. That is not the disorder I am about to speak of; but the other of his species, the enteritis *phlegmonodea*. I say that in colic, we have abdominal pain, constipation, and vomiting. In peritonitis, the functions of the stomach and intestines are not, necessarily, affected: in enteritis they are. There is inflammation, not merely of the peritoneal coat, but of the cellular tissue uniting the several tunics, probably of the muscular tissue also, and often of the whole substance of the bowel at the inflamed part.

When the intestinal channel is any how closed up, and a bar placed to the passage of its contents, the symptoms of colic are very apt to ensue; and at length, the obstacle continuing, fatal inflammation is set up.

The term *ileus* is applied, I believe, to those cases, whether inflammatory or not, in which, by an inverted action of the intestines, their contents are carried, in a retrograde course, into the stomach, and thence out of the body by vomiting.

Having made these explanations, I shall now address myself more especially to the consideration of enteritis.

It is of much practical importance to discriminate between enteritis and the disorders that resemble it; and particularly to distinguish it from colic. When it commences, as it sometimes does, with distinct rigors, and is attended by thirst, a hot skin, and a hard and frequent pulse, there is no room for doubt. But it often begins insidiously, with mere colicky symptoms; the pain, at first, is not much augmented, it may even be somewhat eased, by steady pressure. If we mistake colic for enteritis, the error is of no great moment; but the opposite mistake, which is more common, may be fatal. Blood-letting, and the other remedies of enteritis, will not aggravate the mere colic; they may even, though unnecessary, relieve the patient. Some of the remedies of mere colic are, however, highly dangerous when there is inflammation of the bowel. Physicians may fall into this error; patients who choose to prescribe for themselves, commonly do so. They take stimulants, cordials, carminatives—the pleasantest and nearest at hand is a glass of brandy, or of gin. And in true colic these means are frequently of great service; but they exasperate the symptoms, and increase the mischief when the disease is enteritis. Indeed, treatment of this kind will sometimes urge colic into enteritis. If the case be ambiguous, you must act upon the most unfavourable supposition, and treat the complaint as if you were sure that inflammation was present.

The pain of enteritis is increased by pressure. The pain of colic is not only not made worse, but it is actually mitigated often, by pressure; and it usually intermits entirely. I know that when there is simply flatulent distension of the intestines, pressure does sometimes increase the patient's uneasiness; but the uneasy sensation is very different from that acute sensibility which belongs to inflammation of their peritoneal covering. In enteritis there are also *paroxysms* of severe pain, determined, probably, by the peristaltic movements, or by the temporary distension, of the inflamed parts of the bowel; and the pain has frequently a twisting character: but there is not any thorough intermission. There is a duller abiding pain between the sharper fits. It is to be observed also, as a diagnostic circumstance, that, as in peritonitis, the patient lies on his back, with his knees drawn up, and is fixed in that position, and for the very same reasons. If, in his agony, he tosses about his arms, the trunk is kept motionless, and the respiration is thoracic: whereas in mere colic the mode of breathing is not altered, and the patient is apt to be turning and writhing in all postures, and out of one posture into another.

The nausea and vomiting are often most distressing. The patient not only rejects immediately whatever food, drink, or medicine he swallows, but he has fits of retching when the stomach is empty. In some instances, matters are cast up having the appearance, and something of the odour, of liquid fæces: or resembling, at any rate, the offensive fluids which are found in the small intestines after death in these cases. I cannot say that I have ever seen genuine excrement ejected, unequivocal ordure: yet this may well happen if it be true that clysters, introduced into the rectum, have been voided through the mouth. Such a phenomenon would show that the whole tube was pervious; that there was no mechanical obstruction.

Although the fever, in the outset, may be high, and the pulse strong and hard, it soon becomes (as in all acute abdominal inflammations) small and wiry, or weak, and like a

thread. In bad cases, as the disease proceeds, the abdomen begins to swell, becomes tympanitic; hiccup sometimes comes on; the pulse intermits or beats irregularly; the extremities grow cold; the features are sharpened and ghastly; cold sweats break out; the pain ceases perhaps; and the sphincters relax. The head is generally unaffected. Now and then delirium occurs late in the disease; but much more frequently the intellect remains clear to the very last. Death begins at the heart, and takes place in the way of asthenia.

You may probably have observed that the symptoms which I have been describing are just the symptoms which the surgeon so frequently encounters in cases of strangulated hernia. The symptoms of that surgical complaint are, in truth, most commonly at least, the symptoms of enteritis, caused by the forcible closure of the bowel. Obstruction to the passage of the contents of the gut gives rise to its inflammation. And we often find, after death preceded by the signs of enteritis, an internal mechanical obstruction—an internal strangulated hernia. In some cases, bands or strings of coagulable lymph, the products of bygone inflammation, have formed snares (so to speak) for the gut, which at length they catch and constrict. They do no harm till some coil of intestine gets beneath or beyond them; and then they strangle it, as the phrase is. I have twice seen (as I think I formerly stated) the appendix vermiformis prove the immediate cause of fatal internal hernia. In one of these cases, the person had been a private patient of Dr. Macmichael; and I went with him to examine the body. The free end of the appendix had become adherent to the mesocolon; and so a loop was made, through which a portion of the gut had passed. In the other instance, which I saw in Edinburgh, the appendix was literally tied round a piece of the intestine. Sometimes, again, there is intussusception: the upper part of the tube slips into the lower, fills it up, obstructs it; and inflammation fixes it there. Or it may be that a chronic thickening of the coats of the intestine has narrowed its channel; or a tumour presses upon the intestine from without; or some foreign substance, or morbid accumulation, plugs it up within; in all which cases a chronic disorder passes at length into acute inflammation. A very small hernia at one of the usual orifices—not large enough to manifest itself externally—is sometimes the cause of the obstruction; even though only a portion of one side of the gut may be nipped in the aperture.

Sometimes, but according to my experience less frequently, there is no mechanical impediment to account for the constipation. In all cases, whether there be mechanical obstruction or not, the inflamed portion of the gut is of a red or dark colour; distended by its gaseous or liquid contents; covered often, on its peritoneal surface, with coagulable lymph; or adherent to the contiguous organs. When the gut is mechanically closed, the part which lies on this side the obstacle, as we follow the natural course of the channel, is inflamed; the part which lies beyond it is pale, contracted, and to all appearance, healthy. The line of demarcation is abrupt and strong; and it is determined by the obstacle. And the distinction between the healthy and inflamed portions is usually as sudden and decided, when there is no apparent obstacle. I mean that up to a certain spot the intestine is red like a cherry, or dark like a grape, large, and smeared (perhaps) with lymph or with pus; while immediately beyond that point, and throughout the remainder of its course, it is white, empty, and shrunk up into the semblance of a cord. The pathology of these last forms of the disease is full of difficulty. Some have held that the contracted part was the original seat of disease, namely, of *spasm;* and that the upper adjoining portion of intestine became distended and inflamed in consequence of such spasm. Without attempting to explain these phenomena, which are comparatively unfrequent, I content myself with observing that it is the distended part which is the really inflamed part, and that its muscular coat appears to have lost its natural contractile power—not (I fancy) from overstretching, as now and then happens to the urinary bladder—but from the effect of the inflammation.

The inflamed portion of bowel is often of a very dark colour, and even almost black, from the great congestion of blood in its tissues. Now this black colour has sometimes been erroneously set down as an evidence of gangrene. You must not trust to the mere colour, however. If the coats of the bowel be firm, and if the peculiar odour of gangrene be wanting, you are not to conclude that mortification has taken place, simply from the dark appearance of the intestine.

Enteritis, not dependent upon any mechanical occlusion of the bowel, may arise under the operation of the ordinary causes of internal inflammation: cold and wet, for instance, applied externally, and especially to the feet and legs. Cold is thought to be particularly injurious in this way when the exposure happens soon after a meal: probably because at that time the digestive organs, being in activity, receive a more copious supply of blood.

The mechanical impediments that occur *within* the bowel, are still more various in kind than those which constrict it from *without*. Hardened fæcal matters: intestinal concretions. Some of these are curious, and I shall hereafter have a word or two to say respecting their composition. Persons who are in the habit of taking a good deal of magnesia, or of chalk, to relieve acidity and heartburn, are liable to have these substances accumulate, and become indurated in the intestines. They generally begin to collect, I believe, around some accidental nucleus: and such a nucleus may very readily be supplied. A cherry-stone, a fish-bone, a gall-stone that has found its way into the bowel. Even a pill, prescribed to cure, may thus come to kill one's patient. Dr. Prout was asked to analyze certain odd-shaped, triangular bodies which had been voided at stool, and were supposed to be gall-stones: but he found that they were specimens of Plummer's pill, which had defied the solvent action of the gastric juice, and had passed into, and lingered in, the bowels. Unbruised mustard-seed, and carbonate of iron, are other remedial substances which, injudiciously administered, have collected in hard masses, and caused intestinal disease. It is seldom that we can discover, during life, what is the exact nature of the mechanical obstacle: but I have observed this peculiarity in most of the cases in which I have known it produced by *intussusception*, that after severe and sudden symptoms of obstruction and inflammation, *blood* has been passed by stool. Sometimes the included portion of bowel itself sloughs away, and is expelled *per anum*.

Treatment.—Inflammation of the bowels requires very much the same kind of treatment as peritonitis: indeed, the disease is in most cases peritonitis, and something more. The patient must strictly abstain from every sort of stimulus, and confine himself as much as possible to the horizontal position. He must lose blood also from the arm, and the earlier the better. The lancet is not to be withheld merely because the pulse is small. If the artery becomes fuller, and its beating more distinct, under venesection—nay, if the patient does not become faint—the first bleeding should be a large one. This is not only the safer plan, but in the long run it is the most economical of the blood and strength of the patient. As in simple peritonitis, the abdomen must be covered with leeches, and afterwards with fomentation cloths. Should one spot or region be more painful and tender than another, on that part the leeches are to be accumulated. It may be requisite to repeat the venesection and the leeching once, or twice, or many times; the propriety of such repetition depending upon the urgency and obstinacy of the symptoms, and upon the age and state of the patient. I am speaking of idiopathic enteritis, and of its earlier stages. In the advanced periods, when feebleness of the pulse is associated with tympany of the belly and coldness of the surface, it betokens weakness and sinking, and the tendency to death by asthenia; and, consequently, it then indicates support rather than depletion. So also when the enteritis is not idiopathic, but consecutive—when we have reason to believe that it arises out of a firm mechanical impediment—our employment of blood-letting must needs be modified by that circumstance.

One point in the management of enteritis, requiring great caution and judgment, relates to the exhibition of purgative medicines. The costive state of the bowels is apt to be looked upon as the main evil, and their evacuation as the chief indication of treatment; but great mischief, I apprehend, is likely to arise from the exclusive pursuit of that indication. I am still speaking of the idiopathic disease, where it is presumable that no mechanical obstacle exists to render the passage of the fæces impossible. Purgatives given by the mouth are often rejected by the stomach, with great distress to the patient. If they are retained, and fail to operate, they must do more harm than good. Certainly they should not precede the venesection. I well remember, though it is now many years ago, being myself badly treated for enteritis. Being ill, in a strange place, I sent for the nearest practitioner, who happened to be a very ignorant man. Finding that I was sick, and that my bowels did not act, he gave me, for two or three days in succession,

strong, drastic purges, with no other effect than that of increasing my sickness, and adding to the abdominal pain I suffered. I was then seen by a most intelligent physician (this was before I had paid any attention to physic myself), and the first thing he did was to have me copiously bled; and the immediate effect of that bleeding was to send me to the night chair. And I am persuaded that when evacuations follow the administration of purgatives in such cases, they are owing to the blood-letting and other antiphlogistic measures that are employed at the same time. These are the opinions of Dr. Abercrombie also, who says, "I confess my own impression distinctly to be, that the use of purgatives makes no part of the treatment of the early stages of enteritis; on the contrary, that they are rather likely to be hurtful until the inflammation has been subdued." It is an instructive fact, that when purgative medicines *do* operate during the height of the inflammation, the stools they produce are merely watery; and it is only after the inflammation has been reduced that *fæces* are discharged, and then in such quantity sometimes as to show that they must have been shut up in the bowels during the period of active inflammation.

You should wait, then, for the effect of other measures, blood-letting, leeching, fomentation, before you give active purgatives by the mouth; and in all stages of the disease, whatever laxatives are given should be of the mildest kind, such as are least likely to excite irritation of either the stomach or bowels.

The same objections do not apply to enemata, which soothe while they evacuate the lower parts of the canal. You will read or hear of great service done by an infusion of tobacco, or by the smoke of tobacco, thrown into the bowels through the rectum. Dr. Abercrombie speaks of a weak infusion of the leaves as being a remedy of very general utility. It should, I apprehend, be a *weak* infusion to be at all safe; but, of my own knowledge, I can tell you little or nothing about it. I do know, however, that the injection of a large quantity of merely warm water is often of much use: from two to six pints, for example, thrown up gradually and gently. Indurated fæces are softened and brought away in this manner, and the warm water acts as an internal fomentation.

Of internal medicines I believe the best you can give is a combination of calomel and opium, in such proportions as will restrain the purgative qualities of the calomel. The opium allays pain, and, perhaps, relaxes spasm; mercury tends to arrest the inflammatory action; and the more immediate effects of this combination are often found to be, a settling of the irritable stomach, a disposition to diaphoresis, and an improved pulse.

To sum up then, bleeding, and calomel and opium, are to be resorted to for checking the inflammation; and when that is in great measure abated (and probably not before) it will be right to give some mild laxative to clear out the bowels. There can be none more suitable or convenient than castor oil. But before we venture upon purgatives by the mouth it will be proper to wash out the bowels by lavements of tepid water. These injections *may* sometimes have mechanically a beneficial effect upon the parts concerned in the inflammation; untwisting, for example, a twisted bowel; or setting free the half incarcerated intestine. It is often expedient to administer them through a long tube passed as far as possible into the rectum.

There is one very important point in the treatment of enteritis that I have yet to mention. I have told you that in the advanced stage of the disease symptoms of sinking often come on; a total cessation of pain, failure of the vital powers, and coldness of the body. These symptoms, this collapse and approach to syncope, are generally considered to denote gangrene, and, therefore, a hopeles state of disease. Yet in many cases which have proved fatal after similar symptoms, not a trace of gangrene has been discoverable. So that this unpromising change in the symptoms does not always indicate a morbid condition which is necessarily mortal. And if the patients are to be saved at all, they are to be saved by wine and support. We must combat the obvious tendency to death by asthenia.

"A man, aged forty, was affected with enteritis in the usual form, for which he was treated in the most judicious manner by a respectable practitioner. On the fifth day the pain ceased; the pulse was 140, and extremely feeble and irregular; his face was pale, the features were collapsed, and his whole body was covered with cold perspiration; his bowels had been moved. In this condition (says Dr. Abercrombie, from whom I quote the case) I saw him for the first time. Wine was then given him, at first in large quan-

tities, and, upon the whole, to the extent of from two to three bottles during the next twenty-four hours. On the following day his appearance was improved; his pulse 120, and regular; the wine was continued in diminished quantity. On the third day his pulse was 112, and of good strength, and in a few days more he was well."

Dr. Abercrombie relates other cases to the same effect; and most physicians, I suppose, have occasionally seen such. They teach us that we must not abandon our patients in despair, even under the most adverse circumstances. If diarrhœa should supervene with this state of collapse, opiates must be joined with the wine. External warmth is also a powerful auxiliary.

Mechanical occlusion of intestinal tube.—I know of no cases of disease more painful to witness or to treat than those which result from invincible obstruction of the intestinal tube. They are usually attended, at length, with enteritis; but they differ much in some respects from what I have called idiopathic enteritis. The inflammation is an accident or consequence of the obstruction; or of the means used to overcome it. They happen usually somewhat in this manner. A person thinks it expedient to take some aperient medicine. It has no effect. He repeats the dose. It causes pain and gripings, and probably sickness also; but still the bowels are not moved. Enemata are employed. They bring away, perhaps, some hardened fæces, but there is no further relief. Meanwhile the patient may have a clean tongue, a quiet pulse, a cool and soft skin, and a supple and painless abdomen. Purgatives of a more drastic kind are tried, but tried in vain; and the physician is summoned.

Now the first thing which you have to do, when called to a case of "obstruction of the bowels," is to search narrowly whether there be not some unsuspected external hernia. All delicacy must be waived; and every part of the body, where hernia may possibly show itself, must be submitted to inspection. If none be found, the rectum, and in women the vagina, must be severally explored. Stricture, or a quarry of impacted fæces, may possibly be detected in the one: a uterine or other tumour, pressing upon the bowel, may perchance be felt through the other. I shall never forget the shock I once experienced on being sent for to see a woman, of middle age, who was *in articulo mortis*, and who, as I was told, had for some days been labouring under sickness, pain in the abdomen, and constipation. In her left groin there was a large palpable strangulated hernia, which had not been detected by the practitioner in attendance, simply because it had not been looked for; and which was discovered only when it was too late.

Remember, therefore, that in every case of obstinate costiveness, with signs of inflammation within the abdomen, it is absolutely necessary, for your own credit and subsequent comfort, as well as for your patient's safety, to make diligent and thorough inquiry after such herniæ as may be recognized externally.

But often you find nothing of the sort, and then you are at liberty to prosecute with more energy and decision the purgative plan of treatment. You prescribe strong doses of jalap and calomel; black draughts. The stomach being irritable, you give *pills* of cathartic extract, and repeat them at short intervals; or large doses of calomel, ten grains or a scruple, three or four times in succession. You inject stimulating clysters. Then you are driven to croton oil: and at last, in some vague hope of relaxing spasm, to opiates. If symptoms of inflammation spring up, you put fairly in force the remedies of inflammation; and especially blood-letting. But all is in vain. The medicines are vomited; or, if retained, they serve but to augment the patient's distress, producing or renewing the pain and the nausea. It is extraordinary how comfortable the patient sometimes becomes upon the intermission of these active attempts. Now and then he suffers tormina, or has fits of retching; but in the intervening periods his sensations and outward condition may be those of perfect health; only there is no alvine discharge.

Now, under these afflicting circumstances, the question will force itself upon you—how long am I to pursue the purgative system? Common sense, and common humanity, answer—you must stop it the instant you are convinced that there is a mechanical obstacle, which cannot be overcome. To persist in the use of drastic purgatives after that conviction, is to inflict wanton and needless torture upon the patient. But how are you to know this? That is one difficulty. And how are you, believing that it is so, to satisfy

the patient's friends that his disorder is irremediable; and to resist their importunity to try this and that; and to persuade them to look passively on, while their relative is slowly perhaps, but surely, perishing? These are great and terrible difficulties.

You will be urged by all imaginable suggestions; even the most absurd. Crude mercury may perhaps be one. Pounds of this metal have been swallowed in such cases; in the hope, I suppose, that it would force a passage by its weight. But the obstacle may be in an ascending coil of intestine. And if not, experience does not teach us to put any faith in this rude mechanical remedy. It has often done mischief, and seldom or never done any good. The metal is apt to become oxidized in the body, and then to produce very distressing salivation. This is an evil which I have known to occur, and to trouble the patient greatly, some time after the ineffectual exhibition of large doses of calomel.

Dashing cold water over the abdomen and the lower extremities, is another rough expedient, which is sometimes successful in producing evacuations. It was adopted, after various other measures had failed, in the case in which the bowel was tied down by the adherent appendix vermiformis; and it caused the emptying of that part of the canal which lay beyond or below the internal hernia. It is plain that this partial success can be of little or no use; certainly of none that can compensate for the shock and annoyance of the cold affusion.

Let me not, however, be misunderstood. I do not say that you are to abandon all hope when purgatives (and, if need be, blood-letting) have failed, after a few trials, to obtain evacuations from the bowels. Nor even that you are necessarily, for that reason, to give up the use of purgatives. The intestines may be torpid and insensible to ordinary stimuli, and really require strong rousing. Dr. Abercrombie mentions the case of a gentleman whose bowels were locked up by an accumulation, as the result showed, of black hardened fæces. The obstruction, which had resisted the most active purgatives, and was accompanied by an evident and painful distension of a part of the abdomen, yielded at once to the repeated application of galvanism to that part: each application being immediately followed by a copious evacuation. Here the flagging muscular action was restored, apparently by the galvanic stimulus. Very lately I attended an elderly lady, who from Wednesday morning to the next Monday noon, had had no alvine relief, notwithstanding the employment of the most active cathartics. She suffered frequent paroxysms of pain and vomiting: but the abdomen was scarcely, if at all, distended; nor was it tender. At length she complained that what she vomited was stercoraceous—to use her own words, "what came upwards ought to have passed the other way." It was a thin, brown-coloured, ill-smelling fluid. Dr. Mayo and Mr. Arnott were now associated with me in this case, and they touched and felt the abdomen as I had previously done. The lady observed that their hands were heavy; and she fancied that the pressure they made had displaced something within. And I believe that it was so: for before our consultation in the next room was over, word was brought us that the bowels had acted. She had passed a liquid motion precisely resembling the stuff she had last vomited. The next day, with one of several similar stools, a hard lump was voided, which proved to be a gall-stone, as big as a small walnut.

As large quantities of warm water had without difficulty been injected, I infer that the concretion had been impacted high up in the bowel, probably at the valve of the cæcum. Some years previously the lady had suffered severe abdominal pains, which at the time were ascribed to the passage of a biliary calculus through the gall-ducts.

In this instance there was no evidence of any strong contractile efforts of the bowel above the place of obstruction.

Large enemata—as much as the intestines will patiently receive—gradually and gently introduced, and repeated three or four times a day, may sometimes succeed in breaking down and washing away masses of hardened excrement: and if these enemata are composed of milk or beef-tea, and are suffered to remain as long as they will, they may answer another important purpose; they may contribute sensibly to the nourishment of a patient who cannot retain food in his stomach. They are generally very soothing and comfortable; allaying tormina, and abating sickness: and they are adapted to every stage and variety of the complaint.

There are, however, cases in which we arrive at the melancholy but sure conviction

that some mechanical impediment has rendered the bowel absolutely and hopelessly impassable. We fear this when, the constipation being obstinate, we discover a tumour, or hardness, in some part of the belly: or when we receive a history of some former inflammatory attack, since which the bowels have been habitually difficult to regulate. Our fears are strengthened when the patient feels that the injections reach a certain spot, and there always stop; and that the intestines rumble, and roll, and propel their contents downwards to the same spot, and no further. And this is the distinctive symptom upon which I desire to fix your attention. The abdomen gradually enlarges, especially if the patient is able to retain food; the intestines fill up above the obstacle; and then throes of pain occur, *spasms* the sufferers usually call them, attended with sickness; and during these pangs you may feel, and if the abdomen be uncovered, you may see, immense coils of intestine, as big perhaps as one's arm, rise and roll over, like some huge snake, with loud roarings and flatulence. When this takes place the time for giving purgatives is certainly over. The distended bowel requires no stimulus; it acts, and strives with all its power, but strives in vain, to overcome the opposing barrier. If you would consult your patient's ease, if you would not embitter and abbreviate his slender remnant of life, harass him no further with active remedies. In some cases violent inflammation quickly supervenes, and death arrives in a few days: in others, the patient survives, without any evacuation from his bowels, but with long intervals of ease and comfort, for four, or five, or six weeks. Even a brief respite may be of infinite value; giving time for the arrival of distant friends—for the settlement of worldly concerns—and for preparation of the spirit against the inevitable hour.

It is to these circumstances of irremediable disease that *opiates* are eminently adapted. Even when the symptoms are less desperate, they are not without their use. I cannot better express what I think upon this point than in the words of the first Heberden, who says: "The probability of advantage from anodynes has determined me to recommend them; and experience has strongly confirmed this judgment. Under the protection of an opiate I have successfully given more and stronger purges than would have staid without its help. The patient's strength has been kept up by some refreshing sleeps: and even in hopeless cases, in which the dying person is harassed by unspeakable inquietude, he may be lulled into some composure; and without dying at all sooner, may be well enabled to die more easily. Lord Verulam blames physicians for not making the euthanasia a part of their studies: and surely, though the recovery of the patient be the grand aim of their profession, yet where that cannot be obtained, they should try to disarm death of some of its terror: and if they cannot make him quit his prey, and the life must be lost, they may still prevail to have it taken away in the most merciful manner."

I have spoken of these cases as being irremediable: but perhaps I have spoken somewhat too absolutely. Life may, sometimes, be saved by a surgical operation. The gut may be punctured above the seat of obstruction, and suffered to discharge its contents through what is called an *artificial anus*. This expedient is feasible, however, in those cases *only* in which the obstacle is situated near the termination of the large intestine; *i. e.*, in the rectum, or in the lower part of the descending colon: for here only can we ascertain, with any thing like certainty, the exact place of the impediment, and here only may the colon be pierced, from behind, without injury to the peritoneum. Under no other circumstances, in my opinion, would an attempt to relieve the patient by cutting down upon and opening the bowel, be justifiable. An artificial outlet in the loin would probably be less disgusting to the patient, and less offensive to others, than if it were in front of the body. I am not aware that the operation has ever been done in this country. It has been practised successfully in France. The mode of performing it I am not competent to teach you. In an appropriate case I should think it right to *mention*, though I should be slow to *recommend* this *anceps remedium*. The risk of failure, and the penalty of success, should both be set fairly before the patient. The choice between certain death, on the one hand, and the chance of living with a very loathsome bodily infirmity, on the other, must be made by himself.

For further information upon this subject, I would refer you to an interesting essay, by Mr. Erichsen, published in the 28th volume of the *Medical Gazette*.

Colic.—It remains that I should say something more respecting *colic:* which may exist independently of enteritis, and of mechanical occlusion of the bowel: although the three are very often combined and intermixed in the course of the same disease.

In colic there is pain in the abdomen, constipation of the bowels, vomiting often: and these are symptoms which occur also in enteritis. The pain is a twisting or wringing pain generally, round the navel: and such is the character of the exacerbations of pain experienced when the bowels are inflamed. These are the points of *resemblance* between the two maladies: and it is of much importance, as I told you before, to observe the *differences;* and to note the marks by which the one may be distinguished from the other. It is, then, an essential difference that enteritis is attended with fever, and with tenderness of the belly. The pain is increased by all kinds and every degree of pressure; and the patient, fixed in the supine position, breathes with the intercostal muscles only, and carefully avoids any movement which would call the abdominal muscles into action, and so compress the inflamed bowel. In colic, on the other hand, the circulation is tranquil; there is no fever; and the pain is even mitigated by pressure. The patient will lie on his belly for ease: nay, he will press it, with the whole weight of his body, across the back of a chair, and obtain comfort by that manœuvre. During the paroxysms the pain is often most violent: what the old writers call *dolor atrox*—atrocious pain: but there are intervals of complete ease. Even when the pain is worst, the patient tosses and shifts from one posture to another in search of relief; and he does not wear that anxious and apprehensive aspect which we see in those who are labouring under enteritis. The pain arises, I imagine, from the distension of the bowel, here and there, by gas; or, it may be, from spasm; or from both these states at once. At any rate, it is often associated with audible flatulence, and with evident outward spasm. The abdomen is hard, and drawn spasmodically towards the vertebral column; and its muscles are partially and strongly contracted, gathered up into lumps and knots. None of these circumstances belong to enteritis.

However, there is good reason for believing that, even in mere colic, the pain may sometimes be augmented by pressure. When a portion of the gut has become rapidly distended, considerable uneasiness may result from its forcible compression; although, as Dr. Abercrombie states, the kind of pain can generally, by attention, be distinguished from the sensitive tenderness of an inflamed peritoneum. Luckily, if such pain on pressure should lead us to mistake a case of pure colic for a case of enteritis, the error is on the safe side: and we must always bear in mind the tendency remarked in colic, when the complaint is neglected, or badly treated, to run into actual inflammation. In fact, as any obstacle to the passage of the alimentary matters through the bowels may give rise to colic, colic is sometimes merely the first step towards acute inflammation arising out of a continuance of the obstruction. And having told you that colic may be thus produced, I have at once introduced you to a large class of its causes which have already been spoken of in the present lecture as frequent causes of inflammation also.

Colica Pictonum.—But colic, like inflammation of the bowels, sometimes arises without any apparent or detectible obstruction, of a mechanical kind, to the free transit of the contents of the alimentary tube. And there is one particular form of colic, that requires a separate notice. The colica *Pictonum*—so called from its great frequency, heretofore, among the Pictones, or inhabitants of Poictou—is produced by the slow introduction of the poison of lead into the system.

Now the colic which has this origin is not to be distinguished, in its ordinary symptoms, from any other kind of colic. But the abdominal pain is usually, in such cases, a part only of more general disease. It has received, in different places, a variety of names. Colica Pictonum; the painter's colic; the Devonshire colic; the bellain of Derbyshire; the dry belly-ache of the West Indies. In all cases it acknowledges the same cause—the gradual entrance of lead into the body.

In this country we see the disease more often in painters than in any other persons. They use, as you know, white lead in the preparation of their colours; and they are perfectly familiar with this terrible colic. We see it also in all workmen whose occupations bring them habitually into contact with preparations of lead. No doubt there are very

great differences in the susceptibility of this effect of the poison of lead. Persons have been known to suffer colica pictonum, in consequence of their sleeping for a night or two in a recently painted room. On the other hand, I have myself seen a patient who became affected with the disease, for the first time, after working with white lead for nineteen years. Generally the first attacks of colic are recovered from. The obstinate constipation of the bowels is at length overcome; the patient obtains ease; and forthwith recurs to his previous habits; and after a period, which varies in different individuals, he is again laid up with the colic. Even the primary attacks are usually attended with pains in the head, and in the limbs; sometimes with cramps; sometimes even with epilepsy and coma. At length, in one of these attacks of colic, or after one of them, when the violence of the pain, and the costiveness, have yielded to treatment, the patient finds that he has lost the full power of using one or both of his hands. The wrists, as the patients express it, *drop*. You see at once what is the matter, by the characteristic state of the arms and hands. The extensor muscles of the hands and fingers are palsied: so that when the arms are stretched out, the hands hang dangling down by their own weight; and the patient is unable, by any effort of his will, to raise them. The palsy is local; it does not proceed from any diseased condition of the nervous centres. The affected muscles waste; and the atrophy is very remarkably seen in the bundle of muscles composing the ball of the thumb. Even from this condition the patients often are capable of complete recovery. But if they persist in following their former calling—or if without knowing it, they continue to be habitually exposed to the exciting cause of the disease—they become miserable cripples, fall into a state of general cachexia, and sink at length under some visceral disease. The poison accumulates in the body, and saps the powers of life. Francis Citois, a native of Poictou, who published an excellent and one of the earliest accounts of the disease, in the year 1617, has drawn the following graphic picture of its effects. Its cause was not at that time suspected. Speaking of the wretched sufferers, he says, "Per vicos, veluti larvæ, aut arte progredientes statuæ, pallidi, squalidi, macilenti conspiciuntur; manibus incurvis, et suo pondere pendulis, nec nisi arte ad os et cæteras supernas partes sublatis, et pedibus non suis sed crurum musculis, ad ridiculum ni miserandum incassum compositis, voce clangosâ et streperâ."

The course of the disease is usully such as I have just described it. The colic happens first, perhaps several times: and then arrives the palsy. But in a few instances I have known this order reversed. The wrists have dropped, when there had been no preceding colic.

The great cause of this fearful malady was first made out by our distinguished countryman, Sir George Baker. He set on foot an inquiry into the origin of what was called the *Devonshire colic;* so common was it in that county. He found, first, that it occurred chiefly in persons who drank the *cider* manufactured there; and, by degrees, he traced the source of the malady to the admixture of lead with the cider; either designedly, for the purpose of sweetening it; or by the inadvertent employment of lead in the construction of the cider mills and vats. It was under circumstances of the same kind that the colic of Pictou originated. Preparations of lead were used—not fraudulently, but openly and honestly—to prevent the wines of the country from turning sour; the injurious influence of lead upon the human body not having then been ascertained. So also, equally convincing proofs of the adulteration of *rum* by means of lead, giving rise to frequent attacks of dry belly-ache in the West Indies, are given by Dr. John Hunter, in the *Medical Transactions.* I invite your attention to his papers, and to Sir George Baker's, on this subject. They afford a capital specimen of medical research and reasoning. Various causes, as you may suppose, had been assigned for this disorder. These are one by one investigated, and set aside; until, by this method of exclusion, the real source of all the mischief is detected. Sir George Baker's papers contain a great deal of curious and useful information in respect to the various modes in which this poison of lead may find its way, without being suspected, into the animal economy. The subject is one of vast importance in its relation to medical police; but my limits will not allow me to follow it beyond the point where it ceases to be directly connected with the practice of physic.

Colica Pictonum is seldom fatal as colic; or during the persistence of the abdominal symptoms: yet instances enough of death occurring while the colic was present, but from

other accidental causes, have now been collected, to enable us to say, that no appearances have been met with in the intestinal canal, calculated to explain the pain or the constipation. Andral relates five cases, in which the body was carefully examined after death preceded by the painter's colic. He found neither inflammation, nor any remaining trace of spasm. The intestines were neither dilated nor contracted, but of their natural texture and appearance. Merat, who has written a good treatise on this form of colic, gives the dissections in four fatal cases: fatal, as I mentioned that they sometimes are, by the supervention of coma. There was no discoverable mark of disease; the alimentary canal was empty, and the large bowels contracted; as they were also found to be in rabbits which had died of lead colic. For animals are susceptible of the disease, and it may be produced in them by the slow impregnation of their bodies with the specific poison. Dogs, cats, and rats, that inhabit houses and manufactories wherein lead is much used or prepared, are known to be attacked both with colic and with palsy. With respect to the contraction of the large intestines in these cases, we must not be too ready to attribute it to spasm; for the bowel, when empty, is apt to be contracted.

When the palsy has been of any continuance, the affected muscles not only shrink and waste, but undergo a structural change, which is obvious to the sight. They become pale, almost white, dry. John Hunter examined the muscles of the hand and arm of a house-painter, who died, while thus paralytic, in St. George's Hospital. He found them of a cream colour and opaque; instead of being of a purplish red, and semi-transparent. And, since his time, lead has been detected in the palsied muscles, and in the brain, by chemical analysis. It is doubtless conveyed, by the blood, to all parts of the body: why it fastens solely or chiefly on particular muscles, or particular nerves, nobody knows. The pain it occasions, whether in the abdomen or in the limbs, is generally thought to be neuralgic. It is one of the poisons that do not appear to find a ready exit from the body. Very recently, a most curious symptom, pathognomonic, I believe, of the presence of lead in the system, has been pointed out by Dr. Burton: and now that it has been pointed out, one can hardly understand how it escaped discovery so long. It is a blue or purplish line running along the edges of the gums, just where they meet the teeth. Dr. Burton first noticed this six years ago, but wisely refrained from making his observations public until he had had time and opportunity enough to satisfy his mind that he was not mistaken. A paper of his on the subject was read at the Medical and Chirurgical Society last January (1840). I cannot resist the temptation to read to you the notes of a case which has subsequently occurred to me in the hospital. Mary Ann Davis, a middle-aged woman, presented herself with dropped wrists. It was an exquisite example of palsy of the extensor muscles of the hands and fingers. She could raise her arms, but her hands hung down like the talons of a bird, or like the fore paws of an erect kangaroo. This began nine weeks before. She complained of pain, beginning under the nails, stretching up the back of her hands, and reaching sometimes to the elbows. The bottoms of her feet had also been tender; and at night were burning hot.

Seeing the dropped wrists, we thought immediately of the poison of lead. But the patient was a female. We do not often see these effects of that poison in women; for obvious reasons. At first we could get no clue to the mode in which lead could have found its way into her system. Her husband was a broker. She had not been living in a newly-painted house: and had (she said) no concern with lead in any way. Lead often creeps in, however, through undetected channels, and I could not help suspecting it here. Mr. Pyper, my clinical assistant, soon elicited another part of her history, which added to our suspicions; namely, that before the palsy occurred, she had had pains in the abdomen, and costive bowels, for five days together. Nay, she had a recurrence of colic after her admission. This was a strongly corroborating fact; but what clenched the proof was the discovery of a decided blue rim along the edges of nearly all her gums. This conclusive evidence led to further cross-examination; and at last it came out that some of her sons (she had seven) had occupied their leisure time in the preceding summer with making bird-cages, and painting them green, in the one room in which she habitually lived. The case altogether was a very neat one.

Mr. Tomes pointed out to me some interesting circumstances which led him, at one time, to think it probable that the colour might be produced by some chemical action be-

tween the tartar that forms on the teeth, where they meet the gums, and the lead which pervades the system. This woman's teeth, like those of many in her rank of life, were loaded with tartar. In one place was visible a stump, level with the gum, and surrounded by a ring of tartar; and *there* was also a corresponding border of blue. In other places there were gaps, where teeth once were; here there was, of course, no tartar; and here there was no blue line on the edge of the gum. The blueness may however appear when there is no tartar at all. I do not know exactly in what manner the chemical union arises, but the colour depends, I believe, upon a *sulphuret* of lead, formed somehow by the action of sulphuretted hydrogen upon the metal. It is curious that the *edge* of the gum should always be the part thus marked; the very part which first exhibits the influence of another metal, mercury, when ptyalism is about to ensue.

This discovery of Dr. Burton's is not a mere piece of curiosity, but is likely to be of use in various ways. In the first place, it may settle the nature and cause of many doubtful cases; as it did, indeed, of the one just narrated. The poison of lead produces pains which resemble, and no doubt are sometimes mistaken for, the pains of rheumatism; it has other obscure consequences too: and an inspection of the gums may often greatly elucidate such cases. Dr. Burton finds that small quantities of lead given as a medicine will produce the phenomenon. In two instances it occurred within two days. One of these patients had taken fifteen grains of the acetate of lead in that time; the other twenty-four grains. A correspondent of his produced the blue rim in twenty-four hours; viz. by four doses, of five grains each, exhibited every six hours. He finds too that the line remains distinct after death; even more so than during life. It may afford valuable information therefore to the medical jurist in cases of suspected poisoning with the salts of lead. It is, I say, an *early* consequence of the absorption of lead. It will teach us, as the mercurial affection of the gums teaches us, that the medicine is pervading the system; and admonish us to look out for, and guard against, colic. It may apprise workers in lead that their caution has been insufficient; that the poison has entered; and that they are in peril of belly-aches and palsy. It is a capital diagnostic sign also between colic so arising, and colic from other causes; and between colica pictonum, and the pain of inflammation of the bowels.

In the *treatment* of colic—and especially of the lead colic—the great indication is to get the bowels to act. If the pain of the belly be increased on pressure, if the pulse be at all accelerated, if the face be flushed, and there be the slightest approach to fever, it will be right to take blood from the arm. It is a measure of safety as regards the possible existence of inflammation: and if there be no inflammation present or impending, it will tend to remove the spasmodic state of the muscles which goes along with, and perhaps chiefly constitutes, the disease. External warmth should also be applied; diligent friction, with some stimulating liniment; or, what is much better, a mustard poultice, or a turpentine stupe. My colleague, Dr. Wilson, has been very successful in relieving these patients, by putting them into a hot bath, and having a large quantity of the water in which they are immersed, thrown gradually into their bowels by means of a proper syringe. The bath presently becomes polluted, to the great satisfaction and refreshment of the patient. It will generally be expedient to give a full dose of calomel and opium; ten grains of the one with two of the other. Sometimes the effect of the opiate is to suffer the bowels to empty themselves; showing that the previous difficulty was probably spasmodic. Usually the calomel and opium will soothe the vomiting. the restlessness and the pain; and then a full dose of neutral salts, or of castor-oil, or (if these do not succeed) of the last-named remedy, castor-oil, quickened by one or two drops of the oil of croton, will produce free evacuations from the bowels; and the patient soon returns to his ordinary state of health. It is sometimes necessary to repeat this practice, this alternation of purgatives and anodynes: but when once the bowels have been freely moved, the disease, in general, becomes very tractable.

At La Charité, in Paris, there is what is called a specific mode of treatment followed. It is complicated and rough, but not a whit more successful than the simpler plan which is universally adopted in this country. You may see it described, if you are curious on the subject, in most of the French books. I think it is given in detail in Ratier's *Formulary of Hospital Practice.*

Some have recommended salivation for the cure of the painter's colic, on the principle, I conjecture, of driving out one metallic poison by another. But the two may combine, for aught I know, to plague the patient. The practice is quite unnecessary.

You will be consulted about the *palsy* which arises from lead, and especially to remedy the dropped wrists, which render the patient incapable of earning his livelihood. Now in the early stages of the palsy, and in its primary attacks, you may often succeed in effecting a cure. Electricity has been thought useful, applied in the way of sparks at first, and of slight shocks afterwards, along the muscular parts of the extensors of the fingers. It accelerates the recovery to give the hand and fingers the mechanical support of a splint, made for that express purpose, and so contrived that the hand and fingers are kept extended through the greater part of the day. Patients labouring under this kind of palsy resort to the Bath waters for a cure: and I learn from a gentleman who once held the office of house-surgeon to the hospital there, that the physicans have much more faith in the use of the baths, with shampooing, and in splints and blisters to the palsied muscles, than in electricity. The warm douche is a promising expedient: the electro-magnetic apparatus, perhaps, still more so.

According to Andral and others, who have had more experience of that drug than I have, there is no form of palsy so likely to be benefited by *strychnine* as this which proceeds from the poison of lead. I should recommend you to try the safer methods of binding the hand to a splint, and stimulating the muscles by friction, shampooing, or electricity, before you resort to that active poison.

It is observable of this disease, as of many, and, indeed, of most others, except certain contagious febrile diseases, that when once it has occurred, it is much more liable to occur again, upon a repetition of the exciting cause, than before. It is of very great moment, therefore, that they who are necessarily exposed to the poison of lead—as painters, plumbers, printers (who handle leaden types), colour-grinders, potters, and glass-blowers (who use the oxide of lead in their respective manufactories), shot-makers, workers in lead-mines, and so on—it is of great importance that these persons should be made aware of the means which are best adapted for their protection against the injurious agency of the poison: and we ought to be able to give them advice in that matter. The rules for their guidance are short and simple; and if carefully observed, I believe they will generally prove successful. They resolve themselves into cautions against the admission of the metal or its compounds into the body through any channel.

1. To prevent its introduction through the skin minute attention to *cleanliness* is necessary. The face and hands should be washed, the mouth rinsed, and the hair combed, several times in the day; and bathing and ablution of the whole body should be frequently performed: also, the working-clothes should not be made of woollen, but of strong compact linen; and they should be washed once or twice a week at least; and they should be worn as little as possible out of the workshop: and some light impervious cap might protect the head while the person is at work.

2. Care should be taken that none of the poison be admitted into the system *with the food*. The workmen, therefore, should not take their meals in the work-room, and should be scrupulous in cleansing their hands and lips before eating.

3. The entrance of the poison into the air-passages during respiration should be guarded against as much as possible. Masks have been recommended for this purpose: none, probably, would be more convenient or more effectual, than Mr. Jeffreys' orinasal respirator.

There is a notion prevalent in some places, which apparently has some foundation, that the free use of fat, and of oily substances, as food, is a preservative against the colic. A physician, near Breda, informed Sir George Baker that the village in which he lived contained a great number of potters, among whom he did not witness a single case of lead colic in the course of fifteen years; and he attributed their immunity to their having lived very much on butter and bacon, and other fat kinds of food. De Haen also was told by a physician, the proprietor of a lead-mine in Styria, that the labourers there were once very subject to colic and palsy; but that after they were exhorted by a quack doctor to eat a good deal of fat, especially at breakfast, they were exempt from these disorders for three years. This is a kind of prophylaxis that is very easily adopted.

More recently Liebig has asserted that "the disease called painter's colic is unknown in all manufactories of white lead in which the workmen are accustomed to take, as a preservative, *sulphuric acid lemonade*, a solution of sugar rendered acid by sulphuric acid."

If this be so, the lemonade must protect the system by converting any other salt of lead, which might find entrance, into an *insoluble* sulphate: solubility being requisite to give efficacy to any poisonous substance.

Mr. Benson, the manager of the British white-lead works, in Birmingham, states (in the *Lancet*) that he has tried this method of prevention. Under his direction sulphuric acid was first added to the *treacle-beer*, used as a beverage by the workmen, in the summer of 1841. Lead colic, which had prevailed before "to a distressing extent," soon began to diminish in frequency: and from October in the same year, up to the date of Mr. Benson's communication in December, 1842—a period of fifteen months—not a single instance of the disorder had occurred amongst them. This is very encouraging.

LECTURE LXXII.

Diarrhœa. Sporadic Cholera. Epidemic Cholera.

The morbid *fluxes* which proceed from the long tract of mucous membrane that lies between the stomach and the anus are many in number: and they vary much both in kind and in cause. *Hæmorrhages* are not uncommon. I have already described the disease called *melæna*, which is characterized by the discharge of black semifluid matters, resembling tar, from the bowels, and in most instances from the stomach also by vomiting. The matters vomited, and the matters passed by stool, are composed principally of blood, which has been rendered black, and otherwise modified in appearance, during its progress outwards in the one direction and in the other. Again, hæmorrhage from the bowels is apt to occur in *continued fever;* as I shall show you when we come to that disease. Hæmorrhage takes place also from the rectum in *hæmorrhois*, or *bleeding piles:* a malady that falls chiefly to the care of the surgeon. Blood comes away, too, mixed with a greater or less quantity of mucus, in *dysentery*.

The remaining forms of profluvia from the intestinal canal I shall proceed to consider *seriatim;* at least the most important of them.

Diarrhœa.—There are several very different affections classed together under the head of *diarrhœa:* by which term is usually signified the occurrence of frequent, loose, or liquid alvine evacuations. Thus diarrhœa is a very common symptom of pulmonary phthisis; and this form of the disorder has been already mentioned. It is very often met with also in continued fever, and during the decline of the febrile exanthemata, of which I have yet to speak. But diarrhœa is not unfrequently the main symptom of the illness under which the patient labours; and constitutes, at any rate, the chief object of our treatment. I shall touch briefly on some of its varieties.

In the first place, there is that common form of the complaint which proceeds from over-repletion of the stomach; or from the ingestion of food that is not wholesome: food that disagrees (as the phrase is) with the patient's stomach and bowels at that particular time. We may call it, with Cullen, by way or distinction, diarrhœa *crapulosa;* in which fæces are discharged in a more liquid state, and more copiously, and more often, than is natural. These cases are in truth slight cases of irritant poisoning. The ingesta irritate the mucous surface, and probably the muscular coat also; the secretions of the inner membrane are poured forth in unusual abundance, and the peristaltic motion of the intestines become more strong and active; the object of these changes being that of getting rid of the offending substances: a salutary and conservative effort, which we assist and imitate in our *treatment* of this form of diarrhœa.

The *symptoms* by which this species of diarrhœa is marked, must be well known to us all. There are often nausea; flatulence; griping pains in the bowels, succeeded by stools of unnatural appearance and odour, and of fluid or watery consistence. There are often, also, a furred tongue and a foul breath: but the disorder is attended with little or no fever; the pulse remains of the ordinary frequency; and the temperature of the body does not rise.

Causes.—There are certain things which, more than others, tend when taken into the stomach to cause this crapulous diarrhœa: and there are certain circumstances which increase the disposition to be affected by the ordinary exciting causes.

We frequently see this disorder supervene upon a debauch, in which case the *mixture* of various articles of food, and of drink, each of which in itself might have been perfectly innocent—and the actual *quantity* of the mixed ingesta—have occasioned the irritation and disturbance. But where there has been no intemperance in eating or drinking, some kinds of food are more likely than others, *cæteris paribus*, to provoke diarrhœa. I do not speak of idiosyncrasies, which show the truth of the old proverb, that what is one man's food is another man's poison, and which cannot be reckoned upon beforehand; but I refer to the average of systems and stomachs. And among these indigestible and irritating substances we may place *raw vegetables* of many kinds; such as cucumbers and salads, sundry kinds of fruit, especially if they be hard, immature and acid; plums, melons, pine-apples, nuts, and so forth. Mushrooms may be added to the list, even when they are cooked. *Putrid* food, or food which, in the more refined phraseology of gastronomers, is termed *high*, has the same effect upon some persons: and so, in a particular manner, have some kinds of *fish;* shell-fish, crabs and muscles for instance, in this country; and in other countries, in the West Indies, there are several species of fish which are actually poisonous, and cannot be safely eaten at all. A similar disorder is frequently produced in children by any sort of food, other than the natural sustenance furnished by the mother. The new kind of nutriment disagrees with them: and the very same thing is apt to occur in adult persons. An article of diet which is perfectly wholesome and digestible, and which the stomach bears well after a little habit, will sometimes cause griping and purging, when it is taken for the first time. It is upon this principle that the diarrhœa to which Englishmen are subject upon their first visiting the towns upon the continent, is to be explained. I do not know that it is so, but I think it very likely that Frenchmen, and Germans, and Italians, suffer in the same way when they first come to this country, and adopt our habits and regimen.

Another curious exciting cause is to be found in certain *mental emotions*, and especially the depressing passions; grief, and above all, fear. A sudden panic will operate on the bowels of some persons as *surely* as a black dose, and much more *speedily*. Among the circumstances which *predispose* most persons to this kind of malady, we may particularly specify *season*—the hot weather of summer and autumn. And it is probably consistent with the experience of most of you, that the atmosphere of the dissecting-room has a similar tendency.

Treatment.—Now this diarrhœa, from occasional irritation, produced by the presence of substances that offend the stomach or bowels, will generally cease of itself. The purging is the natural way of getting rid of the irritant cause. We may *favour* the recovery by diluent drinks, and by making the patient abstain from all further use of food which is not perfectly easy of digestion; and we may often *accelerate* the recovery by sweeping out the alimentary canal by some safe purgative, and then soothing it by an opiate. Or we may give the aperient and the anodyne together, and the one will not interfere with the operation of the other. A table spoonful of castor oil, with six or eight minims of laudanum dropped upon it: or from fifteen grains to a scruple of powdered rhubarb, with half as much of the *pulvis cretæ compositus cum opio*. By some such medication as this emptying the bowels, and quieting them, the cure is generally accomplished with ease, and speedily: *tuto, cito, et jucunde*.

We sometimes however meet with cases in which diarrhœa *runs on:* the stools being composed of fæcal matter in an unnaturally fluid state; and the precise condition on which this disposition to an over-loose state of the bowels depends, escaping detection. If the disorder be slight, it will often yield to the astringent and bitter medicines. The infusion

of cusparia, and the tincture of cinnamon, supply a convenient formula. If it be more severe, or obstinate, we have recourse to chalk mixture, which neutralizes acidity; combined with catechu, which is a direct astringent of the tissues; and with laudanum, which calms irritation. And in extreme cases the sulphate of copper has been found to have a powerful effect in restraining the flux. It is apt to gripe, and should be combined therefore with opium. A quarter of a grain of each, in a pill, given three or four times a day, I have frequently found successful, when previous attempts to remove the diarrhœa had failed.

Cholera.—I have alluded to the influence of *hot weather* in predisposing the system to be affected by the exciting causes of diarrhœa. And there is a complaint—of which diarrhœa is one prominent symptom, but which is something more than mere diarrhœa—that shows itself in this country, more or less every autumn, and prevails extensively in some years, as a minor epidemic. It is rightly enough named *cholera;* for it is attended with, and consists mainly of, a remarkable flux of *bile.* Sydenham held that the disease is limited to the month of August; and that bowel affections, with vomiting, occurring at other times, are not genuine cases of cholera. But this was one of that great man's crotchets. The symptoms that mark this complaint are vomiting and purging of liquid matter, deeply tinged with, and principally composed of, bile; violent pains in the stomach and bowels; cramps of the legs and of the abdominal muscles; a great depression of the vital powers, and a tendency to syncope or collapse.

The attack is generally sudden. At first the contents of the alimentary canal are evacuated; and then a quantity, an enormous quantity sometimes, of a turbid, yellowish, acrid fluid, is expelled with violence both from the bowels, and by vomiting. The patients complain of a burning sensation in the epigastrium. As the vomiting and purging go on, clonic spasms of the lower extremities, and especially of the gastrocnemii, occur; the surface of the belly is drawn up into knots: and after a while, the patient, exhausted by the pain and the spasms, and still more so by the copious discharges, grows cold and faint. Sometimes actual syncope happens: and sometimes death.

Death, however, is an uncommon event of this form of cholera, in this country.

The chief cause of cholera, such as has now been described, appears to be casual exposure to cold, after a continued high temperature of the atmosphere: and the great irritation of the stomach and bowels evinced by the symptoms, proceeds from the presence of bile in the intestines in undue quantity, and rendered more acrid than usual by some morbid alteration of its quality. The attack seems to be often *determined* by some of those causes of irritation which I just now mentioned when speaking of simple diarrhœa; and particularly by imprudence in eating and drinking.

I believe no better *treatment* can be followed in this disease than that long ago laid down by Sydenham. He observes that any attempt to stop the purging and vomiting by strong drastic aperients, under the notion of expelling the irritant matter, would be like endeavouring to extinguish fire by pouring oil upon it; and that to try to lock up the acrid discharges in the alimentary canal by means of narcotics or astringents, would be equally hurtful. He therefore was accustomed to dilute the contents of the stomach and bowels by emollient drinks, and injections, especially by chicken broth; and so to favour their expulsion: and when any faintness or sign of sinking began to show itself, to administer *laudanum* in full doses. We are seldom summoned to these cases in the outset. Generally the vomiting and diarrhœa have continued for some hours before we see the patient; so that it is expedient to give the opiate as soon as we can. If the stomach be very irritable, solid opium in the form of pill may be preferable to laudanum; or an opiate clyster—or an opiate suppository—may be introduced into the rectum. When the skin is cold, and the pulse sinking or irregular, carbonate of ammonia, or brandy and water, may be given by the mouth; and a mustard poultice, or a bag of hot salt, or a moist and hot flannel sprinkled with oil of turpentine, should be applied to the abdomen. The cramps of the extremities may be relieved by diligent friction with the hand; or some stimulating liniment may be rubbed upon the affected muscles. When the collapse is great, the patient should not be allowed to raise himself out of the horizontal posture, lest fatal syncope should follow. Opium, however, is our sheet-anchor in this complaint: it sustains the flagging powers, while it quiets the gastro-intestinal irritation.

After an attack of severe cholera, the patient is apt to be left extremely feeble; with soreness of the muscles of the trunk and limbs: and sometimes, symptoms of *inflammation* of the mucous membranes will supervene; pain and tenderness of the belly, a white tongue, thirst, and fever. And these symptoms may require some of the *remedies* of inflammation.

Epidemic cholera.—Such is the disease which has long been familiar to English practitioners, as *cholera:* but about twelve years ago, this country was visited by a severe epidemic disorder, which was also called cholera; or by way of emphasis, *the* cholera; or sometimes *spasmodic* cholera; or *Asiatic* cholera; or *malignant* cholera. The symptoms of this new disease resembled, in some points, those of the old-fashioned cholera: but differed from them in more, and in more important, particulars. So that the application of the term cholera, or cholera morbus, to both these morbid affections, is very much to be regretted, for it has produced a great deal of confusion and inconvenience.

I scarcely know how to name the newer and severer disorder. I have no right to alter the received nomenclature; and choosing from among the many appellations which have been given to the complaint, that epithet which seems the least objectionable, I may call it *epidemic* cholera; although this term is objectionable, since the other malady, the *English* or *sporadic* cholera, is sometimes also epidemic.

The *epidemic* cholera so far resembled the *sporadic*, that it was attended by profuse vomiting and purging, by extreme prostration of strength, and by cramps. But it differed remarkably in *these* respects; in the circumstance that the matters ejected from the stomach and bowels contained no bile (and this alone is a good reason against calling the disease *cholera*); in the early supervention of the symptoms of collapse; and in the great mortality of the disorder.

The amount of the fluid matters thrown up from the stomach and discharged by the bowels, was really in many cases wonderful. At first, perhaps, the patient would have so copious a stool—a consistent dejection it might be, but so large in quantity—as to lead him to conclude that the whole contents of the intestines had been evacuated at once. Yet soon afterwards a turbid whitish liquid would again and again pour from his bowels in streams; and be spouted from his mouth as if from a pump: not in general with much effort, but easily and abundantly. The matters thus discharged were thin, and for the most part of a whitish colour, like water in which rice has been boiled; without fæcal smell; and containing small white albuminous flakes. There were some varieties in the evacuations, but the kind I have mentioned, resembling rice-water, was the most common and the most characteristic: and however *else* their sensible qualities might vary, *this* circumstance was universal, that they contained no *bile*.

With all this there was early sinking, and collapse, as it was called. This term *collapse* expressed a general condition, made up, in the most exquisite cases, of the following particulars:—A remarkable change took place in the circulation, and a striking alteration in the appearance of the patient. The pulse became frequent, *very* small and feeble, and at last, even for hours sometimes, extinct at the wrists. The surface grew cold; and in most, or in many instances, blue as well as cold. The lips were purple; the tongue was of the colour of lead, and sensibly and unpleasantly cold to the touch, like a frog's belly; and the breath could be felt to be cold. With this coldness and blueness there was a manifest shrinking and diminution of the bulk of the body. The eyes appeared sunk deep in their sockets; the cheeks fallen: in short, the countenance became as withered and ghastly as that of a corpse. The cadaverous aspect that sometimes precedes death in long-standing diseases, would come on in the course of an hour or two, in this complaint. If the physician left his patient for half an hour, he found him visibly thinner on his return. The finger nails became blue; the hands and fingers shrivelled, white, corrugated, and sodden, like those of a washerwoman after a long day's work. The skin was bathed in a cold sweat. The voice became husky and faint. So peculiar was this change, that the sound was spoken of as the *vox cholerica*. These are the symptoms which the single word *collapse* was meant to express.

Another very striking feature of the disorder was the muscular cramp; affecting the muscles of the thighs and calves of the legs, rendering them as hard and rigid as wood;

and drawing up into lumps the muscles of the abdomen. These spasmodic contractions were attended with severe pain, and constituted the greater part of the patient's suffering. During the continuance of the symptoms that I have been endeavouring to describe, not a drop of urine was passed or secreted. One man, who was under my own observation and care, and who recovered, did not void a drop of water from Sunday morning till the afternoon of the following Wednesday.

Even in the extreme state of collapse the intellect remained quite clear: the patients would continue to talk rationally to the last moment of their lives; and, for the most part, they seemed singularly indifferent and apathetic about their condition.

In the fatal cases—and a very fearful proportion of the whole number *were* fatal—death took place sometimes in the course of two or three hours; and it was seldom delayed beyond twelve or fifteen. In those that recovered, the favourable symptoms were the cessation of the vomiting, purging, and cramp: the return of the pulse, and of warmth to the surface; the disappearance of the blueness of the skin, and of the hippocratic countenance; the reappearance of bile in the alvine evacuations; and the restoration of the secretion of urine.

The course of the symptoms varied a good deal in different persons. Sometimes the vomiting and purging soon ceased, and sometimes there was neither sickness nor diarrhœa at all, but rapid collapse and sinking. These were thought the most formidable cases. However, the peculiar secretions were *poured forth*, in some, at least, of the instances in which none of them were *ejected from the body*. A patient died of cholera in the Middlesex Hospital without any vomiting or purging: but on examining the dead body, we found the intestines quite full of the rice-water serous fluid. Sometimes the cramps were not very troublesome. The cutaneous blueness was not a universal phenomenon. The patients were in general tormented with thirst: and when attempts were made to bleed them, the blood was found dark and thick, like treacle, and scarcely moving, if moving at all, in the veins: in some cases it could not be made to flow out. Considerable hurry and anxiety of the breathing were also symptoms that I omitted to mention before.

Examination of the dead bodies threw no light, that I know of, upon the nature of this frightful disease. The alimentary canal generally was found to contain a white liquid, having whiter flakes in it; such as had previously issued from the bowels: and the mucous glands of the intestines, both the solitary and the agminated, were unusually large and conspicuous. The veins were loaded with thick, black, tar-like blood; and the urinary bladder was always found empty, and contracted into the size of a walnut. Even when the blue colour had existed in a marked degree during life, it often quickly disappeared after death. And another most singular phenomenon was occasionally remarked in the dead body. A quarter, or half an hour, or even longer, after the breathing had ceased, and all other signs of animation had departed, slight, tremulous, spasmodic twitchings and quiverings, and vermicular motions of the muscles would take place; and even distinct movements of the limbs, in consequence of these spasms.

The disease, of which I have drawn but a faint outline, was not known in this country till the autumn of the year 1831. There are persons, I am aware, who hold that it has always existed among us; only not in such numerous instances as at that period; and they appeal to Morton, and other early writers on the diseases of this country, in support of their opinion. But the malady was too striking to be overlooked, or ever forgotten, by any one who had once seen it. Certainly, till that year I never saw any thing like it. To be sure I had not at that time been very many years in practice here. The late Dr. Babington, however, told me that it was quite new to *him*. He had, for a very long period, been in extensive practice, in those parts of the metropolis and its vicinity where the epidemic cholera raged most; and when it first came among us he had the curiosity to ask every medical man whom he met, whether he had seen any case of the cholera; and if the answer was "*yes*," he went on to inquire whether, before that year, the person had ever met with the same complaint; and the reply was always, without a single exception, "*no*." Yet I say there were, and are, a few practitioners who denied, and deny, that it was any thing more than the common and well-known English complaint, raging with unusual frequency and violence.

But we have evidence of a different kind of the newness of the epidemic cholera to

these kingdoms. Its approach was discerned afar off, as distinctly as a storm is foreseen by the rising of the clouds from the horizon in the direction of the wind. The disorder began to rage with terrible severity, in India, in the year 1817. I do not mean that it then broke out there for the first time. It had visited those regions again and again before. But from its irruption in the year I have mentioned, when it committed frightful devastation in the armies in the north-eastern districts of India, its course can be distinctly traced to our own shores; towards which it approached with slow and halting, but with sure steps, in a north-western direction. From India it spread to Persia; and thence to Russia; and across through Poland to Germany: and at length it was found at Hamburgh. It was predicted before that time, that the distemper *would* at length reach Great Britain. Our government had even sent two physicians into Russia to investigate its nature, in the fearful anticipation that its march across the earth would continue progressive; and accordingly, at the expiration of fourteen years, it made its appearance on the *eastern* coast of this country; in Sunderland: and in due time extended over every part of these islands. I say its arrival had been *foreseen* and *foretold;* and it is absurd to suppose that a vast number of persons would fall sick; and die, with symptoms quite strange to the great mass of practitioners here, merely to fulfil this prediction.

The progress of the disorder did not end here. Crossing the Atlantic, it invaded America; turning, at the same time, in a south-easterly direction, it ravaged France and Spain, and the north coast of Africa, and Italy.

Moving thus onward, as it did, in defiance of all natural or artificial barriers, under opposite extremes of temperature and climate, in the teeth of adverse winds, over lofty mountain chains, across wide seas, through "hot, cold, moist, and dry"—in what manner, you will probably ask, was this wasting pestilence *propagated?*

Upon this point various and discordant opinions are entertained. Many persons believe that the complaint spread by contagion: more, however, that it was not contagious at all, but arose from some deleterious cause with which the general atmosphere of the place was pregnant. Now I cannot reconcile the phenomena of the appearance and extension of the malady with either of these hypotheses, *exclusively*. It must, I think, be granted, that the complaint, in every instance, was excited by the application of some noxious material to the body, some positive poison. It is certain, also, whichever hypothesis may be chosen, that many more individuals were exposed to the agency of this poison, than were injuriously affected by it. This exemption from the disease no more invalidates the doctrine of contagion, than it invalidates the doctrine of some diffused atmospheric influence: nay, it is more explicable upon the former than upon the latter supposition; for while many may avoid a specific contagion, all are immersed in, and all breathe, the common atmosphere. But the exemption shows *this:* that the exciting cause, to be effective, required a fit recipient: that the susceptibility of being hurt by the poison in its ordinary dose and intensity varied much in different persons; and in the majority was very faint, or awanting. It is clear that the poison travelled. It is equally clear to my mind, that it was *portable;* and therefore communicable from person to persons. I even believe that it was capable of being conveyed, and was actually conveyed, from one spot to another, by persons who were themselves proof against its effects. The innumerable authentic instances of coincidence, in point of time, between the first outbreak of the disorder in a particular place, and the arrival at that place of some person or persons from an infected locality, prove that the poison could be thus carried. Of this direct importation of the disorder into new and distant places, by infected individuals, and of its subsequent extension from those individuals to others who had intercourse with them, you may see a vast number of examples, collected by Dr. James Simpson, in the 49th volume of the *Edinburgh Medical and Surgical Journal.* The evidence there adduced of the portability of the poison is abundant, and to my mind, irresistible. Whether the malady was contagious in the same sense in which small-pox is contagious—whether, I mean, the cholera poison had the power of multiplying and reproducing itself in the human body, as yeast multiplies itself during the fermentation of beer—is a different and a much more doubtful question. A disorder may be contagious, without this property of reproduction in the animal fluids. The itch is contagious. The itch is produced by a minute parasitic animalcule, whose existence has, of late years only, been assured to us by the microscope.

Suppose that these itch insects could fly, or were capable of being wafted through the air, they would then represent what is conceivable enough of the subtle exciting cause of cholera. Between the two epidemic distempers, influenza and cholera, there were numerous and striking points of similitude or analogy. They have observed the same, or very nearly the same, geographical route. Both, issuing from their cradle in the east, have traversed the northern countries of Europe, till, arriving at its western boundary, they have divided into two great branches; the one proceeding onwards, across the Atlantic, the other turning in a retrograde direction, towards the south and east. The main differences between them have been, that whereas the poison of influenza spared very few of the community, inflicting a disease which, of itself, was seldom fatal; the poison of cholera, on the contrary, smote very few, but with so deadly a stroke that as many sunk beneath it, probably, as recovered. Both were *general* disorders, affecting the whole system, but in both the most prominent of the symptoms had reference, in the majority of cases, to the mucous membranes: to those of the air-passages in the influenza; to those of the alimentary passages in the cholera.

Now this strong analogy has been made use of as an argument that the cholera was not contagious. "The influenza (say the objectors) had no contagious properties; therefore it is, *à priori*, likely that the cholera had none." But I demur to the major proposition. Cullen thought the influenza was contagious, and I adverted, in a former lecture, to some facts which favour that belief. Supposing it, however, to be so, the proof of its contagious property must, from the very nature of the case, be extremely difficult. Its visitations are so rapid, widely spread, and multitudinous, that there is no time for its transference from house to house, or from person to person; yet it may be nevertheless transferable. Its inherent rate of locomotion outstrips and precludes the tardier conveyance of the poison by man. Its contagious qualities (granting them to exist) are hidden in its universality, and can seldom be traced but by accident. I therefore esteem this argument from analogy as worthless; and my own creed respecting the cholera is, that it *was* contagious, in the limited sense already explained; but that its contagious power was not very great: that a comparatively small part of the population, of this country at least, was susceptible of its operation; and that few were in much danger of suffering from exposure to the physical cause of the disease, except under circumstances of predisposition. At the same time I believe that a great majority of the cases of cholera were not attributable to direct contagion, but to the poison diffused through the atmosphere. There is nothing inconsistent in the supposition that this noxious matter travelled sometimes by its own peculiar powers, sometimes made use of vehicles.

This, I say, is my creed upon the vexed question of contagion. Respecting the special nature of the poison I can only guess; and my guessing, as you may have perceived, takes the same direction as before. I adverted, when speaking of the influenza, to what Dr. Holland has called "the hypothesis of insect life as a cause of disease." I shall not repeat the observations I then made; but I would refer you, for much curious thought and information upon the subject, to Dr. Holland's very interesting essay. The hypothesis in question squares more readily than any other that I know of, with the ascertained history of the disorder: with its origin, after an unusually wet season, in the low marshy country, and hot atmosphere of Bengal: with its irregular but continuous migrations: with its dying away after a while, and its occasional and partial revivals. But still, remember that we are dealing merely with an *hypothesis*.

Whatever obscurity may overhang the *exciting* causes of the epidemic cholera, we are quite sure that certain circumstances exercised a strong *predisposing* influence upon the human body, to render it more than usually susceptible to the disease. The predisposing causes, as might well be imagined, were such as tended to debilitate the system: and therefore *poverty*, which implies scanty nourishment, and frequently also the confinement of several persons to a narrow space, and want of fresh air; poverty which includes these and other evils, was found to predispose the body to a ready reception of the malady. But to *intemperance*, more than to any other *single* cause, may the proclivity to become affected by this species of cholera be ascribed; and especially to the intemperate and habitual use of distilled spirits. This fact was peculiarly manifested in the selection, by the disease, of its victims in this country; and it has been remarked almost everywhere else.

I have all along spoken of the visitation of epidemic cholera in the past tense, because, for the last ten or eleven years, we have heard but little of it. Yet we can scarcely venture to hope that the stranger pest has altogether forsaken us, for we have had slight sprinklings of the disease in and near London, every summer, I believe, since 1832; but it has never again been extensively prevalent or epidemic. Certainly it dealt lightly, upon the whole, with our country. It was much more general, and more widely fatal, in France, which it visited subsequently to its arriving here: it was very destructive also in its subsequent course, both westward and toward the south-east.

The epidemic cholera made its attack in two different modes. In one it seized upon the patient suddenly, and without warning. This was comparatively rare. Much more commonly the specific symptoms were preceded, for some little time, even for some days perhaps, by diarrhœa. And this I take to be the most important practical fact that was ascertained during its prevalence among us. When the disease was once fairly formed, medicine had very little power over it; but in the preliminary stage of diarrhœa it was easily manageable. Unfortunately people are inclined (especially those classes of the community among whom the cholera most raged) to regard a loose state of the bowels as salutary; and to make no complaint of it, and to do nothing for it: or, in other cases, they conceive it to proceed from some peccant matter within, which requires to be carried off, and they take purgative medicines to get rid of it. Both of these are serious and often fatal mistakes. Mere neglect of the diarrhœa frequently permitted it to run into well-marked and uncontrollable cholera; and the employment of purgatives hastened or insured that catastrophe. The proper plan of proceeding, I am convinced, was, to arrest the diarrhœa as soon as possible after its commencement, by astringents, aromatics, and opiates. You may object perhaps that the cases that were cured in this way were not cases of cholera at all, and never would have been; but simple ordinary diarrhœa. It is impossible to *prove* the contrary, no doubt; but the presumption is strong that the diarrhœa would, in many, and perhaps in most instances, have run on, if not checked, into the more perilous form of the disease. In many places, when, taught by experience, the authorities established *diarrhœa dispensaries*, to which those attacked by looseness of the bowels were warned and invited to apply, that the looseness might forthwith be corrected; in many such places the cholera, which had before been cutting the inhabitants off by scores, and hundreds, began instantly to decline in frequency. I venture to advise you, supposing the disease should reappear, or whenever in the autumn a suspicion arises that this form of cholera is present in the community, not to try, in cases of diarrhœa, to carry off the presumed offending matter, but to quiet the irritation, and stop the flux as soon as you can.

But when the regular symptoms, peculiar to the severe form of cholera, had set in, medicine, I repeat, had very little influence upon it: and accordingly, as might have been expected, a hundred different cures of the disease were announced, most of them all but infallible. Some persons held that timely bleeding would save the patient; others relied confidently upon mustard emetics. Hot air baths were manufactured, and sold to a great extent, to meet the apprehended attack in that manner without delay. Certain practitioners maintained that the disease was to be remedied by introducing into the system a large quantity of neutral salts, which were to liquefy and redden the blood, and to restore the functions of the circulation. But of this practice it was said in a sorry but true jest, that, however it might be with pigs or herrings, *salting* a patient in cholera was not always the same thing as *curing* him. In a great number of the sick the blood was mechanically diluted by pouring warm water, or salt and water, into their veins. Some physicians put their trust in brandy, some in opium, some in cajeput oil, which rose to I know not what price in the market; some, again, in calomel alone.

Now, I would not willingly mislead or deceive you on this point, by speaking with a confidence which I really have no warrant for, of the success or propriety of any of these expedients. I believe that each in some cases did good, or *seemed* to do so, but I cannot doubt that some of them did sometimes also do harm. I had not more than six severe cases under my own charge: and I congratulated myself that the mortality among them was not greater than the average mortality. Three died, and three (I will not say were cured, but) recovered. The three that died I was called in to see when the disorder was at its

height: in each case it went on with frightful rapidity, in spite of all the means adopted, and proved fatal a few hours afterwards. The three that recovered I saw somewhat earlier, but still not till the specific symptoms were present: one was a girl in the hospital. They all recovered under large and repeated doses of calomel. Yet (as I said before) I do not venture to affirm that the calomel cured them. In the first case which was treated in that way, I merely followed up the plan that had been begun by Dr. Latham, who had visited the patient for me when I was accidentally absent. I found that he had felt better, less sick and less faint, after taking half a drachm of calomel at a dose; and I repeated the same dose many times, for after every dose his pulse rose somewhat, and he appeared to rally. This was the same man whom I mentioned before as having made no urine from the Sunday to the Wednesday: all that time he kept discharging rice-water stools. At last, on the fourth day he passed a *little* water, and his alvine evacuations became rather more consistent, and began to look *green:* and from that time he gradually got well. Afterwards I treated my hospital patient in the same way, and with the same event. Yet I will not pretend to say that these persons might not have done quite as well if they had been left entirely to themselves.

Some of the expedients recommended had certainly a very marked and immediate effect upon the condition of the patients, especially the injection of warm water into the veins. Many instances of this were related at the time. One I myself saw. The patient was a young man, who was nearly moribund apparently. His pulse had nearly, if not quite, disappeared from the wrist; he was very blue, and his visage was ghastly and cadaverous: in one word, he was in an extreme state of collapse. Out of this he was brought in a few minutes by injecting warm water into one of the veins in the arm. The pulse again became distinct and full; and he sat up, and looked once more like one alive, and spoke in a strong voice. But he soon relapsed; and a repetition of the injection again rallied him, but not so thoroughly: and in the end he sunk irretrievably. Dr. Babington told me of a patient whom he saw, speechless, and all but dead, and whose veins were injected. He then recovered so as to sit up, and talk, and even joke, with the bystanders: but this amendment did not last either. Yet even this temporary recovery might be sometimes of great importance: might allow a dying man to execute a will, for example. And some of the persons thus revived got ultimately well. We had for some time a woman in the Middlesex Hospital acting as nurse, who had been rescued, when at the verge of death in cholera, by the injection of her veins.

It was remarked of those who recovered that some got well rapidly, and at once; while others fell into a state of continued fever, which frequently proved fatal some time after the violent and peculiar symptoms had ceased. Some, after the vomiting, and purging, and cramps, had departed, died comatose; *over-drugged* sometimes, it is to be feared, by opium. The rude discipline to which they were subjected might account for some of the cases of fever. And the process of artificially replenishing the veins was certainly attended with much danger. The injection of *air* with the water—inflammation of the vein from the violence done to it—an over-repletion and distension of the vessels by the liquid—*might*, any one of them, and sometimes, I suppose, *did*, occasion the death of the patient. Never, certainly, was the artillery of medicine more vigorously plied—never were her troops, regular and volunteer, more meritoriously active. To many patients, no doubt, this busy interference made all the difference between life and death. But if the balance could be fairly struck, and the exact truth ascertained, I question whether we should find the aggregate mortality from cholera, in this country, was any way disturbed by our craft. Excepting always the cases of preliminary diarrhœa; just as many, though not, perhaps, the very same individuals, would, probably, have survived, had no medication whatever been practised.

I do not know that I have any thing more to say that could be of any use to you, in respect to the epidemic cholera.

LECTURE LXXIII.

DYSENTERY. DIARRHŒA ADIPOSA. INTESTINAL CONCRETIONS. WORMS.

ANOTHER of the morbid fluxes from the alimentary tube, of which I have yet to speak, is *dysentery*.

Its characteristic symptoms are, griping pains in the abdomen, followed by frequent, mucous or bloody stools, straining, and tenesmus. In chronic cases pus is sometimes discharged from the bowels. The acute form or stage of the disease is attended with fever.

The differences between dysentery and diarrhœa are obvious enough. Both of them may be accompanied by griping pains: in both the stools are frequent and loose: but in diarrhœa they are fæcal; in dysentery there is retention of the natural fæces, or they are expelled, from time to time, in small, hard, separate lumps, termed *scybala*. Again, straining, and tenesmus, and the excretion of mucus, which is often tinged with blood, form no necessary features in diarrhœa; whereas in dysentery these symptoms are prominent and constant. These nosological distinctions are true and useful, although in our actual intercourse with the sick we do not find them always or strictly observed. Some of the worst forms of dysentery *commence* with the ordinary symptoms of diarrhœa.

Dysentery consists, essentially, in inflammation of the mucous membrane of the large intestines; yet not, I apprehend, of the whole of that long surface indiscriminately. Observation of the course of the disorder, during life, and of the morbid appearances visible after death, leads to the conclusion that in simple dysentery, marked by tormina and tenesmus, and frequent dejections of sanguinolent mucus without fæcal matter, the inflammation chiefly affects the *rectum* and the *descending colon*. When the earlier portions of the large intestines are involved in the diseased process, the stools at the outset are often composed in great measure of excrement in an unnaturally fluid state, and mingled with blood and slime. We generally speak of these circumstances as constituting *dysenteric diarrhœa*.

Slight and simple dysentery may occur and run its course with very little or no disturbance of the circulation. When it is acute and severe, it is attended with more or less pyrexia. The acute disease may terminate in recovery; or in early death; or in chronic dysentery, which usually, in the end, is fatal.

The wards of our metropolitan hospitals place frequently under our notice severe cases of chronic dysentery in the persons of soldiers and sailors, who bring the disease home with them from hot climates. With these exceptions, dysentery is, now-a-days, neither a very common nor a very serious disorder in this country. I say now-a-days, for the time was when it raged in London like a plague. The present Dr. Heberden, in his valuable essay, *On the Increase and Decrease of different Diseases*, shows, that in the seventeenth century the number of deaths set down, in the weekly bills of mortality, under the titles *bloody flux*, and *griping in the guts*, were never less than 1,000 annually, and in some years exceeded 4,000. For five-and-twenty years together, viz., from 1667 to 1692, they every year amounted to above 2,000. During the last century, the number gradually dwindled down to twenty. Dysentery is one of the pests of hot climates. In all tropical regions at certain seasons of the year it is very prevalent and destructive. But it is in fleets and armies, and especially among troops in actual service, that the distemper most displays its terrible power. There is no single malady which is so crippling to an army in the field as this. Sir James M'Grigor, to whom was entrusted the superintendence of the

medical department of the army on "the two greatest services on which the military force of this country has, of late years, been employed, namely, that in Walcheren, and that in the Peninsula," calls dysentery "the scourge of armies," and the "most fatal of all" their diseases. In two years and a half, the British army in Spain lost no less than 4,717 men by this complaint.

How are these facts to be explained? Wherefore is dysentery, which was so familiar to our ancestors, so happily rare among us? Why does it thus wait upon and afflict the march of armies? Upon what depends its frequency in hot climates? We may expect to obtain some answer to these questions by searching into the *causes* of the disorder.

It has been ascribed to exposure to wet and cold; to the use of unwholesome food; to the agency of malaria; to contagion.

Weather and season have a manifest influence in the production of dysentery. In temperate climates, like our own, it is an autumnal disorder. In tropical countries it is observed to be more common and more severe when rains succeed to long-continued drought. In respect to this, as to other bowel affections, a high diurnal temperature of the air appears to be the predisposing, and exposure to cold the exciting cause. I stated, on a former occasion, that great vicissitudes of temperature are very frequent and very pernicious, even under the torrid zone. Scorching days are followed by extremely cold nights. The dysentery which arises under these circumstances is apt to run on into the ensuing winter. Soldiers in the field against an enemy are peculiarly obnoxious to the agencies which favour or generate the complaint. Marching, or engaged in actual conflict, during the day; bivouacking at night, often in the open air, and under every variety of weather; ill-provided too often with clothes and bedding; their food scanty, precarious, or of bad quality; seizing the many opportunities which their dreadful trade supplies of license and intemperance; depressed, it may be, by disaster or defeat: we need not wonder either at the prevalence of dysentery among them, or at its untractableness while they remain subject to the same morbid influences. Neither can the causes be warded off from the patient, nor, in general, can the patient be removed from the causes. Yet occasions do arise which show distinctly enough this alleged relation of cause and effect. *Præsens morbum facit —sublata tollit.* Take, on the one side, the following facts from Sir John Pringle's book *On the Diseases of the Army.* The men who had fought at Dettingen lay that night on the field of battle, without tents, exposed to a heavy rain. For the next night or two they encamped on better, but still wet ground; and they wanted straw. Nearly half of these troops were soon after affected with dysentery; while three companies which had not been engaged in the battle, nor exposed to rain, nor lain wet, escaped the complaint entirely. Take this converse fact, related by Desgenettes. Four hundred of the French "army of Egypt," reduced to a state of extreme weakness and emaciation by dysentery there contracted, embarked at Alexandria on their return towards France; were carried away, in short, from the alleged causes of their disorder. Nineteen died at the very outset of the voyage; which had, however, so good an effect upon all the rest, that before they reached Malta they were thoroughly convalescent.

The very frequent coincidence or alternation, in some places, of dysentery with intermittent fever, has given rise to the opinion that both these diseases are alike attributable to the malarious poison. But dysentery prevails where there is no other evidence of the presence of malaria. You may recollect that when we were upon the subject of ague, I showed you that its repeated paroxysms were attended with extreme and increasing congestion of blood in the internal organs; of which congestion the tumid spleen, the ague-cake, was an effect and a token. Now whatever gorges the splenic vein, gorges its tributary, the inferior mesenteric, which carries the blood from the rectum and the descending colon. Upon such congestion of the mucous membrane inflammation is readily engrafted; and in this indirect way dysentery may be said to result from the marsh effluvia. Ague is an effect of malaria; and dysentery is, sometimes, a sequela of ague. In precisely the same manner, dysentery is apt to supervene, in hot climates especially, upon *hepatic* congestion and disease.

That dysentery is, in itself, a *contagious* malady, we have no satisfactory evidence. In its sporadic form, in this country, we never see it spread from person to person. But it is a prominent symptom in some epidemic visitations of continued fever, which un-

doubtedly is contagious. To this fact I am inclined to attribute the notion, formerly much more common than it now is, that simple dysentery is catching.

The remarkable decline of dysentery in this metropolis, has been contemporary with that of other severe disorders; and it is due to the same combination of causes. For nearly two centuries we have had no *plague* among us. *Agues*, formerly very rife in London, have almost disappeared. *Continued fevers*, which used to break out annually in hot weather, are comparatively unfrequent. I believe that we may trace these great blessings to an event which was regarded, at the time, as a national judgment; I mean the great fire that, in 1666, consumed every thing between Temple Bar and the Tower. The streets and houses thus destroyed had been filthy in the extreme, close, densely crowded, and consequently most unhealthy. The impurity of the air excited, perhaps, some complaints; and it certainly predisposed those who dwelt in it to various kinds of disease, "the seeds of which (says Dr. Heberden) like those of vegetables, will only spring up and thrive when they fall upon a soil convenient for their growth." To the better construction of the houses and of the streets in the rebuilt city; to the increased means of ventilation; to the general formation of drains and sewers; to the more copious supply of water; and to the more temperate and cleanly habits of the people, we may fairly ascribe our present exemption from dysentery, from ague and continued fever which are often the parents of dysentery, and from the plague itself. In many parts of this overgrown place there is still much room for improvement.

The pyrexia that accompanies dysentery sometimes begins before the local symptoms declare themselves; more frequently it succeeds their manifestation. Occasionally the fever runs high, the pulse is hard and frequent, the skin hot, the face flushed, and the tongue furred; and the patient complains of headache and thirst. But in this, as in other abdominal diseases, the pulse soon becomes small and weak, the strength rapidly declines, and the temperature of the body sinks.

In acute cases the pain is often severe; but it is subject to remissions and exacerbations. It occupies the hypogastrium, or some part of the course of the colon, where there is usually more or less tenderness on pressure. The patient is tormented by a sensation as if there was some excrement ready to be dislodged; goes perpetually to the night-chair, and is irresistibly impelled to strain violently to get rid of the irritation. But the efforts are ineffectual; he discharges but little; and what is voided is either altogether a jelly-like mucus (in which case the complaint has been called the *dysenteria alba*, and the *morbus mucosus*), or more commonly it is mucous and bloody (the *bloody flux* of our old authors), mixed with films and membranous shreds. In many of the dejections there is no genuine fæcal matter at all; or the small indurated balls which I just now mentioned come away occasionally. Frequently the ejected mucus is variegated in colour, green, or black, or reddish, like the washings of meat, and horribly fœtid. Sometimes pain and difficulty in making water are added; there is *dysuria*, the irritation of the rectum being reflected upon the bladder through the lower portion of the spinal cord. Sometimes the stomach sympathizes, and nausea and vomiting ensue. With all this local suffering there is a continuance of febrile distress; the patient passes sleepless, or dreamy and disturbed nights, and is low-spirited and desponding. In the fatal cases the pulse becomes very small and rapid, the features sharpen, and the surface grows cold. Death begins at the heart.

Inspection of the dead body discloses more or less ulceration, chiefly of the large intestine. The glands that are scattered over its surface are enlarged and prominent, looking somewhat like small-pox pustules, for which indeed they have been mistaken. They probably form the foci of most of the ulcers, which are sometimes narrow and oblong, lying across the gut; sometimes very large and irregular, with here and there islands or ridges of thickened mucous membrane. In the worst cases the whole extent and circumference of the bowel presents, internally, one irregular, confused, and tattered mass of disorganization.

When submitted to early *treatment*, and when its exciting causes can be averted, or avoided, dysentery is not an intractable disorder. Sir James M'Grigor remarks of the camp dysentery in the Peninsula, that it had two stages, which it was of consequence to note, because they required different and almost opposite modes of treatment: the inflammatory stage, and the stage of ulceration. A plan proposed by Dr. Somers appeared to

Sir James so judicious, and proved so successful, in the first attacks of the pure unmixed disease, that he recommended its general adoption in the army. It was this.

First, the patient was freely bled. Immediately afterwards twelve grains of Dover's powder were administered. This dose was repeated three times, at intervals of one hour. Plenty of warm barley-water was at the same time given, and profuse sweating encouraged for six or eight hours. A pill, containing three grains of calomel and one of opium, was exhibited every second night; and in the intervening days two drachms of Epsom salts dissolved in a quart of light broth. The venesection was repeated, while the strengh and the pulse permitted it, until the stools were free or nearly free from blood; and Dover's powder, as a sudorific, was always given after the blood-letting. When the pains were great, and attended with much tenesmus, the warm bath gave instantaneous relief. "This plan being steadily persevered in for a few days, the inflammatory diathesis of the intestinal canal, which had excited symptomatic fever throughout the general system, was found gradually to yield, and make way for returning health."

If the disease was not cut short by this method, but advanced into the second stage, and became chronic, the most effectual remedies appeared to be laxatives, and opiates, given alternately; and combined with such medicines as promote perspiration. The abdomen should be swathed with flannel, or covered by a warm adhesive plaster. Much benefit may be obtained from the employment of clysters, if there be not too much tenesmus to admit of the introduction of the pipe of the injecting syringe. Warm starch, with laudanum in it—not exceeding in quantity a couple of ounces, lest the irritable bowel should expel it again—will sometimes afford signal relief. Or if the pain and tenesmus are so great that a clyster-pipe cannot be used—or the enema is not retained—a grain or two of solid opium inserted into the rectum, beyond the sphincter ani, will often allay the distress. The food should be farinaceous and simple; and great care must be taken during the convalescence to prevent a return to improper diet, and a fresh exposure to cold.

There is one important point in the treatment of dysentery, concerning which a striking discrepancy of opinion exists, even amongst practitioners who have had large experience of the disease: I allude to the employment of mercury as a remedy. I have no data for settling the question: but the amount of evidence appears to be against its indiscriminate use. It seems (as we might expect) to be powerful both for good and for evil. Sir James M'Grigor has probably hit the distinction which should guide us to prescribe or to withhold this drug. It was, he tells us, when the dysentery was complicated with disease or disorder of the *liver*, that mercury proved so highly useful: when along with the dysenteric symptoms there were present a dull pain in the hepatic region, and in the right shoulder, a yellowish brown colour of the skin, and of the conjunctiva, and uneasiness when the patient lay in any other posture than on the right side. He adds: "In the early stage of the acute and unmixed disease, and before venesection has been performed, mercury will aggravate the symptoms. In the more advanced stage of the disease, particularly when there is hectic fever, with extensive erosion or ulceration of the intestine, it is invariably found to hurry it on to a fatal termination."

The sporadic dysentery which we chiefly see in this country seldom requires the lancet. Leeches, however, are to be applied, in the track of the colon, wherever there is much tenderness on pressure. A full dose of castor oil may then be given; and after that an opiate. It is the practice of some physicians to prescribe laxatives and opium together; but in this complaint it is better to alternate them. Opiate enemata are of service for relieving tenesmus. These remedies will be much assisted by the warm bath; by hot fomentations to the abdomen; and by such means as promote the natural secretions of the skin. If there be any reason to suspect that the portal system is gorged with blood, complete relief to the dysenteric symptoms may often be obtained by the practice which I recommended as proper in melæna; viz., the exhibition of five grains of calomel at bedtime, and of a senna draught the next morning, for two or three days in succession. Should the symptoms still drag on, it may be necessary to give mercury, even to the extent of making the gums tender; but it should be introduced gradually. Equal parts of *hydrargyrum cum cretâ* and of *Dover's powder*, constitute a very good combination either in pill or powder for such forms of the complaint. But in the milder cases of

simple sporadic dysentery there is no occasion, I had almost said there is no excuse, for giving your patient a sore mouth by the lavish employment of mercurial remedies.

Diarrhœa adiposa.—The fluxes of which I have hitherto spoken have all consisted in an immoderate discharge of some of the usual contents or secretions of the alimentary tube, in an altered and unnatural state. But matters are sometimes voided from the bowels totally unlike any of the healthy discharges. *Adeps* is not an intestinal excretion; yet it is sometimes passed, in great abundance, by stool. Many unquestionable instances of this are on record, both in ancient and in modern literature. Though I have never met with one, and therefore have but little to say on the subject, I must not pass it over altogether. A certain quantity, sometimes it has been a *large* quantity, of oil, of liquid fat, has been poured forth, in a sort of diarrhœa. Sauvages was aware of the disorder, and calls it, in his *Nosology*, *diarrhœa adiposa*. In a paper in the *Medico-Chirurgical Transactions* upon this subject, Dr. Elliotson refers to an example of it described by Tulpius, in which a woman discharged every day, for fourteen months, a considerable quantity of yellow fat, that lay upon the fæces like melted butter. When voided into a vessel of water it floated, like oil, upon the surface; and when cold it assumed the consistence and appearance of fat. Like fat, it was very inflammable, and burned with a bright flame. With all this there was no kind of distress, nor any wasting of the body; and the patient was in excellent health sixteen years afterwards. Dr. Elliotson had a case of this kind under his own care. The man had also diabetes and phthisis. The symptoms were precisely the same as those described by Tulpius. Dr. Prout and Dr. Faraday analyzed portions of the adipous matter, and they pronounced it to be genuine fat. Mr. Lloyd, of St. Bartholomew's Hospital, has given us the details of a case in which the evacuation of grease was associated with jaundice. The excretion looked like melted fat, but when cool had the consistence of butter. It swam on the surface of water, melted at a moderate heat, and burned readily. In this instance the head of the pancreas, and the duodenum, were involved in a mass of scirrhous disease. And this is a very curious fact: for Dr. Bright also states that in three different persons, each of whom he had known to pass fat from the bowels during life, and whose bodies he had the opportunity of examining after death, he found scirrhous disease of the pancreas, and fungous disorganization of the duodenum.

The remarkable coincidence, occurring so often, of these fatty discharges with cancerous disease of the pancreas and duodenum, is well worthy of being borne in mind; although it may not, as yet, afford any explanation of the phenomenon.

All that we know of the disease seems to amount to this: that it is not a *common* complaint; and that it is not necessarily a *fatal* complaint: for persons who have passed great quantities of fat in that way have lived in good health for many years afterwards. Yet though not *necessarily* fatal, it has frequently been found associated with incurable malignant disease in the duodenum and pancreas. Dr. Prout also informs us that in cases in which a similar oily fluid has been passed through the *urethra*, the *kidneys* have been found in a state of organic malignant disease.

With respect to the treatment of such cases, all the hints I can give you are such as are furnished by the two following facts:—

Mr. Howship, in his book on morbid anatomy, mentions the instance of a lady who was affected with this diarrhœa adiposa, and parted with vast quantities of fat; and who was cured upon the principle of *similia similibus curantur*, for she recovered after swallowing a pint of sweet oil. And Dr. Elliotson, acting on this hint, gave *his* patient, who was labouring at the same time under diabetes, a quarter of a pint of olive oil; and the voiding of fat greatly diminished from that time, and soon ceased entirely.

Whether these were really cures, or whether they were coincidences, is a question which we want larger experience to help us to determine.

Intestinal concretions.—When I was speaking of the causes of enteritis, I adverted to the presence of *foreign substances*, as they are called, in the bowels, and to *intestinal concretions*.

There are some points connected with these subjects which I had not then leisure to mention, but which you ought not to be ignorant of.

Intestinal concretions are very common in some of the lower animals—in horses and oxen especially. Most of you have seen, I dare say, immense intestinal calculi of this kind, and great numbers of them, in the museum of the College of Surgeons. The old remedies called *bezoars* were of the same nature.

They occur also, these intestinal calculi, in the human entrails, and in various parts of them; chiefly, however, in the cæcum and large intestines, but sometimes in the stomach: indeed, very large ones have occasionally been met with in the latter organ. Bonetus describes one which weighed nine ounces, and was as big as a hen's egg. Generally they are few in number in the same person; one only, perhaps, exists, or two or three. Yet as many as thirty have been found together in the stomach in one case; and in another case nine. One of the Monros of Edinburgh (Monro primus) detected twelve in the colon of a boy, during life, by the touch. Monro secundus took a concretion that weighed four pounds from the colon of a woman. They have been known to measure as much as eight inches in circumference. In the twenty-fourth volume of the *Edinburgh Medical and Surgical Journal* is an account of one long one, or, perhaps, of three that had become united together, weighing twelve ounces. Mr. Turner, of Keith, has very lately published the case of a man who passed fourteen large intestinal concretions.

Now what are these substances, and how do they get there? What is the pathology of the malady? Why, they seem to be formed, in many instances, by the deposit of saline particles, intermixed with animal matter, upon and around some accidental nucleus which has entered the alimentary canal, and there stopped. Sometimes a gall-stone forms the nucleus: the centre of the calculus has several times been found to consist of pure cholesterine. Those matters over which the gastric juice has no power, and which pass the pylorus unchanged—such as the stones of fruit, husks of grain, many unbroken seeds, portions of bone, and the like. Other of these intestinal concretions are evidently composed of a mass of short fibres, matted, or interwoven together, after the manner of *felt*. These calculi have a somewhat soft and velvety feel, yet are too hard to be much compressed. Sometimes they involve a nucleus, and sometimes they do not. Their composition has been discovered in rather a curious manner. Mr. Clift, who, as you know, has long had the main charge of the Hunterian Museum, after attentively examining some of the specimens there collected, fancied that they might be formed somehow of the beards of oats; and the late Dr. Wollaston, at Mr. Clift's suggestion I believe, undertook to analyze them somewhat more rigidly; and he found that Mr. Clift's conjecture was well founded. If you have ever looked closely into the structure of an oat, which has been separated from its husk, you may have noticed that one end of it is formed somewhat like a tiny brush; made up of very minute needles or beards. Dr. Wollaston found that these ends were identical in their shape and composition with the fibres of the intestinal concretions.

The accuracy of the result of this analysis is singularly confirmed by the fact that this particular kind of intestinal calculus is almost peculiar to the bowels of Scottish people; among whom, as you know, oats form a very common article of diet, in the shape of oat-*meal*.

Concretions of the same species have also been found in the intestines of Lancashire persons; and they also use oatmeal a good deal as food. Mr. Children gives an account of some in the Philosophical Transactions for 1822. The fibres were cemented together by mucus; and the concretions contained also albumen, phosphate of lime and soda, and common salt.

I mentioned formerly the danger which attends the incautious or excessive use of magnesia, whether for summer complaints of for urinary disorders. When this substance is taken habitually, and when due care is not taken to insure its habitual expulsion from the intestines, it is liable to accumulate and concrete there, especially in the cæcum and colon. Large masses of this kind have been met with, composed almost entirely of carbonate of magnesia.

And the habitual use of any other indigestible substance may have the same ill consequence. The seeds of figs; unbruised mustard seeds, which (as I mentioned before) are

taken daily by some persons; the woody knots found in certain pears; all these have been known to form the material of concretions, or of hard injurious masses in the bowels.

Now concretions of this kind come at length to produce symptoms by the pressure and distension they occasion, by the ulceration to which they sometimes give rise, and, above all, by the obstacle they oppose to the passage of the contents of the intestines. They generally cut the patient off by exciting inflammation.

But they may exist for a long time without producing any definite symptoms. And when symptoms do arise, or when we ascertain that such concretions have formed, we are often at a loss for a remedy. From the *colon* we may hope at last to dislodge them: by mechanical means when they are near the outlet; by frequent injections of warm water, or soap and water, whereby they may be softened or broken down, and washed out, when they are beyond the reach of the finger, or of instruments passed into the rectum.

We have an illustration of the patience of the alimentary canal under the presence of these masses, in what often happens when foreign bodies of some magnitude are swallowed and remain in the tube.

In one of the earlier volumes of the *Medico-Chirurgical Transactions* you may read the history of a celebrated knife-eater. A sailor, in a drunken bravado, swallowed a clasp-knife. This was followed by no immediate bad consequences, and he used to brag of the feat he had performed. And afterwards, either to satisfy the scruples of those who did not believe his assertions, or for the sake of rewards which some people were thoughtless and cruel enough to offer, or to win wagers, he stupidly repeated his folly, till he had swallowed (I think) thirteen knives of various kinds and sizes. They killed him at last; and their remains were found in various parts of the alimentary tract. But he had no serious symptoms for some time.

Mr. Wakefield has given us an account of a culprit, confined in the Cold Bath Fields Prison, who had swallowed seven half-crowns before his incarceration. One day out they all clattered into the pan of his night-chair.

I saw a prisoner myself, last autumn, in the Penitentiary, who, after some sickness, and tenderness of the belly, voided a half-crown from the rectum. This was in November, 1839. He had swallowed the piece of money two years and a half before—viz., in March, 1837; and, until within a week of his passing it, he had enjoyed excellent health.

Worms.—Before I proceed to any of the other viscera of the abdomen, I may as well take such notice as the nature and limits of these lectures admit and require, of the subject of *worms:* in which subject the intestinal canal is more concerned than any other part of the body. It seems a strange, as it is a somewhat humiliating fact, that the human body should furnish food and a habitation for many of the inferior creatures; not only after death, but while it is yet alive. The parasitic animals which thus prey upon man have been much studied from time to time, and especially of late, in their relations to natural history: and some of the facts that have been ascertained respecting them you ought to be acquainted with. But I shall pursue the subject no further in this place than it concerns us as pathologists and physicians. Its natural history will, no doubt, be fully taught you by the professor of comparative anatomy.

First, then, it is a notorious fact that numerous parasites do crawl over our surface, burrow beneath our skin, nestle in our entrails, and riot, and propagate their kind, in every corner of our frame: producing oftentimes such molestation and disturbance as require the interference of medicine. Nearly a score of animals belonging to the interior of the human body have been already discovered and described: and scarcely a tissue or an organ but is occasionally profaned by their inroads. Each, also, has its special or its favourite domicile. One species of *strongle* chooses the heart for its dwelling-place, another inhabits the arteries, a third the kidney. Myriads of minute worms lie coiled up in the voluntary muscles, or in the cellular tissue that connects the fleshy fibres. The *guinea-worm* and the *chigoe* bore through the skin, and reside in the subjacent reticular membrane. *Hydatids* infest various parts of the body, but especially the liver and the brain. A little *fluke*, in general appearance much like a miniature flounder, lives,

steeped in gall, in the biliary vessels. Even the eye has its living inmates. But it is, I repeat, in the alimentary tube that we are most apt to be plagued with these vermin.

Independently of minute scientific divisions into genera and species, there are some broad lines of distinction between these creatures. Thus, some kinds of worms occupy, as I have said, the interior of our bodies; these are called accordingly *entozoa:* some dwell externally, and are named *ectozoa;* or, more properly perhaps, *epizoa.*

There are five sorts of intestinal worms, sufficiently common to make it likely that you will meet with some or most of them in your future practice. I shall, on that account, direct your attention first of all to them.

1. A frequent tenant of the human intestines is the round worm, so like in shape, size, and general appearance to the common earth-worm. It is from this species, no doubt, that the whole class are called *worms.* This round worm is often denominated a lumbricus; but that is erroneous: it is a species of ascaris, and it has been named by naturalists the *ascaris lumbricoides*—the ascaris that is like a lumbricus.

2. The *ascaris vermicularis;* or the *oxyuris vermicularis.* These animals resemble slender maggots rather than worms. They are often called simply *ascarides:* or, in the vernacular, *thread-worms:* and they are very much like bits of white thread.

3. The *tricocephalus dispar;* also a small worm, but longer than the last; its vulgar denomination is accordingly the *long* thread-worm.

4 and 5. Two species of *tænia;* long, flat, articulated animals, resembling pieces of tape. The *tænia solium,* or common tape-worm of this country; and the *tænia lata,* or broad tape-worm.

Of all these I proceed to mention a few more particulars.

The *ascaris lumbricoides,* or round worm, is, I say, very like the common earth-worm, and used to be thought identical with it. It runs from five or six inches to about a foot in length, and it is of a reddish-brown colour, with a tinge of yellow. The female worm (for they are of both sexes) is much more common than the male, which is smaller also, and may be distinguished by a curved state of its tail and by the genital organs. Sometimes young ones are met with, about an inch and a half long.

I shall not go into any minute description of the anatomy of these worms. You cannot mistake them, except for earth-worms; and the points of distinction between the two, when known, are easily perceived. The earth-worm, then, is redder than the intestinal worm, and less pointed at its two ends. The mouths of the two differ much; that of the earth-worm is a short longitudinal fissure, or slit, placed on the under surface of its small rounded head. In the ascaris lumbricoides, the mouth is situated at the extremity of the worm, is of triangular shape, and is surrounded by three tubercles.

It is curious that similar differences, only reversed, exist in respect to the other aperture of the alimentary canal, the anus. In the earth-worm this is terminal, at the very end of the cylinder: in the ascaris it is a transverse slit *near* the extremity, and on the under surface of the animal.

Again, the earth-worm has rows of little projections, like bristles, upon its under surface; feet they may be called, for they appear to serve the purposes of locomotion. In the parasite there is nothing resembling this.

By attending to these plain marks, you may avoid being deceived by impostors, who pretend that they are afflicted with worms, and to prove their case bring you an earth-worm or two in a bottle.

The *habitat* of these worms is in the small intestines. They may, and do, pass upwards into the stomach, or downwards into the large bowel: in either case they are generally soon voided. Sometimes they are vomited up: but they have been known to *creep* into the œsophagus, and thence into the nostrils. Andral states that he saw a case in which a child was strangled by one of these worms, which had turned back and become entangled in the larynx. They have been found also in the excretory ducts of the liver. This Andral has witnessed; as has also Dr. Baron in this country.

It was formerly thought these animals were capable of perforating the coats of the intestine: but that opinion is now generally exploded. They do not appear to have the means, if they possess the inclination, to bore through. What gave rise to this notion was the

circumstance of their sometimes passing out of the bowel, through ulcerated or other openings, into the peritoneal sac, or into the vagina or bladder; or outwards through hernial apertures.

The number of these worms existing at the same time in the same person is very variable. The late Dr. Hooper mentions a child, eight years old, a girl, who voided upwards of 200 in the course of one week. An instance is recorded of a soldier who passed 367 in six days. Another patient got rid of 460 in a fortnight.

Fifty or sixty have been found in the same dead body. They often lie in packets. The corresponding portion of mucous membrane has in some cases been red, in others quite natural. Sometimes two are met with; sometimes one only. So that we cannot infer with certainty that because one such worm has been voided, more remain behind; although that is always probable.

This worm is more common in the early periods of life than afterwards.

The other species of ascaris, the *ascaris vermicularis* or thread-worm, resembles the former in some respects, but differs from it remarkably in size. Here also the female is longer and larger than the male; the one being perhaps half an inch in length, the other not two lines, and very slender.

The thread-worms live principally in the rectum, and sometimes exist there in vast numbers; thousands: and they pass out, or are ejected, matted together with mucus in the shape of balls, or entangled in portions of excrement Sometimes they emerge of their own accord, and crawl about the neighbourhood, getting into the vagina in females, and even into the urethra, and causing intolerable irritation, itching, and distress.

They are seen, when recently expelled, to be very lively; moving their anterior extremity about continually. To this restlessness and activity the animal owes its name, which is derived from the Greek word ασκαριζειν, to leap. The Germans call it *spring-wurm*.

This worm also belongs chiefly to infancy and childhood. It does sometimes infest adults; but generally as the patient grows older the animals cease to trouble him, whether curative means are employed or not. Bremser, however, knew a person eighty years old, who was nearly killed by them.

The third kind of these round worms is the *long* thread-worm; the *tricocephalus dispar*. It is from an inch and a half to two inches in length. One extremity, that to which the head belongs, is extremely fine and small; and then suddenly bulges out into a thicker body. The thinner portion is about twice as long as the thicker. Its name is derived from this variation of size. θριξ, a hair, and κεφαλη, the head; the portion to which the head is appended being as fine as a hair. At one time the head was mistaken for the tail, and then the animal was called *tricuris*, from θριξ, a hair, and ουρα, the tail. The thicker or body part is rolled up in a spiral form, especially in the male, the female being straighter. This worm is of a white colour, unless tinged by its food. It also affects the large intestine as its place of abode; but the opposite end of that gut, the cæcum, is its favourite spot. It is sometimes met with in great numbers, attached to the mucous membrane by its head; the body hanging loose.

Although generally overlooked, it is said to be extremely common, and to occur in most bodies. I have seldom seen it; but then I have never hunted for it. It infests the dog, the fox, the monkey, and other mammalia.

This species of entozoon attracted a good deal of attention about seventy or eighty years ago; it being then first observed in Germany during the prevalence of an epidemic fever, which was characterized by profuse mucous diarrhœa. Rœderer and Wagler have given an excellent account of this disorder, under the title of *morbus mucosus*. It was thought to have been excited by these worms, which were found in abundance in the cæcum after death. This opinion must have been erroneous, for the animals were known, in other places, long before: and they produce, in general, no inconvenience.

The two *tænia* are more formidable beasts. With a general resemblance between them, there are strong particular distinctions.

The tænia solium, or common tape-worm, has a minute hemispherical head, and a long

flat body, of a whitish colour, composed of many pieces curiously articulated together. The articulated pieces are quadrilateral, very short and small in the creature's neck; become gradually square as the distance from its head increases; and at length are longitudinally oblong. These portions, or *joints* as they are called, have foramina on their margins, leading to ovaries within. The foramina, which are very conspicuous, are placed alternately on the one side of the animal and on the other: on the right edge of one joint, on the left of that next to it. This arrangement is, however, subject to occasional irregularities. Each joint is let in, as it were, to that immediately in front of it; and the connection between them is not very firm It is less firm in proportion as the animal is older, and as we approach its posterior extremity: so that the segments are apt to come away, by stool, separately. They have somewhat the appearance of the seeds of cucumbers or gourds; and the parasites, for that reason, are sometimes called *cucurbitine* worms. Blumenbach and others have supposed that each articulated piece was a distinct worm: but that is not the case. The head of the animal, and its peculiar terminal segment, forbid this belief.

The common tape-worm is very narrow and thin towards its anterior extremity; one-third or one-quarter of a line perhaps in breadth. At its broadest part it may be from three to six lines wide.

The young tænia *seem* to be merely wrinkled; but they are really articulated. The question has been started, whether the lost joints are ever reproduced. It is believed that no new joints are formed, but that the original ones are gradually more and more developed. The animal is hermaphrodite.

Specimens of this worm are preserved, upwards of twenty feet in length. Much exaggeration seems to have existed formerly about its size. It has been said to measure 150, and even 300 feet. In all probability separate portions of several have been estimated as forming parts of one and the same worm. There is one case well authenticated (it is cited by Bremser from Robin) in which a tapeworm was found to extend from the pylorus to within seven inches of the anus; adhering firmly to the mucous membrane all the way. The animal has the power of motion. Its movements are felt by the patients, within them; when recently expelled, and placed in tepid water, it may be seen to shorten itself; nay, portions protruding many feet from the anus have been known to draw themselves back again.

This kind of worm is more frequent in adults than in children: yet it is sometimes met with even in the fœtus. It is exceedingly common in dogs. It has been badly named *ver solitaire*, for it is not always single. It is not only found in company with different worms, but also with others of its own species. Its natural place of abode is the small intestines: but it extends sometimes into the large, and sometimes into the stomach. Vandoverer declares that after an emetic one of his patients vomited forty Dutch ells of the worm, and might have god rid of more "if he had not been afraid of puking out all his guts, and for that reason bit the worm off."

The tænia lata, or broad tape-worm, has often been confounded with the tænia solium; yet there are striking differences between them; respecting which, for all practical purposes, it is enough to say that the heads (as viewed through a microscope) are very dissimilar; that the joints of the tænia lata are shorter and broader, and adhere together in a different manner; and that the pores leading to the oviducts are situated not on the edge of each joint, but in the centre of its flat surface. This variety is not so easily broken across as the former; and therefore its segments are less liable to be voided in a separate form. It is probably shorter also than the tænia solium. Fifteen feet have been supposed its average length. Marvellous stories, however, are told on this head. Boerhaave declares that he effected the expulsion of one, which was 300 ells long, from the bowels of a Russian.

The geographical distribution of these two species of tænia forms a curious part of their history, and throws some light upon the doubtful question of their origin. In England, Holland, and Germany, the tænia solium is common, and the tænia lata very rare. In Russia, Poland, and Switzerland, it is just the reverse: the tænia lata prevails; the tænia solium is seldom seen: while in France the one species is nearly as frequent as the other.

LECTURE LXXIV.

ENTOZOA CONTINUED. HYDATIDS. TRICHINA SPIRALIS. THE GUINEA WORM. STRONGULUS GIGAS. ORIGIN OF ENTOZOA. QUESTION OF SPONTANEOUS GENERATION. GENERAL SYMPTOMS OF THE PRESENCE OF INTESTINAL WORMS. PARTICULAR SYMPTOMS, AND REMEDIES, OF THE COMMON ROUND WORM, OF THREAD-WORMS, OF TAPE-WORMS.

AT our last meeting I gave a summary description, sufficient however for our purposes as medical practitioners, of the five kinds of *vermes* which are most common in the human intestinal canal. There are yet a few more of these entozoa which are curious and interesting enough to deserve a brief notice.

Hydatids—animals like bags or bladders of water—are of very frequent occurrence in various parts of the body. They are also called *acephalocysts*, headless bags. These are not to be confounded with enlarged graafian or other vesicles, nor with morbid serous cysts in general. They look like, or rather they are, spherical sacs having one aperture only, and containing a thin colourless liquid. They are usually found congregated, sometimes in vast numbers, within a large cavity or cyst, to which they are not attached. This is a consequence of the peculiar manner in which the animals are propagated. The wall of the cyst is laminated, and the young hydatids bud forth from between its layers. In the species which most commonly infests the human frame, they are born into the cavity of the parent: in some other species they are detached externally. We find therefore a parent bag, full of other smaller bags; which, again, are pregnant, as it were, with their own offspring, the grandchildren of the primary cyst: and so on, somewhat after the manner of a nest of pill-boxes. Minute in their origin, these parasites may thus increase and multiply till the original cyst attains an immense size, and at length destroys life by its bulk and pressure. Of course the immediate consequences of such pressure will depend greatly upon the parts occupied by the hydatids. You may readily imagine what kind of symptoms are likely to ensue, when they are lodged within the abdomen, within the less yielding thorax, within the unyielding skull. They are more common in the liver than in any other single organ. I lately mentioned the case of a woman, Harriet Baldwin, who died in the Middlesex Hospital, and whose liver contained thousands of these globular bodies. The enlarged gland had completely sealed up, by its pressure, a portion of the inferior cava.

We can seldom tell that hydatids exist in the body until we see them; nor, if we knew of their presence, could we propose any rational method of cure. It has been fancied that a galvanic current, or an electric shock, passed through the organ containing these creatures, might kill them, and so at least prevent their increase: or that they might be poisoned by drugs that are not seriously prejudicial to man, such as mercury or iodine. But these are mere dreams of our baffled art. They sometimes open a way for themselves to the surface, and escape through an ulcerated outlet: and sometimes they are let out, to the surprise perhaps, of the operator, who only knew that he was dealing with an abscess which required puncturing. Mr. Arnott put a lancet into a fluctuating tumour in the epigastrium of one of my patients; very offensive pus issued, with the shrivelled skins of sundry defunct hydatids. The cyst was situated, I believe, in the liver. Although the orifice was slow to heal, the patient ultimately got well.

There are single cyst-like bodies, with short retractile necks, bearing the generic name

of *cysticercus*. One species of this kind, the cysticercus *cellulosus*, inhabits the interfascicular cellular tissue of the muscles. It is rare in the human subject, but frequent in the pig; giving rise to that condition of the muscles which is familiarly known as measly pork. This is one of the internal parasites with which the organ of vision is liable to be infested. A most remarkable instance occurred a few years ago, in Glasgow. In the eye of a child, who had suffered repeated attacks of ophthalmia, Mr. Logan discovered one day, to his extreme astonishment, a semitransparent body, about two lines in diameter, floating unattached in the anterior chamber. It seemed almost perfectly spherical, except that from its lower edge there proceeded a slender process, of a white colour, with a slightly bulbous extremity, which appeared to be heavier than the globular part, for it was always turned downwards. This head or neck, was seen to project or elongate itself from time to time; and occasionally it was drawn up and completely hidden in the cystic portion. When the patient sat still, in a moderate light, the animal covered the two lower thirds of the pupil. "Watching it carefully (says the gentleman who has recorded the case), its cystic portion was seen to become more or less spherical, and then to assume a flattened form; while its head I saw at one moment thrust suddenly down to the bottom of the anterior chamber, and at the next drawn up so completely as to be scarcely visible." The child's head was now turned gently back, and instantly the hydatid revolved through the aqueous humour, so that its head fell to the upper edge of the cornea, now the more depending part. Upon the child again leaning forwards, it settled, like a little balloon, in its former position; preventing the patient from seeing objects directly before her.

The animal was carefully watched for three weeks; and no other change was noticed than a slight increase in the bulk of its cystic portion. In six weeks it had evidently grown bigger, the eye became injected, and the iris less free in its movements; and pain ensued. Extraction of the hydatid was attempted; but the patient was unruly; the lens was forced out, and the animal ruptured and expelled in shreds: the iris became entangled in the wound of the cornea, and vision in that eye was spoiled.

Trichina spiralis.—There is a very singular microscopic parasite, the *trichina spiralis*, dwelling in myriads, sometimes, in the muscles of the living human body. It was first described, I believe, by Mr. Hilton, of Guy's Hospital, and afterwards more fully by Professor Owen in 1835. Mr. Wormald, the Demonstrator of Anatomy at St. Bartholomew's Hospital, sent to that gentleman a portion of human muscle, which presented a singular speckled appearance, as if it were mouldy. Mr. Owen found that each speck was a shuttle-shaped cyst, containing a very minute cylindrical worm, coiled up in two, or two and a half, spiral turns. The worm measures, when unrolled, no more than $\frac{1}{30}$th of an inch in length, and $\frac{1}{700}$th of an inch in diameter; and of course requires, for a satisfactory examination, to be seen through a microscope. The longer axis of the containing cyst lies between, and parallel to, the fibres of the muscle. Fourteen similar instances have since come to Mr. Owen's knowledge.

This is a very strange kind of parasite. One would imagine that the presence of innumerable living beings, in or between the muscular fibres, would be likely to give rise to symptoms. We might expect pain, or muscular debility, or embarrassed movements; yet no indication of the presence of these worms seems to have been afforded, in those instances in which the condition of the subject in whom they were found was known, during life. The principal points that have hitherto been made out appear to be the following:—

1. The muscles thus beset with parasites are the voluntary muscles: and those which lie superficially are fuller of the worm than the deeper seated. The pectoralis major, latissimus dorsi, and other large flat muscles, usually present them in great abundance. They have been detected in the muscles of the eye; and even in those belonging to the little bones of the ear, and of whose action we are wholly unconscious. They occur also in the diaphragm, in the muscles of the tongue and of the larynx, in those of the soft palate, in the constrictors of the pharynx, in the levator ani, in the external sphincter ani, and in the muscles of the urethra. They have not yet been seen in the muscular tunic of the stomach and intestines, in the detrusor urinæ, or in the heart. Mr. Owen makes

this interesting remark—that all the muscles infested by the trichina are characterized by the striated appearance of their ultimate fasciculi: whereas the muscles of organic life, which the animal does not inhabit, have, with the exception of the heart, smooth fibres, not grouped into fasciculi, but united reticularly.

2. It appears, also, from what has been hitherto observed of these entozoa, that their presence in the body is unconnected with age, sex, or any particular form of disease. They have been concomitant with cancer of the penis, tubercles of the lungs, exhaustion of the vital powers by extensive ulceration of the leg, fever, combined with pulmonary phthisis, aneurism of the aorta, sudden depression or collapse after a comminuted fracture of the humerus, diarrhœa. They have also been met with in the muscles of a man who, while in the apparent enjoyment of robust health, was killed by a fracture of the skull.

The *Filaria Medinensis*—*Dracunculus*—or *Guinea-worm*—has its residence in the subcutaneous cellular tissue. It is a long, slender, round, uniform animal, like a fiddle-string, or a piece of bobbin; as you may see in the specimens before you. Its length varies from five or six inches to twice as many feet. Men's lower limbs, their feet and legs, are the parts most commonly possessed by this worm; but it occurs also in the scrotum, in the parietes of the belly, in the arms, beneath the conjunctiva of the eye, and in almost every superficial situation. It is sometimes solitary; but several may coexist or succeed each other in the same individual; nine or ten perhaps. A Dr. Marrudri, a friend of the celebrated Clot Bey, had suffered from twenty-eight of them in succession.

This entozoon is endemic in the hot intertropical regions; in Asia and Africa; upon the coast of *Guinea*, whence its trivial name. It sometimes abounds after the manner of an epidemic. Sir James M'Grigor tells us that the 86th and the 88th regiments, stationed at Bombay, were much plagued by this pest. The 86th was free from it upon entering the fort, in September, 1799; and so continued till the setting in of the monsoon in 1800. In the course of the monsoon nearly 300 of the men were attacked. The 88th regiment relieved the 86th. No case of Guinea-worm appeared among them for nearly a month after their coming into the barracks at Bombay, in October, 1800. In the latter end of November, they embarked for the Egyptian expedition; and in the course of the voyage, in one ship alone 199 men out of 360 were crippled and laid up with this loathsome disease. It was thought to be infectious. The artillery-men, who were kept separate, escaped. Of 181 instances, of which Sir James M'Grigor gives a tabular account, the feet were the parts affected in 124.

These animals sometimes remain for a long while in the cellular tissue without producing inconvenience, and therefore without betraying their presence. Hence they are sometimes brought over to this country. The great navigator, Dampier, had no symptom of a Guinea-worm which he carried about with him, until about half a year after he left the place in which he contracted it. Sometimes the parasite is quiet and harmless for a still longer period; in one recorded instance it was latent and dormant for three years.

The symptoms which do at length arise are the following: itching of the part affected; a sensation as if there were something creeping under the skin; sometimes a cord-like ridge can be felt in the track of the worm: at length a vesicle, or a pustule, or a little boil forms, from which, when it breaks, the head of the animal protrudes. This process is often attended with fever: and in certain parts of the body the local suffering is considerable; the cellular tissue sloughs; and sometimes dangerous hæmorrhage occurs.

The only treatment which these cases appear to admit of, is the gradual and careful extraction of the worm. Lest that part of it which already protrudes should recede, or be broken, it is gently wound, day by day, round a small stick, or a little roll of adhesive plaster; pains being taken not to pull upon it so much as to risk its being torn asunder. The roll is protected by a bandage. Whenever, by accident, the animal is broken, very serious consequences are said to ensue; violent inflammation of the part, abscesses and sinuses, and high irritative fever. This mischief is ascribed by some to the presence of dead animal matter, by others, of young filariæ, in the subcutaneous tissue: for I should have mentioned that the Guinea-worm is viviparous; and although neither digestive organs nor generative organs have yet been discovered in its structure, it is sometimes found

stuffed internally with a countless offspring. The extraction is tedious work. Where, indeed, the cellular tissue is very loose, as in the scrotum, the worm is occasionally drawn out at the first attempt: but the process sometimes occupies weeks; and its average period appears to be not less than ten days. When the course of the animal is quite superficial and obvious, the natives are accustomed to make an incision in the skin, at about the middle point, and to pull the worm through from both ends. When once it is out, the parts presently heal. All other medication (and much has been tried) has been found useless; except, perhaps, the administration of assafœtida; and that not as a means of cure, but of prevention. It is said that the Brahmins in India, who are in the constant habit of using this drug, are exempt from attacks of the dracunculus. Cleanliness was also found, in the army, to be a considerable protection.

There is a species of filaria peculiar to the eye; and another to the bronchial glands: but these are extremely rare.

Strongulus gigas.—The urinary organs have their parasites also:—of which I shall specify but one, and that chiefly on account of its strange lurking-place, and remarkable size. I allude to a species of strongle which sometimes occupies the human kidney, and which is no uncommon tenant of the same organ in various animals; the horse, the bull, the dog, the wolf, the polecat, and the otter. In the human subject its length varies between five inches and a yard, and it is sometimes half an inch in diameter. There is a specimen nearly of that size in the Hunterian museum. It may well be called the *giant* strongle, *strongulus gigas*. Fancy a creature as big as a snake coiled up in one's kidney. It gives rise to no distinctive symptoms, although, as you may suppose, it causes much renal distress; hæmaturia, retention of urine, and great suffering in its passage out of the body, either through the natural urinary channels; or by abscess and ulceration through the back.

Origin of entozoa.—With respect to some, at least, of the parasitic animals that I have been describing, those I mean which are found shut up in close chambers, our first feeling is that of wonder how they came there. Into all parts, from which a road is open to the external surface, we can conceive that living creatures may enter, or their eggs be carried. But how can either animals or ova find their way, unperceived, into the substance of the liver and of the voluntary muscles, into the eye, into the brain? The whole matter is obscure, yet interesting.

With respect to the common hydatids, it has been conjectured—and the conjecture does not seem improbable—that they are not parasites, nor distinct animals in any sense; but merely certain of those primitive nucleated *cells*—from which the microscope asserts that all the varied tissues of the body are originally formed—rendered gigantic and monstrous by some erring or morbid action of the vital forces. Except in size, the cell and the hydatid are declared to be alike: alike in shape, alike in the mode of their growth and multiplication. But this view of the matter, granting it to be the true one, does not relieve our difficulty; for within these very hydatids, distinct living animals have been found: parasites of the second order, entozoa of an entozoon, if the hydatid be itself a separate animal—tenants of a formation-cell, if the hydatid be only an extravagant development of the primary corporeal structure.

Within several of the transparent hydatids which were taken from the liver of the woman Baldwin, a number of small, opaque, white grains were visible. These were examined by Mr. Tomes and myself, under the lens of his powerful microscope. They were plainly minute animals: baglike, with an orifice or mouth which, in some instances, protruded a little from the bag, in others was evidently contracted and drawn inwards. Around this orifice was arranged a circlet of small, flat, spearshaped rays, somewhat like a Vandyke collar. Many of these rays, or spines as they have been called, were detached, and lying loose in the surrounding liquid. Being very ill-informed in this department of natural history, I paid less attention to these creatures than they deserved;—fancying indeed that they might be common and well-known, or rather that they were juvenile hydatids. I have become aware of my mistake, through perusing a paper, by Mr. Curling in the twenty-third volume of the *Medico-Chirurgical Transactions;* where he more

minutely describes precisely similar phenomena. Mr. Curling shows that these included animalcules are parasitic vermin which infest a peculiar species of hydatid, called the *Echinococcus Hominis*.

Now whatever hypothesis we may adopt respecting the nature of hydatids themselves, the puzzling question still remains, whence originated the living beings inclosed within them? How got *they* thither?

It was the opinion of Linnæus, and of other natural philosophers of his time, that the *intestinal* worms were really terrestrial or aquatic animals which had been accidentally swallowed, either while young and small, or in the antecedent state of ova. It was even pretended that these animals had been recognized and detected out of the body, in stagnant waters. But later inquirers, especially Bremser and Rudolphi, have completely disproved this notion. After dedicating twelve years of his life to the observation and study of entozoa, Bremser was satisfied that no creatures identical in structure with the intestinal worms are ever met with out of the body, except such as have come from the intestines of man, or of some other animal; and conversely, that no terrestrial or aquatic worms are ever found living within the bodies of men or of animals, unless they have been directly and plainly received from without.

But, then, is it not possible that, as Boerhaave supposed, aquatic or terrestrial reptiles, casually entering the body from without, being placed under entirely new and unnatural conditions, may have attained a monstrous growth, and undergone metamorphoses such as we know that some of the lower animals, by change of circumstance, do undergo; as the tadpole becomes a frog, the maggot a butterfly?

To this theory there are strong grounds of objection. First, there is no ascertained relation (as in the other cases) between the structure of intestinal worms, and of animals having an independent existence out of the body: and they are never caught (as they would surely sometimes be) in the transition state—the intermediate condition. Secondly, opposed to this "accidental" hypothesis are also the facts that certains species of worms infest certain species of animals only: that in the same animal different species of worms occupy (as we have seen) special parts of the alimentary canal; have each their peculiar habitat: that worms and animals of external origin mostly die as soon as they are received into the digestive organs; while the true intestinal worms perish whenever they are delivered therefrom. Thirdly, the circumstances that these worms not only live but breed within the human bowels, and that they are met with even in the intestines of the unborn fœtus, are very adverse to this theory of an accidental *error loci*.

But, to give up the notion of a metamorphosis, may not intestinal worms spring from specific germs or ova introduced from without, not casually, but in accordance with a natural law: germs or ova which find in the interior of living bodies the only conditions that admit of their development, the only soil in which they are capable of germinating, the only nest in which they can be hatched? I confess that such is my own belief. We have something like this, at least in that common affection of horses called the "bots." A species of œstrus or gadfly deposits its eggs upon the animal's hide, where they cause, I presume, some irritation, which induces the horse to lick that part with his tongue. The eggs are thus conveyed into the mouth, whence they reach the stomach. There they are converted into larvæ, and affix themselves to the parietes of the stomach. At length, when they are ready to undergo their final metamorphosis, they are detached from the interior of the stomach, pass along with the food and fæces through the intestines, and are ejected from the rectum with the dung.

Why, it is asked, may not similar phenomena take place in the human body? There can be no doubt that we every day swallow, inadvertently, numerous ova, of various kinds. It is supposable enough that sometimes the digestive organs may, and sometimes they may not, have the power of decomposing or expelling these ova.

It is quite certain that what are generally called *spurious* worms may have that mode of origin in the body. Thus, Dr. Elliotson states that he had once a patient, an infant, who discharged from the bowels a dozen live larvæ, or maggots, of the common fly. The child had eaten part of a *high* pheasant some months before. There was, in that case, this instructive circumstance. The infant had been suffering under a chronic cough, but as soon as those larvæ were got rid of, the cough ceased. Dr. Elliotson says that he saw

them in the napkin, moving about in the fæcal matter, just as they might have done if they had never been in the child's body. The same physician tells us that he has twice known, in two different patients of his, a living caterpillar to be discharged from the intestines. One of the patients was a woman who had been in the habit of eating cabbage-stalks while she was washing them for the pot. The moth lays its eggs on cabbages, and she no doubt had swallowed some of them, and one had hatched within her. In the ninth volume of Dr. Duncan's *Medical Commentaries* is a precisely similar case. A boy, after a dose or two of calomel and jalap, discharged from the rectum very many caterpillars, all alive, and full of activity. He had been in the habit, when in the garden, of eating young cabbage-leaves. Till this habit began he had enjoyed good health. While the animals were within his bowels he suffered severely; had locked jaw; and fell into a state resembling coma. Upon their expulsion he recovered perfectly. Centipedes have in like manner been vomited, and voided from the bowels. But the most wonderful instance of this kind that ever was heard of, is related by Dr. Pickells in the *Transactions of the King and Queen's College of Physicians in Ireland*. A young woman, of melancholic disposition and chlorotic appearance, had been in the daily habit, from some superstitious motive, of drinking water mixed with clay taken from the graves of two priests who lived and died in the odour of sanctity. In this way she probably imbibed the ova of the insects which subsequently issued from her body. In the course of about three years and a quarter, she discharged, partly by vomiting, but chiefly *per anum*, upwards of 2000 beetles, and their larvæ, most of them alive. Dr. Pickells counted more than 1300. Larvæ, and pupæ, and perfect insects, all came forth simultaneously. Some of them ran off, as soon as they were vomited, into holes in the floor; and two large winged insects were so lively and vigorous, as immediately to fly away. These strange births were preceded and attended by a complicated and distressing train of symptoms; a gnawing, and sense of something creeping at the pit of the stomach, vomiting of blood, amenorrhœa, hysterical convulsions, headache, retention of urine, and sometimes a degree of mental derangement. She was at length freed from this disgusting malady by large doses of the oil of turpentine.

These spurious worms differ from the true intestinal parasites in this—that the human alimentary canal is not their *only*, but their accidental and unusual nidus. Nevertheless, their occasional presence, alive, in that place, adds to the probability that some of the entozoa may be originally ectozoa.

Question of spontaneous generation.—The main difficulty, however, respects those animals which occupy shut cavities within us, or are imbedded in our solid organs: and this difficulty forms one avowed ground of the theory of *equivocal generation:* which means the spontaneous production of living creatures, independently of any germ, or egg, or parent. The vulgar suppose that dirt engenders fleas, that maggots result from the putrefaction of flesh, that eels arise, of themselves, in and out of mud. In other words, they infer the spontaneous origin of those creatures, of which they cannot or do not trace the procreation by pre-existing parents: and philosophers and men of science have done the same. They will not believe that which they cannot see. Now this doctrine of equivocal generation shocks, I confess, my mind, and offends my reason. If well founded, it strikes at the root of that great argument of Natural Theology, which deduces the existence of a First Intelligent Cause, from the marks of adaptation, design, and contrivance, so manifest throughout the visible universe. Observe the demand which this doctrine makes upon our faith. In defiance of all experience and analogy in respect to creatures which our finite senses are competent to examine, it calls upon us to believe that living beings of complex and intricate, yet definite and harmonious structure; provided with a digestive apparatus, with instruments of locomotion, with generative organs; of various species; in many instances of separate yet answering sexes; that not one or two of these beings, nor a pair or two, but beings and pairs innumerable, are daily formed by the casual concourse of "organic molecules." The obscurity that hangs over the origin of the entozoa is not indeed the only, nor I think the chief ground upon which the notion of spontaneous generation rests. You are probably aware that minute animalcules, so minute that most of them cannot be seen without a microscope, soon become abundant in water wherein vegetable or animal matters have been dissolved by infusion. Such animalcules are therefore called *Infusoria*. How do they come there? There are two suppositions open to us.

One is, that they are formed by the fortuitous union of organic atoms contained in the infusion. The other is, that they proceed from ova or germs existing in the liquid, or floating always in the atmosphere, and ready to quicken whenever they light upon their proper element. That the ova of animals which are themselves visible only by the aid of a microscope, should be absolutely invisible by us, is not surprising. We may conclude that the latter supposition is the most true, if we can show that when these ova or germs are excluded, all the other conditions of the production of infusoria being present, no animalcules appear. Now Spallanzani long ago found, by careful trials, that no animalcules were discoverable when the access of air to the infusion was completely prevented. But it has been objected to his experiments, that the presence of atmospheric air may be one of the essential conditions upon which the requisite combination of the organic molecules depends. Air, solar light and heat, and organic matters in solution being given —does animal (or even vegetable) life ever result? That is the question. The experimentum crucis has been made, and has answered "No," as I lately learned from one of Professor Owen's admirable introductory lectures, by whom, I am glad to find, this uncomfortable doctrine of equivocal generation is strongly discountenanced. The experiment to which I refer was conducted by M. Schulze, of Berlin. I will read you his own account of it, as I find it recorded in the *Edinburgh New Philosophical Journal.*

"The difficulty to overcome consisted in the necessity of being assured, first, that at the beginning of the experiment there was no animal or germ capable of development in the infusion; and secondly, that the admitted air contained nothing of the kind. For this purpose I filled a glass flask half full of distilled water, in which I mixed various animal and vegetable substances; I then closed it with a good cork, through which I passed two glass tubes bent at right angles, the whole being air-tight. It was next placed in a sand-bath, and heated until the water boiled violently, and thus all parts had reached a temperature of 212°. While the watery vapour was escaping by the glass tubes, I fastened at each end an apparatus which chemists employ for collecting carbonic acid; that to the left was filled with concentrated sulphuric acid, and the other with a solution of potash. By means of the boiling heat, every thing living, and all germs in the flasks or in the tubes, were destroyed; and all access was cut off by the sulphuric acid on the one side, and by the potash on the other. I placed this easily moved apparatus before my window, where it was exposed to the action of light, and also (as I performed my experiment during the summer) to that of heat. At the same time I placed near it an open vessel, with the same substances that had been introduced into the flask, and also after having subjected them to a boiling temperature. In order now to renew constantly the air within the flask, I sucked with my mouth, several times a day, the open end of the apparatus filled with solution of potash; by which process the air entered my mouth from the flask through the caustic liquid, and the atmospheric air from without entered the flask through the sulphuric acid. The air was of course not at all altered in its composition by passing through the sulphuric acid in the flask, but if sufficient time was allowed for the passage, all the portions of living matter, or of matter capable of becoming animated, were taken up by the sulphuric acid and destroyed. From the 28th of May till the beginning of August, I continued uninterruptedly the renewal of the air in the flask, without being able, by the aid of the microscope, to perceive any living animal or vegetable substance, although during the whole of the time I made my observations almost daily on the edge of the liquid: and when at last I separated the different parts of the apparatus, I could not find in the whole liquid the slightest trace of infusoria, of confervæ, or of mould. But all three presented themselves in great abundance a few days after I had left the flask standing open. The vessel which I placed near the apparatus contained, on the following day, vibriones and monads, to which were soon added larger polygastric infusoria, and afterwards rotatoriæ."

This experiment confirms the belief which various other facts had suggested—that the different kinds of entozoa are not parentless animals, and that they somehow find their way into the body they inhabit from without. The lowest of the infusoria are of fixed and determinate species; and Ehrenberg states that even the minutest monads possess a complicated organization. It may be asked, concerning both them and the entozoa, why, if they ever arise spontaneously, should they be furnished with a generative apparatus? Again, some of the entozoa abound in certain places, and strangers coming to those places

appear to contract them there. The dracunculus was thought by the soldiers in India to be communicable from person to person, as the itch insect, and the chigoe, to both of which it has some analogy, certainly are. The infant filaria probably creeps in through the skin without causing any noticeable pain. Even that monster among the entozoa, the tape-worm, invades the bodies of those persons who visit the countries to which it belongs. I told you before that, when tape-worm occurs in Germany, it is always the tænia solium; when in Switzerland, almost always the tænia lata. Now the celebrated Soemmering was afflicted by one of these beasts; and he was by birth a German: yet the worm that he voided was of the foreign species, the tænia lata. He had resided, however, for some time in Switzerland; and there, we can scarcely doubt, he caught the ovum, or the young one, of the parasitic animal. Mr. Abernethy once told me the following curious story:—A shepherd had to drive a flock of healthy sheep to a distant part of the country. The journey occupied two or three days. On the road one of the animals broke its leg, and was carried the rest of the way on horseback. All the flock, except this hurt individual, was turned for one night into a marshy pasture. The broken limb was set, and the patient got well; and was the only one of the whole flock that did not subsequently become affected with the rot; the only one that escaped having flukes in its liver. Is it not presumable that the ova of these parasites were swallowed with the herbage cropped by the sheep in the damp meadow? The germs of the entozoa which dwell in closed chambers, and within the solid viscera of the body, are probably carried thither by the blood.

Upon the whole, we may reject the hypothesis of equivocal generation, and fall back upon the Harveian axiom, taken in its most extended sense, of *omne vivum ex ovo*. If I have digressed somewhat in order to set before you the grounds of my own belief in this matter, the interest and importance of the subject must be my excuse.

What I have further to say will relate exclusively to the intestinal worms of the human body: their predisposing causes; the symptoms they occasion; and the modes of getting rid of them.

Predisposing causes.—However much we may be in the dark as to the exact mode in which these parasites reach their habitations, we do know *something* of the circumstances that appear to favour their production and multiplication. They certainly prevail *most* in persons who, from whatever cause, are weak and unhealthy; and particularly in scrofulous children. There are, however, many exceptions to this: they are not uncommon in individuals who are robust and vigorous. Intestinal worms of all kinds, are more abundant in some situations than in others; especially in places that are low and moist. They are accordingly very frequent in Holland, and in some parts of Switzerland. Wherever there is much debility of the digestive organs—in leucophlegmatic habits—in persons who secrete habitually a large quantity of mucus—worms are apt to congregate. The children of the Negroes in the West Indies are wonderfully infested by them.

General symptoms.—It more concerns us to inquire into the general symptoms, through which the existence of worms in the alimentary canal may be ascertained, or suspected.

Those symptoms are very multifarious; and, for the most part, very equivocal. I know of none that can be reckoned certain or pathognomonic, except the actual appearance of one or more of the animals, or of portions of them, in the excrements of the body. Yet that they do give rise to a variety of morbid phenomena—which morbid phenomena are, however, liable to be produced by other causes also—there can be no question.

The most common of these are well known to all nurses and old women: such as colicky pains, and swelling of the belly; picking of the nose, in consequence of itching and irritation there; itching of the fundament; a foul breath; grinding of the teeth during sleep; a variable and capricious appetite, sometimes voracious and insatiable, sometimes none at all; and irregular bowels.

Worms sometimes give rise to strange, and even severe, *nervous* symptoms, explainable upon the principle of the reflex office of the spinal cord. We conclude that the symptoms are owing to worms in such cases, because they cease when the creatures are got rid of. Some examples of this I have already noticed. Thus Dr. Elliotson's infant patient lost a chronic cough upon the expulsion of the live larvæ of the common fly. Bremser gives a very similar case. A child of eleven, afflicted with tænia, had a troublesome dry

cough. It was observed that the cough was suspended for two months, just after a very large portion of the worm had been brought away by anthelmintic medicines. This kind of coincidence happened, not once only, but three or four times; and at length, when the whole of the worm had come away, the cough was permanently cured. I mentioned, some time ago, the frequent association of intestinal worms with epilepsy, which is then of the eccentric form: and I stated that a certain nobleman voided some kind of worm (a tape-worm, I think) from his bowels, and was thenceforward free from epileptic fits, under which he had long laboured. A curious circumstance, illustrating the fact that irritation of the mucous membrane of the alimentary tube may affect distant parts, is quoted by Dr. Joy from Albinus. A soldier received a wound, which led to the formation of an unnatural anus, in front of the abdomen, and in the track of the colon. Through this opening the mucous membrane of the bowel sometimes protruded; and whenever it was out, and exposed to the contact of cool air, the patient began to cough; and continued to do so till the mucous surface was warm again. Partial palsy, amaurosis aphonia, and other nervous symptoms, occasionally depend upon the presence of worms in the intestines.

But let us examine into the symptoms which are more or less proper to particular species of intestinal entozoa; and into the treatment which they severally require.

Particular symptoms and remedies, of the common round worm.—A variety of symptoms are ascribed to the *ascaris lumbricoides*. Dr. Baillie says that the most characteristic are a tumid belly, emaciated extremities, offensive breath, and a deranged appetite. To these may be added colicky pains of the abdomen. When these animals get out of the small intestines, and ascend into the stomach or œsophagus, they may occasion pain, nausea, vomiting, even convulsions. They have caused death, as I mentioned before, by crawling into the biliary ducts, and into the chink of the glottis. Sometimes, on the other hand, they emerge, unexpectedly, from persons who had received no previous notice of their presence within.

This, the commonest parasitic tenant of the human bowels, is also a troublesome one to eject. A great variety of anthelmintics have been cried up as successful against it; but brisk purgatives, and bitter medicines in the intervals, have the best evidence in their favour. These animals seem not to like steel; and my own plan of assailing them is that of purging the patient from time to time by calomel and jalap, and administering, three times a day, some preparation of iron; the sulphate, or the muriated tincture. I believe that most of the patent worm medicines consist of mercury, jalap, and scammony, given in strong doses. The fœtid drugs, assafœtida, galbanum, valerian, are often given. Cowhage also, the dolichos pruriens, which is supposed to tease the skin of the parasite no less than that of the human worm; and tin-filings, which are thought to bruise or lacerate the offenders, are favourite remedies with some persons. I have never tried them. The oil of turpentine I have not found so successful in expelling this species of ascaris as I shall presently show you that it is against the tape-worm. Croton oil has been much commended, either given by the mouth, or rubbed upon the abdomen. Common salt, coloured by cochineal, and exhibited every morning in half-drachm doses, was found by Dr. Rush to be very successful.

Particular symptoms and remedies of thread-worms.—The symptoms produced by the *ascaris vermicularis* are itching and irritation about the anus, especially in the evening, and aggravated by the warmth of the bed, and by whatever overheats the body. I would refer you to a paper by Dr. Heberden on this subject, in the first volume of the *Medical Transactions*. The case he describes is the more valuable, as it was related to him by a *physician* who was all his life plagued by these thread-worms. Generally, however, they infest children; and become fewer, and at length disappear, as childhood passes into youth. When they do accompany life through its several stages, although they are a source of serious annoyance and suffering, they do not appear to shorten the duration of the patient's existence.

To introduce at one end of a tube, several yards long, substances which are intended to act upon animals that live quite at its other end, would be a very round-about course. Whether a purgative effect, or a specific destructive effect, be the object, enemata are preferable to medicines given by the mouth. *Bitters* offend and destroy these little worms.

I have relieved many patients from their tormentors by prescribing simply the infusion of quassia as an injection. Tobacco clysters are praised; but the remedy is a hazardous one. Dr. Darwall says of an enema composed of half an ounce of the muriated tincture of iron mixed with half a pint of water, "there are few cases so obstinate that this will not suffice to overcome." Lime water, injected into the rectum, forms another effectual remedy for ascarides, and (as pharmacologists love to speak) a rather *elegant* one.

Thread-worms may be scooped out of the rectum, with the finger. Old women fish for them with a piece of fat meat, or a candle, wherewith the entangled worms are drawn out of the bowel. Perhaps, in troublesome cases, the plan laid down by Martinet is as good as any. He recommends three successive injections: the first merely purgative; the second specific (common salt in solution, cold vinegar and water, lime water, some bitter infusion); the third, oleaginous and soothing. Oil often allays the itching. This teasing symptom may sometimes be quieted by applying a towel, wetted with cold water, to the fundament, while in bed.

With some means of this kind for expelling the worms, appropriate measures should be combined for improving the general health.

I know of no signs by which the presence of the *tricocephalus dispar* is revealed; and I am equally ignorant of any remedies for it.

Particular symptoms and remedies of tape-worms.—That a *tape-worm* is within, we know when joints of it are voided. Numberless symptoms have been ascribed to this huge internal parasite. The following are probably the most distinctive. Uneasy feelings in the epigastrium, which often abate or are removed by eating; the appetite generally craving, but sometimes bad; itching of the nose and of the anus; nausea; colic; giddiness; a sour breath. Less frequently loud borborigmi occur; and sometimes convulsions.

Louis has watched and recorded, with his accustomed minuteness, the symptoms of ten cases, in the wards of La Charité. Seven of the patients were males, and three females. The youngest was a boy of twelve, the son of another of the patients; the oldest was seventy-four. Most of them were in comfortable circumstances, and had been habitually well fed. The greater number of them had for some time been passing fragments of tape-worm, with their stools, in their clothes, and in their beds. In one of the cases the articulations had been twice only detected in the stools, and each time upon the operation of a purgative.

In all the patients but two, the other symptoms commenced when the evacuation of the fragments commenced. This renders it probable that the worms begin to give annoyance when they get into the large intestine. The temporary relief that results from the expulsion of portions of the animal strengthens that supposition. The case is mentioned in the *Medico-Chirurgical Journal* of a man who was in the habit of freeing himself from large fragments of tape-worm by introducing a stick into his rectum, and twisting the worm round till it broke.

The chief symptoms observed in Louis's cases were colicky pains of the abdomen; itching of the anus, and of the end of the nose; uneasiness in the epigastrium; and deranged digestion and appetite.

Pain in the abdomen occurred in *all* the instances: but it differed in different cases, both in degree and in kind. It was intermittent; and mostly felt towards the flanks.

There was itching at the margin of the anus in seven of the ten cases; itching of the nose in four. With one exception only, itching was present in one or the other, or in both of these situations.

The appetite was craving in one patient; unaffected in four; variable or bad in all the rest. In all, slight emaciation was observed. In all, the pupil of the eye was of its natural dimensions. This is noticed, because dilatation of the pupil has been set down as one of the symptoms of tænia.

Louis thinks that the following combination of symptoms indicates with tolerable certainty the presence of some kind of worm in the intestines. Pain in the belly; colic of various degrees of intensity, unaccompanied by diarrhœa; itching about the anus, and at the end of the nose. If pains in the limbs, lassitude, and nervous symptoms exist also, the diagnosis is strengthened.

We have a very effectual remedy for tænia—at any rate for the tape-worm of this country—in oil of turpentine, given in large doses. The anthelmintic virtues of this substance were not generally known till a paper on the subject, by Dr. Fenwick, of Durham, was published, in 1811, in the *Medico-Chirurgical Transactions*. A sailor, plagued by this parasite, had noticed that large fragments of the worm were passed whenever he had swallowed an unusual quantity of raw gin. Thinking that a stronger spirit might have a stronger effect upon his internal enemy, he tried a glass of turpentine, which completely cured him. This practice was then taken up by some unprofessional persons, who gave turpentine, with similar success, in several cases. At last Dr. Fenwick, in conjunction I believe with my friend Dr. Southey, investigated the subject; and when they had satisfied themselves of the value of the remedy, the result of their inquiries was communicated by Dr. Fenwick to the late Dr. Baillie, in a letter which was read before the Medical and Chirurgical Society. But there is nothing new under the sun. Fifty years ago, Mr. Malden, in the *Memoirs of the London Medical Society*, recommended the same remedy, in the same doses, for the same purpose. But his recommendation had been neglected, or forgotten.

The dose of turpentine is from half an ounce to two ounces. It may be given in combination with castor oil; or castor oil may be administered afterwards to assist its purgative effect. It should be taken in the morning, fasting: and no drink should be admitted into the stomach until the medicine begins to operate, lest sickness and vomiting should be provoked. The worm generally is voided, dead, within an hour or two.

The inconveniences of turpentine as a remedy are its nasty taste, the sort of intoxication it is apt to produce, the distressing sickness it excites in some persons, and the strangury it sometimes occasions. This effect of it, however, is less common from a large than from small doses of the oil. The bowels should be kept open with castor oil, so long as the urine retains the violet smell, which indicates the presence of the turpentine in the circulation.

Upon the continent a celebrated empyreumatic oil, called Chabert's, is in great repute. It contains turpentine; but is still more nauseous than it. One part of the empyreumatic oil of hartshorn is mixed with three parts of oil of turpentine. After the mixture has stood for three days, three-fourths of it are to be distilled over, in glass vessels, by means of a sand bath. Chabert was a veterinary surgeon; and had used this remedy with remarkable success upon domestic animals, cows, dogs, and sheep. Bremser, after testing its safety by taking it himself, administered it, he tells us, to no less than 500 individuals troubled with tænia. Among these were two children, a year and a half old. He found it both a powerful and a permanent cure. Of the whole number treated there were but four who required a second course of the remedy. It seems to kill both the worms and their ova. The dose is two teaspoonfuls, night and morning, until four or six ounces have been taken; a purgative being from time to time interposed. If that dose produces any confusion of head, it must be diminished.

Bremser admits that this curative process is tedious, but affirms that, on the other hand, it is safe, and but little inconvenient. When the patient has continued free from any symptoms of tape-worm for three months, he concludes that the cure is absolute. Other practitioners agree with him in attesting the efficacy of this oil; but are not so confident about its perfect safety and convenience.

Another great remedy is the bark of the pomegranate-root. This is at least as old as the time of Celsus. It has long been employed in India. Its value has only been recently appreciated in France; and in this country it is not much used, I believe, even yet. You may read a very instructive account of its effects in a paper communicated to the Medical and Chirurgical Society by Mr. Breton. He boiled two ounces of the fresh bark of the root in a pint and a half of water, till the decoction was reduced to three quarters of a pint. Two ounces of this, cold, he gave to a patient who had tape-worm, and repeatad the dose every half hour until four doses had been swallowed. About an hour after the last dose, an entire tænia was voided, alive.

The bark dried in the sun he found still more active. A stout man, forty years old, took a decoction prepared in the same way as the former, only with the dried instead of the fresh bark. Three hours after the first dose, a live tape-worm came away, nineteen feet two

inches long. The medicine thus prepared seemed to be too strong; the patient was sick, giddy, and trembling for several hours. He, also, had taken four doses.

To two boys, of seven and ten years of age, he began early in the morning to administer one ounce of a decoction made of half the previous strength, every half hour, for six times; and then stopped. In the middle of the day he resumed the medicine in half-ounces doses. Giddiness and faintness soon came on; and, about five o'clock, each of the patients passed a tænia of the solium kind.

A scruple of the powder was given, mixed with water, every hour, for five successive hours, to a boy of nine. Forty minutes after the last dose a living tænia was expelled.

The same quantity was given to a girl, ten years of age; beginning at eight, and repeating the dose hourly till twelve o'clock. At twenty-four minutes past one she voided a living tænia lata: and the next morning, at nine o'clock, a dead tænia solium. This curious case shows that the two species may exist in the same person at the same time.

Mr. Breton relates other examples; but those which I have cited are quite sufficient to demonstrate the power of this substance. The bark appears to act upon the worm as a poison. In tepid water tænia will live for several hours. When they are plunged into the aforesaid decoction, they writhe and manifest great uneasiness. Between the first dose of the medicine and the expulsion of the worm the shortest period appears to have been three hours, the longest twenty-five.

Louis's ten cases, before referred to, were all treated by a nostrum called the potion of M. Darbon. It proved successful in all. Eight or ten ounces of it were taken in the morning, before breakfast; and the cure was accomplished by that single dose. It is said to be quite safe, to have no very decided taste, and to cause no further inconvenience than a slight colicky feeling, and uneasiness in the epigastrium, less than is produced by many a purgative: and even these sensations were probably owing to the movements of the worm; for when, after the lapse of four months, the dose was repeated, it was followed by no inconvenience at all. The medicine is not strongly cathartic, and sometimes requires the aid of a lavement. In each of these cases the expelled tænia were apparently dead. Their *heads* were detected in the evacuations: in one instance seven heads were seen. Louis says that within a few days at furthest after the discharge of the worms, all the previous symptoms of their presence ceased; and the patients improved in respect to appetite, digestion, complexion, flesh, and strength. They all remained well four months afterwards, and then the potion was again administered; but it brought away no more worms. Some of the patients, who had previously tried other modes of cure, had never enjoyed so long an interval of freedom.

Various other remedies have been employed, and employed with more or less success. One of them, the root of the male fern, formed the basis of a nostrum, called Madame Nouffer's, which was so highly thought of, that in 1776 the King of France gave that lady some hundred pounds sterling for the secret of its composition. I think, however, you will find the oil of turpentine equal to the cure of tape-worm in most instances. Should it fail, or should circumstances forbid its use, I would advise you to resort to the bark of pomegranate root. It seems probable that M. Darbon's remedy, if its nature were divulged, would supersede all others.

LECTURE LXXV.

DISEASES OF THE LIVER. ACUTE INFLAMMATION. ABSCESS OF THE LIVER. CAUSES AND TREATMENT OF ACUTE HEPATITIS. CHRONIC HEPATITIS. JAUNDICE. ITS SYMPTOMS, CAUSES, AND SPECIES.

FROM the alimentary canal I go to the other organs directly or indirectly concerned in the digestion and assimilation of our food. And, first, let us look at that large gland, the liver, of which the main office, so far as we can perceive, is the secretion of bile.

The liver is liable to various forms of disease: but it is not so frequently in fault as many would have us believe. It is often blamed most gratuitously and unjustly.

The researches of Mr. Kiernan, recently published in the *Philosophical Transactions*, have paved the way for a better understanding in future of its pathology. If I did not feel myself too much circumscribed by the remaining limits of this course of lectures, I should be glad to attempt to assist you in construing Mr. Kiernan's somewhat difficult, and very valuable paper. But doubtless you are made familiar with the anatomy of the organ, as described by him, in other lectures: and I must content myself with noticing, as we go along, one or two points, in respect to its morbid appearances, concerning which, before Mr. Kiernan took the subject up, great mistakes prevailed, even among the most celebrated pathologists.

I shall follow the usual order, and consider, first, *inflammation* of the liver, which may be either acute or chronic.

These are, both of them, diseases that are much more common in warm climates than here.

Acute hepatitis.—Of well-marked acute hepatitis the symptoms are fever, with pain and a sense of tension in the right hypochondrium, inability to lie on the left side, difficulty of breathing, a dry cough, vomiting, hiccup.

You will not find all these symptoms present in every case: yet they are all worth attending to.

The pain is sometimes sharp and pricking, like a pleuritic stitch: sometimes dull and tensive. In the former case the peritoneal covering of the gland is supposed to be affected; in the latter its parenchyma. The pain sometimes extends to the right clavicle and to the top of the right shoulder. This circumstance was noticed by Hippocrates; and I have referred to it before as a good example of what are called sympathetic sensations. The existence of this pain makes it probable that the inflammation affects the convex surface of the liver. Occasionally the *left* shoulder is painful: the left lobe of the liver being involved in the disease. The pain in the right side is aggravated, often, by the movements of the diaphragm in respiration; and this explains the embarrassment of the breathing, and the short dry cough. Why the patient cannot well lie on the left side is obvious enough: all the connections of the inflamed organ are then put upon the stretch by its weight. There are, however, some exceptions to this rule. On the under or concave part of the liver lies the pyloric extremity of the stomach; and that viscus often sympathizes with the hepatic inflammation: nausea and vomiting occur; and hiccup. The thoracic symptoms on the one hand, or the stomach symptoms on the other, may be expected to predominate, according as the convex or the concave surface of the organ is chiefly the seat of the inflammation.

The situation of the pain, the cough, the short and shallow breathing, used to puzzle the older observers: and they confess their occasional inability to determine whether the

inflammation was situated in the lower lobe of the right *lung*, or in the *liver*. But now-a-days we need have no difficulty in making the diagnosis. The ear will tell us, if we employ auscultation and percussion, whether the contents of the chest or of the belly are suffering: and my own experience has taught me that sharp pain, with feverishness, occurring in the debatable ground of the right side, denotes pleuritic inflammation far more often than it does hepatic.

Jaundice is an *occasional*, but by no means a *necessary* effect or accompaniment of hepatitis; whether acute or chronic: and, therefore, what I have to say of that symptom I shall give under a separate head.

Abscess of the liver.—Acute hepatitis may terminate in resolution; or it may terminate in diffused suppuration; or, what is more usual, in the formation of a circumscribed abscess, or of abscesses, in the liver. In this climate we do not often meet with hepatic abscesses; but they are very common in hot countries: and some of the most interesting events of the disease have relation to the progress of these collections of matter. When they approach the surface of the liver, adhesions generally take place (in virtue of that conservative principle of which we so constantly discern the working) between the diseased organ and the neighbouring parts. If no such adhesion occurred, the matter would at length burst into the cavity of the peritoneum; and this *does* sometimes happen. The peritonitis which is thus, suddenly, set up, is almost always fatal. I referred, however, in a former lecture to one instance in which it was recovered from. Sometimes the adhesion is effected between the liver and the parietes of the abdomen, and the abscess points *externally*, and may be opened by the surgeon. Such a case occurred in my neighbourhood last year. Sometimes the liver glues itself to the stomach, or to the intestines; and then the abscess breaks into the alimentary tube, the matter is evacuated by vomiting, or by stool: and all goes on tolerably well again. In other instances the agglutination is to the diaphragm, which is perforated, and the pus makes its way into the sac of the pleura, or through the lung into the bronchi, and so out by the mouth. I have seen three examples of this myself. In one the abscess originated in the formation and degeneration of hydatids in the liver; and the patient (a woman, she was under Dr. Macmichael's care in the Middlesex Hospital) spat up quantities of yellow fluid, consisting partly I suppose of pus, but chiefly of pure bile. The examination of the dead body ultimately demonstrated the nature of that case. The second of the three patients got well: yet I cannot doubt that, bating the hydatids, the disease was essentially of the same kind. It also occurred in a woman, whom I admitted into the hospital. She was a kitchen-maid at the Thatched House Tavern in St. James's street; and she had received a violent blow at the back part of the right hypochondrium. This was followed by pain there and by fever; and at length she began to have cough, dyspnœa, and expectoration. She brought up an abundance of frothy mucus of a bright yellow colour: not at all resembling the rusty sputa of pneumonia, but exactly of the tint which bile would give to it. At the same time large crepitation could be traced from the bottom to the top of the right lung. I fully expected that this woman would die: but by degrees the yellow expectoration ceased, all the auscultatory signs gradually disappeared, and she recovered perfectly.

It is plain that in such instances as this, there must be adhesion of the *lung* to the diaphragm also: but cases have occurred in which the matter burst into the cavity of the pleura, and presently destroyed the patient by suffocation.

The event of the third case is still *sub judice*. A gardener, forty-five years old, was attacked, four weeks before he came under my care in the hospital, with severe pain in the right loin. Just at the edge of the short ribs, and not far from the vertebral column, on the right side, I found a large elastic swelling, very tender, and of a bluish red colour. As the patient's urine was most remarkably loaded with amorphous deposits, my first suspicions turned towards the kidney. It soon, however, became evident that, although the tumour was below the diaphragm, the parts above that muscle were involved in the disease. The right half of the thorax was dull to percussion; no vesicular breathing was audible there, but some scattered crepitation and bronchophony. Four days subsequently to his admission the patient began, during an effort of coughing, to discharge almost in a stream, from his mouth, a considerable quantity of gray, pultaceous fluid, of the consistence of gruel, and horribly fetid. Altogether the amount of matter thus expectorated was esti-

mated at two quarts. The pain then ceased; and the swelling was observed to be less. After three days more, it was deemed proper to puncture the tumour, and matter was let out having precisely the qualities of that which he had ejected through the mouth. The patient remained eleven weeks in the hospital, the expectoration becoming less unnatural, and the discharge from the abscess gradually diminishing. He then chose to depart. Some months afterwards he applied for re-admission, and again staid with us a week or two. His health had much improved during the interval: but the wound in his back was still open, and he still continued to expectorate somewhat: and yellow bile was frequently to be seen, both on the dressings which covered the puncture, and in the vessel into which he spat. I think it probable that this man may eventually recover, at the expense, however, of a permanently damaged lung.

Rigors occurring during the progress of hepatitis should make us suspect that suppuration is taking place: if the pain is thenceforward mitigated, or exchanged for a sense of weight, and hectic fever sets in, we may be tolerably certain that pus has formed.

Of the several courses taken by an hepatic abscess, that towards the surface of the body is the most common; and it is the only one with which we are able to interfere. And the chief thing we have to look to, is not to interfere too soon. The adhesion of the inflamed organ to the wall of the abdomen is the indispensable condition, not of success only, but of safety, where the question arises of puncturing the tumour. Without such adhesion the pus will be transferred from the cyst to the cavity of the peritoneum; or if the abscess be not reached by the scalpel, that cavity will be laid open. Now it is not easy to ascertain whether there be adhesion or not. Certainly the operation ought not to be attempted until the parts above the abscess are very thin, and are verging to a point; and even then, unless there were some distinct purpose, besides that of saving a little time, some urgent distress or danger to relieve, I think such abscesses might be more prudently left to themselves. Dr. Gregory, of Edinburgh, used to mention a case in which, as fluctuation was palpable, and the tumour pointed, it was proposed to open it; but the patient died, somewhat suddenly, before the operation could be performed; and inspection of the dead body showed that no adhesion existed. Mr. Malcolmson has lately published, in the *Medico-Chirurgical Transactions*, two or three instances of a like kind. These are circumstances which teach us to be cautious about recommending the operation.

A case is recorded of the bursting of an hepatic abscess into the pericardium; another into the vena cava. Facts of this kind constitute mere medical curiosities, and have no practical bearing.

It is clear, both from the size of the organ, and from its situation in the body, that an abscess in the liver can never be otherwise than gravely hazardous. Yet many recover from them. Much evidently depends upon the manner and direction in which the pus seeks a vent. The most desirable road for its exit is one which it sometimes takes, but which I have not yet mentioned. It occasionally flows out into the duodenum through the excretory ducts of the liver, when these happen to have been laid open by the suppurating process. Next to this we may hope for its discharge through the adhering parietes of the abdomen; next by a breaking of the abscess into the alimentary canal. The escape of the matter through the air-passages is fearfully perilous; and its entrance into the shut serous sacs, or into the great blood-vessels, almost necessarily fatal.

I have spoken of abscess in the liver, as an event of acute inflammation of that organ. Such inflammation is apt to arise, in tropical countries, after exposure to cold. In any climate it may be excited by a blow, or other mechanical injury. But abscess in the liver seems to be far more frequently the result of other remote causes. You will call to mind those collections of matter which form in the liver, as well as elsewhere, in consequence of suppurative phlebitis. There is, moreover, an acknowledged connection between hepatic suppuration and *dysentery*. In hot climates the two are often found coincident. It has commonly been thought that, in these cases, the affection of the liver happens first, in the order of time; that the hepatic disease, interfering with the freedom of the portal circulation, occasions congestion of the submucous capillary blood-vessels, and so disposes the membrane to take on inflammation under the influence of any slight exciting cause. In some recent lectures, however, delivered before the College of Physicians, Dr. Budd

has proved, by a careful collation of a large number of authentic instances, that the relation of these two morbid conditions to each other, is exactly the reverse: that the dysentery is the primary disorder, and the hepatic abscess the secondary; the link of connection between them being the same as between suppurative inflammation of a vein, and the formation of pus in parts more or less remote. The blood, in its return through the veins which are tributaries of the vena portæ, carries with it, if not pus, yet some vitiating ingredient from the inflamed membrane: and this vitiating ingredient provokes inflammation in the capillary vessels of the liver.

This, to the best of my knowledge, is a new, and certainly it is an important, view of the pathology of hepatic abscess. I trust that, ere long, Dr. Budd will lay before the public the evidence upon which it is founded.

Treatment of acute hepatitis.—Acute hepatitis, when it occurs, requires vigorous treatment in the outset. Our object is, if possible, to prevent suppuration. Blood should therefore be freely taken from the system by venesection, and from the neighbourhood of the inflamed part by leeches. I will not weary you by going over the old ground that we have so often trodden already, nor repeat observations which have been many times made respecting the methods, and the requisite amount, of this great antiphlogistic remedy. Depletion of the portal vessels may also be indirectly obtained by purgatives; especially by such purgatives as produce copious and watery stools. The neutral salts are therefore proper in this disorder. It may be, as some suppose, that they operate beneficially, as counter-irritants, upon the duodenum; but their effect in draining the veins that feed the vena portæ, and thereby relieving the hepatic congestion, is more obvious and more intelligible. These saline medicines should be much diluted; and their action may be quickened, if that be necessary, by adding the infusion of senna.

After blood-letting has been duly performed, and the force of the inflammatory action has been broken, blisters may be applied to the right hypochondrium: and I believe that repeated blistering is more serviceable than a single blister kept open by savine ointment.

Some difference of opinion prevails among medical men in regard to the employment of mercury in the *outset* of acute hepatitis. I cannot pretend to offer you the results of my own observation on this point, but I find that the best authorities, among those who have had to treat the disease in hot climates, are *against* its use at the *very first*, as being stimulating to the liver. I suspect that this is a piece of theory: but at all events, after the first violence of the inflammation has abated, that remedy is not to be omitted, either in the acute, or in the chronic form of the disorder, to be mentioned presently: only in the one case it should be so administered as to affect the system as speedily as possible; in the other it is to be introduced with a slowness which bears a proportion to the pace of the disease.

When suppuration has taken place, or is unavoidable, when the patient ceases to complain of pain, but has in its stead a feeling of weight in the hypochondrium, and becomes distinctly hectic, a corresponding change must be made in the treatment. Active depletion is no longer admissible; you must sustain the strength by a more nourishing diet, and prescribe some tonic remedies; the sulphate of quina, with sulphuric acid; or the nitro-muriatic acid, which enjoys a considerable repute, greater perhaps than it merits, for the relief of liver complaints.

Chronic hepatitis.—Acute inflammation of the liver is apt to degenerate into *chronic*. Chronic inflammation may also arise under the circumstances that give birth to the acute form. Chronic hepatitis, again, is not unfrequently produced by the presence of specific disease in the liver; of what is called carcinoma; of scrofulous tubercles. Melanosis and hydatids are both of them of common occurrence in the same part; and they may give rise to symptoms, or they may not: and when these morbid conditions do declare themselves by external signs, those external signs are very much the same as belong to chronic hepatitis. The precise diagnosis is exceedingly obscure; the symptoms point distinctly to the liver as the *seat* of the disorder; but as to its exact *nature*, we must often be content with probabilities alone.

The symptoms of chronic hepatitis—or of the chronic forms of disease to which I have alluded, when they show themselves by symptoms—are (I give you them in Cullen's

words) "some fulness and some sense of weight in the right hypochondrium; some shooting pains felt at times in that region; some uneasiness or pain felt on pressure in that part; some discomfort from lying upon the left side; perhaps some degree of jaundice; and sometimes a certain amount of fever combining itself with more or fewer of these symptoms." In short, they are just the symptoms of acute hepatitis occurring in a minor degree.

Chronic affections of the liver are sometimes attended with an *increase*, and sometimes with a *diminution*, of its *size*. When it is augmented in bulk, its place and enlargement may be ascertained by palpation and percussion; nay, the magnified gland may sometimes be seen, extending beyond its proper situation in the hypochondrium, and passing far down into the abdomen. I have known the liver reach to the right groin: and when its left lobe is affected, it will sometimes stretch across towards the lower part of the left side of the belly. On the other hand the liver may shrink into a much smaller space than it naturally occupies. These small livers are usually hard. Interfering more with the portal circulation than many enlarged livers do, they are more frequently attended with dropsy of the peritoneum.

The "hobnail" liver, the *cirrhose* of modern French writers, is nodulated as well as hard. The irregularity of its surface may be so great as to be perceptible to the touch. I fully described this condition of the liver when I was upon the subject of passive ascites, of which it is the most common cause.

I mentioned also, some time ago, the "fatty" liver, so fequently found associated with pulmonary consumption. The liver in this state is soft, enlarged, smooth on its surface, and of a buff or tawny colour throughout. Mr. Bowman has lately shown that these changes are owing simply to the unwonted abundance of certain small granules of fat, of which, in the healthy organ, each lobule contains a few only. If in a phthisical patient we find the liver palpably enlarged, we may guess that it is encumbered with this interstitial fat: but there are no symptoms peculiar to the fatty liver. As to its cure, we are quite helpless: and the same may be said of the hobnail liver, as well as of all those forms of disease in which the organ is loaded with specific deposits.

Dark masses of extravasated blood are sometimes found interspersed through this gland, and then, by an absurd perversion of language, the patient is said to have had "apoplexy" of the liver.

The same causes which produce acute hepatitis, acting in a less intense degree, will excite chronic inflammation of the same textures. Intemperance also, and particularly the habitual and excessive use of alcoholic liquors, certainly tends to generate hepatic inflammation, especially in its more chronic form. We see this even here, and it is still more strikingly perceptible in warm climates, as you may learn by reading the works of those persons who have had experience of the diseases of India. Dr. William Ferguson, for example, who was for some time chief of the medical staff of the windward and leward islands in the West Indies, observed that there was a regular increase and aggravation of these chronic affections of the liver among the troops after they received their monthly pay, when they drank great quantities of ardent spirits; arrack in the East Indies, and rum in the West.

There has long been supposed to be what is called a *gin-drinking* liver, in which a section of that gland presents an appearance very closely resembling the section of a nutmeg; and a good deal of useless speculation has been employed as to the nature of the change which has taken place in such cases. Mr. Kiernan has clearly shown that the nutmeg aspect of the liver is produced by mere congestion of blood. Congestion of the liver is, indeed, very likely to arise under the daily stimulus of distilled spirits, but it arises under various other circumstances besides; and therefore it is no safe test of the intemperate habits of the party. And of this we had sufficient evidence before Mr. Kiernan's observations were published. Again and again have I met with the nutmeggy liver, strongly marked, when there was reason to believe that the possessor of it had never transgressed the strictest rules of temperance in drinking: in young persons, for example, of both sexes, who certainly never had been dram-drinkers. Disease of the heart is a very obvious, and a very common cause of hepatic congestion.

You are aware that the congestion occurs under two forms, according as the branches of the hepatic vein, or of the vena portæ, are gorged. If both these sets of vessels are

full, the liver is universally red. If the hepatic vein alone be the seat of the congestion, then in the centre of each lobule we see a red speck, surrounded by yellowish matter; the specks are isolated, the yellow colour is arranged in a sort of net-work. Whereas, if the portal system be greatly engorged, the red streaks will be continuous, and the yellow portion hemmed in by them, and isolated. I show you these distinctions in some specimens prepared by Mr. Kiernan himself.

I should have stated, when speaking of the signs of chronic hepatitis, what is singularly true of chronic liver affections in general, that they are apt to be attended with much languor and lassitude, and a remarkable depression of spirits; and with that sort of dread, and apprehension of impending evil, which I mentioned as being a striking feature of hypochondriasis: the very derivation of which term marks its frequent connection with hepatic disorder. There is sallowness of the complexion also; and sometimes emaciation.

The same remedies are adapted to the chronic as are proper for the acute inflammation of the liver: the comparative mode of their exhibition, however, differs somewhat.

Blood-letting is not often necessary or advisable, except when more violent aggravations than usual of the inflammatory symptoms supervene. Topical bleeding, and blistering, are more expedient.

But the two main remedies to be tried in this complaint are mercury, and the saline purgatives, given in small doses, and repeated for a long time together. Five grains of blue-pill every night, or every night and morning; and as much of the sulphate of magnesia as will produce one or two watery stools every day, for weeks, perhaps, in succession. Patients are not so well content to bear this discipline when it is administered in boxes and phials, *as physic:* but they have more faith in the natural mineral waters: so that a residence at Cheltenham, or some such place, is exceedingly proper to be recommended in these cases; where the daily use of the waters may keep up a continual drain on the system of the vena portæ; and where relaxation from business, the amusements that are constantly going on, with change of scene and of society, may contribute to dissipate the hypochondriacal feelings which are so apt to render the subjects of chronic hepatic disease supremely wretched.

Moderate exercise, in the open air, on horseback and on foot, should be encouraged. There is no doubt that hepatic as well as gastric derangements are fostered by sedentary habits. Tepid bathing is another expedient from which benefit may be hoped. In many instances it will be proper to make trial of Scott's nitro-muriatic bath.

Iodine has been thought of much use in certain kinds of hepatic disease; in those kinds especially which are connected with enlargement of the viscus. The iodide of potassium; or a mixture of the iodide and of iodine; or some of the combinations of iodine and mercury; may be given in such cases: or the *unguentum iodinii compositum*, or the *unguentum hydrargyri iodidi*, of the Pharmacopœia, may be rubbed night and morning upon the hypochondrium. I have not seen much benefit from these forms of medicine myself, in such cases; but they are said, by persons of experience and credit, to have been successful in their hands.

Taraxacum is also a drug which has been much employed in liver complaints since Dr. Pemberton's book on the diseases of the abdomen was published; but it is very doubtful, in my mind, whether it ever does much good. The Germans are very fond of giving the *muriate of ammonia* in small and frequent doses. They have the same belief in the virtues of this salt, in various disorders, as the English practitioners have in those of mercury; and what is curious, they attribute to it some specific agency on the functions of the *liver*.

In the account which I have now given of the principal diseased conditions of the liver, I have not included, nor dwelt upon, all the changes of structure and appearance to which that organ is liable. There are various conditions which disclose themselves by no intelligible symptoms during life, of which the nature has not yet been determined, and of which the cure still remains to be discovered. At this advanced period of the course, and with no time to spare, I do not think it necessary or right to trouble you with the unprofitable discussion of matters that are not strictly practical.

Jaundice.—I have mentioned *jaundice* as an occasional symptom both of acute and of chronic inflammation of the liver. But jaundice is spoken of, in general, as constituting, itself, a distinct form of disease. If we consider it in that light, its diagnosis is most easy. We have only to look upon our patient to know what is the matter with him But jaundice depends upon various and very different morbid conditions; and looking to those conditions as the true objects of diagnosis, we find that the real nature of a given case of jaundice is often involved in very great obscurity.

Symptoms, causes and species.—Let us first consider the constituent features of jaundice, whether it be regarded as a *disease* or as a *sign of disease.* They are, yellowness of the skin and of the eyes; whitish fæces; urine having the colour of saffron, and communicating a bright yellow tinge to white linen.

The characteristic yellow hue is owing, no doubt, to the presence of bile, or at any rate of the colouring matter of the bile, in the circulating blood. And the deep tint of the urine is evidently derived from the same source. On the other hand, the paleness of the fæces is to be ascribed to the want of bile, which always exists in healthy and natural excrement. This last symptom is not, however, a constant one; there may be jaundice while bile appears in the stools. I shall explain how this is supposed to happen presently.

It has been made a question how the bile, or its colouring matter, comes to be visibly present in the blood, or rather in many of the tissues supplied by the blood, and in several of the other fluids of the body. The general opinion, and probably the true one, is, that the bile, after being secreted in the liver, is reabsorbed, and carried into the circulation, and so conveyed to the surface, and to the parts in which the change of colour is observed. This explanation is consistent with all the phenomena which we notice in the disorder.

But another theory has been broached on this subject; first, I believe, in this country, by Darwin. It has been more recently revived by M. Chevreul, who is followed by Mr. Mayo. These pathologists are of opinion that the bile is formed, not by the liver, but in the blood: that the office of the liver is to strain off or withdraw the bile from the circulation, constantly, as fast as it is formed; just as the perpetual elimination of urea from the blood appears to be one great purpose of the kidneys. They hold, therefore, that jaundice manifests itself whenever the due separation of the bile from the blood is suspended or imperfect. Failing of its natural vent, this peculiar substance accumulates in the blood, seeks other outlets, is deposited in various places, and, in fact, partly escapes through unaccustomed channels. They speak of jaundice as a symptom of *suppression* of bile, while others consider it as a sign of *retention:* using these words, suppression and retention, in the sense in which they are applied to the secretion of urine. They maintain that the proper function of the liver, the abstraction of bile from the blood, may be arrested by alterations of the substance and structure of that gland; or by the obliteration or obstruction of the gall-bladder or ducts, impeding or forbidding the removal of the bile already formed; or by some obscure influence of the nervous system upon these organs. They introduce the last kind of cause with the view of explaining those cases, which certainly occur, in which jaundice is the result of severe bodily pain or strong mental emotion. Nay, on their supposition, we might even suppose that the yellowness is sometimes due to a spontaneous and unwonted *abundance* of the elements of bile in the blood: in which case we need not wonder that jaundice should go along with perfect integrity of the biliary apparatus.

The advocates of the first mentioned supposition are aware of the difficulties opposed to its reception, by the occasional absence, even in cases of well-marked jaundice, of all organic disease or apparent obstruction to the excretion of the bile. They show that in very many cases there is some obvious mechanical obstacle to the efflux of the secreted fluid: and in those instances in which none can be discovered after death, they conceive either that the ducts of the liver had been temporarily plugged up by inspissated bile, or a sort of biliary sand—or that they were closed, for a time, by spasm—or by some morbid condition of the duodenum.

Now of these two hypotheses, that which ascribes the yellowness to the reabsorption of secreted bile, is, to my mind, the most probable and the simplest: and I cannot see that it

is attended with *more* difficulty than the other. But you may choose for yourselves between them: and I pass from this digression to a somewhat closer examination of the principal circumstances noticeable in the complaint. Its technical appellation, I should observe, is *icterus*, which is the Greek name for a bird with a yellow plumage, the galbula, or golden thrush; the sight whereof, by a jaundiced person, was death (Pliny tells us) to the bird, and recovery to the patient. Various other terms have been applied to the disorder, most of them having reference, like jaundice itself (from the French *jaune*,) to the unnatural colour. *Morbus arquatus*, from its exhibiting some of the bright hues of the rainbow; *aurigo*, from its resembling gold; and we hear the common people say, now-a-days, such a one is as yellow as a guinea. The Latins spoke of it also under the title of *morbus regius:* why they so called it we learn from the following curious passage in Celsus, giving an account of the pleasant regimen, fit for Royalty itself, to be adopted by those who labour under the malady. "Per omne vero tempus utendum est exercitatione, frictione: si hiems est, balneo; si æstas, frigidis natationibus; lecto etiam et conclavi cultiore, lusu, joco, ludis, lasciviâ, per quæ mens exhilaretur: ob quæ *regius* morbus dictus videtur."

The whiteness of the stools I have mentioned as being a very common but not a constant appearance. It clearly depends upon the absence of bile. But sometimes there is bile in the discharged fæces, and at the same time the yellow colour of the skin and eyes. This probably depends upon the circumstance that some branches of the hepatic ducts are obstructed while the others are free; and thus the bile that is secreted is, in part, reabsorbed into the blood, and in part carried off into the intestines. In a former lecture I stated that one of the uses of the bile appeared to be that of stimulating the bowels to action: it is the natural purgative. Accordingly, in most cases of jaundice, the bowels are costive. But neither is *this* uniform. In some of the worst cases, wherein the jaundice depends upon hepatic disease, which is connected with disease also of the mucous coat of the intestines, there is constant diarrhœa.

In some instances the yellowness of the skin is at first attended with itching, which is occasionally so intolerable as to require the employment of opiates to allay it. In most cases there is no itching at all. The bile never fails to appear in the urine, which is in itself dark, and when collected in considerable quantity in a deep vessel, even *black;* and which tinges any white substance that is dipped into it of a bright yellow. The urine which thus sometimes seems black, may be proved to derive that appearance merely from concentration of the yellowness, by pouring a little of it into a shallow white dish, or by diluting it with water; when the brilliant yellow tint will become manifest. Bilious sweat sometimes occurs, staining the patient's linen yellow. The saliva, in some jaundiced persons, has the same yellow tinge, and a distinctly bitter taste. It has been said that the milk of women who are nursing is made yellow in this disorder. Dr. Heberden, however, states that he never witnessed this; and he had known a woman with a very deep jaundice upon her, suckle her infant for six weeks together with no apparent bad effects upon its health. One man assured him that his tears were yellow. You are aware of the vulgar notion that to a jaundiced eye all things appear yellow. It is an old notion, for we find it expressed by Lucretius:—"Lurida præterea fiunt quæcunque tuentur Arquati." Heberden was disposed to regard this as a mere poetical fiction. But certainly it is sometimes, though very rarely indeed, a fact. Two women, whom he considered however to be of little credit, told Heberden that objects appeared yellow to them. I have been assured of the same thing by a medical man who experienced it in his own person. If I do not mistake, Dr. Mason Good saw all things yellow when he was jaundiced. Dr. Elliotson has had some very interesting cases of this phenomenon. One of his icteric patients declared that objects seemed yellow when looked at with one eye, but not with the other; and in the eye that perceived the yellow tint he observed two large red vessels running towards the cornea. And in one or two instances, which he met with afterwards, of yellow vision with both eyes in jaundiced patients, he found inflammation, or distended blood-vessels in both eyes. This very morning I saw in the hospital a patient of Dr. Wilson, a middle-aged woman, affected with jaundice. She affirms that all objects seem yellow to her vision. In both eyes there are several varicose and singularly tortuous vessels, proceeding across the sclerotica towards the cornea, and some of them reaching

its margin. It seems probable therefore that the ophthalmic vessels, in their natural state, do not permit the colouring matter of the bile to pass through them; but that when they become enlarged by disease, so as to admit the colouring particles of the blood, they may also give a passage to the yellow colouring matter, which tinges the humours of the eye: and in that case the objects seen through the yellow fluids would appear like those viewed through a piece of yellow stained glass. This is a point which is worth your attention in future.

The shades of yellowness are different in different patients. Those who are pale and fair present a bright lemon colour. But in those who are florid, or whose cheeks and skin are flushed with fever, the tint will more resemble that of the Seville orange. Again, if the patient be naturally swarthy, or if his visage is livid or dusky through imperfect arterialization of his blood, the superaddition of jaundice will give him a greenish hue. These differences result from natural or acquired differences of complexion, antecedent to the icterus. But sometimes the bile that is reabsorbed is vitiated and dark; and we may have, for that reason, as Dr. Baillie has pointed out, cases of green or black jaundice. You will remark that from whichever cause the green or dark colour arises, whether from a mingling of the yellowness of the bile with the blueness of lividity, or from the circulation of green-coloured bile, such cases are especially unpromising cases.

Icterus depends, as I have said, upon various and different internal causes: and frequently we cannot determine at all, until death affords us the means of inspecting the parts concerned in its production, what the precise exciting cause may be; even when it is simply mechanical. Any kind of pressure made upon the excretory ducts of the liver will produce it: and such pressure may be exercised by tumours seated in the liver itself, or by a scirrhous pylorus; or by specific disease situated in the head of the pancreas, of which I have seen several examples; or by a diseased condition of the duodenum: and these possible causes of a detention of the bile in its receptacle should always be borne in mind when we are investigating an obscure case of jaundice.

The impediment, in the cases just supposed, is external to the ducts; but they may be obstructed within, plugged up by inspissated bile, or by a biliary calculus. This forms one of Cullen's species of icterus—the *icterus calculosus*. The concretion is most commonly situated, I believe, in the ductus choledochus; sometimes, however, in the cystic, and sometimes in the hepatic duct. The pain that attends the passage of a gall-stone through these ducts is often dreadful. Perhaps there is no pain to which the body is subject that is more severe. You will not wonder at this, when you consider that through a tube, of which the natural size scarcely exceeds that of a goose-quill, there sometimes passes a stone as big as a walnut. The common duct has been found so dilated as readily to admit one's finger. Cullen's definition of this species is "Icterus, cum dolore in regione epigastricâ, acuto, post pastum aucto, et cum dejectione concretionum biliosarum." Now the last of these circumstances, the voiding of biliary calculi by stool, may happen over and over again, without its being noticed, and it does not help us at all to judge of the nature of the complaint at its commencement, while the gall-stone is still in the ducts. With the pain, which is not constant, but comes and goes, there is much nausea and vomiting; and sometimes hiccup; and the matters vomited are usually very sour. The patient is flatulent, and dyspeptic; languid, and gloomy. At length the concretion passes into the intestines; the pain suddenly ceases, and all is soon well again. Attacks of this kind, having happened once, are very apt to be repeated.

Now this pain you might readily mistake for the pain of inflammation, were it not marked by these two circumstances—the absence of tenderness and the absence of fever. Pressure, instead of augmenting, usually mitigates it. The patient keeps his hand firmly applied to his epigastrium; or rests, perhaps, the weight of his body upon some hard substance placed beneath his stomach. I speak now of the beginning of the attack, before there has been much retching; for a degree of tenderness of the abdominal muscles is often produced by repeated straining and vomiting. The pulse is unaffected, or I should rather say it is not accelerated, during the pain: occasionally it is even slower than natural, and the skin cold. Though there be no inflammation, rigors may occur; just as they sometimes happen when a solid substance—a bougie to wit—is passed through, and distending the *urethra*.

Nevertheless, inflammation does sometimes arise, and then the pulse becomes frequent, and the skin hot, and thirst and headache are complained of, and the epigastrium is tender; and if blood be drawn it exhibits the buffy coat. Sometimes the gall-stone makes its way, by ulceration, through the contiguous structures, and so is discharged outwardly, or into the bowels. In such cases there must have been inflammation.

As jaundice often occurs without any pain, so a gall-stone may enter and pass through the ducts, and produce pain, when there is no jaundice. The cystic duct alone may be blocked up, and that portion only of the bile be prevented from escaping which is accumulated in the gall-bladder. It is probable that reabsorption of the contents of that cistern is not very active. Or a calculus of an angular shape may stick in the common duct, and thus impede, without entirely stopping, the egress of the bile. Dr. Heberden thought that gastrodynia was not unfrequently owing to biliary concretions: founding his opinion upon the fact that many persons suffer, for months or years, under occasional attacks of epigastric pain, which is at last associated with jaundice. But after all, this might happen from progressive disease in the stomach itself; and it is a pity that Dr. Heberden's views were not fortified by dissections.

When once a large calculus has forced its way through the natural channels of the bile, they remain permanently dilated; and smaller stones may be afterwards voided without pain or other notice of their passage. There are persons who get rid of scores of them in this way, during the course of their lives.

Sometimes a large concretion, after its extrication from the biliary passages, lodges in the more capacious intestines, and gives rise to serious obstruction there. I mentioned, recently, one case of this kind which had fallen under my own notice. But in general the concretions are presently voided with the stools: and they should always be looked for. The patient is much gratified by *seeing* that his enemy has been expelled; and also by the proof he thus obtains of the sagacity and judgment of his physician. The fæces should be mixed with water, upon the surface of which any gall-stones, since they are specifically lighter than that fluid, will float. I never but once succeeded in thus catching a concretion in the evacuations of a patient, whose symptoms had led me to search for it.

We often find gall-stones, even in vast numbers, in the gall-bladders of persons who during their lifetime had never been known to suffer pain about the liver, or to have jaundice, or to exhibit any token of the presence of such concretions. We infer from this that, while they remain in the reservoir of the bile, they are harmless; and that the suffering and the hazard they occasion are mechanical consequences of their transit through the gall-ducts. I have heard of an instance in which upwards of 1,300 gall-stones were taken from a human gall-bladder after death.

These gall-stones are not, as you might suppose, mere lumps of inspissated bile. There are, I believe, concretions of that kind, but they are very rarely met with in the human subject. The ordinary calculi consist, in a great measure, of a peculiar substance, *cholesterine*, which exists in a state of solution in healthy bile, but which, in some morbid conditions of that fluid, being released from its solvent, assumes its proper crystalline form. Very little is known respecting the circumstances under which the change takes place. Cholesterine, Dr. Prout tells us, is the product of some modification of the oleaginous principle. Biliary concretions seldom form in children. They are much more common in women than in men. They occur most frequently in persons who are corpulent, lead sedentary lives, use generous fare, sleep much, and neglect their bowels: all which things foster or denote a torpid and congested state of the hepatic system. Cattle are said to be subject to biliary calculi when shut up in stalls during the winter, and to lose the complaint when they are again turned out into the pastures in the spring. Hence the absurd notion, countenanced even by Van Swieten, that *grass* is a good remedy for jaundice.

Another variety of jaundice, also noticed by Cullen, is supposed to depend upon mere spasm of the gall-ducts. "*Icterus spasmodicus*, sine dolore, post morbos spasmodicos, et pathemata mentis."

Now the existence of this cause is hypothetical. The gall-ducts, though not distinctly muscular, possess a vital power of contraction. I am not aware that the disease has ever been clearly traced to a connection with "morbi spasmodici." It is an alleged cause which we can neither prove nor disprove. Certainly the "pathemata mentis" play their assigned

part: fits of anger, and of fear, and of alarm, have been presently followed by jaundice: and it has also been produced by great bodily suffering, by a severe surgical operation, or, perhaps, by the dread which attended it. Mr. North witnessed a case in which an unmarried female, on its being accidentally disclosed that she had borne children, became in a very short time yellow. A young medical friend of mine had a severe attack of intense jaundice, which could be traced to nothing else than his great and needless anxiety about an approaching examination before the Censor's Board at the College of Physicians. There are scores of instances on record to the same effect: and *this* is observable of such cases, that they are often fatal, with head symptoms: convulsions, delirium, or coma, supervening upon the jaundice. But with respect to the immediate cause of the icteric symptoms, they may, I say, depend upon a spasmodic constriction of the gall-ducts. Mr. Mayo has suggested another cause, viz., the sudden formation of bile in unusually large quantity in the blood, by some influence propagated through the nerves.

Jaundice sometimes supervenes upon violent and long-continued vomiting; in which case the extremity of the gall-ducts is supposed to have been compressed by the coats of the duodenum.

Jaundice may also occur, as I stated before, as a symptom of acute or chronic inflammation of the liver; and then its treatment will merge in that of the primitive disease that has given rise to it.

Icterus occasionally comes on during pregnancy; and disappears after child-birth. The pressure of the gravid uterus may thrust other organs, a loaded colon for example, against the liver, and so impede the passage of the bile. The little exercise that pregnant women are apt to take, and the costiveness that frequently attends their condition, may have some influence in causing the *icterus gravidarum*.

All systematic writers follow Cullen in making jaundice a common disorder among newly-born children. The *icterus neonatorum* occurs, they say, a few days after birth; is not attended with any suffering, or obvious disturbance of the bodily functions; and soon disappears. Now there seems reason to believe that this is not icterus at all; and has no relation to the biliary organs. The surface of the infant, at its birth, is frequently of a deep red, from hyperæmia or congestion of blood; presenting a condition which falls little short of a mild but universal bruise. By degrees the redness fades, as bruises fade, through shades of *yellow* into the genuine flesh-colour. Such, I am assured by those who are more conversant with these matters than myself, is the pathology of the icterus infantum. Of course true jaundice may, as well as most other complaints, befall the earliest period of life; but I conceive that it seldom does.

The *prognosis* in jaundice is generally favourable; except when it depends upon structural disease of the liver, or supervenes suddenly upon some great mental or bodily shock. In both these cases the prognosis is bad, or doubtful. It is better, in that variety connected with hepatic disease, if the disease proceeds from some known cause, by which a low degree of inflammation has been produced; and the cause is such as can be avoided for the future. Just, indeed, as in chronic hepatitis, of which the icterus is simply an occasional symptom. The prognosis is worst of all in old persons, when the constitution is impaired, and there is no obvious cause for the disease; and particularly when the colour of the skin is greenish, or approaching to black.

LECTURE LXXVI.

TREATMENT OF THE VARIOUS SPECIES OF JAUNDICE. DISEASES OF THE GALL-BLADDER; OF THE SPLEEN; OF THE PANCREAS. DISEASES OF THE KIDNEYS. NEPHRITIS AND NEPHRALGIA. PHENOMENA CONSTITUTING A "FIT OF THE GRAVEL." DIFFERENT KINDS OF GRAVEL. DISEASED STATES OF THE URINE. DESCRIPTION AND REMEDIES OF THE LITHIC, PHOSPHATIC, AND OXALIC DIATHESES.

IN the last lecture, after describing the symptoms, causes, and treatment of acute and chronic inflammation of the liver, and after pointing out various other forms of chronic disease to which that organ is obnoxious, I spoke of *jaundice*. I offered you some comments upon its phenomena; and I indicated several different internal conditions upon which it may, in different cases, depend: and the lecture was closed with some brief hints respecting the *prognosis* of icterus. I have yet to consider the plans of treatment best adapted to the several varieties of the complaint.

Treatment.—Some kinds of jaundice are obviously and absolutely irremediable. From others the patients recover, whatever treatment may be adopted, or without any treatment at all. Hence, as is customary in such circumstances, remedies the most worthless and absurd are extolled for their efficacy against jaundice. The patient gets well, and the drug last tried is held to have cured him. *Post hoc, ergo propter hoc*, is an argument more often applied I believe to the variations of disease, than to any other class of events.

In that species of icterus which occurs, sometimes, in connection with acute or chronic inflammation of the liver, the treatment must be such as I yesterday recommended for acute and chronic hepatitis. Mercury forms an essential part of that treatment; and, unless the disease yielded sooner, I should urge the remedy until its effect upon the gums, and, therefore, its presence in the circulating blood, was apparent.

But to the icterus calculosus, mercury is not so well adapted. What we want is, not a more plentiful or a more healthful secretion of bile, but to get rid of the mechanical impediment to its excretion; or, at any rate, if that cannot be accomplished, to ease the acute sufferings of the patient. Should fever attend the passage of a gall-stone, or should the epigastric pain become epigastric tenderness, leeches may be applied, or a vein may be opened. The abstraction of blood may prevent any thickening of the distended gall-ducts; or it may perhaps relax their spasmodic closure around the calculus. But, in general, blood-letting is not of service nor requisite in this variety of jaundice. Our great resource for relieving the pain, and for loosening the presumed spasm, is opium, given in full doses; and I can add but little, with any advantage, to the directions laid down, on this head, by Dr. Heberden. "This pain (says he) can only be assuaged by giving and repeating opium and its preparations, as often as the continuance of the pain requires them; and because this pain is very apt to return, the patient should always be advised to keep by him, as long as the distemper lasts, pills of pure opium, each weighing one grain —or what is equivalent to them—that no time may be lost in quieting a sensation which is so difficult to endure. One of these pills may be taken as soon as the pain comes on, and it may be repeated once or twice in the course of two hours, if the pain require it; and I have often found it both safe and necessary to give much more."

This plan, of giving opium in the form of *pills*, is the more judicious, because, from their small bulk, they are more likely to be retained than draughts would be. Sometimes the stomach is so irritable as to reject even a pill. I would add, therefore, to Dr. Heberden's recommendations, that of throwing an opiate injection into the rectum; half a

drachm, or a drachm of laudanum, mixed with a small quantity of warm gruel. Another very useful expedient is the warm-bath. If this cannot be readily procured, hot fomentations to the epigastrium, the mustard poultice, the turpentine stupe, are valuable substitutes for it. Dr. Prout states that he has seen more alleviation afforded by large draughts of hot water, containing the carbonate of soda in solution (one or two drachms to a pint), than by any other means. "The alkali counteracts the distressing symptoms produced by the acidity of the stomach; while the hot water acts like a fomentation to the seat of the pain. The first portions of water are commonly rejected almost immediately; but others may be repeatedly taken; and after some time it will usually be found that the pain becomes less, and the water is retained. Another advantage of this plan of treatment is, that the water abates the severity of the retching, which is usually most severe and dangerous where there is nothing present upon which the stomach can react. This plan does not supersede the use of opium, which may be given in any way deemed most desirable; and in some instances a few drops of laudanum may be advantageously conjoined with the alkaline solution, after it has been once or twice rejected." The pain having been quelled, the bowels should be swept out by a brisk purgative.

When jaundice appears to have been suddenly engendered, by moral causes, the rationale of its production is obscure; and the treatment has a corresponding uncertainty. The bile, retained or readmitted into the blood, is supposed to operate, as a poison, upon the nervous system. But the mental state which precedes and seems to occasion the jaundice, may possibly be itself the cause of the nervous symptoms that follow. In other forms of the malady patients remain intensely yellow, often for a long time together, without becoming comatose, delirious, or convulsed. Not that this is conclusive. We know that a given poison may influence different persons, very differently. The same dose of opium that will put one man to sleep, will stimulate a second to madness, and will have no sensible effect upon a third. In the very complaint before us, one patient is tormented with a universal itching, which we attribute to bile in his blood; and ten others remain free from that disagreeable feeling. If we were sure that the bile was the material cause of all the cerebral symptoms, we might hope to draw some of the poison off by bloodletting: but we are not sure of this; and reasoning upon the matter helps us not much towards the cure. The lesson which experience has furnished amounts to no more than this: that active purging is sometimes followed by evident amendment, and ultimate recovery. I would bleed also, if the *pulse* warranted venesection, but not otherwise. In all the varieties of what, from its intensity and rapid accession, I may call acute jaundice, purging is strongly indicated: and we sometimes succeed in rectifying the whole morbid condition by thus applying a sudden *wrench* (so to speak) to the biliary organs; by giving, for instance, half a scruple or a scruple of calomel, and, a few hours afterwards, half an ounce of castor oil, with half an ounce of spirit of turpentine.

When *green* jaundice arises from hepatic disease, we can only palliate. Mild laxatives and anodynes comprise all that such a state admits of. For the icterus *gravidarum*, delivery is the natural cure; it may sometimes be removed by the careful employment of aperients.

Diseases of the gall-bladder.—The *gall-bladder* has its own diseases, which I do not stop to investigate, for they seldom become the objects of specific treatment. Sometimes it is found shrivelled up, and nearly empty: sometimes enormously distended; sometimes ruptured. Of these conditions, the distension of the gall-bladder is the only one that we can ever expect to recognize in the living body. The bag then projects beyond the edge of the liver, and is palpable externally, forming an elastic tumour in the right side. Authors lay down marks for distinguishing a distended gall-bladder from abscess of the liver, and from a hydatid cyst; but they are not much to be trusted to; nor is the precise diagnosis of any great moment. The practical rule seems to be that, when the swelling is adherent to the parietes of the abdomen, we may puncture it, whatever is its nature: but under no other circumstances.

Diseases of the spleen.—I have already, incidentally, described most of the morbid

states of the *spleen* which are susceptible of relief from medicine: especially the enlargement of that body constituting the *ague-cake* of the fens, and occurring in connection with intermittent fever; and that other kind of enlargement which sometimes goes along with hæmatemesis and melæna. The spleen is liable to tubercles also; to deposits of other specific tumours, and of bone; and to softening of its substance.

The best remedy for the ague-cake is the remedy for intermittent fever, *quina*. Purgatives also have the effect of reducing hypertrophy of that curious organ. One caution enforced by Dr. Abercrombie, is that in splenic disease, mercury should be sedulously avoided, or rather such an employment of mercury as would risk tenderness of the gums. Dr. Williams, of St. Thomas's Hospital, states in a recent publication, that he has made many trials of the *bromide of potassium* as a remedy in various disorders; but that he has satisfied himself of its utility only in cases of diseased spleen. Of this I know nothing.

Diseases of the pancreas.—Again, it may seem a slight to the *pancreas* to pass it over without noticing the diseases to which it is subject. But really those diseases appear to be but few; and they do not signify their existence by any plain or intelligible signs. I have, eight or nine times perhaps in my life, met with carcinomatous deposits in the pancreas. In every instance the head of the gland, that extremity which lies next to the bowel, has been the exclusive or the principal seat of the disease. I have known this change in the pancreas cause jaundice by obstructing the bile ducts; and I have known it produce enormous and slowly fatal distension of the stomach by compressing the duodenum, and so preventing the free passage of the aliment through that gut. As to remedies for pancreatic diseases or disorders, I do not know of any.

Diseases of the kidneys.—Diseases of the *kidneys*—and disorders of their function—and alterations in the fluid they secrete—require more consideration. And I proceed at once to the subject of their *inflammation;* to *nephritis:* and it will be practically convenient to take *nephralgia*, or pain of the kidney, into the account at the same time.

Nephritis and nephralgia.—Nephralgia is commonly, but not always, produced by the transit of a urinary calculus from the pelvis of the kidney, through the ureter, towards the bladder. This constitutes what is called, in common parlance, *a fit of the gravel.* The symptoms are these:—pain, sometimes dull, but more frequently very severe, in the loins, usually on one side, and descending often along the track of the ureter of the same side; numbness of the corresponding thigh; in the male, retraction, and perhaps pain, of the testicle; a frequent desire to make water, which is generally high coloured; nausea and vomiting.

If to these symptoms there be added pyrexia, we learn the important fact that inflammation is present: we have the symptoms of *acute nephritis.* The passage of gravel from the kidney sometimes does, and sometimes does not, provoke inflammation of the gland. Nephritis is very seldom idiopathic. It may sometimes arise under the influence of cold: more frequently it is excited by calculous matter lodged in the kidney: by a blow or fall upon the loins; by the internal administration of cantharides, or of turpentine. It is to the presence of fever that we look, to establish the inflammatory character of the renal affection.

Nephralgic pains require to be distinguished on the one hand from rheumatic, and on the other from colic pains. In lumbago there is pain in the back, and it may or may not be attended with fever; but the pain usually affects both sides, and is aggravated by such movements of the body as call the muscles of the loins into action, particularly by stooping. It originates, frequently, in some strain or effort, of which the patient is made painfully conscious at the time. It is seldom accompanied by any notable trouble of the urinary functions. When rheumatic pain extends from the back into the thigh, it mostly follows the course of the great sciatic nerve, and is felt down the outer part of the limb; whereas the pain that accompanies nephritis or nephralgia shoots rather along the track of the anterior crural nerve. Lastly, lumbar pain, depending upon rheumatism, is not attended with nausea and vomiting.

The pain of colic *is* often associated with sickness and retching; and it may occupy

those parts of the abdomen which correspond to the place of the ureters. The urinary functions are undisturbed; and this is a capital point of distinction. The numbness of the thigh, and drawing up of the testicle, are sufficiently characteristic, when they happen; but they are frequently altogether absent.

Some years ago I was sent for by an exceedingly intelligent surgeon, who had been one of the house-surgeons at the Middlesex Hospital. I found him in bed. He told me he had pain in the abdomen. It had begun in the morning in the situation of the right kidney, and soon extended round to the right side of the abdomen and the groin. Two days before, he had experienced a similar attack of pain in the renal region, stretching round into the hypogastrium. When I saw him he described the pain as lying more round the umbilicus than elsewhere; and he expressed a strong persuasion, from the feelings which attended it, that it would be removed by free action of the bowels. But he felt nausea; and had vomited some medicine which he had taken. He had no fever, no retraction of the testicle or numbness of the thigh, and the pain was not increased by pressure. Neither had there been any marked irritation of the bladder. He said, indeed, when I questioned him on that point, that he *thought* he had made water *rather* more frequently than usual the day before. I mention this case to show you the occasional obscurity of the symptoms. Here a well-instructed medical man believed that nephralgia, existing in his own person, was colic. To my judgment, however, it seemed most probable that a small calculus had been passing from his kidney towards and perhaps into his bladder. I may as well tell you the event of the case, which interested me a good deal; for it exhibits the train of symptoms that are apt to ensue after nephritic attacks: although in this instance they were but slightly pronounced. His bowels were well acted on by a purgative, and the next day he was free from pain, and apparently well.

Two days after this, he had more frequent calls to pass urine than were usual with him, and having done so on one occasion, he presently felt the want again, and then passed a little blood. The urine had been of a clear amber colour throughout. At the expiration of two or three days more he called upon me to say that after making water he had perceived in the vessel a small crystallized mass, which he took out, supposing it to be (what it very much resembled) a fragment of sugar candy. In fact he had been eating sugar candy, and thought some portions of it had fallen down between his waistcoat and shirt, and afterwards into the chamber-pot. He had the curiosity, he said (some misgiving he must have had too, for I had told him my own opinion of the nature of his attack), to put a small crystal from this fragment into his mouth; and as it neither tasted sweet nor dissolved, he suspected it might be a urinary concretion, and brought it to me. And sure enough it was so; a piece of very pure oxalate of lime, which he had been fortunate enough thus to get rid of. It was a quarter of an inch in length, and less than one-eighth of an inch broad, consisting of an aggregation of small crystals. It was exactly similar in appearance and colour to a piece of brown sugar candy of the same size. It would pass, longways, into a large crow-quill.

That it was oxalate of lime was proved in this manner. A little separate crystal was heated to redness on a piece of platinum foil, by means of a spirit lamp and blow-pipe. By these means the oxalic acid was destroyed, and quick-lime left. This residue, moistened, and pressed into a powder on a piece of turmeric paper, gave the characteristic brown colour.

You see, then, that a nephritic affection may be mistaken for an attack of colic. In reference to practice, it would indeed be a mistake of no great importance, since the remedies that are proper in the one case are generally proper, or not improper, in the other. If the pain be attended with fever, antiphlogistic measures are alike indicated in each of the two diseases.

The numbness of the thigh, and the drawing up of the testicle, are analogous phenomena to the pain which affects the shoulders in hepatic disorders. Irritation of one extremity of a nerve, situated internally, and belonging to an organ which is not endowed with a high degree of sensibility, causes sympathetic sensations in the sentient extremities of other branches of the same nerve, or of communicating nerves.

And this sympathetic affection of distant parts is sometimes attended (as I formerly observed) not merely with pain, but with some degree of inflammation also. The testicle occasionally swells, and becomes tender, during a nephritic attack. On the other hand,

as the nerves which communicate with those of the testicle or thigh may or may not be implicated in the renal disorder, so we see how it happens that these curious symptoms, so instructive when they do occur, may frequently be wanting; as they were in the example I just now detailed to you.

When the symptoms I specified in the outset are attended with fever, we conclude that we have to deal with nephritis: and when inflammation of the kidney, however produced, lasts for a certain period, without abatement, *suppuration* is to be dreaded. Such suppuration is marked, sometimes, by the supervention of rigors, by throbbing perhaps, and it may be by a remission of the pain: but I believe it may take place without throwing out any such signals. Nay, I think it probable that inflammation, confined to the parenchymatous substance of the kidney, may arise, and run through all its stages, without denoting its presence or progress by any noticeable local signs; and that the sharp and peculiar symptoms ascribed by authors to acute nephritis manifest themselves only when the investing membrane of the gland, or its pelvis and excretory tubes, are involved in the inflammatory process. However this may be, suppuration leads to ulceration, to the formation of renal fistulæ, to the establishment of a purulent discharge, and hectic fever; and finally, in most cases, to a fatal event; whether the inflammation was at first idiopathic, or dependent on a calculus.

I may illustrate these remarks, by stating the heads of a case which has occurred to me since this course of lectures began. I admitted Caroline Barnard, a married woman, forty years old, into the hospital, on the 18th of October. Among other things she complained of pain in the situation of the right kidney. She had been ill six weeks, and at the commencement of her illness her urine had been very turbid, as indeed it still was; and she had experienced much pain and difficulty in passing it, and after it had passed. From that time she had frequent nausea and retching, and occasional numbness of the right thigh. She had been losing flesh fast; and her pulse was frequent. There was some tenderness discoverable in the right renal region; and after a time a manifest fulness there, and hardness; and at length œdema of the integuments and *extreme* tenderness. She suffered also well-marked hectic fever, and had severe and repeated rigors. On the 4th of November, after a careful examination of the right loin, we satisfied ourselves of the presence of matter. I got Mr. Arnott, therefore, to see her, and to put a lancet into the abscess; and a large quantity of faint-smelling pus came out. She was greatly relieved by the operation; and a purulent discharge, mixed with shreds of cellular membrane, came away in abundance for some days: but in time the discharge ceased, the swelling subsided, and the opening healed. We began to hope that it had been merely an abscess in the *neighbourhood* of the kidney, irritating it and affecting its functions. But in three weeks after the abscess was punctured, the swelling was found to have recurred; and she again began to suffer much. The tumour was again opened, and pus of a more offensive character than before evacuated. In the early part of December she sunk.

We found the right kidney small, collapsed, and hollow; in some parts a mere flabby bag. On its posterious surface there was an opening, which formed a communication between the interior of the kidney and the abscess in the cellular tissue, which had pointed externally. The pelvis of the kidney was much dilated; and the substance of the gland destroyed to a considerable extent, by suppuration and ulceration. The ureter, where it left the pelvis of the kidney, was found to be impervious.

The other kidney was much enlarged; but of quite healthy and sound structure. That kind of compensation had occurred which I formerly mentioned as not unusual when, of double organs, one has been rendered incapable of its natural functions, and the other takes up its duty, and performs a two-fold amount of work. The organ of which the function is thus increased, becomes hypertrophied. This woman did not die because there was not urine enough secreted; but she sunk under the wasting purulent drain, the irritation and pain she suffered, and the protracted hectic fever. In this instance the inflammation and suppuration occurred independently of the formation of calculous matter.

Sometimes the pus finds its way out of the body through the natural passages, and appears in the urine. This woman's urine was thought, by some of the pupils, to contain pus. It was quite thick, and of a yellowish colour. But heat rendered it transparent. You

must not judge by a cursory look at the water. The effect of heat proved that the yellow material was not pus; the impervious condition of the ureter showed afterwards that it could not have been.

When calculi exist in the kidney, they often betray their presence there, by causing *bloody* urine. But bloody urine may proceed from various causes; and in conformity with my usual custom, I shall by and by offer you some general remarks on hæmaturia, as one of the hæmorrhages.

I showed you at our last meeting, that gall-stones might inhabit the gall-bladder in considerable numbers, and be quite harmless, unless they attempted to escape from their prison, through the very narrow channel of egress from it; and I intimated that the same observation was often applicable to urinary concretions. Renal calculi do indeed, in many cases, produce abiding uneasiness, or frequently recurring pain, in the situation of the affected kidney, bloody urine, and gastric disturbance, especially when the concretions are shaken or displaced by sudden jolts or jarring movements of the body; or when the system is deranged by intemperate habits. But in many other instances these calculi cause no pain or annoyance, so long as they remain in the kidney: although they inflict horrible suffering, in general, while, for the first time, they are forcing their way along the narrow ureter. A concretion cannot be formed in a moment; yet the attack of pain often comes on in a moment, without any previous warning. After a while it remits, perhaps as suddenly; the calculus having passed (it may be presumed) from the ureter into the bladder; and then indications, more or less palpable, usually begin to declare themselves of its presence in that reservoir. Moreover, it is not uncommon to find calculi in the kidney after death, of the existence of which there had been no symptom manifested during life.

The *treatment* of nephritis—or of the nephralgia calculosa, when accompanied by fever, or occurring in young, strong, and plethoric persons, is just such as would be proper in cases of severe colic, or enteritis: and therefore it is that any mistake between these disorders at the outset is not of so much practical consequence. The objects of treatment are, to arrest the inflammatory process; to quiet existing irritation; and to obviate any fresh causes of irritation. Venesection, therefore, in proportion to the strength of the patient and the violence of the symptoms, will sometimes be proper: and it will always be advisable to take away blood freely from the neighbourhood of the suffering part by cupping. Warm fomentations; the warm-bath; the injection of warm water into the bowel; these are expedients of which practical men acknowledge the value. The warm enemata not only clear out from the large intestines any irritating matters they might contain, but, from the proximity of the colon to the kidney, they perhaps have the effect of an internal fomentation. It is desirable also to get the bowels well acted upon by purgative medicines as soon as possible: the relief that follows free alvine discharges is often very marked. There is sometimes a difficulty, from the irritability of the stomach, in administering purgatives by the mouth. Calomel, however, will often be retained, when other substances are rejected. It is generally considered of importance to give those purgatives only which are not likely, after being absorbed into the blood, to irritate the urinary passages. On this account the *saline* purgatives are to be avoided. Nothing is so good as castor oil, if the stomach will bear it; or infusion of senna, with manna, may be used; or, if the stomach be very queasy, *pills*, composed of cathartic extract, and calomel.

When there is no fever, *i. e.*, when the case is one of nephralgia, and a calculus is passing, after the intestinal canal has been cleared by a purgative, it will be necessary to give opium in full doses to allay the pain: and it may either be administered in the form of pill through the stomach; or introduced into the rectum.

Gravel.—When a person suffers what is called a fit of the gravel, the pain, I say, is at length very suddenly relieved, in general, in consequence of the calculus having emerged from the ureter and entered the bladder. We judge that this has taken place, first, by the cessation of the pain; and secondly, by the supervention, sooner or later, of the symptoms indicative of stone in the bladder: viz., a more than usually frequent inclination to make water; pain, referred to the extremity of the urethra, especially just after passing urine;

and stoppages and renewals of the stream of water while the patient is endeavouring to void it.

The time which a calculus takes in travelling from the kidney to the bladder varies a good deal. The painful journey may be over in a few hours; or it may last two or three days. More rarely the symptoms continue, with irregular intervals of comparative quiet, for weeks. And sometimes, notwithstanding the peculiar pain, which amounts to torment, all morbid symptoms cease, and yet no calculus has passed, apparently, into the bladder: none, *i. e.*, of the symptoms of stone ensue; no calculus is voided by the urethra; and none found in the bladder when the patient at length dies.

What is the explanation of these circumstances? Why, as calculi have been discovered in such cases in the *kidney*, it has been supposed that a concretion may get into the very beginning of the ureter, where it is a little larger than elsewhere, and give rise to the peculiar symptoms, yet never pass fairly into that narrow tube; but at length fall back again into the pelvis of the kidney: when the symptoms cease.

But the same symptoms undoubtedly occur, occasionally, when there is no calculus at all. Sir B. Brodie has referred to this form of complaint. In people who live intemperate and luxurious lives, pain is apt to seize upon one renal region, and to extend round and downwards into the groin; and these symptoms will be followed by frequent, difficult, and painful micturition, the urine being unusually acid, high-coloured, and sometimes turbid. The whole irritation appears to be produced by this unhealthy urine: at least the complaint vanishes after cupping the loins, purging, the warm-bath, and two or three full doses of colchicum given at short intervals.

Sometimes the little stone becomes immovably wedged in the canal of the ureter. If it completely shuts the tube, the urine accumulates behind it; and that portion of the ureter dilates. The obstruction usually proves fatal, by its influence upon the functions of the kidney, and thereby upon the whole economy. But if the urine finds a passage by the side of the impacted concretion, this danger is averted, or postponed.

When we have reason to believe, from the nature and course of the symptoms, that a calculus has come down from the kidney, and lodged in the bladder, then it becomes an object of deep interest to the practitioner, and of fearful importance to the patient, to try all means to bring about its expulsion before it grows too large to be voided. For grow it almost surely will, by the continual accretion of earthy matter upon its surface, if it remains long in the bladder. We know that it *may*, at first, be voided, provided the urethra be in a healthy and natural state: that whatever has passed through the ureter, may pass through the urethra also.

The objects to be kept in view are these: first, to procure a plentiful secretion of bland urine, wherewith the bladder may become filled; secondly, by lulling the sensibility of the parts concerned, to prevent or lessen that spasmodic effort of the sphincter of the bladder, which the presence of the calculus is apt to provoke; and, thirdly, to ascertain that the channel of the urethra is open and unimpeded.

To effect the first of these purposes, the patient should be instructed to drink freely of diluent liquors; such as barley-water, or linseed-tea, in which may be mixed a small quantity of the sweet spirits of nitre. To fulfil the second, he should take a full dose of opium at bed-time. By these means the pain and irritation which may have been produced by the calculus, will be soothed; and the bladder will gradually fill. He should then make water, having first placed himself in such a position that the outlet of the bladder shall be at the lowest part of that receptacle. He may stand up, and lean forwards; or it may be well to make water while kneeling, in a warm bath. If these expedients are not presently successful, the urethra may be cautiously expanded, and habituated to the contact of a solid body, by the daily introduction of a full-sized bougie. Sometimes the calculus will follow the bougie, as it is withdrawn, through the urethra. In this way the patient will have a fair chance of getting rid of the stone. In this way a very near friend of my own, a physician now practising in this town, did expel a formidable, though not very large, piece of rough oxalate of lime several weeks after its entrance into the bladder. Out it came at last, with a smart clink, which was music to his ear, against the chamber-pot. A gentleman was not long since sent up to me from Kent, by a former pupil of this College, with the following history. About a month before, he had been suddenly attacked with

acute pain in the loins, extending forwards into the left flank and pelvis, with nausea and vomiting. For nearly ten days these symptoms continued to occur at intervals; then they ceased; and then he began to be troubled by a frequent and very urgent inclination to make water, and by pain after voiding it, just above the arch of the pubes. I gave him directions, in accordance with the plan just now mentioned; and wished him to allow some surgeon to explore the contents of his bladder. To this he would not, as yet, he said, consent. I saw him on the 2d of August. He returned into Kent the next day. On the 5th, while taking a walk, he was seized with a most imperative desire to make water, but found that he could part with none. Concluding that a calculus had entered, and stopped up the urethra, he was proceeding homewards, but was soon constrained again to try to empty his bladder: and then he had the satisfaction of feeling, and seeing, a stone fly out with great force: but, as he had turned towards a hedge, he could not find it. From that moment he was quite easy.

When a calculus of a certain size has once traversed the tubes that lead respectively to and from the bladder, others sometimes follow it with more ease. I show you here a large concretion which was passed, or pissed if you will, by a patient of mine without his knowing it. He is subject to epilepsy, which is probably eccentric, and excited by renal disease. He is closely and anxiously watched by his wife. One day last year she noticed that the urine he had just voided was slightly tinged with blood: and she then found this oblong stone, which is composed of lithic acid, in the vessel.

If the renal calculus, after it has reached the bladder, cannot be got rid of by the expedients I have been recommending, the question arises, whether medicine can do any further good, or whether the patient is to be delivered over to the surgeon.

Most of these small concretions admit of being mechanically crushed into smaller fragments, which are then readily washed out by the stream of urine. Larger stones are extracted entire, through incisions of the bladder. Yet there are many cases in which, for various reasons, surgery declines to attempt the removal of vesical calculi. Medicine still offers to these unfortunate patients the means of mitigating, at least, their sufferings. But it often can do more than this. It is very important for you to know that judicious medical treatment may retard or prevent, and that injudicious medical treatment may promote and hasten the enlargement of such calculi. Let us briefly consider the principles by which our judgment and our practice, in this serious matter, must be guided.

I have described a fit of the *gravel*. We say that a patient has the gravel when he passes concrete matter with his urine, whether in the form of powder, of grit or sand, or of more massive calculi. We do not apply that term to the cases in which the urine is clear when recently voided, and warm; but throws down an earthy sediment as it cools: which sediment redissolves if the urine be again artificially heated. Now besides the different forms which the gravel assumes, of powder, sand, and little stones, there are (as you may have guessed from certain terms that I have been obliged to employ) several *kinds* of gravel; differing, I mean, in their chemical composition. The main signs—the pain, the sickness, the affection of the testicle, the subsequent bladder symptoms—are much the same, whatever be the nature of the solid matter that descends from the kidney, and lingers in the bladder. But other circumstances differ widely. The qualities of the water previously to the formation, and to the discharge, of the sabulous matter; the state of the system at large. And it is quite impossible to treat cases of calculus in the kidney, or of stone in the bladder, with propriety, or safety, without constant reference to the condition of the urine. The morbid states of that secretion are of the greatest interest. I cannot undertake to enter upon the subject in much detail. Yet some outline of it I must attempt, especially where it touches upon points of practice.

Diseased states of the urine.—You know, probably, that the urine voided by a person in health always exhibits *acid properties*, always turns litmus paper red. Not that healthy urine contains a free acid; but only (according to Dr. Prout, who is the great authority in these things), that certain of the alkaline and earthy bases are not exactly neutralized, but exist in the state of supersalts. The pure lithic acid is nearly insoluble; but the lithate of ammonia is very readily soluble: and it is this which reddens the vegetable blues. Now, whether out of the body, or within it, the lithate of ammonia will, of course, be decomposed, if any acid be present in the urine, for which ammonia has a stronger affinity than

it has for the lithic acid; and the latter will be thrown down, in the form of a red sand: little crystals, in point of fact, they are, very much like, in shape, size, and colour, to particles of Cayenne pepper. I show you some collected by one of my out-patients at the hospital. He must have passed a peck of it while under my observation: and I am sorry (having lately lost sight of him) that I did not procure a large quantity for the museum.

Lithic diathesis.—Now this lithic acid, or red sand, or gravel, is liable to form in the kidney, if not in the bladder, and to concrete into calculi; and the calculi once formed, or, indeed, any solid substance, will constitute a nucleus, upon and around which a further and repeated incrustation of a similar nature is almost sure to take place. You will at once perceive the importance of doing nothing to aggravate this disposition to deposit lithic acid; but of trying to prevent, or stop it. If there be symptoms of stone in the kidney, or in the bladder, and we have reason to believe that it consists of lithic acid, there are medicines which would tend to render matters worse, and there are others of which the effect would be to correct the lithic acid *diathesis* as it is called. But how are we to know whether the presumed calculus be of that kind or not? or, rather, how are we to know that the lithic diathesis exists? Why, we learn that it exists by noticing the habitual qualities of the urine, and the habitual state of the patient's general health.

In the urine of persons who have the lithic diathesis, you will find that there are frequent deposits, after it has become cool, of reddish sediments, looking like brickdust, and, therefore, called *lateritious*. These sediments consist chiefly of the lithate of ammonia, tinged with certain colouring matters in the urine. Sometimes pure lithic acid appears, in the shape of fine sand, or in larger crystals. The urine itself is bright, of a dark coppery colour, like brown sherry. It is more acid than the urine of health, and gives to litmus paper a deeper shade of red. It is apt, too, to fall below the average quantity.

The presence of this diathesis is likewise accompanied, and so far denoted, by a tendency to feverish and inflammatory complaints. The patients are troubled with acidity of stomach and heartburn; many of them are subject to gout or rheumatism. They are mostly also indolent and luxurious, or intemperate in their mode of life. Adults are peculiarly obnoxious to this condition of the system after the age of forty. But children, up to the period of puberty, are very liable to have lithic acid gravel.

Whenever a paroxysm of nephritic pain befalls a person whose time of life, whose habits, the characters of whose health, and the habitual qualities of whose urine, are such as I have been describing, you may conclude that the concretion which has occasioned the symptoms is of the lithic acid kind: and you may expect that such attacks will recur; for it is observed of these lithic acid renal calculi, that they are generally numerous in the same individual. I speak of the *habitual*—or of the *frequently recurring*—qualities of the urine: for a deposit of lithic acid gravel, or of superabundant lithates, may occur to the healthiest individual, under accidental and transient disturbing causes. Many persons will tell you that their water becomes turbid with red sand whenever they have a cold. Febrile and inflammatory ailments will produce the sediment: even too full a meal: or exercise taken immediately after a full meal. In all such cases it seems probable that the customary evolution of lactic acid through the skin is somehow prevented: in consequence either of a check given to the perspiration, or of imperfect assimilation of the food. The lactic acid, thus diverted from its natural emunctory—or some other acid generated within the system—is determined to the urine, and precipitates the lithic.

Treatment.—Now the formation of lithic acid may be controlled by the exhibition of alkaline remedies. You will find that free livers use alkalies to neutralize the excess of acid which results from their intemperate habits; the carbonates of soda, or of potass. They do this, without any reference to the appearance of their urine, to prevent or appease the uneasy feelings produced by a debauch. But it is of importance to be aware that one of these alkalies is preferable, for the purpose of obviating the lithic acid diathesis, to the other. Soda will sometimes combine with the lithic acid, and form an insoluble salt, as hard, and as pernicious, when deposited around a nucleus, as the lithic acid itself. With potass there is no such danger. If it should combine with the lithic acid, the resulting salt is perfectly soluble, and will pass away dissolved, in the urine. Magnesia is also a good medicine in such cases; but it has this disadvantage, as I showed you indeed before, that it is apt, when taken habitually, to cause *intestinal* concretions: and these

may be as dangerous as the urinary ones. One of the best modes of giving the bicarbonate of potass is in the common saline draught. The stomach has the power, apparently, of destroying the *vegetable* acids: and the remedial properties of the bicarbonate become thus equally certain with those of the pure alkali, while it is much less likely to derange or disagree with the stomach. Of course the mode of living ought to be changed when the lithic diathesis prevails; the patients should dine moderately and plainly, eating of one dish, and avoiding acids, and all articles of diet likely to generate acid in the stomach; saccharine substances, therefore, and fermented liquors. But, as I mentioned in a former lecture, they will not, if they can help it, give up their accustomed indulgences: and they attempt, and we attempt, but the attempt is often made in vain, to *remedy* disorders, which might with ease and certainty have been *prevented*.

You must take care not to give these alkaline remedies too long: nor in too great quantity. You must not push them to such an extent as entirely to destroy the acidity of the urine: for if you do, your patient is exposed to the same danger as before, but from an opposite cause. A *white* sand or gravel will be apt to form in the alkaline or neutral urine: and this will collect itself, by the force of aggregation, around any existing calculus, or foreign substance. The white deposits consist mainly of the triple phosphate of ammonia and magnesia; and if you examine collections of urinary calculi, you will find that they are sometimes made up of concentric layers; and one layer may be composed of lithic acid, and the next of the triple phosphates; and so on, as the condition of the urine has alternated. You must test the urine therefore, and see that it still reddens litmus, though perhaps faintly. The saline draught has always a tendency to make the urine alkaline; and thus it is, probably, that it proves of use in febrile disorders: but it is an absolute poison to those whose urine is already alkaline. Colchicum has a similar tendency to diminish the acid reaction of the urine. So has mercury. And I may tell you—speaking generally of morbid states of the urine—that it is much more easy to correct too great acidity than to rectify the opposite condition. We can almost always make acid urine neutral or alkaline: but to render alkaline urine acid is often beyond our power.

It is scarcely less important to attend to the functions of the *skin*, in persons having the lithic acid diathesis, than to the functions of the stomach. In cold weather, warm clothing must be enjoined; and the avoidance, in all weathers, of such exposure to cold as might suppress or materially lessen the amount of healthy perspiration.

Phosphatic diathesis.—You will have gathered, from what I have already said, that there is a morbid condition of the system, the opposite of that in which the lithic diathesis prevails. The phosphatic diathesis, namely; in which there is a readiness to deposit *white gravel;* composed of minute shining crystals of a triple salt, the phosphate of ammonia and magnesia. The way in which this is formed, according to Dr. Prout, is as follows. Healthy urine contains the phosphate of magnesia, which is very soluble; and, therefore, is dissolved in that fluid. But, under certain circumstances, the urea of the urine becomes decomposed in the kidneys, and ammonia is extricated, which combines with the phosphate of magnesia, and forms an *insoluble* triple salt. Sometimes with the triple phosphate just mentioned, there is also an admixture of phosphate of lime.

It is a fact of great practical importance, that the tendency to the formation of the phosphates goes along with a debilitated condition of the system. Persons who have been rendered weak and feeble by overmuch toil, by mental anxiety, by insufficient nourishment, are very apt to pass water that is alkalescent or but faintly acid, and to exhibit the tokens, in their urine, of the phosphatic diathesis. They are, for the most part, cachectic, sallow, languid, spiritless, exhausted, the urine itself is pale, copious, slightly turbid or opaline, of a low specific gravity, and it does not smell like healthy urine: sometimes it has somewhat the faint odour of weak broth. It is occasionally alkalescent when voided; never more than slightly acid. As the urine cools, the white sand is thrown down; and in many cases a sort of film is formed upon the surface of the water, exhibiting, as you see it in different lights, all the colours of the rainbow: an iridescent pellicle. This has been found to consist of the triple phosphate. If you skim the pellicle off, by placing a bit of paper under it, and then suffer the paper to dry, you may distinctly see the little crystals. Urine of this kind speedily grows putrid and highly offensive. Sometimes it has a strong ammoniacal smell. You may estimate the intensity of the phosphatic disposition

by the rapidity with which the urine becomes alkalescent. Occasionally the salt is so abundant that it is thrown down while the urine is still in the bladder; and the last portions of the issuing stream look milky.

Any thing which tends further to depress the powers of the system will aggravate the phosphatic diathesis. When you find that your patient passes urine such as I have been last describing, which does not redden litmus paper, but on the contrary turns litmus paper that has been reddened by a weak acid, blue again, or even in some cases is alkaline enough to make turmeric paper brown—in such cases you must cautiously abstain from all remedies that are calculated to lower the vital powers; from saline draughts, and alkalies of all kinds; from mercury and colchicum; from bleeding; and even from active purgation; or you will add to the patient's dangerous weakness; and promote the more abundant deposit of the alkaline phosphates. But you may do more than abstain from what is hurtful: you may counteract the phosphatic tendency by a generous diet and by the exhibition of tonic medicines; bark, wine, and acids; the muriatic acid, or the nitric, may be given in such cases with vast advantage sometimes. Opium is also a remedy to be employed in this form of disease. No single drug probably has so much power in rendering alkaline urine acid, as opium. And it is indicated for other reasons; it composes the nervous anxiety to which these patients are mostly a prey. Mental relaxation—freedom from care—the relinquishment of all exhausting habits and pursuits—these too are points of vast importance, whenever they are attainable.

I should state that the tendency to deposit the mixed phosphates, though sometimes idiopathic, is much more often consequent upon local disease in some part of the urinary organs, epecially in the bladder and prostate gland. It is also a frequent result of certain injuries of the back.

Oxalic diathesis.—There is yet another diathesis sufficiently common and important to claim your best attention. I mean the *oxalic:* in which there is a tendency to the formation, in the kidney, of the oxalate of lime, or *mulberry* calculus; an epithet derived from the occasional resemblance of the concretion to that fruit, in respect to colour and inequality of surface. This diathesis is not so obvious as the other two, but it is no less real.

The urine differs much in its sensible qualities from that of both the preceding varieties. Unlike the urine of the phosphatic diathesis, it is bright and clear; unlike that of the lithic, it is remarkably free from sediment. The mulberry calculus is solitary also; or recurs at long intervals; and the diathesis prevails chiefly during the prime of life. In both these particulars the contrast with the lithic diathesis is striking.

The persons who manifest this disposition are usually dyspeptic; sometimes very much so; sometimes very slightly. They are uneasy during the assimilation of their meals; suffer flatulence when the stomach is empty; prefer vegetable diet to animal; are fond of sweets, especially of sugar. They are liable to boils and carbuncles, and to scaly cutaneous eruptions. According to their original temperament, they are nervous and irritable, or dejected and desponding in mind. A nephritic attack relieves them from all this discomfort for years perhaps. When the oxalic diathesis is strongly marked, the skin, Dr. Prout says, "is apt to assume an unnatural appearance difficult to describe, but the colour of which may be said to vary from dull greenish yellow in the sanguine, to dark olive or livid in the melancholic temperament."

The formation of the oxalate of lime within the body depends, according to Dr. Prout, either upon the non-assimilation of oxalic acid taken with the food, or upon the mal-assimilation of saccharine aliments. Hence, as a general rule, both curative and prophylactic, *sugar* and other saccharine substances should be rigidly excluded from the diet of these patients. They should avoid, also, all kinds of fermented liquor. The young stalks of the *rhubarb*-plant, which of late years have come into such general use in this country for tarts in the spring; and *sorrel*, of which our neighbours, the French, consume a good deal in salads, and in other ways; both contain oxalic acid; and *hard water* contains lime. Dyspeptic persons who drink such water, and eat such articles of food, and are thus daily introducing, without suspecting it, the constituent ingredients of the mulberry calculus, are very likely indeed to incur the pain, and the exceeding peril, of a renal concretion of that

kind. You must see, therefore, the great importance of detecting the oxalic diathesis; and of forbidding, to those who have it, all such viands as contain the oxalic acid, and of recommending them to use pure water, even distilled water, for drinking. Animal food, and the stronger farinaceous matters, are best for them.

Dr. Golding Bird refers the oxalate of lime to a different source: maintaining that it results from a re-arrangement of elements of urea, whereby oxalate of ammonia is formed in the first instance; and afterwards oxalate of lime, by the decomposition of the calcareous salts natural to the urine.

Agreeing with Dr. Prout that the mulberry *calculus* is not of very frequent occurrence, —Dr. Bird nevertheless finds that small crystals of the oxalate of lime are extremely common: although from their transparency, and from their having nearly the same specific gravity with the urine in which they exist, they do not disclose themselves to the naked eye, nor sink down in manifest deposit. They are made plainly visible by the microscope.

The same writer states that the persons whose urine is thus charged with crystals of oxalate of lime are, for the most part, highly sensitive and irritable, hypochondriacally apprehensive of impending evil, full of gloomy fears concerning their bodily and mental powers, dyspeptic, weak, and usually emaciated.

Treatment.—With respect to direct remedies for this diathesis, Dr. Prout tells us that he has seen more benefit derived from the mineral acids, alone or combined with tonics, than from any other. But the effects of these acids must be watched: and when they begin to produce a deposit of the lithate of ammonia, or of lithic acid, their use must be suspended. He recommends to patients who happen to be at a distance, the muriatic, or nitro-muriatic acid, till the lithate of ammonia, or lithic acid, begins to appear in the urine; or for a *month*. "By adopting," he says, "such a course of acids three or four times in the year, and by carefully-regulated diet, I have seen the diathesis gradually subdued, and at length removed altogether." Dr. Bird also testifies to the efficacy of similar measures.

These observations will serve, I hope, in some degree, as landmarks, to guide your treatment of patients labouring under renal or vesical calculi, or presenting symptoms such as warrant the apprehension that such fearful disorders may occur. It is impossible for me to do full justice to this interesting subject in these lectures; and I must refer you, for more minute information respecting it, to Dr. Prout's invaluable volume; to Sir Benjamin Brodie's most instructive book on the *Diseases of the Urinary Organs;* and to a short but excellent series of Lectures, published in the *Medical Gazette*, by Dr. Golding Bird, *On the Physical and Pathological Characters of Urinary Deposits.*

LECTURE LXXVII.

Suppression of Urine. Diabetes: Qualities of the Urine; Symptoms; Anatomical Appearances; General Pathology of the Disease; Treatment Diuresis.

Systematic writers have adopted the term *Ischuria* to express that condition in which no urine is voided. It includes, therefore, those cases in which no urine is secreted; and those in which, although secreted, it is not discharged from the body. Now these two conditions are exceedingly different from each other in most respects; and I shall prefer making use of the two plain English names, *suppression* of urine, and *retention* of urine. Even these terms are sometimes confounded with each other. In *suppression*, the secretion is suspended: in *retention* it may be as active as ever. Retention of urine is a surgical case; involving points of great practical interest. Suppression belongs to the physician: and the technical term for it is Ischuria *renalis*. It is sometimes spoken of as

paralysis of the kidney; a phrase to which I object, because I think palsy is a word which ought to be restricted to a loss of power over the muscular fibre.

This affection usually occurs in persons who are advanced in life, and inclined to corpulency. Why it should be so I cannot tell you, but such is the fact, as stated by most observers. Sir Henry Halford has related one of five instances of this disease that he had met with in the course of seven-and-twenty years. He says it was an exact copy of all the others that had fallen under his notice: and as his account of the general course of the symptoms coincides with the statements of other writers, I may give you his narrative, in lieu of a formal description.

"A very corpulent robust farmer, of about 55 years of age, was seized with a rigor, which induced him to send for his apothecary. He had not made water, it appeared, for 24 hours. But there was no pain, no sense of weight in the loins, no distension in any part of the abdomen:—and therefore no alarm was taken till the following morning, when it was thought proper to ascertain whether there was any water in the bladder, by the introduction of the catheter: and none was found. I was then called (says Sir Henry), and another inquiry was made, some few hours afterwards, by one of the most experienced surgeons in London, whether the bladder contained any urine or not: when it appeared clearly that there was none. The patient sat up in bed, and conversed as usual, complaining of some nausea; but of nothing material in his own view: and I remember that his friends expressed their surprise that so much importance should be attached to so little apparent illness. The patient's pulse was somewhat slower than usual; and sometimes he was heavy and oppressed."

"I ventured to state (continues the author) that if we should not succeed in making the kidneys act, the patient would soon become comatose, and would probably die the following night: for this was the course of the malady in every other instance that I had seen. It happened so: he died in thirty hours after this, in a state of stupefaction."

This is the curious and important point in the history of such cases. If no urine be separated from the blood, coma soon supervenes, and death. It is believed that these consequences result from the detention of *urea* in the system. Urea is a mere excrement, which, in health, is removed from the blood by the kidneys, as fast as it enters that fluid. When it is not so carried off, it accumulates in the blood, circulates with it to every part of the body, and acts as a poison, especially upon the brain. This is one of several cases, showing that the carrying fluid of the body may become the vehicle of disease and death, if it be not duly purged of deleterious matters which pertain to the unceasing processes of organic life. If *carbonic acid* be not extricated by the lungs, the animal functions are as certainly and almost as speedily extinguished by that gas, as the flame of a taper might be. And we have recently seen that when the outlet from the liver is shut up, when the blood is not purified from the excrementitious *bile*, the powers of animal life are weakened, and sometimes utterly and rapidly destroyed.

Suppression of urine, for a considerable time, is not, however, necessarily or universally fatal. Patients labouring under the epidemic cholera would secrete not a drop of water for some days; and yet recover. It was remarkable how entirely free such patients were from any approach towards coma. Was the urea here drained off from the blood in the enormous and unnatural flux from the stomach and bowels? I think it probably was: but I do not know that any chemical search was ever made for that substance in the fluids so effused. There are, however, some very singular instances on record of persons who have passed days and even weeks without secreting urine; and without showing any other indication of impaired health. What degree of credit such narratives deserve I do not know; but assuming that there was neither fraud nor mistake, it may be suspected that either the natural secretion was compensated by some vicarious or supplemental discharge; or that a *small* quantity of urine was actually separated by the kidneys. "If any water, however small the quantity (remarks Sir Henry Halford), had been made in these cases, I should have thought it possible that the patient might have recovered: for it has often surprised me to observe how small has been the measure of that excrementitious fluid which the frame has sometimes thrown off, and yet preserved itself harmless. But the cessation of the excretion *altogether* is universally a fatal symptom *in my experience*, being followed by oppression on the brain." The same eminent physician states that in

three of his five cases there was observed a remarkably strong urinous smell, in the perspiration, for twenty-four hours before death. This I believe is of common occurrence in such cases. Other patients have vomited, or passed by the bowels, watery matters possessing some of the sensible qualities of urine: and a urinous fluid is said to have been found in the ventricles of the brain in some of the fatal examples.

I have spoken of suppression of urine as a malady, though it probably is never any thing more than a symptom. Yet it is one of those symptoms which from our uncertainty respecting their origin and determining cause, we are obliged to treat, and to study, as if they were substantive diseases. In the only well marked instance that I have seen of suppression of urine coming on in an apparently healthy person, some blood had appeared in the urine for a day or two before the secretion was totally suspended; and the kidneys were found gorged with blood. Extreme congestion, or inflammation, of the substance of the gland, is probably at the bottom of many of these cases. The same train of symptoms supervene not unfrequently upon organic renal disease. They happen, too, when the ureters become impervious, from disease, or from impacted gravel. In this condition urine continues to be secreted, for a time at least, and distends the ureter behind the seat of the obstruction. The apoplectic state which ensues may arise from a reabsorption of the secreted fluid; or, in consequence of the obstacle, the secretion itself, after going on to a certain point, may stop, and then the case becomes a case of suppression.

Respecting the treatment of this most formidable condition, I can say but little. Cupping upon the loins, venesection if warranted by the state of the pulse, the hot-bath, sudorific medicines, purgatives, and large warm enemata, seem to me the kind of remedies indicated. To endeavour to force the secretion of urine by strong stimulating diuretics, would strike one, *à priori*, as being hazardous. Yet this practice has its advocates; and should experience declare in its favour, theoretic objections ought to be disregarded. If benefit is so to be obtained, certainly the best drug for our purpose will be cantharides. Dr. Elliotson refers to some examples of its success in the hands of Sir Astley Cooper; and afterwards of another practitioner who took the hint from Sir Astley. He suggests that as the tincture of cantharides is a very uncertain preparation, the remedy should be given in the solid form, a grain at a time, and that a large blister should be laid upon the loins. Beyond these hints I am unable to give you any assistance towards the management of this obscure but serious complaint.

Diabetes.—The opposite condition of the kidney, in respect to its peculiar function—that, I mean, in which its secretion is largely and morbidly *augmented*—is scarcely less fatal than the total suppression we have just been considering, but it is not so rapidly fatal. When the amount of urine secreted and passed is permanently too great, when it is constantly running off, as it were, from the system, the patient is commonly said to have *diabetes:* from διαβαινω, to pass through.

But it is not every case of an excessive flow of urine that deserves to be called diabetes. Great quantities of aqueous urine are passed by hysterical and nervous patients. We all make more water in cold weather than in warm; the functions of the skin and of the kidney compensating each the occasional defect of the other. Certain drugs and articles of diet are also well known to cause a temporary excess in the amount of urine secreted.

Qualities of the urine.—In fact, although the quantity of urine voided is the most obvious and striking symptom of diabetes, its definite and characteristic symptom is a most remarkable change in the *quality* of that liquid: in its becoming loaded with sugar. You will find, indeed, two species of diabetes mentioned by many authors, the diabetes *insipidus*, and the diabetes *mellitus*. The former term ought, in my opinion, to be abolished. If it refers merely to an unnatural abundance of urine, not otherwise differing in its composition from healthy urine than in containing a large proportion of water—by calling such a state diabetes, we link together in the same genus two essentially different conditions. In true diabetes the urine is never without sugar. The quantity may indeed be small; and it may not be sensible to that coarse test, the *taste:* but modern observers

almost all agree in rejecting any species of diabetes, in which the urine is not at all saccharine.

The sensible qualities of diabetic urine differ strikingly, in many particulars, from those of the urine of health. Its chemical qualities differ strikingly too, as I have already told you; but it is in one particular only. Fortunately no extraordinary skill is required to recognize the morbid secretion.

Diabetic urine is light coloured, and transparent; of a pale straw, or greenish tint. Its odour is peculiar. According to Dr. Prout the scent somewhat resembles that of sweet hay, or that of milk, but to my nose it is more like the faint smell of certain apples, or rather of an apple chamber. Its taste is, more or less decidedly, sweet. Notwithstanding its limpid and aqueous appearance, diabetic urine is remarkably heavy.

It was long believed that the quantity of urea in diabetic urine was reduced much below the natural standard; and that the sugar was somehow formed at the expense of the urea. Dr. Prout, in his earlier researches, always detected a little, and but a little, of this peculiar principle. Later observations have shown, however, that the urea is not so scanty; nay, that it is generally as abundant as in the urine of health, and sometimes even more so. The presence of the sugar conceals the urea; interferes with the action of the ordinary tests of that substance. By certain modes of procedure, which I need not stop to describe, the urea may readily be discovered: and it is often found, I say, to be rather excessive than deficient. The usual saline matters belonging to healthy urine are present also in that of diabetic persons; and in the same *relative* proportions; but, as might be expected, their *absolute* amount, in a given quantity of the liquid, is very much diminished. In short, the only essential deviation from the standard chemical constitution of the urine is, that it holds in solution a quantity of sugar. This explains its peculiar odour, its sweetness, and perhaps its excessive quantity. It accounts also for another very characteristic property of diabetic urine; I mean its high specific gravity. In general, you know, the specific gravity of the urine is inversely proportional to the quantity secreted in a given time; the more copious and dilute it is, the lighter it is. But in diabetes, so strong is the saccharine impregnation that the specific gravity more than keeps pace with the increased quantity of the liquid secreted. The specific gravity of diabetic urine is always much higher than that of healthy urine.

The *quantity* of urine secreted and voided is sometimes enormous: far more than could be supplied by the quantity of fluid taken as drink, although that, as I shall presently explain, is excessive too. A healthy person passes from one to three or four pints of urine in the twenty-four hours. The quantity, as you well know, is liable to considerable variation: perhaps the average may safely be laid at about forty ounces. But patients in diabetes will void forty *pints* in the same time. I have myself seen twenty-six; thirteen or fourteen are not uncommon; and cases are recorded by writers of credit and veracity, in which seventy pints were passed daily. Nay, one Italian author declares that two hundred pints have been discharged in that time.

The saccharine matter thus held in solution may be obtained in its solid form by evaporating the urine. I have seen large flat cakes of beautifully crystallized diabetic sugar. It differs somewhat from common sugar, the produce of the sugar-cane; and approaches more nearly to the sugar of grapes. By rapid evaporation of the water a thick syrup is procured resembling treacle: but Dr. MacIntyre, who has presented to our hospital-museum some very perfect specimens of this sugar, prepared by Mr. Blandford, informs me that to get it well crystallized, the evaporation in a steam bath should be stopped while the urine is still of thin consistence. It may be quickly reduced to one half, perhaps, of its original quantity. Then it should be set aside, in shallow plates; and in the course of ten days or a fortnight the sugar will be deposited in a regularly crystalline form.

The sugar is sometimes so abundant, that it undergoes a rude crystallization as the urine dries, whenever it happens to fall. A girl who was in St. Bartholomew's Hospital, while I was a student there, observed that if her water was accidentally sprinkled upon her black stuff shoes, every drop left a white powdery spot behind it. So also an aged patient under Dr. MacIntyre's care expressed to him her alarm at finding that her black worsted stockings were covered with a white dust, from the same cause. A man, recently under my charge in the Hospital, complained that two pair of his black cloth trousers had

been spoiled in a similar manner. I remember hearing from a diabetic patient in the Edinburgh Infirmary, that his attention was first drawn to his urine by the number of flies and wasps which its sweetness attracted to the chamber-pot.

This daily produce of sugar from the laboratory of the human body, is surely a very singular and surprising phenomenon. Sugar is not a constituent of healthy urine. Dr. Prout (who is more consulted on this subject than any one else, and whose experience in respect to it is commensurably great) says that he has never known saccharine matter to occur in the urine of any other animal than man. I once had a coach-horse which I supposed might have diabetes. He was a greedy feeder, and drank eagerly, yet he grew thinner and thinner; and wherever I had occasion to stop, there he invariably began to stale: so that I became thoroughly ashamed of his leaking. Dr. Prout was good enough to examine his urine for me. It contained no sugar, but its healthy properties were much changed: it had less than the natural quantity of hippuric acid, and more of earthy matters. The disease, he tells me, is known at the Veterinary College; whence specimens of such urine have been sent to him for inspection: but it is not true diabetes.

The unnaturally high specific gravity of diabetic urine is a constant quality; and you must attend to this, for it is almost always a faithful index, not only of the presence, but of the severity of the disorder. Dr. Prout places the specific gravity of healthy urine between 1015 and 1025, that of distilled water being represented by 1000. Different authors vary somewhat in their estimate of the natural standard; but we may be content to follow Dr. Prout. He says that the specific gravity of diabetic urine has been stated to vary from 1020 to 1050: that he has many times seen it higher than this, but very seldom so low. In fact it ranges generally between 1030 and 1060: and the average may be taken at 1040.

Symptoms.—So much with reference to the quantity and qualities of the urine discharged in this complaint. It is attended, however, as you may suppose, with other and important symptoms.

As so much fluid is evacuated from the body through this channel, it might be expected that the *other* channels for the excretion of liquid matters would be comparatively dry: and so they are. The skin is arid, harsh, and unperspirable. The patients tell you that they never sweat: that they cannot get into a perspiration. This is a very general symptom: yet in some few patients, especially as the period of the complaint draws near, the surface readily becomes humid. Again, the bowels are mostly costive, and the fæces remarkably solid and free from moisture. The tongue is dry, parched, and sticky; sometimes unnaturally red and clean: and the waste of watery particles from the system seems to be felt and expressed by the inordinate thirst which the patients suffer. Their drought is often insatiable. I remember one girl telling me that when she was debarred from an excess of water to drink, she would get up if she heard it raining in the night, and catch some of the descending drops to satisfy the tormenting sensation of thirst. And another patient, a very sensible fellow, informed me that, believing it could not be good for him to drink so much, and feeling no confidence in his own resolution to refrain, he was in the habit of betaking himself in the summer time to the fields and dry pastures, where no water was at hand to quench his strong desire for it. The appetite for food is often, but not always, equally keen: and the patients, especially those in the lower ranks of society, are apt to think, while they wonder at their weakness, that there cannot be much the matter with them, since they continue to eat and drink so famously.

Again, the enormous daily drain upon the system may be expected to cause various symptoms and sensations which may all be referred to debility and defective nutrition. Dr. Henry published a table, showing the quantity of solid extract in every wine pint of urine of different specific gravities from 1020 to 1050. Taking 1040 as the average specific gravity, and ten pints as the average quantity of the urine discharged daily, the patient would in this manner lose, every twenty-four hours, 15 ounces 7 drachms—or more than a pound and a quarter—of solid materials.

We need not be surprised, therefore, at the hunger, the wasting, the hectic fever, the feeling of emptiness and sinking at the stomach, the debility, the chilly state of the body, and especially of the extremities, the aching and sense of weariness in the loins and legs, the aversion to exercise, the loss of virility; all of which symptoms are generally present.

I may add, to complete the picture, some others, enumerated by Dr. Watt, and confirmed by Dr. Prout, and consistent with my own experience of the disease. They are, uneasiness in the stomach after meals, flatulence and acid eructations, dimness of vision, redness of the whole interior of the mouth, sponginess of the gums, looseness of the teeth, and some degree of irritation and inflammatory redness about the external orifice of the urethra: these last are symptoms noticed in persons dying of inanition. Again, listlessness and depression of spirits, weakness and peevishness of temper: "the once vigorous mind becomes feeble, oblivious, and vacillating—the once amiable temper, fretful, suspicious, and intolerant." With all this there is a peculiar faint and unpleasant odour of the breath and person; an odour which Dr. Prout says is hay-like, which some call melleous, but which reminds me, as I said before respecting the urine, of the smell of a room in which apples have been kept. I have recognized the complaint, upon first entering the sick chamber, by this peculiar scent.

Diabetes is generally a chronic disorder, creeping on at first insidiously, and spreading itself, under judicious management, over many years. Yet it is sometimes fairly entitled to be called an acute disease; for it occasionally breaks out suddenly, is attended with much febrile disturbance, and runs a short course, uncontrolled by any treatment. One such instance I have seen. Much more frequently it proves fatal through the supervention of some organic mischief, such as debility is calculated to foster and develop. It often becomes associated in its progress with pulmonary disease, especially with tubercular phthisis. So common is this, that some persons have thought it universal. But it is not so. I have myself witnessed more than one or two dissections of persons dead of diabetes, whose lungs did not contain a single tubercle. Sometimes the disease terminates in incurable dropsy: and sometimes the patient is cut off suddenly, either by apoplexy, or by some peculiar affection of the stomach.

There is some kind of connection between diabetes and certain affections of the skin, and of the subjacent reticular membrane. Dr. Prout remarks that it usually *follows* cutaneous complaints, but *accompanies* or *precedes* those which involve the cellular tissue. Persons have been known to lose chronic eruptions upon the supervention of diabetes. On the other hand, carbuncles and malignant boils are frequently the companions of that disorder.

Anatomical characters. The examination of the body throws little or no light upon the pathology of diabetes. We naturally look with interest to the kidneys. But we find nothing there to explain the symptoms noticed during life. What I have usually remarked has been a deep purplish red colour of the kidneys, which were veined and vascular, but not otherwise altered in texture. Andral and others tell us that the kidneys are found hypertrophied in diabetes. But hypertrophy, and unnatural vascularity, are circumstances which we are not surprised at when we reflect upon the vastly increased quantity of work which the glands have been performing. We must regard both these unnatural conditions rather as being the consequence, than as being the cause, of the morbid flow of urine. In one instance, after sudden symptoms of gastritis, which followed the incautious potation of strong ale, I found the mucous membrane of the stomach distinctly inflamed in its cardiac portion. I have found also the mesenteric glands diseased, converted almost entirely into bone. But neither of these changes are constant. They were purely accidental in those particular cases.

Pathology.—What, then, is the origin and source of this complaint? whereabouts in the body is the sugar formed? is it made, by the kidneys, from the blood? or is it made, from the food, by the stomach; and carried into the blood to be simply cast out through the urinary channels? or is it elaborated in some intermediate stage of what Dr. Prout has called the secondary assimilation; which includes the formative and the destructive processes that take place in the body subsequently to the act of sanguification? These questions, which are full of interest, have been much debated; and until very lately pathology was very unequal to their solution. It was naturally thought that, if the sugar pre-existed in the blood, and was only withdrawn from it by the kidneys, it would be discoverable in the blood. Yet able chemists sought for it there in vain. Hence it was inferred, that by some new combination of its elements, saccharine matter was actually formed in the kidneys. The chemistry and the reasoning were both faulty. Sugar has,

now, been detected in diabetic blood. It is detected with some difficulty, partly perhaps because its presence is masked by the albumen of the serum, but partly because its quantity is small; and its quantity is small because it is continually decanted out of the blood, as fast as it enters, and with it a profusion of water also, through the kidneys. In this respect the sugar and the urea are alike. They are both excretions which the blood is in haste to cast forth. It would seem also as if the sugar necessarily carried with it a large quantity of aqueous fluid from the blood, and was simply diuretic. When the amount of sugar eliminated is diminished, as by certain remedies it may be, the quantity of urine diminishes too.

Traces of sugar had, I believe, been found in the blood by some previous inquirers: but it is to Mr. M'Gregor, of Glasgow, that we are indebted for the full exposition of this interesting fact. His researches, published last year (1837) in the *Medical Gazette*, have thrown a new and strong light upon the pathology of diabetes.

By a peculiar process, he did, I say, that which many preceding chemists had failed to accomplish; he detected sugar in the serum of the blood of diabetic patients. The serum had a milky appearance, he says; and I have seen that myself: its specific gravity was above the healthy standard. Having coagulated the serum by heat, he carefully dried it; then cut the dried mass into very small pieces, and boiled them in distilled water; and lastly, he evaporated the decoction to a certain point. To the liquid thus concentrated he added a portion of yeast, and the presence of sugar was manifested by the fermentation which ensued, and which lasted for several hours. Yeast, I should tell you, is a most delicate test of sugar, and will readily detect half a grain in two ounces of liquid.

Mr. M'Gregor went a step further back. He obtained, by means of an emetic, the digested food from the stomachs of two men who had dined two or three hours before. One man was in health; the other had diabetes. In each case the food had been of the ordinary kind. Applying, after due preparation, the test of yeast, he found that the vomited matters fermented strongly; especially those from the diabetic patient.

Then he varied the experiment. Thinking that the sugar, in these cases, might have been introduced in the vegetable portion of the food, he adopted precautions to exclude that possible source of fallacy. He administered to a healthy man, and to a diabetic man, a vomit, and a purge; to clear out the alimentary canal. Next, he fed them upon roast beef and water, and nothing else, for three days. Then, three or four hours after a meal, the contents of their stomachs were procured by the operation of the sulphate of zinc, as an emetic; and treated as in the former case. What the healthy man vomited did not ferment at all. What came from the diabetic patient fermented "pretty briskly."

The fault, then, we may safely conclude, lies in the *digestive* organs. Instead of healthy and nutritive chyle, saccharine matter is prepared by the stomach, and enters the circulation. That which should be converted into muscle, and fat, and bone, and nerve, and membrane, is hurried out of the system, as sugar, with the urine. Thus far we see our way with tolerable clearness. But *why* the stomach should cease to perform its accustomed chemistry upon the food, and even upon sugar itself as an article of food, we have not yet learned. That single important step is still wanting to the complete solution of the pathological problem.

I may mention that Mr. M'Gregor carried his inquiries further still, and met with some curious results. He examined the saliva; the fæces; and the sweat. He found sugar in the saliva. He could find none in the sweat. The fæces of one patient, allowed to dry spontaneously, become covered, after the lapse of some time, by distinct crystals of sugar. And yeast having been administered to two patients, in ounce doses, after each meal, had soon to be discontinued, because the patients, to use their own expressions, felt as if they "were on the eve of being blown up."

Causes.—We know but little about the *causes* of diabetes. It is not a very common disorder; and in those who become afflicted with it, there probably has existed a predisposition to it. Dr. Prout remarks that the complaint runs sometimes in families and is inherited. I had under my own observation, for some time, three children, two brothers and their sister, all affected with diabetes. The same author mentions among the predisposing causes, long-continued intemperance, and especially the immoderate use of spirits, severe evacuations, excessive labour joined with a poor acescent diet. Distress and anxiety

of mind are held also, and justly I think, to be among the predisposing causes. It occasionally seems to be produced, at once, by the operation of some exciting cause, such as exposure of the body to cold; or the drinking of large draughts of cold fluid while the person was hot and perspiring. Dr. Bardsley states that, in twelve instances of the disease which had fallen under his own notice, the patients attributed their ailment to one or the other of these two causes. Now these are common causes of disease; and that a predisposition does exist is probable from the fact, that where the exciting cause has acted on several individuals at the same time, one alone has become affected with diabetes. There is a narrative illustrating this, by Sir Henry Marsh, in the third volume of the *Dublin Hospital Reports*. A patient of his traced the apparent origin of his diabetes to exposure to wet, cold, and privation, at sea, while in imminent danger of shipwreck. Another of the crew fell ill of ague. Others escaped entirely, or had only common colds.

Treatment.—If the account which I have been obliged to give you concerning the intimate nature, and the causes, of this curious malady should appear unsatisfactory, so also, I fear, will what I have still to say respecting its cure. I dare not affirm that diabetes, although it seems a merely functional disorder, has ever been cured. Dr. Prout has known some few patients, two perhaps, or one, recover. Apparent recoveries—nay, apparent cures—are not very uncommon. And this it is of great importance to know. Remedies are not uselesss because they fall short of their full scope. It is better to keep a man on the edge of a precipice, if you cannot pluck him away from it, than to let him fall over. And many diabetic patients are kept in this predicament of dangerous safety. There are certain remedies that exercise a strong controlling influence over some of the most prominent and troublesome of the symptoms; and that sometimes even restore the patient to a state which he mistakes for health; and which a medical man, unwarned of its fallacious character, might also mistake. The urine may recede within the natural limits. There may remain *one* morbid circumstance only, and that of a nature easily overlooked; indeed it is sure to escape observation if it be not especially searched after. I allude to the unnaturally high *specific gravity* of the urine. So long as the density of the urine continues permanently and decidedly above the healthy standard, there is no real security. The smallest disturbing cause, exposure to cold, an intemperate meal, unusual exertion and fatigue, sudden or strong mental emotion, may bring back all the symptoms in their former severity. If these and similar hurtful agencies can be averted, life may sometimes be prolonged, with much comfort, for many years.

There are three objects to be kept in view, in the treatment of every case of diabetes.

First, to restore the defective power of the digestive apparatus:

Secondly, to cut off, or restrict as much as possible, the supply of saccharine matter from without:

Thirdly, to mitigate or remove the most distressing symptoms.

If we could achieve the first of these objects, the other two would fall out of sight; for the disease, which is really a variety of dyspepsia, would be cured. But hitherto all the resources of our art have, in this respect, been baffled. Our main hope of ultimate success must lie in the regulation of the *ingesta;* whereby, also, the second indication is to be fulfilled. *Some* of the food is, in every case, carried to the proper account, or the patient would speedily die. If we can succeed in directing a sufficient amount of healthy nutriment to the organic tissues of the body, the draining away of any superfluous sugar will be borne well enough.

About the beginning of the present century, Dr. Rollo discovered and taught that a diet composed exclusively of animal matters had a signal effect in reducing the quantity and in diminishing the sweetness of diabetic urine. Mr. M'Gregor's experiments tell us why this is. Animal food furnishes but scantily the materials for the formation of sugar. "The saccharine alimentary principles are chiefly derived (says Dr. Prout) from the vegetable kingdom, and indeed constitute what may be called, by way of distinction, *vegetable aliments.*" If, then, we can exclude aliments of this kind, and confine the patient to animal food alone, we thus cut off the supply of the *materies morbi;* and without indeed curing the disorder, suspend its worst effects. But unluckily very few persons can long endure this mode of living. So far as they can endure it, they are comparatively safe. We are

obliged to relax, in some degree, the rigour of our rule; and it is curious to observe how suddenly and decidedly the saccharine properties and the quantity of the urine are augmented, when, by stealth, or by permission, the patient adds to his meal the smallest portion of vegetable food—even a biscuit or two.

We must therefore contrive to vary the animal diet as well as we can; encouraging the patient by a free license to choose among the different kinds of meat, game, poultry, and eggs, in their diversified modes of preparation, and admitting into his bill of fare as small an admixture as possible of vegetable substances. Green garden-stuff, the oleracea, spinach, cabbage, celery, and the like, may be taken with less risk of increasing the saccharine matters in the system, than potatoes, and those other articles of vegetable diet which contain a notable proportion of sugar, gum, or starch. All kinds of fruit must be forbidden. You will seldom be able to debar your patients entirely from bread: none should be allowed but such as is well fermented, and somewhat stale, or thoroughly toasted; and even that as sparingly as may be.

It is also of much importance to admonish the patient, whose appetite is generally ravenous, against eating too large a quantity, even of animal food, at any one time. Not only is the digestion still further weakened and oppressed by an intemperate meal, but the patient's life may be put in peril by every such act of unwise indulgence. Of this I have witnessed one example, and have heard of several more.

The quantity of drink should likewise be limited It may properly enough consist of animal broths: and these should be taken *tepid*, for they are then more likely to be taken in moderation. The patients must, however, and will, have something else, to slake their urgent thirst I have found (acting upon a suggestion of the elder Dr. Latham) that distilled water, acidulated with phosphoric acid, appeases, more than most things, that painful sensation. The water of the Bristol Hotwell, which contains carbonate of lime in solution, is praised by Dr. Prout for the same purpose.

Dr. Christison gives an important caution in respect to drinks; viz., that, when the thirst has already been much indulged, the quantity of liquid taken must not be greatly reduced all at once. Sudden failure of the vital powers has not unfrequently ensued upon abrupt changes of that kind.

Very numerous are the remedies which have been tried, and which have been recommended, for this disorder. I shall notice those only of which I have had personal, and in some degree favourable experience

The first of these is *blood-letting*, which has been strongly advocated by Dr. Watt, of Glasgow, and by Dr. Satterley, formerly one of the physicians to the Middlesex Hospital. It is not a remedy which would naturally occur to one's mind as being likely to prove of service in such cases; but both the authors I have mentioned speak of it in terms of high commendation. They affirm that, under small and frequent bleedings, the strength increases; the clamminess of the mouth, and dryness of the skin, diminish; and the blood, by degrees, assumes the buffy coat. I have once seen the method of frequent blood-letting fairly put to the test. At first the patient did seem to be benefited by it; but she ultimately died: and I am satisfied that her death was accelerated by one bleeding too many, or by too large an abstraction of blood at one time.

This measure has the best chance of being useful, when the malady is recent, and attended with febrile disturbance. In chronic cases, in old persons, and whenever the debility is already great, venesection can seldom be requisite or proper; although even then, as Dr. Prout remarks, it may be borne better than one might expect. Local bleeding is, however, of much service in relieving local uneasiness. Leeches may be applied to the epigastrium, if the patient has tenderness there, or complains of a sense of fulness or of burning in the somach. Cupping to the loins, if they greatly ache.

Opium is a treasure to us in this disorder. It quiets the nervous irritability of the patient, allays many of his most distressing sensations, and restrains in a remarkable manner the morbid profluvium from the kidneys. But you must not suppose, from observing these favourable changes, that you are curing the disease by it. It appears to control the diuretic influence of the sugar in the blood; but it does not banish the sugar itself. And, as far as my experience goes, Dr. Prout is in the right when he states that moderate doses of opium generally suffice to check the excessive discharge. Five grains

of Dover's powder, for example, three times a day, will do as much good, and on the other hand be productive of far less inconvenience, than larger quantities of that narcotic substance. The sudorific properties of this compound are thought to render it eligible: although it has seldom any apparent effect, in that way, in diabetes. If the ipecacuan which it contains should disagree with the stomach, an equivalent quantity of any other preparation of opium may be substituted for it.

There is another remedial measure which has also, in some cases at least, a most beneficial influence on the condition of the patient; I mean forced perspiration—perspiration induced by the hot air bath. Of this I have seen some striking examples. A very well-marked case of diabetes came under my care in the Middlesex Hospital several years ago. A vapour, or hot air bath, had just then been constructed in the hospital, and I thought it a good opportunity for trying whether the suspended functions of the skin might not be restored, and the extravagant action of the kidneys perhaps corrected, by that powerful mode of exciting perspiration. I should tell you that other plans of treatment had already been put in force, with but partial advantage. It would occupy more time than I can now spare to enter upon the details of this case; but I will read to you the man's own statement, which he wrote down before he left the hospital, in evidence of the benefit he derived from the *sudatorium*.

"The urine" (these are his words) "is reduced more than one-half, and does not contain much sweetness, but sometimes tastes salt, with a mixture of bitter. My stools, which were dry, and like balls packed together, are now quite natural. The pains in my limbs are entirely removed. My spirits, which were very much depressed, are now revived, and cheerful. The unpleasant aching of my kidneys, of which I spoke little lest I should be cupped in the loins, is now removed; only I feel weak there. I am cured of the pain in my stomach, and the circuitous working of the wind in my bowels, which formed lumps in my belly as it passed, resembling those formed by the cramp. I have likewise got rid of the palpitation at my breast, which was accompanied with a sort of dread. My breathing is much improved; perspiration, in a great measure, restored; and my skin, which was dry, is now become moist. I sleep well at night, whereas I *could* not sleep more than two or three hours out of the twenty-four. My thirst, which was excessive, has ceased to be troublesome."

This man, who, in the statement I have just read, has so graphically described his own morbid sensations, and the relief from them which he had obtained, left the hospital thinking himself well: *but*, the specific gravity of his urine remained above 1030.

In about half a year afterwards, he went one evening to Hyde Park to see some fireworks, got wet feet, and began to cough. The diabetic symptoms returned more severely than ever; and he soon died. I found his lungs stuffed with tubercles.

In furtherance of the principle upon which the use of the hot bath is recommended, the other well-known methods of promoting the natural functions of the skin should be followed; friction, and more especially warm clothing.

Steel is sometimes singularly beneficial in repairing the strength, and enlivening the spirits; as indeed it is well known to be in other forms of disease attended with a copious and permanent drain upon the system, and with a diminution of red blood. Of course it may be combined with opium, or with any other medicine which the circumstances of the patient may render needful.

I need scarcely say that the bowels require attention. Not that active purgation is advisable, but simply their regulation. Castor oil, rhubarb, aloes, lenitive electuary, are more appropriate in these cases than the purgative salts, which are apt to be diuretic also.

There is one other drug from which I think I have seen the happiest effects. I mean the *creasote*. I first became acquainted with its virtue, from prescribing it, almost accidentally, until a consultation could be arranged with Dr. Prout, for a child, eight years old, in whom the disease was well marked, and who had been brought to London from the country for advice. She had been rapidly wasting away for ten weeks, was extremely feeble, soon tired, very thirsty, especially at night, and had (what was also new to her) an enormous appetite. She was passing from three pints to two quarts daily of pale urine, having a specific gravity greater than 1040. I desired that her diet should be as exclusively animal as she could bear, and that she take one minim of creasote, suspended by

means of mucilage in an ounce and a half of water, three times a day. Curiously enough, the child liked the tarry flavour of the medicine. Upon this plan, with gentle aperients occasionally to regulate her bowels, she remained for upwards of a twelvemonth. Her urine soon fell in quantity within the limits of health, and in density to about 1030. She regained her lost flesh, strength, complexion, and spirits, and grew considerably. At length she suddenly sunk under an obscure affection of the chest. A brother of this little girl has lately manifested unequivocal signs of the same complaint; and in him it appears to have been equally checked by the same method of treatment. I have detected sugar in the urine of another boy belonging to the same family.

There were two points in this young lady's case which deserve a passing remark. As her urine diminished in quantity it began to deposit the lithates. This Dr. Prout considered a very promising earnest of a return to a better diathesis: and I find that in cases which have crept on insidiously from the first, he is accustomed to *date* the malady from the time when lateritious sediments, previously common, disappeared from the water.

The other point was, that calomel always sickened this child, and aggravated all the diabetic symptoms. This is consistent with Dr. Prout's experience, who tells us that he has never seen mercury do good in diabetic disease; but on the contrary almost invariably do mischief. I would advise you to read his judicious observations upon the too common abuse of that mineral.

I am quite aware of a possible fallacy in the case I have been mentioning, with respect to the efficacy of the creasote. With the use of that drug was associated a stricter adherence to a purely animal diet; and it is impossible to estimate with precision the separate effects of these two remedial measures. I am, however, disposed to believe that the creasote, by its well-known preservative property, checks the conversion of the food into sugar. And I think it is a mistake to go on increasing the dose. To most stomachs it proves irritating and hurtful when pushed beyond a certain small amount. Dr. Mac Intyre tells me that he has found the creasote very useful in diabetes. I must however acknowledge that, in common with others, I have sometimes been totally disappointed by it.

Diuresis.—For many years of my professional life I had not met with an instance of what has been called diabetes *insipidus:* from which I infer the rarity of that disorder. I shall use the term *chronic diuresis* to express this affection.

Very recently a marked instance of such chronic diuresis has presented itself in the hospital. A boy, eleven years old, not unhealthy looking, but lean, was admitted there under my care. He was much troubled by thirst; and by frequent micturition, which, even in the night, disturbed him many times. His bowels were costive, he had a capricious appetite, and his skin was dry. He voided during the twenty-four hours several pints, seldom less than nine or ten, of simply dilute urine, of a faintly yellowish hue, and having the specific gravity of 1002. Sometimes, indeed, it was found to be scarcely heavier than distilled water. In other respects he appeared tolerably well. It was supposed that he had been affected in this way for about twelve months; his desire for drink having been the first symptom noticed.

During his residence, of many weeks, in the hospital, under my observation, I made trial of every plan and drug that I could think of, for repressing the unnatural flux of urine. Nothing did him any good; some things, I fear, by disturbing his stomach and bowels, did him some temporary harm. He went out much as he came in.

Although I can tell you neither upon what this disorder depends, nor how it may be remedied, I mention the case because it afforded me a proof that liquid may be absorbed into the body from the atmosphere; either by the external skin, or by the pulmonary mucous membrane, or by both these surfaces. Dr. Prout, to whom I showed both the urine and the patient, advised that, for a time, his supply of drink should be limited. Accordingly, very much to the poor boy's sorrow, he was put upon a daily allowance of a pint and a half. I have no doubt that my injunctions were strictly observed, both by himself and by the nurses. Nevertheless, without losing flesh, or weight, he passed, during the corresponding twenty-four hours, ten pints and a half of urine.

I got evidence of the same fact in another way also; namely, by weighing the boy at

short intervals: although the experiment was not repeated so often as I wish it had been. I give you the results of one of these trials.

Immediately after he had emptied his bladder he was found to weigh 3 st. 8 lb. 0 oz. 3 dr. Three hours subsequently, having taken nothing in the interim, he weighed 3 st. 9 lb. 0 oz. 2 dr. Then he voided 16 oz. of urine: after which his weight was again 3 st. 8 lb. 0 oz. 3 dr. So that he must have imbibed about a pound of liquid in that brief space of time.

In this instance there was merely an excess in the aqueous ingredient of the urine; the solid matters were apparently there, in their due proportion to each other, but in a very small ratio to the water.

But the aqueous ingredient may be in excess, while the absolute quantity of *urea* is deficient. On the other hand, with an excess of the watery material, there may be an excess also in the quantity of urea it contains.

Dr. Willis has distinguished these three varieties of chronic diuresis by the terms *hydruria*, *anazoturia*, and *azoturia*, respectively. Although I am no friend to the multiplication of technical names, I must tell you that the distinctions expressed by these terms are real, and of some importance. Anazoturia we shall find to be often symptomatic of a peculiar organic disease of the kidney, which I hope to describe in the next lecture.

Azoturia, which is accompanied by an unnaturally high specific gravity of the urine, is apt, on that account chiefly, to be mistaken for diabetes. As recoveries from it are not uncommon, it may be suspected that some of the boasted cures of diabetes were cures of this less serious disorder. In the one case, the yeast test detects the presence of sugar in the superabundant urine; in the other case it finds none.

LECTURE LXXVIII.

ALBUMINOUS URINE. MEANS OF DETECTING THE ALBUMEN. WHAT IT IMPORTS. ANATOMICAL CHARACTERS OF BRIGHT'S KIDNEY. SYMPTOMS TO WHICH THIS RENAL DISEASE GIVES RISE. NATURE OF THE AFFECTION.

ANOTHER morbid condition of the urine, imperatively demanding your attention, is that in which it is habitually impregnated with *albumen*. This albuminous condition is much more common, and in general not less serious, than the saccharine condition which I described yesterday.

There is no albumen in healthy urine. Neither can we recognize its presence, in any urine, by mere inspection. We detect it by certain tests: and I will tell you, in the first place, what these are; and how to use them.

Means of detecting the albumen.—Albumen—of which we have so familiar an example in the "white" of eggs—begins to pass from the fluid to the solid state at the temperature of 160° *Fahrenheit*. When diluted it may require for its complete coagulation the heat of 212°. Hence one simple and easy test of its presence. We discover that albumen is contained in the urine, by heating that fluid to the boiling point. This is most conveniently done in a small glass tube, by the flame of a spirit lamp. It is seldom that any preparation of the suspected urine is requisite. It may, perhaps, be hazy in consequence of its containing *mucus;* and if its transparency be much troubled, it will be well to filter the fluid before testing it. When, as sometimes happens, albuminous urine is already turbid from the presence of the lithates, these dissolve as the heat is applied, and the urine first becomes clear; and then, as the temperature rises, the albuminous opacity begins to be visible.

The phenomena observable in the heated urine vary in different cases, chiefly by reason of the variable amount of albumen. The whole is sometimes converted into one gelatinous mass: but this is uncommon. Usually the albumen first appears in the form of a

whitish cloud, of which the constituent particles multiply, and collect in proportion as the quantity is considerable, into small curdy fragments or flakes. These soon subside to the lower part of the tube, leaving the supernatant liquid clear. The amount of albumen is of course to be estimated by the portion of the tube that it occupies.

Now this test, by heat, is not conclusive, nor sufficient. There are circumstances that may impede or prevent its effect in coagulating albumen, which, nevertheless, is present. On the other hand it may, under other circumstances, produce a fallacious appearance of albumen where none exists.

Albuminous urine has often a less acid reaction with litmus paper than healthy urine. The reason of this I will explain presently. When recently discharged from the bladder the urine may be neutral or even alkaline; or it may become neutral or alkaline, by spontaneous decomposition after it has left the bladder. In any case, the urine thus alkaline or neutral will not coagulate when heated, even though it may be full of albumen. Again, although there may be no albumen, heat may cause a flaky precipitate, consisting of the earthy phosphates.

We avoid, or remedy, these sources of fallacy, by testing the suspected urine with nitric acid also, which has the property of precipitating the albumen in a flaky or pulpy form. It will thus detect albumen when the tested urine is alkaline; and by restoring its acidity, it will make the albumen discoverable by the test of heat. It has likewise the effect of redissolving the spurious precipitates which may be thrown down by the application of heat, and consequently of showing that they *are* spurious.

Nitric acid alone, however, is not, any more than heat alone, an unequivocal touchstone of the presence or absence of albumen: for it may occasion a flaky precipitate of lithic acid, when there is no albumen. But this defect is compensated by the complementary criterion of heat; the precipitate being redissolved by raising the temperature of the urine, while any coagulated albumen remains insoluble.

Dr. Christison states that, in his experience, "sometimes nitric acid added in excess did not separate albumen which had been present in large quantity"—a fact which he thinks "is probably to be ascribed to the albumen itself having undergone more or less decay, along with the other principles of the urine." Hence the urine should, if possible, be examined before it has become decomposed by lapse of time. If, however, you employ both these tests with different portions of the same urine at the same time, and with the same portion in succession, you will avoid all risk of mistake.

Other tests there are, frequently spoken of, and sometimes recommended: particularly the ferrocyanate of potass, corrosive sublimate, creasote, and oxalic acid. They are unnecessary, in addition to heat and nitric acid; and they are liable to fallacies from which these last, when combined, are free. Unless you are expert chemists, you had better avoid them.

Bright's kidney.—Now it is quite certain that the presence of albumen in the urine does often accompany and bespeak a very serious organic disease of the kidney. For this disease we have no appropriate name. I wish we had. Some call it *granular degeneration* of the kidney, but the epithet granular is not always applicable. It is most familiarly known, both here and abroad, as *Bright's kidney*, or *Bright's disease;* after the eminent physician who, in 1837, first described it, and showed its great pathological importance. These are odd-sounding and awkward terms; but in the lack of better, I must employ them.

Anatomical characters.—It is very difficult to describe, in words, the anatomical characters proper to this renal disease; for they are neither very definite nor very constant. The description that I am about to attempt will be made more intelligible by Dr. Bright's plates, and those of M. Rayer, which are both before you.

The morbid appearances presented by the substance of the kidney are such as denote some change in its intimate structure. Its cortical (or secreting) portion is the primary and chief seat of this degeneration; yet what is called its medullary (*i. e.* its excreting) part, is also sometimes implicated, but in a less degree.

These morbid appearances relate to the size, figure, and consistence of the kidney; to the colour and condition of its surface, and of its interior. With respect to some of these points there is much variety in different cases; and studying this variety under the light

which is thrown upon it by the clinical history of the disease, we have reason to believe that it is connected with different stages of the disorganizing process. Thus if we look to the *size* of the diseased organs, they are sometimes much larger than natural, sometimes of the ordinary magnitude, sometimes considerably smaller. The average weight of the adult human kidney is four ounces. M. Rayer has met with some, in this disease, weighing twelve ounces. Both the increment and the decrement of the natural bulk belong principally, if not altogether, to the outer secreting portion of the gland. If a longitudinal section of the exaggerated kidney be made, its cortical part is seen to be unduly broad: and the same part is disproportionally narrow when the whole organ is smaller than common. For this reason, in the latter case, the radiating medullary portions approach nearer to the surface than they are observed to do in a healthy kidney. And it furthermore appears that the enlargement is most commonly coincident with the earlier, and the contraction or shrinking with the later, stages of the renal disease.

The *consistence* of the diseased gland is variable also. Sometimes, and for the most part in the earlier periods, it is soft and flabby: sometimes, and especially in the later periods, it is remarkably compact and hard. The size and the consistence of the kidney are, in most cases, inversely proportioned to each other.

Again, the *form* of the kidney, in the disease in question, often undergoes some modification. As the special change proceeds, the exterior of the gland shows a tendency to become indented by linear depressions, and to present a lobular shape. This, however, is by no means a constant phenomenon, even in the most advanced stage of the malady.

When its proper investing tunic is stripped off—and less distinctly *through* the same tunic, before its separation—the surface of the kidney appears mottled, marbled, or stained; of a yellowish gray colour in one place, and of a dark or purple tint in another. Occasionally it is pale throughout its whole extent; more commonly of divers hues, and variegated with little streaks, which are portions of vessels containing red blood. Sometimes the surface is curiously speckled: often uneven as if strewed with prominent grains; in some instances quite rough and scabrous. These several unnatural appearances are usually the more conspicuous, in proportion as the complaint is the more advanced.

The most uniform, however, and the most characteristic of the morbid appearances, are those presented by the cut surface of the kidney, when it has been divided into two symmetrical portions by a longitudinal incision. We then perceive that the cortical substance is the main seat of the morbid alteration. It has lost, in a greater or less degree, its proper red colour and uniform aspect. Sometimes it puts on a speckled or granular appearance; but this, in my experience, is less common than a pale, nearly homogeneous surface, somewhat like the section of a parsnip. Its natural striæ are confused or obliterated. The incised surface gives one the notion of some deposit, whereby the original texture of the part is obscured. The blood-vessels seem, many or most of them, to have been emptied by compression, or to be blocked up by yellowish solid matters; while the healthier pyramidal masses belonging to the medullary portion of the kidney are displaced, and pushed aside, or encroached upon by the same yellowish matter, which sometimes interposes itself between, and opens out, their radiating tubuli. Together with these changes of appearance and structure, I have several times found the veins that emerge from the kidney firmly plugged up by coagula of blood.

In some rare cases the kidney is studded, both on its surface and throughout its interior, with numerous small cysts or cells, containing a thin transparent liquid. These cysts have been inaccurately termed hydatids. It is not at all uncommon to meet with one or two such cysts in this diseased state of the organ.

It has been made a question whether the various appearances which I have been attempting to describe, and which sensibly differ in degree and combination in different cases, are characteristic of different morbid conditions, or merely of different stages and varieties of the same essential change. Our knowledge of the subject is scarcely sufficient to supply a positive solution of this question. Excepting perhaps the cysts, my own *opinion* is, that they are all accidental forms and effects of one and the same morbid process. At the same time I ought to tell you, that both Dr. Bright and Dr. Christison appear to incline to the opposite conclusion.

There is still another state of the kidney, very different to the eye from any that I have

yet mentioned, but which has been thought, and which I think, to be, in some cases at least, the first stage of all in the disorganizing process. This state, which I referred to when speaking of suppression of urine, may be briefly described in two words—*sanguine congestion*. The whole organ is gorged with blood, which sometimes drips freely from it when it is cut open. The kidney is in general large, somewhat flabby, of a deep dark red, even of a chocolate or purplish colour, nearly uniformly diffused, except that the cut surface is usually diversified by still darker tuft-like spots, which have been ascertained to be the Malpighian bodies filled with blood. This change from the natural appearance of the kidney is evidently of a recent kind; and the symptoms that have been observed to belong to it are these:—Fever, preceded often by rigors; uneasiness or dull pain in the loins; nausea and vomiting; a very scanty secretion of urine, which is sometimes tinged with blood, and always albuminous; occasionally complete suppression of urine. To these symptoms there is presently added, in most cases, sudden and general anasarca—what is commonly called inflammatory, active, or febrile dropsy. If the secretion of urine be entirely suspended, death soon ensues by coma, as I explained to you yesterday; but if not, the disorder is frequently fatal by the supervention of some acute internal inflammation; pleurisy, or pericarditis, or pneumonia, or peritonitis. Many persons recover completely from the condition expressed by this combination of phenomena. Many seem to recover, but bear about with them the germs or beginnings of those more chronic and latent changes which constitute "Bright's kidney."

Symptoms.—And what are the signs which indicate, to an instructed eye, the presence of those changes? Some of them are precisely the same, in kind, as those which denote the acuter disorder; only mitigated in degree, and of slower march and succession. The patients are subject to obscure lumbar pains; to sickness from time to time, and retching; and their urine is apt to be red, brown, or dingy, as well as albuminous, from the intermixture of some of the colouring matter of the blood. They are obnoxious to inflammations of the serous membranes also; and more particularly to head affections, of which they often die; drowsiness, convulsions, apoplexy. And, to finish the resemblance, many of them, ay, most of them, become at length anasarcous. Besides these symptoms there are others which are not seen in the acute malady; because it is acute. Gradually increasing pallor is almost constant; disease of the heart is common; and the skin, in general, even in the absence of fever, is remarkably dry and unperspiring. The patients are troubled by a frequent want to make water; by flatulence of the stomach and intestines; and by caprice of the bowels, which are sometimes obstinately costive, sometimes prone to diarrhœa.

Now it is worth your while to remark, with respect to this category of symptoms, that (the state of the urine excepted) they have no special *primâ facie* reference to renal disease. They are all common enough in various other complaints. In truth they are mere secondary consequences of Bright's disease; and in so far as they are symptoms of it, they are *indirect* symptoms. Before Dr. Bright no one perceived, in such symptoms, any indications of disease of the kidney. The primary and fundamental organic malady reveals itself by no indirect symptoms, excepting those which are furnished by the urine.

Albuminous urine.—Seeing, then, that this peculiar disease of the kidney is coupled with effects so grave and perilous, and seeing that one of its most positive and distinctive marks is an albuminous state of the urine, two questions of great interest at once present themselves.

1. Does albuminous urine *always* imply the presence of Bright's disease?
2. Is Bright's disease, when present, *always* accompanied by albuminous urine?

To both these questions the answer is—no

It is certain that some articles of food have the effect, in some persons, of rendering the urine for a time albuminous: perhaps it would be more correct to say that certain forms of indigestion cause this change. Albumen has also been detected in the urine under that general state of irritation produced occasionally by mercury, or by a blister to the skin. In the crisis of some febrile disorders, and in some cases of pregnancy, the same phenomenon has been observed. Whenever blood, proceeding from any part of the long tract of mucous membrane which lines the urinary organs, mingles with the urine, that fluid of necessity contains albumen, and coagulates if tested by heat or by nitric acid.

On the other hand, when the kidney is really affected in the way already described, the admixture of albumen with the urine is apt to disappear, for a while, even suddenly. I have known it vanish for several hours, immediately after the effectual application of a hot air bath; and after profuse purging by a full dose of elaterium. Sometimes it is absent for a longer period.

Another important question, therefore, now arises. Finding albumen in the urine, how are we to know whether it does, or does not, indicate the presence of Bright's kidney?

We may judge, in part, by frequently testing the urine, and noticing whether the albuminous impregnation be transitory or persistent. In part also we judge by the absolute amount of the albumen in a given measure of urine. If the urine be deeply charged with that unnatural ingredient, the presumption is strong that the kidney disease is in progress; and when that disease is confirmed, another remarkable change is found to have taken place in the urine. Its specific gravity is very low; and strikingly in contrast with that of diabetic urine. This is therefore a very strong additional diagnostic circumstance.

On Dr. Prout's authority we have assumed the specific gravity of healthy urine to range between 1015 and 1025. Other writers make it higher. But the urine voided in Bright's disease is sometimes as low as 1004; and its mean specific gravity does not exceed 1013.

I need scarcely again remind you, that the question of specific gravity must always be viewed in relation to the absolute quantity of urine secreted. The specific gravity depends, of course, upon the proportion of the solid constituents of the urine contained in a given quantity. If the aqueous portion be augmented, the effect upon the absolute density will be the same as if the solid contents were proportionally diminished. But when, as frequently happens in certain stages of this renal disease, the specific gravity decreases while the quantity of the urine decreases also, that conjunction of phenomena becomes especially significant.

The density of the urine being thus unnaturally low, notwithstanding the addition of the new substance, albumen, it follows, as a matter of inference, that the solid constituents proper to healthy urine must be sensibly diminished: and they are found, in fact, to be so. These solid ingredients consist mainly of urea, and of certain salts. The aggregate solid contents amount, in health, to sixty-seven or sixty-eight parts in every 1000. In Bright's disease the amount has been ascertained to be diminished to twelve or fourteen parts, and even, in an extreme case, to less than this—to about six parts.

The urine contains, then, albumen; and it is deficient in urea. These two facts suggested, naturally enough, to M. Solon, and to others, the notion that the albumen might be formed, by a sort of conversion, at the expense of the urea; since these substances, by a slight alteration in the ratio of their elements, pass respectively each into the other. But it is not so. Dr. Christison had observed many years ago, that when the urine was deprived of the greater part of its urea, the quantity of albumen contained in it was small; and, on the other hand, in cases where the urea was considerable in quantity, the albumen was plentiful also. In a recent work on this subject, the same physician states that the whole of his subsequent experience has been in conformity with this observation.

It being certain, therefore, that the albumen is not vicarious of the urea, what (you may ask) becomes of the urea? It is detained in the blood; and may readily be recognized there in considerable quantity: and herein lies, as I conceive, the secret of the secondary affections which belong to this disorder, and of its great fatality. The body is poisoned in detail by the retention of its own excrements. The blood not being duly purified through that great emunctory, the kidneys, is spoiled for its purpose of nutrition. Besides containing urea, it undergoes other and more manifest changes. Its proportion of fibrin varies; and it gradually becomes poor in colouring matter; the serum is less albuminous also, and of a lower specific gravity, than in health. The quantity of albumen in healthy blood averages from sixty-five to sixty-nine parts in 1000. In this malady Dr. Babington has found it reduced to sixteen parts. The average specific gravity of healthy serum is 1030; but in Bright's disease it descends to 1024, 1020, and even to 1013. Now Dr. Christison has made out the very interesting fact, that there is a definite inverse

ratio between the coagulability of the urine, and the density of the serum. The more albumen there is in the former of these fluids, the less is there in the latter, and the lower is its specific gravity. So that the deficiencies of the one fluid balance the superfluities of the other. All this is very different from what takes place in diabetes, in which sugar is excreted with urine that is otherwise healthy: whereas, in Bright's disease, urea, which ought to be discharged, remains in the blood; and albumen, which ought not to be separated, is taken from the blood, and carried out with the urine.

Stages.—I have now described the changes presented by the kidneys in this disorder, the symptoms which attend it, and the morbid conditions both of the urine and of the blood. But these all vary and fluctuate at different periods of the complaint. I must next, therefore, endeavour to state what has been ascertained of its course and progress.

When the chronic disorder is not a legacy left by the more severe and acute form of disease which I have termed febrile dropsy, it is apt to creep on very insidiously, and to escape our notice—and its history is not fully known. It will be enough if I distinguish two stages of the malady—the early, and the advanced.

In the early stage the urine is generally scanty. Instead of about 40 ounces in the twenty-four hours, the patient voids 16, 12, 8, or even so little as 2 or 3 ounces. Sometimes the secretion is nearly or quite suppressed: and then the head seldom fails to become affected in the way already described. The urine has also an unnatural appearance. It is red, or dark, obscurely turbid, like muddy beer. It froths more than usual; and if you blow into it through a tube, you raise bubbles similar to those which may be formed in soapy water. Its specific gravity is somewhat, yet not greatly reduced; about 1021, perhaps; it is seldom at this period so low as 1016. It contains an abundance of albumen.

At the same early period, blood drawn from the arm exhibits the buffy coat. The serum is much diminished in density, and contains a considerable quantity of urea. There is no decrease in the fibrin; perhaps it is a little augmented: and there is no great change in the amount of colouring matter.

In the more advanced stages of the disease, the quantity of urine is frequently not below the standard of health; and it sometimes considerably exceeds that standard, so as to constitute one variety of chronic diuresis (*anazoturia*), which some call diabetes insipidus. It is usually pale, slightly opaque, and of a very low specific gravity; 1014, 1010, 1007. Once, when the quantity of the urine was *not* in excess, Dr. Christison found the specific gravity to be no more than 1004. There is a corresponding reduction in the natural solid ingredients of the urine. Albumen, too, is present, but more uncertainly than in the earlier periods: fluctuations in this respect are more common than before. It is a mistake to suppose that the amount of albumen increases as the disorder advances. The contrary rule would be more near the truth. In general the albumen is plentiful and almost constant in the outset of the malady; less surely present as it proceeds; and sometimes entirely absent in its latter periods: and it is of importance to remark that the alteration in the specific gravity follows the opposite law. The declension of density, so far from being corrected, augments with the progress of the disorder. Hence the one of these morbid phenomena is a valuable check upon the other, considered as an index of what is going on in the kidney.

And another fact, which it is essential for you to know and to remember, is, that in any stage of the disease, the supervention of febrile disturbance, from local inflammation or whatever other cause, tends to renew, for the time, those qualities of the urine which belong to the early period.

Meanwhile, the disease advancing, the serum of the blood recovers more or less its lost specific gravity, in proportion to the decrease of albumen in the urine. The quantity of fibrin seems, in some cases, to diminish. But the striking and most characteristic change is the rapid disappearance of the colouring matter, the hematosin, as it is called. This may at length be so much reduced, as to form less than a third of the healthy average. If venesection be occasionally employed, this process of depravation is accelerated; but it takes place whether blood be artificially withdrawn from the system or not. "I am acquainted," says Dr. Christison, "with no natural disease, at least of a chronic nature, which so closely approaches hæmorrhage in its power of impoverishing the red particles

of the blood." Hence the peculiar pallid or dingy hue of the patient's skin; the leucophlegmatic and even waxy aspect which invariably stamps the victims of this complaint.

These characters, then, of the urine and of the blood, when rightly compared and interpreted, reveal not only the existence of the renal disease, but also, with much probability, the stage or degree that it has reached.

Secondary affections.—Let us next review, a little more in detail, those secondary affections which I have already pointed out as being incidental to the subjects of this renal malady. They are of much consequence: for, in the course of the disease, more or fewer of them are almost sure to occur; most of them are productive of very serious distress; and some of them place the patient's life in immediate jeopardy, and often bring it to a premature end. Moreover it is by these secondary affections that our suspicion of the primary disease upon which they depend is, in general, first awakened: and it is to the prevention or the removal of these same secondary affections that our curative endeavours must chiefly be directed

The most common, and practically the most important, of them all, is anasarca; but of this, although I mention it first, I shall postpone for a while, the further consideration.

Another very common, and very important secondary complication, is the occurrence of what we compendiously call *head symptoms:* various manifestations of derangement in the cerebral functions; headache, drowsiness, delirium, epileptic seizures, apoplexy. So frequently indeed is the death of the patient preceded by coma, with or without convulsions, that Dr. Christison considers this to be the "natural termination" of the disease, or "the mode in which it proves fatal when life is not cut short by some other incidental or secondary affection." Of seventy fatal cases observed by Dr. Bright, death was ushered in by well-marked cerebral symptoms in thirty.

I have already told you the circumstances under which these affections of the brain usually arise. They almost always follow any great diminution, or the entire suspension, of the secretion of urine. But this rule is not so strict as to admit of no exception. Occasionally, but I believe very seldom, the urine, in this disorder, is reduced to a very small amount, while the head remains undisturbed. Of this Dr. Christison has recorded a remarkable instance. One of his patients voided no more than two ounces of light urine daily, for nine days before his death; yet he continued sensible to the very last minute of his existence, and died simply of inanition. Sometimes apoplectic symptoms occur, and carry the patient off, although there has been no extreme or material reduction in the quantity of urine.

Now when death has thus taken place in the way of coma, and the case had been complicated with anasarca, and serous liquid is found accumulated in unnatural measure in the cerebral ventricles, and in the tissue of the pia mater, it seems reasonable to ascribe the coma to the presence and pressure of that liquid. The dropsy has extended to the brain. And this view of the matter is strengthened by the connection which may sometimes be noticed between the accession of coma and the visible increase of the dropsy in other parts of the body. My own experience accords entirely with that of Dr. Christison, as expressed in the following statement. "If the dropsical fluid be allowed greatly to accumulate, drowsiness, the first symptom of the affection of the head, very soon makes its appearance in the generality of cases, and it will speedily pass to fatal coma, if not controlled, but the removal of the dropsy will usually remove the drowsiness."

To many cases, however, this explanation will not apply, there being no morbid collection of water within the skull, nor any other appreciable change there; nor, perhaps, any dropsy elsewhere. In such cases we refer the ultimate symptoms, the stupor and the death, to the poisonous influence of the urea in the unpurified blood upon the organs of animal life. Yet this explanation also has its difficulties. Urea must often circulate with the blood without affecting the brain. Dr. Christison states that he has repeatedly known the daily discharge of the solids of the urine to be reduced, for weeks together, to one-fourth of the natural amount, while, moreover, analysis of the blood showed that it was loaded with urea, without the appearance of any head symptom. Dr. Bright also relates a case to the same purpose. A person labouring under this disease of the kidney lived for four or five years under his occasional observation. The blood was analyzed in the earlier stage, and

found to contain a large quantity of urea; as much as the urine itself contained. Yet this patient had no *fits* till towards the close of his life.

I have sometimes fancied that the pale and watery condition to which the blood is at last reduced, may have something to do with the stupor and coma. I showed you, some time ago, when speaking of spurious hydrocephalus, that similar symptoms are apt to ensue, in conjunction with a similar defect of hematosin. It would seem that, under such circumstances, the functions of the brain are exercised irregularly, languidly, and at length not at all, in consequence of the failing supply of its appropriate stimulus through the arteries.

Another striking circumstance observable in this disease, is a readiness of various organs of the body to inflame, and particularly of the serous membranes. According to M. Solon, who has lately published a thick volume on *Albuminurie*, this disposition has not been so manifest in France; but of its frequent appearance in this country I can add my own testimony to that of Dr. Bright, of Dr. Christison, and of Dr. Gregory. Such intercurrent acute inflammation is not an uncommon cause of the patient's death. The pleura appears to be much more often affected in this manner than either the peritoneum or the pericardium.

It follows from this tendency, that when we come to inspect the dead body, we seldom find the kidney to be the only part in which structural changes are plainly visible. Most commonly evident traces of disease are met with in various organs.

Disorder of the stomach and bowels is certainly a frequent companion of the renal malady: nausea, vomiting, flatulent distension, diarrhœa.

It would appear, however, that these incidental and secondary complications prevail with irregular frequency in different places. They are probably determined, in some measure, by local and peculiar agencies. Thus vomiting and diarrhœa have been more familiar to the Edinburgh observers than in London to Dr. Bright, or in Paris to M. Solon: while the headaches and coma, so often witnessed by the British physicians, have been comparatively uncommon in France.

Disease of the heart, if not a secondary consequence, is a very frequent accompaniment of Bright's kidney. It is probable that the cardiac disease, and the renal disease, have sometimes no connection in respect to cause and effect, but are both results of some common cause; of habitual intemperance, for example.

I am, however, of opinion, that the renal malady has a direct tendency, by its effect upon the blood, to generate disease of the heart. It induces anæmia: and anæmia, as I showed you on a former occasion, implies debility of the muscular texture of the heart, and leads to dilatation of its cavities; and the weak muscle, becoming irritable also, grows thicker as it labours more. In fact, this is the kind of cardiac disease which, more than any other, has been found coincident with the peculiar change in the kidney. Among 100 cases, recorded in a tabular form by Dr. Bright, there were 27 in which no affection of the heart could be detected. In 52 instances the heart presented the characters of hypertrophy, and of these no fewer than 34 were free from any trace of valvular disease. Among these 34 there were 11 cases of disease affecting the aorta: in the remaining 23 no cause for the existing hypertrophy and dilatation could be found in the heart itself, or in the great blood-vessels. The true cause may therefore be reasonably supposed to have been the renal disease, operating upon the involuntary muscle through the quality of the blood.

Whether the renal disease be ever produced by the cardiac, is more questionable. In the acute renal affection, when it proves early fatal, the kidney is always found to be gorged with blood. And the customary intermixture of blood with the urine warrants the belief that the same condition was present in the patients who have recovered. From this state of engorgement springs, apparently, the subsequent series of changes It is therefore a plausible conjecture that whatever tends to produce congestion of the kidney, tends, also, to aggravate, and may even cause, the peculiar changes in question. I need not now tell you that disease of the heart does frequently occasion congestion of the venous system, and gorge the viscera with blood. Under this influence the liver often *enlarges*. On the other hand, disease of the heart, even such as gives rise to venous congestion and to dropsy, often lasts long, and proves ultimately fatal, without the occurrence of albuminous urine, and without any appreciable change of structure in the kidney.

Pain or tenderness of the loins, is sometimes, and sometimes only, an accompaniment of the renal disease. This symptom is more often present in the early than in the later stages of the malady. It occurred in one-third of twenty-eight cases narrated by M. Solon. Dr. Gregory noticed it in the half of his patients.

Causes.—The *causes* of the disease of which I have been endeavouring to sketch the outline, are often obscure. Its more obvious symptoms, in the chronic form of the malady, have been observed, in very many instances, to begin soon after the exposure of the body to wet and cold under unfavourable circumstances. But it is by no means certain—indeed the probabilities preponderate on the other side—that, in these instances, the renal disorder had not previously existed in a latent state.

It is certain, however, that the acute kidney affection, which may be considered identical with febrile dropsy, does often arise under similar circumstances of exposure, and is attended with a marked disturbance of the functions of the kidneys. And Bright's disease, in its chronic form, has been noticed as occurring in persons who had previously suffered, and had apparently recovered from, an attack of febrile dropsy. Are we not warranted in believing that the recovery was *imperfect* in such cases? that the kidney had sustained irretrievable injury? and that the disease, although from the treatment employed, or by lapse of time, it had become tranquil or latent, was ready again to give indications of its existence upon any repetition of its exciting cause?

Again, it is matter of common observation that intemperate habits have often preceded the development of this disease. Yet we may conclude that intemperance in drinking is rather a predisposing than an essential cause, from the fact that the malady is not unknown among children, and other persons whose manner of life has been strictly sober. I had lately an example of this in a young girl, fifteen years old, who had never menstruated. And this leads me to remark that the renal disorder has been known, in many instances, to follow a sudden check or suppression of the catamenia. It has sometimes seemed to owe its origin to blows received upon the loins.

The complaint happens at all ages: less often, however, in extreme youth than afterwards. Sabbatier records that he saw, while in the service of M. Baudelocque, a young infant affected with anasarca and albuminous urine. The first case described by M. Solon is that of an infant, seventeen months old, in whom similar symptoms appeared shortly after exposure to cold and wet. In 1838 a boy between five and six years old, anasarcous, and passing bloody and albuminous urine, was in the Middlesex Hospital, under the charge of my colleague Dr. Wilson. M. Constant, in the *Gazette Médicale* for 1835, cites the case of a child of five years of age; and M. Rayer gives two plates, representing the kidneys of two children, the one five and the other six years old, who both died of dropsy with albuminous urine, the sequel of scarlet fever. In each of these the changes described by Dr. Bright were well marked, and the bulk of the kidney was considerably increased.

The malady is, however, much more common in adults: not, in all probability, because the kidney is more readily susceptible of it at one period of life than another, but because, as life advances, the circumstances which tend to produce or to foster it become of more frequent operation; intemperance, exposure to great vicissitudes of temperature, and (perhaps) disease of the heart.

It occurs, I presume for the same reasons, oftener in men than in women.

Dr. Christison suspects that Bright's kidney happens chiefly in persons of scrofulous habit; and he found it, in several instances, coincident with phthisis pulmonalis. My own experience would not have led me to that opinion. I partake in M. Solon's doubts, whether the coexistence of the pulmonary consumption and of this renal malady is more than casual. Dr. Bright tells us that "the instances in which phthisis, or any form of scrofulous or tubercular disease, has been connected with the renal affection, have been decidedly rare."

Nature of the disease.—What, after all, is the true character and essence of the organic metamorphosis which constitutes this formidable disorder, Bright's kidney? All that has been ascertained of its early stages, of its course, and of its causes, furnishes to my mind a strong presumption that the structural change, in all its varieties of aspect, may be ultimately traceable to an undue accumulation and stagnation of blood in the blood-vessels

of the kidney. Those curious arterial bunches, the Malpighian bodies, appear especially to be overfilled and obstructed. Rayer calls the complaint *albuminous nephritis;* and perhaps the congestion (which unquestionably is present in what I consider the acute form of the malady) may sometimes pass into chronic inflammation. We do not, however, find that it ever terminates in suppuration: yet suppuration is no uncommon event of true inflammation of that part, excited by violent injuries, or by the lodgment of calculi within it. It seems to me more probable that the mischief done to the kidney is owing to extreme congestion, and its usual consequences—the oozing forth of the blood in substance, or of some of its constituents, into the interstitial textures, as well as into the excretory tubes of the kidney. The appearance of these ingredients of the blood, and even sometimes of blood itself, in the urine; the increased size of the gland in the earlier stages; the various shades of colour which its surface and parts of its interior present, as the colouring matters of the effused fluids are more or less absorbed; the impermeability of those altered parts by artificial injections; the ultimate shrinking and hardness of the organ as the disorder becomes chronic, and absorption proceeds; these are all consistent with this theory. It is plain that the morbid conditions of the urine depend, in part at least, upon the mechanical transudation of certain portions of the blood, which pass through the kidney unchanged, as through an inert filter. Mixed with the urine we find serum, with its albumen, and its salts, which diminish the acidity of the mixture, or even render it neutral; and in many cases we find more or less of the colouring matter also of the blood. Those portions of the extravasated fluid which have no outlet of escape, solidify, and thus obliterate the natural texture of the part they have invaded. The obstruction of the emergent veins of the kidney by firm clots of blood is in harmony with the same supposition.

When the kidney is thus spoiled, its natural function is imperfectly or but partially performed. The change which it should effect upon the blood, by purifying it from urea, fails to be accomplished. The albuminous impregnation, and the other altered qualities of the urine when voided, may be explained either by supposing that the secreting power of the whole gland is interfered with, but not absolutely suspended; so that the urine is incompletely elaborated: or, by supposing that portions of the gland are spoiled, and portions remain sound and effective; that true urine is formed by the healthier portions, and mixes with the constituents of the blood which pass, as such, through the diseased portions. The latter of these hypotheses is most in accordance with the fact that, in the advanced stage of the disorder (when we may conceive the spoiled parts to have become mere solid unchanging masses) the albumen is apt to disappear from the urine: and also with the fact, that complete recovery does, sometimes, appear to take place; in which cases we may imagine that, although a small portion of the substance of the gland has undergone irremediable change, enough of it remains healthy to serve the wants and purposes of the economy.

I have yet something to say respecting the *dropsy*, which is so common an accompaniment of these renal changes; but I must defer it till to-morrow.

LECTURE LXXIX.

ANASARCA; ITS CONSIDERATION RESUMED. DISTINCTION OF CHRONIC GENERAL DROPSY INTO CARDIAC AND RENAL. CHARACTERS AND SIGNS OF EACH OF THESE VARIETIES. TREATMENT.

WE were yesterday occupied with the circumstances of that remarkable disease, which has never received a good, distinctive name, but which is sometimes called *Bright's* disease, after the distinguished living physician who first recognized and described it, sometimes yellow degeneration of the kidney, sometimes granular degeneration. I endeavoured to represent to you, by words and by drawings, the morbid appearances that are most commonly noticed in the several stages of the complaint: which morbid appearances may, one

and all of them, be traced (as I think) to congestion of the kidney; to the detention of blood in that organ, and to the physical consequences of such detention. I spoke of the symptoms which appear *essential* to the malady, and which consist in certain striking changes in the urine and in the blood of the patient. I mentioned also the symptoms which are *incidental* to the renal disease. But of one of those incidental symptoms, or secondary consequences, I postponed the full consideration till to-day: I mean the *anasarca*, with which most commonly, yet by no means always or necessarily, it is complicated.

This is, for several reasons, a very important symptom. It is usually the first thing that prompts us to suspect, and to inquire after, the renal malady. It was through his researches into the relation subsisting between chronic dropsy and the conditions of the urine, that Dr. Bright was led to the discovery of the associated affection of the kidney. The dropsical accumulation adds greatly to the patient's distress, and sometimes constitutes nearly the whole of it. It adds proportionally to his danger. Moreover, it is that consequence of the renal disorder over which our curative measures have the most control. Indeed, under this complication we have, practically, to consider the remedies of the dropsy, distinct from the remedies of the renal change.

In most cases, at the outset at least of the dropsy, the skin is dry, and the urine is scanty: and the anasarca is observed to increase, or to decrease, as the quantity of urine diminishes or augments. The aqueous fluid, which should escape from the surface and through the kidneys, collects in the subcutaneous cellular tissue. As the disorder advances, the tendency to effusion of serum through the sides of the blood-vessels is probably increased, not only by the sluggish movement of the blood in the veins, from progressive debility of the heart, but also by the cause of that debility, the thin and watery condition of the blood itself; a condition which I yesterday pointed out to you as one of the most uniform and striking effects of the primary disease.

And here I again take up the subject of anasarca and general dropsy. You will remember that, in the earlier part of the course, I entered somewhat fully into the general pathology of dropsies. At the same time I promised you that I would afterwards, and when you were better prepared to understand them, endeavour more fully to explain some grand distinctions which have been found to exist between different forms of general dropsy. Having now, at last, brought before you all the organic changes which are apt to give rise to anasarca, I am in a condition to redeem that promise. In doing so I shall probably have to remind you of some things which you have already heard from me.

Anasarca, you will please to recollect, signifies the filling up of a considerable part, or of the whole, of the subcutaneous cellular tissue, with serous or watery fluid: and when to this is added a collection of liquid in the large serous cavities also, we call the complaint *general dropsy*.

It is obvious that this condition may exist, and in nature it does exist, in various degrees: from slight infiltration of the cellular tissue, scarcely noticeable until, after some hours passed in the upright posture, it accumulates in visible œdema about the ankles—to the other extreme, in which the integuments are every where stretched to the utmost even to bursting; the insteps bulging upwards; the legs and thighs enormously enlarged, cylindrical, unshapely, and exhibiting partial vesications: the surface of the trunk of the body capable of being kneaded and moulded like dough; the skin of the penis distended, and in consequence of its confinement by the frænum, twisted and circumvolved so as materially to impede the outward passage of the urine; the scrotum, as big as a child's head, preventing the miserable patient from approximating his thighs, and from lying upon either side; the hands swollen; the face and neck bloated. With all this, the peritoneum is generally full of liquid, and at length the pleuræ; and as the scene is about to close, there is water in the ventricles of the brain, and an anasarcous pia mater.

Now from whatever cause this watery condition of the whole body may arise, the effects resulting *from the presence of the water* are the same. And of what do patients in this state usually complain? Why of shortness of breath, and of palpitation of the heart; of a sense of impending suffocation if they attempt to lie down, or actively to bestir themselves; of tightness and distress across the epigastrium, relieved somewhat by eruc-

tation, augmented by food and drink; of weight and stiffness of their limbs; and, sometimes, of drowsiness.

The explanation of all this is easy and obvious. The shortness of breath may be accounted for on various grounds: by œdema of the lungs themselves, a state that is revealed to us through auscultation; by water in the pleuræ; by the pressure upwards of the diaphragm which embarrasses still more the labouring heart and lungs; and this upward pressure is increased by any kind of distension or repletion of the stomach, diminished when the stomach is collapsed and the upright position is maintained. The heaviness and want of pliability of the unwieldy limbs are, like the rest of the phenomena, purely mechanical. All parts are oppressed by the unnatural load of water.

But we must look beyond the dropsy: and inquire whether the complaint has set in suddenly, and simultaneously with febrile disturbance, invading all the districts of the body at once, and quickly reaching its present degree; or whether it has crept upon the patient slowly and by stealthy steps: whether it has had any obvious or probable exciting cause; or whether it has approached insidiously we know not whence or why: whether (in a word) the case be one of active and febrile dropsy, or of chronic and passive.

Now setting aside, for the present, any more particular consideration of the acute or febrile form of general dropsy, and contemplating those forms only which are chronic, we find that all, or nearly all of them, may be arranged in two great classes; those which depend upon disease or debility of the *heart* forming one class, those which depend upon disease of the *kidney* constituting the other. To these classes we accordingly apply the terms *cardiac dropsy*, and *renal dropsy*. They are often combined in the same individual; but taking the pure cases of each form, we may proceed to inquire into their peculiar features, how they may be distinguished, and what differences of treatment they may require.

And first of *cardiac dropsy*.

The mode in which disease of the heart may occasion general dropsy has been sufficiently explained already. We infer that the dropsy, in a given case, has this origin, if we find that thoracic symptoms, such as cough and dyspnœa, preceded the dropsy: or if we perceive direct signs of cardiac disease, such as distended jugular veins, irregular movements of the heart, unnatural impulse, altered sounds: or if we trace the history of some previous acute disease affecting especially the left side of the thorax: or if we learn that the patient has formerly suffered acute rheumatism: or if the patient's age is so much advanced as to make it probable that some of those organic changes in the heart and large blood-vessels are in progress, which are almost natural in the decline of life. And our inference is confirmed if there be no discoverable indication of renal disease.

But we see many persons who labour unequivocally under organic disease of the heart, yet who survive, even for many years, without becoming dropsical. The interesting question therefore arises, of what kind of heart-disease is dropsy a consequence and symptom? You already know the answer. It is such disease as offers a certain amount of permanent obstruction to the passage of the venous blood. Hence dropsy is especially associated with dilatation of the right chambers of the heart. It would not be correct to say that the anasarca is *dependent* on such dilatation, for the dilatation itself is at once an effect and a sign of impeded transmission of blood from the right side of the organ. Nor is such dilatation a necessary attendant on the general accumulation of water. The impediment may be sufficient to gorge the right cavities, while it is yet too slight in amount, or too recent in duration, to have dilated them.

What, then, are the physical conditions which oppose to the blood in the veins such an impediment as we are now considering? The two great vital organs contained within the thorax, the heart namely and the lungs, form different parts of one common mechanism, the object of which is to supply every tissue of the body with blood that has recently been purified by exposure to the air: and these organs, thus closely related in their functions, are moreover so reciprocally dependent, that structural disease occurring in the one tends to produce disease, sooner or later, in the other also.

And I wish you again to observe the order and direction in which disease is, almost always, propagated from one part of this apparatus to another. It is a backward direction —opposite, I mean, to the course of the blood. There are, strictly speaking, two hearts,

which lie side by side, in respect to their anatomical position, but which, reference being made to their vascular inter-communication, are really separated from each other by the lungs. The great veins precede, and the great arteries follow, this chain of connected organs. Disease occurring in any one part of the chain becomes a cause of consecutive disease in the part immediately behind it. And this law obtains, as I have shown you before, in regard to the several chambers of the heart, considered as a single organ.

Thus, structural disease situated at the aortic outlet of the heart, and of such a kind as to hinder the exit of the blood from the left ventricle, gives rise to permanent changes in that ventricle; to hypertrophy with, or less commonly without, dilatation. The hypertrophy is strictly a compensating and conservative change; and when it is exactly proportioned to, and keeps pace with, the impediment which has given it birth, so as precisely to balance and countervail it, no delay takes place in the stream of arterial blood, and the injury is, as yet, confined to the left ventricle. That chamber is remodelled, and adapted to its purpose by the *vis medicatrix naturæ;* and no other evil manifests itself than, perhaps, some slight encroachment and pressure upon the neighbouring parts, in consequence of the augmented volume of the heart.

So long as the mitral valve remains healthy and effective, it offers a barrier of protection against the extension of the disease in the direction which is retrograde to the course of the blood. But at length, in most instances, the stress becomes sensible further back. The left auricle and the pulmonary veins become choked and distended; the blood is detained in the lungs. Then commences *dyspnœa:* at first occasional only, whenever the heart is tasked with the conveyance of a greater quantity of blood in a given time than usual, as in brisk movements of the body, or sudden emotions of the mind; or when it is oppressed by circumstances that diminish the capacity of the chest; by a full meal, therefore, by flatulent distension of the stomach and intestines, by the recumbent posture. Afterwards the shortness of breath becomes more or less constant and distressing.

Now this loaded and embarrassed state of the lungs, even when it is permanent and has reached a considerable degree, may exist without materially interfering with the functions of the right or venous heart; for the pulmonary plethora may be relieved by increased secretion from the bronchial mucous membrane. Dyspnœa, even when it has become habitual, may precede for some time any appearance of dropsy.

At last, however, the effects of the original evil augmenting and extending, the right ventricle also becomes unable duly to propel its contents into the pulmonary vessels; it continues morbidly full, is first distended occasionally, then permanently, and at length really dilated; and with that dilatation we have a turgid venous system, of which we see a part in the prominent veins of the neck.

In this way then may be explained a series of symptoms which you will often witness, and be consulted about, in persons who are growing old. You will find irregularity of the pulse: preternatural impulse perhaps of the heart; occasional shortness of breath; large crepitation, habitually audible in the lower portions of the lungs; more or less expectoration, sometimes tinged, sometimes even loaded, with blood. Eventually the ankles begin to swell; and the patient (if his life is not cut short earlier in some other way) becomes by degrees decidedly and universally dropsical.

Many of the direct signs of diseased heart may exist, therefore, while there is no anasarca: intermissions and irregularity in its movements, palpitation, the impulse proper to hypertrophy. But when dropsy has supervened, we may expect also the signs which denote dilatation of the right chambers. The heart is heard and felt to beat beyond the præcordial limits; the pulsations become feeble and unequal, if they were not so before; the patient is liable to fluttering palpitations, to extreme and panting dyspnœa on the slightest exertion, even on taking food into the stomach, or adopting the recumbent posture; his skin assumes a dusky hue, and his lips and extremities are apt to be livid.

In these cases the anasarca first becomes manifest about the ankles. During the earlier stages the œdema disappears in the night, and returns towards the next evening. It is sometimes confined, for a long while, to the legs; but ultimately it creeps up towards the trunk of the body; the loins and flanks become doughy, the scrotum fills, and water collects in the serous bags of the abdomen and thorax. In extreme cases the dropsy is universal, pervading the cellular tissue of the head and face and upper limbs.

As the accumulation of serous liquid is commonly gradual, the reticular tissue, partly perhaps through maceration, but chiefly from continued pressure, and stretching, loses its elasticity; and the œdema is soft, and *pits* readily.

Sometimes, the fluid continuing to increase, the cuticle is raised by it, and large vesications take place on the limbs; or some part of the cellular tissue sloughs, and a breach of surface is made, and the fluid drains off by this vent in great abundance, to the signal relief of the patient. This beneficial accident furnishes us with a valuable practical hint.

Such, then, is the consummation of disease commencing in the left heart, and working its gradual way, through the lungs, to the right heart. But the object may originate at a less distant link in the chain. The circulation may be checked, first of all, in the intermediate lungs: and it may be worth our while to consider, for a moment, the relations which subsist between general dropsy, and certain *pulmonary* diseases.

Whenever, in pneumonia, a large portion of one or of both lungs becomes impervious to air and to blood—or when pleurisy fills one side of the thorax with liquid, which, by its pressure, shuts out both air and blood at once from one half of the respiratory apparatus—the egress of the blood from the right heart, and, therefore, from the venous system, must be checked. We might expect that dropsical effusion would be the result of these changes; and in truth it does sometimes occur. That it does not happen more frequently is to be attributed, I believe, to the free evacuations and the strict abstinence, which are early put in force in those complaints, and which relieve the venous plethora before it produces effusion.

So, again, lungs that are hollowed out into large cavities, or rendered solid over a wide space by numerous tubercles, are manifestly incapable of admitting into their vessels from the right ventricle the ordinary quantity of venous blood. In these cases, however, the whole mass of blood is diminished, and kept within the limit which does not imply distension of the veins, by the constant agency of various causes; by the imperfect nutrition consequent upon abdominal disease; by the sometimes copious expectoration; by the wasting diarrhœa; by the profuse nocturnal sweats. Accordingly anasarca is an unusual symptom in pulmonary phthisis, or shows itself in the latter periods only of the disease, in the form of œdema of the legs: and its occurrence then is mainly owing to the debility which affects, in common with the other muscular parts of the body, the moving organ of the blood.

The pulmonary disease which more commonly and certainly than any other, though often very slowly, leads to dropsy, is *emphysema* of the lungs. I showed you, some time ago, that in this morbid condition many of the smaller blood-vessels of the lung become gradually obliterated; and when the disease is extensive and advanced, large portions of the organ are visibly white and bloodless. Meanwhile the nutrition of the body is not impaired; the same quantity of blood continues to be returned towards the heart, but it finds not a ready entrance into the pulmonary blood-vessels, when delivered from the right ventricle. A certain amount of accumulation becomes habitual in that chamber, and in the great veins; and at length the capillary vessels feel the mechanical congestion, and more or less anasarca ensues.

Renal dropsy.—So much for dropsy that is purely cardiac. Let us next consider the circumstances from which, during the life-time of the patient, we draw the conclusion that the dropsy he exhibits is of renal origin.

There is nothing, that I know of, very peculiar or distinctive in the characters of the anasarca itself. When the renal complaint sets in suddenly, and with acute symptoms, dropsy usually supervenes *soon*. So also, during its more chronic progress, anasarca is apt to show itself, or to increase, whenever local inflammation, or febrile disturbance, happens to arise. The more rapid and copious the effusion, the less do the dropsical parts pit upon being pressed. It may be said also of this renal form of dropsy, that accumulation in the larger serous cavities is not, in general, a prominent feature.

If we find, upon due scrutiny, no material or adequate embarrassment of the respiratory functions, no deviation from the natural sounds of the heart, no derangement of its regular movements, no alteration in the force of its pulsations, or in the space over which they can be felt and heard, no distension of the large veins of the neck—then we have strong reason for suspecting that the anasarca is connected with some vice of the kidney.

But we cannot infer, from the *presence* of heart-symptoms, that the kidney is free from disease.

Our judgment is guided, or assisted, in some degree, by the *complexion* of the patient. When general dropsy depends upon disease of the heart, the cheeks and lips are occasionally florid, often purplish or livid, frequently dusky and loaded. Sometimes (as in chlorotic women, where the heart may be temporarily distended without any positive organic disease, and the blood is thin and poor) the face and mucous membranes are pale: but in the renal variety of dropsy there is a very characteristic hue; an evident lack of red blood, indeed, in the capillaries, but withal an unhealthy dingy sallowness, significant, to a practised eye, of some deep-seated alteration of structure.

Our suspicion that the kidney is the organ primarily in fault is strengthened, if we trace certain accidents in the history of our patient. An attack, for example, of illness, attended, perhaps, with temporary swelling of the body and disturbance of the urinary functions (acute dropsy, in short,) soon after some exposure, under unfavourable circumstances, to the influence of cold; either applied to the external surface, or to the stomach by a draught of cold drink. For, as I told you yesterday, there is reason to believe that in *acute* dropsy is often laid the foundation of those peculiar changes in the kidney which, since they were first pointed out by Dr. Bright, have been chiefly studied in their connection with *chronic* dropsy. That as rheumatic carditis may occur, and become latent as to its effects for some time, and yet implant the germs of future cardiac dropsy, so the stress or strain which befalls the kidney in cases of febrile anasarca, may set on foot a morbid process that long works silently and unobserved, but at last declares its operation by symptoms; the reproduction of the dropsy in a more chronic form being the most significant symptom of all. The acute attack may have been forgotten; there may have been no obvious (though there may have been ill-understood) indications of the renal affection; and its existence has been, therefore, unsuspected.

The discovery of intemperate habits would also be of importance in aid of our diagnosis, if these same habits had not a like influence in causing disease of other organs as well as of the kidney, and especially of the heart. There may, however, be no such episodes as these in the patient's history. The dropsy may have come on immediately after some exposure, or obviously injurious influence, yet not with acute symptoms, and in a temperate subject: in which case it is probable that the renal disease had pre-existed in a latent state. Or the anasarca may have stolen on by degrees, without any apparent cause.

But the most conclusive evidence of the renal disease is to be found in the conditions of the urine: which were fully explained to you in yesterday's lecture. I shall therefore only add four rules upon the subject, which have been laid down by Dr. Christison, and generally acquiesced in by Dr. Prout. These rules assert that organic disease, or granular degeneration, or granular disorganization of the kidney—all which phrases we may condense into the compendious expression "Bright's kidney"—is present;

1. In most cases of febrile dropsy, including those which are consequent upon scarlet fever. To this proposition I have already given my full assent.

2. In all cases of anasarca, where the œdematous parts are elastic, and do not pit upon pressure. I am not so sure of this rule. I believe that the firmness of the œdematous parts has relation rather to the rapidity with which the effusion has taken place, than to the local origin of the dropsy. In proportion as the accumulation in the cellular tissue has been chronic and gradual, does the swelling receive and retain indentations from external pressure.

3. In most, or all cases of dropsy attended with diuresis, provided the urine be not saccharine. Such cases, Dr. Christison truly remarks, are far from being uncommon. Of course the diuresis here spoken of is such as occurs independently of diuretic remedies.

4. In all dropsies associated with urine of a very low specific gravity (say below 1010), and not exceeding the natural standard of quantity, whether it be albuminous or not. To the last two rules there are probably very few exceptions.

The dropsy which is dependent upon "Bright's disease" yields, not unfrequently, to treatment; but it is prone to recur. Of the renal disease itself the prognosis is, no doubt, very unpromising. Yet I do not think so despairingly of these cases as I once thought; and as many think. I have known a few instances of what seemed complete recovery

after well-marked symptoms of the disorder. Certainly success enough has followed careful treatment to encourage us to use all diligence in prosecuting such remedial measures as are already known, and in devising new ones.

I have been speaking of dropsies that are purely cardiac, and of dropsies that are purely renal. But I have already told you that disease of the heart and disease of the kidney, frequently go together; and I have endeavoured to estimate their relation, in such cases, to each other. When both organs are structurally affected, the disposition to dropsical accumulation must evidently be augmented. What share they have, respectively, in producing the dropsy, it would be very difficult, and practically it is not very important, to determine.

Treatment.—In chronic general dropsy of a purely cardiac origin, the kidneys, being sound, offer the most convenient and eligible channel for carrying off the superfluous water. Diuretic medicines, therefore, rank among the most important of our curative expedients. When they fail to act, or prove insufficient for the purpose sought, we next have recourse, the state of the bowels permitting, to drastic or hydragogue purgatives.

Diuretics are notoriously of most uncertain operation; sometimes completely answering our wishes, oftener perhaps disappointing them altogether. When the urine is strongly acid, and deposits, on cooling, a sediment like brick-dust, it may be well to try, at first, the alkaline diuretics, and particularly the salts of potass. Nitre added to the common saline draught; or a combination of the acetate and bicarbonate of potass; or the bitartrate in small doses; or the iodide of potassium; or the liquor potassæ. In my own experience the tincture of squills also has seemed to correct this superacid and turbid condition of the urine, while it increased its quantity.

The benzoate of ammonia is another salt which I have found to operate very powerfully, in several instances, as a diuretic.

Digitalis sometimes promotes, in a remarkable degree, the flow of urine; and this, in my judgment, is its most useful and manageable property. Small quantities of the tincture, or of the infusion, may be added to other formulæ. Or the powdered leaves may be combined in pills. But one of the best modes of exhibiting digitalis for this purpose is to give larger doses of the infusion, half an ounce, for example, in some cordial water, at intervals of four or six hours, till three doses have been taken in succession; and then to pause and note its effects; and to repeat the three doses, or not, accordingly.

The spirit of nitrous æther, and the compound spirit of juniper, have both of them well-marked diuretic properties, and may, with propriety, be added to most of the liquid formulæ for augmenting the discharge of urine. And, as vehicles for more active, or more concentrated ingredients, those vegetable infusions or decoctions should be chosen which are reputed to possess similar virtues; such as the decoction of broom-tops, or of juniper-berries, or of winter-green, or the infusion of buchu.

Squills, turpentine, the tincture of cantharides, are drugs of a more stimulant nature, more peculiarly adapted to cases in which there is no febrile disturbance, and the kidneys are obstinately inactive.

Sometimes a combination or farrago of diuretic substances proves more efficacious than larger doses of any of the ingredients administered singly: and the operation of some of these combinations is undoubtedly quickened and exalted, in many instances, by the addition of mercury. A fluid drachm of the officinal solution of the bichloride in each dose of a mixture; or small quantities of calomel or of blue pill when the medicines are given in a solid form. A very useful pill of this kind, much recommended by the late Dr. Baillie, consists of three or four grains of the pilula hydrargyri, mixed up with one grain of the dried powder of squills, and half a grain of the dried leaves of digitalis; to be given twice or thrice a day. Dr. Baillie states that squills and digitalis are much less effectual by themselves, than when combined with mercury.

In choosing purgative drugs to aid the effect of diuretics in carrying off the dropsical fluid, or to take their place when these fail to act, we select those which produce copious and watery discharges from the bowels. A combination of jalap and cream of tartar has been long and deservedly esteemed for its excellent operation in this way. Gamboge is also a good cathartic. It may be given two or three times daily, in grain or two-grain doses, with a drachm of cream of tartar, suspended in two ounces of peppermint water. Or

half an ounce of cream of tartar, mixed in six ounces of peppermint water, may be administered in one dose every morning. Croton oil, and elaterium, are still more powerful evacuants of serous liquid from the intestines. One or two drops of the former, or from a quarter of a grain to a grain of the latter, will be about a proper dose. It is astonishing how much relief to the feelings of the patient, and how great a diminution of the dropsical symptoms, are sometimes obtained by these active cathartics. Patients will earnestly beg for a repetition of them, even when their operation is attended, for the time, with considerable pain or sickness, and much general distress.

In addition to these measures for the removal of the collected water, attention must be paid to the actual condition of the heart. If the dropsy has been the result of anæmia, or of cachexy of the system, you must endeavour to strengthen your patient, and to repair his impoverished blood, by nutritious food and tonic medicine, and especially by the administration of steel. I told you formerly that preparations of iron have an exceedingly good effect, oftentimes, even in cases of *organic* disease of the heart, consisting in dilatation and tenuity, and consequently weakness of its muscular parietes.

On the other hand, if there be violent palpitations of the heart, with a strong heaving impulse, you may appease the excessive action, and afford sensible comfort to the patient, by applying leeches, from time to time, to the præcordia.

Of the *renal* variety of chronic general dropsy, whether pure or mixed, the treatment is less accurately ascertained. The uncertainty which has perplexed men's minds respecting the nature of the renal disorder has extended, in some measure, to their choice of remedies for it.

Whenever (in renal dropsy) acute symptoms and febrile disturbance occur, much relief may be expected from the abstraction of blood. When drawn from a vein, it usually shows the buffy coat. The existence at the same time of pain in the loins, would indicate the propriety of applying cupping glasses to that part. Nevertheless, the impoverishing effect of the disease itself upon the blood, and the probable dependence of some of the more distressful and alarming symptoms upon the thin serous condition of the circulating fluid, as well as the increased facility with which the dilute blood may transude outwards—these are circumstances which should induce every cautious practitioner to have recourse to this heroic remedy, only when it is clearly *demanded*.

One definite object, in the renal as well as in the cardiac variety, is to remove the dropsical fluid, from which the danger and the suffering often chiefly proceed. But it is a more nice question, when the *kidney* is involved in the disease, how this is to be accomplished. Can we, with the same safety as in cardiac cases, employ diuretics? It has been thought that we cannot. As the primary morbid state of the kidney is certainly often, and probably always, a state of congestion, it has been feared that direct diuretics, such as are calculated to cause, keep up, or augment congestion of the kidney, or to stimulate and irritate that organ, would be likely to accelerate the disorganizing process of which it is already the seat.

Now, although these views are theoretical, and their justness has not yet been established by positive proof, you had better, when you can, observe the caution which they suggest. You had better endeavour to empty the distended cavities, and to relieve the loaded cellular tissue, through the bowels, or through the skin. Sometimes, however,—more often indeed than in cardiac dropsy,—we have the untoward complication of irritable bowels, or habitual diarrhœa: and then drastic cathartics are inadmissible. But when this complication is not present, they are eminently useful.

Much benefit is sometimes derived from measures that act powerfully or steadily upon the cutaneous transpiration; and especially from warm, or hot-air, baths. The hot-air bath is, in many respects, preferable to the common warm water-bath, and even to the vapour bath. Upon the principle of heterogeneous attraction, the escape of the liquid from the surface of the body is more promoted by a dry heat than by water artificially raised to a high temperature, and even than by an atmosphere made moist, as well as hot, by vapour. The risk, moreover, of exposure to cold, and the inconvenience and hazard of fatigue, are much less: for the hot air can be brought, with but little trouble or expense, to the patient as he lies in bed. I have often applied the *sudatorium* to patients

labouring under renal dropsy, and they have obtained much relief from it: but the relief is seldom of itself sufficient, or of more than temporary duration. Still it is an expedient that should never be neglected; and in pursuance of the same indication, diaphoretic medicines should at the same time be diligently exhibited. Dr. Osborne states that when the renal disease has been uncomplicated with other organic mischief, he has always found the dropsy to disappear upon the re-establishment of the functions of the skin.

These measures failing—as fail they often will; and diarrhœa forbidding the use of drastic purgatives; or drastic purgatives and diaphoretics together proving insufficient; we *must*, even in renal dropsy, choose the least of two evils; or rather we must incur the risk of one possible and contingent evil, for the chance of obtaining what, if obtained, is a certain and positive benefit: we must endeavour to remove the dropsical accumulation by means of *diuretics*, whether these accelerate the progress of the disease in the kidney or not.

Such diuretics, therefore, are, in the first instance, to be selected, as seem the least likely to stimulate the kidneys injuriously. The bitartrate of potass has been found one of the most certain and useful. Digitalis also is esteemed safer, and therefore more proper for this purpose, than many others: and the simultaneous exhibition of these two has, perhaps, the surest effect of all.

When diuretic medicines act as we intend them to do, and increase the quantity of urine, they are commonly of great service by reducing the dropsical swelling. But they are apt to be very capricious and disappointing. In some unhappy cases of renal dropsy I have tried every known form and combination of diuretics, without augmenting the secretion from the kidneys. And sometimes, although a plentiful discharge of urine takes place, no impression is made upon the anasarca.

It is yet an unsettled question whether mercury be advisable, or even admissible, in these cases. The current of medical opinion sets against it; perhaps too strongly. It has been observed that salivation is apt to be produced by a small quantity of this drug; and to be unusually troublesome and severe, without bringing any commensurate advantage. I mentioned to you formerly Dr. Farre's opinion, that mercury has the property of rapidly destroying red blood: if so, it must be regarded rather as an ally, than as an antagonist, of this malady. On the other hand, some patients have appeared to recover altogether, after passing though a furious salivation. One of the reputed virtues of the mineral is that it promotes interstitial absorption—a property which the changes apparent in the kidney in renal dropsy would seem to render valuable.

When internal remedies prove ineffectual, and outward applications to procure sweating miss their aim, we turn to those mechanical expedients which (in either variety of general dropsy) often afford ease, and prolong life, and may even sometimes, perhaps, achieve a cure.

I have told you that the tense and stretched integuments occasionally give way; the cellular tissue sloughs, and from the breach thus made water wells copiously forth, and great relief ensues. Sometimes, though very rarely, the whole of the accumulated liquid has so escaped, and the dropsy has not reappeared. The sore has healed, and the natural cure has been complete.

This spontaneous mode of draining away the liquid has been imitated by art. For the unwieldy legs become painful as well as cumbrous; the integuments threaten to inflame, or to mortify; and if we can diminish the tension by removing a portion of the included fluid, we avert or lessen this danger. The penis and scrotum also in the male, and the labia pudendi in the female, become, in many cases, enormously swollen, and hinder the exit of the urine, which is therefore spilled upon the tumid parts, rendering them erythematous and raw, to the grievous aggravation of the patient's sufferings.

Now seeing that vesications sometimes form upon the dropsical limbs, and give vent, in some degree, to the fluid, practitioners have been induced to follow that indication by exciting artificial blisters. But they are highly dangerous; leading often to gangrene of the surface thus inflamed. Within my recollection it was the custom to make incisions, with a lancet, in the œdematous legs. These gashes seldom healed again; but degenerated into sloughing sores; and not unfrequently they hastened the dissolution of the patient.

A vast improvement upon these expedients is the modern practice of acupuncture, which consists in perforating the integuments here and there with a fine needle.

It is surprising how much fluid may be let out in this way; and how much relief may be bestowed by this trifling operation. The liquid trickles rapidly forth; and I have known it soak through the patient's bed, and form a pool on the floor of the room. In one instance, a physician being the patient, the limpid fluid which thus oozed from a puncture in the thigh was caught, and collected in a glass, by means of a little gutter of oiled silk. It was found that ninety minims, or a fluid drachm and a half, escaped in a minute; which is at the rate of eleven ounces and a quarter in an hour: and this drain went on for upwards of four hours.

The surface on which these punctures have been made sometimes becomes red; erysipelas supervenes, which it is difficult to arrest, and the patient sinks. In some of these cases the same event would probably have occurred, even although no punctures had been made, from mere tension of the integuments, and the progress of the disease. When such appearances present themselves, the affected limb should be kept in the horizontal position: and strips of linen, wetted with a solution of goulard, should be applied to the inflamed surface.

Under the old system of incisions it was found (and reason would teach us to expect this) that there was more hazard of sloughing when they were made on the legs, than on the thighs. The risk is much less when needles are used. But even these punctures are not to be made without attending to certain precautions. They should not be too near each other: an inch and half, at least, should intervene between them. Neither should they be too numerous, nor too deep. The depth must depend upon the circumstances of the case; and especially upon the place where the puncture is to be made. The needle must not be pushed so deep as to penetrate or wound any fascia; for the danger of subsequent inflammation would thereby be increased.

The peritoneum may, at the same time, require to be emptied in the same mechanical way, by help of a trocar. I am always loth to recommend tapping, until the symptoms actually call for it, and until all other means of dispersing the water have been tried in vain. But the circumstances that warrant or demand the performance of the operation, the dangers that attend it, and the means of obviating these dangers, were fully pointed out when we were on the subject of ascites.

By whatever means we may succeed in getting rid of the dropsy, there will still remain (except in the comparatively few cases that are unconnected with organic disease, and depend simply upon debility and anæmia) the necessity for guarding against the re-accumulation of the water, by remedial measures addressed to the faulty organs. You may sometimes keep the disease of these organs in check, even when you cannot cure it.

In cardiac dropsies, besides the medicines already specified, undeviating temperance and regularity of life must be enjoined; and the patient should carefully, and always, avoid all active motion or exertion of the body, and all strong emotion of the mind; whatever, in short, might tend to hurry the circulation. You will scarcely be able to enforce this prudence, without plainly showing the patient the risk he will incur by its neglect.

In the renal variety of the disorder, in addition to the appropriate remedies already enumerated, particular attention must be paid to the avoidance of all exposure to cold and vicissitudes of the weather, and to keeping the surface of the body warm. Such patients should be constantly clothed in flannel from head to foot. To those who are able to choose their place of abode, I should strongly recommend resort to a warm climate. Some benefit may also be hoped for from counter-irritation—blisters or issues to the loins.

The diet in the chronic forms of the disease should be nutritive, but unstimulating. M. Solon suggests that if, in the renal cases, urea be detected in the blood, the patient should be restrained from too animalized a diet. Dr. Budd has had the same thought, and has put to the test, I believe, in the Hospital-ship Dreadnought, the utility of withholding all articles of food that contain azote. I have found this restriction entirely useless in one painful case, in which it was fairly enforced. In fact, the principle of such restriction appears to be wrong: the urea is furnished to the blood, not in the primary assimilative process, but in that which is secondary and destructive.

One more point, and I have done. Much unnecessary penance used to be imposed

upon dropsical persons, by stinting their allowance of drink. It was natural to suppose that the accumulation would increase in proportion to the quantity of liquid swallowed. But experience has shown this opinion to be erroneous, and

"crescit indulgens sibi dirus hydrops"

has ceased to be more than a poetical doctrine. The patient may safely be allowed to exercise his own discretion in this respect. When the peritoneum is full, distress is apt to ensue upon the distension of the stomach by drinks; but this source of suffering is soon discovered and avoided. The sick man is better able than his physician to judge which evil is the greatest; the torment of unslaked thirst, or the discomfort that may be produced by its immoderate indulgence.

LECTURE LXXX.

CHYLOUS URINE. HÆMATURIA; ITS DIAGNOSIS, GENERAL AND PARTICULAR; LOCAL DISORDERS OF THE URINARY ORGANS ON WHICH IT DEPENDS; TREATMENT. ABDOMINAL TUMOURS.

I HOPE I have convinced you that the morbid conditions of the urine are worth studying. You have heard, probably, of the quacks who call themselves "water-doctors," and who pretend that, by mere inspection of the urine of a patient living at a distance, they can tell what is the matter with him, and how he may be cured. This skill, which looks like conjuration, the scientific physician of the present day does really possess. Of some very important forms of constitutional disorder, and of some specific local maladies, he reads the sure evidence in the sensible and chemical qualities of the secretion from the kidneys. And I do not hesitate to say, that a rightly instructed person might form a more accurate opinion respecting a sick man fifty miles off, and prescribe for him more judiciously, upon being furnished with a phial of his urine, than some practitioners whom I have known could do, if they had the patient bodily before them. You may learn much (and so, no doubt, you ought) by prying into the arcana of the night-chair: but you may learn more, I am persuaded, by the habitual perusal of the chamber-pot.

I have not yet quite done with the subject.

Chylous urine.—Sometimes urine is voided which appears to contain *chyle*. It looks white and milky, and stiffens as it cools into a tremulous jelly, like *blanc mange*, and takes the shape of the vessel into which it was passed. The coagulum gradually separates again into two portions: one of which is liquid and whitish, and when left at rest for a few hours throws up to the surface a sort of creamy matter, containing (as cream does) a butyraceous or oily principle; the other is a delicate fibrinous mass, of flesh-like appearance, having a red tinge from the presence of some of the colouring matter of the blood. This is the character of the urine passed a few hours after a full meal. When the patient has long been fasting, the urine is simply opalescent, and the coagulum small and partial.

Dr. Prout attributes this curious deviation from the natural qualities of the urine to a double fault; first, in the organs of assimilation; secondly, in the functions of the kidney. The chyle, from some derangement of the assimilative process, is not raised to the blood standard; and being unfit for its purpose, is ejected through the kidneys; and these organs, instead of converting it, as usual, into the lithate of ammonia, suffer it to pass through them unchanged.

Of this rare disorder I have not met with an instance. Dr. Prout has seen more or less of thirteen cases. Five of the patients were males, eight females. Two of the males, and one of the females, were below the age of puberty. Seven of the cases occurred in

natives of the East or West Indies, or in persons who had lived for many years in hot climates. Mr. Thomas informs me, that during a residence of ten years in Barbadoes, he saw at least a dozen well-marked examples of chylous urine in negroes. It would seem, therefore, that a tropical climate predisposes certain individuals to this affection. In one case, drinking cold water while the body was warm seemed to have been the exciting cause; and exposure to cold was thought to have had something to do with the attacks in other cases.

The general health suffers less than you might suppose. Two of the females, while labouring under the complaint, became pregnant, and brought forth healthy children: and one of the two lived nearly twenty years, with the disorder upon her all the time.

In the slighter cases there is usually some degree of feverishness, some uneasiness in the back and loins, some thirst, a dry skin, and torpid bowels. When the malady is more severe, the symptoms approach to those of diabetes; the thirst is more urgent, the appetite unnaturally craving; and there is some degree of emaciation and debility. In this severer variety the urine is apt to coagulate before it leaves the bladder; and the patient experiences difficulty in passing it, the urethra being blocked up by the clots. Dr. Prout states that he has known this to constitute the most troublesome symptom of the disease. In one of the fatal cases the body was examined, and the kidneys found perfectly healthy. Occasionally the complaint ceases of its own accord, even for years, and then recurs, without any apparent cause. We may conclude from these circumstances that it is a purely functional disorder.

With respect to treatment, little can be said. Dr. Prout has found several things of temporary service, in the chronic state of the affection. The mineral acids; astringents, such as alum, and the acetate of lead; opium; counter-irritation. But the suspended symptoms have always soon returned.

Hæmaturia.—Lastly, the urine may contain *blood:* and I proceed to consider some of the phenomena that occur in connection with *hæmaturia;* under which term I would comprehend every kind of bleeding from the urinary organs.

Blood alters, of course, the colour of urine with which it is mixed; giving it, in some instances, a bright red tinge, and causing it, in others, to assume a dark hue; to become brown, like coffee, or even to approach to blackness. Hence we are sometimes too ready to conclude that urine of a distinctly red colour, or so very dark as to appear almost black, derives its peculiar tint from blood that has somehow mingled with it.

But, in truth, urine may be perfectly red, or nearly black, and yet be quite free from blood. There are certain substances which, when taken as food, invariably impart a red colour to the urine. One of these is the prickly pear, or Indian fig as it is sometimes called, the *cactus opuntia* of botanists. When the Spaniards first took possession of America, many of them were alarmed by observing that they passed what they supposed to be bloody urine; but it was soon discovered that the red colour of the secretion was owing to the liberal use they made of that fruit. Dr. Hennen, in his book on *Military Surgery*, quotes a precisely similar example from Elliot's Journal of his Travels for determining the boundary of the United States. He says that his "people ate very plentifully of this substance at an island of the Mississippi (Kayo-ani), and were not a little surprised the next morning at finding their urine appear as if it had been highly tinged with cochineal. No inconvenience resulted from it. It would seem (he continues) that the juice of this plant may be analyzed into a crimson dye by other processes besides that of the cochineal insect."

Another vegetable substance with which, in this country, we are more familiar, and which will produce the same effect, is *beet-root*. Desault relates the case of a person who noticed that he every morning voided urine of a deep red colour; exactly such as would result from adding fresh blood to that liquid, except that no deposit took place. The man became frightened at this, and consulted M. Roux, who, after some examination, began to suspect that the water owed its red appearance to some other cause than an admixture of blood. It turned out that his patient was in the habit of supping every night upon the red beet-root; and as soon as, by M. Roux's advice, he relinquished this article of diet, he was freed at once from his supposed bloody urine, and from his fears.

A similar tinge is said to be given to the urine by the use of *madder* as food, by some species of strawberries, and by drinks made of sorrel.

It is right that you should know these facts: for by eating large quantities of such substances, and by complaining of sensations which they do not really feel, impostors may endeavour, without any difficulty, or pain, or danger to themselves, to deceive others into a belief that they are suffering under some serious and disqualifying disease, and are proper objects of charity. Moreover, a knowledge of the efferts of these vegetable matters may sometimes enable you, as in the case mentioned by Desault, to remove unfounded anxiety and alarm from the minds of persons who are innocently and unconsciously giving themselves red urine.

The natural tint of the urine inclines towards redness, independently of any admixture of blood, in many instances of fever, and of acute inflammation. Occasionally urine of a pink colour is passed by persons who are subject to obstinate dyspepsia connected with organic disease. This pink tint is most apparent when the water is contained in an opaque, shallow, white vessel.

Again, urine of so deep a colour as to be called, in common parlance, *black*, may or may not owe that hue to the presence of blood. When blood is the cause of the unnatural colour, the blackness must be ascribed to the chemical action of some free acid upon the blood: as I showed you formerly to be the case with blood that is vomited, in hæmatemesis.

I also pointed out to you some little while since, that the urine, in jaundice, sometimes *seems* to be black, when it is collected in large quantity, and in a deep vessel. This colour is merely a concentration of yellowness, as appears at once by diluting the urine with water. It then assumes a bright yellow colour. The aspect of the skin in icterus will always secure you against mistaking or overlooking this cause of black urine.

There has been observed, also, though very rarely, a form of black urine, depending upon the presence in that secretion of a peculiar principle to which Dr. Marcet gave the name of melanic acid. The only specimen of really black urine that I ever saw, was shown to me by Dr. Prout; who knew nothing, however, of the circumstances under which it was voided. It appeared to me to be full of coal-dust.

With these two exceptions, almost all urine that is of a very dark or blackish colour owes that quality to the circumstance of its containing blood, which has been more or less altered, by various causes, from its original appearance.

When blood is present in any considerable quantity, a portion of it subsides to the bottom of the vessel, and may be recognized without any difficulty. And even when there is not enough blood to give a marked and characteristic deposit, a very small admixture of it will be found to disturb the natural transparency of the urine, rendering it of a smoke-brown, or dull cherry colour: whereas the reddish or pink urine which contains no blood is clear and untroubled; and if, on cooling, it throws down a sediment, that sediment may be redissolved by heating the urine—a result which does not take place when a portion of blood has been deposited. Another rough test is, that a mixture of urine and blood tinges a piece of white linen dipped into it, of a red colour. But the simplest and most satisfactory criterion is afforded by gradually raising the suspected urine to the boiling temperature. If it contains blood, a grayish brown flocculent precipitate, consisting of coagulated albumen tinged with the colouring matter of the blood, will form, and gradually subside, and leave the supernatant liquid clear, and with its natural tint.

We have by no means accomplished the diagnosis when we have merely ascertained that there is blood in the urine; and that the case is a case of hæmaturia. The question remains, of what is such hæmaturia a sign? The blood emerges from the urethra, but it may have been poured out at any point of a long and somewhat complex tract of mucous membrane. It may have proceeded from one or both of the kidneys; from each or either ureter; from the bladder; from the prostate gland; or from the urethra.

Hæmaturia strictly idiopathic must be very rare. Cullen remarks that neither he nor any of his friends had ever met with an instance of it. I shall mention presently the only example of hæmorrhage from the urinary organs, apparently idiopathic, that has fallen under my own notice.

Blood is excreted with the urine in that acute affection of the kidney which I have

already spoken of as lying at the root of most, if not all cases of febrile dropsy. The albuminous urine proper to the chronic form of Bright's disease sometimes contains the colouring matter as well as the serum of the blood. Hæmaturia is occasionally, I believe, vicarious of some other hæmorrhage, and especially of bleeding from the hæmorrhoidal vessels: so that it is always right, in obscure cases, to inquire whether the patient has been habitually subject to hæmorrhage from the rectum; and if so, whether that hæmorrhage is suspended. These cases have even been called *hæmörrhoides vesicæ*.

Hæmaturia occurs also, independently of any strictly local complaint, in the course of certain disorders which affect the system at large; especially in scurvy and purpura hæmorrhagica. Bloody urine is sometimes a symptom, and one of the most fatal augury, in typhus fever, small-pox, measles, and the plague.

But setting aside these more general forms of hæmaturia, let us inquire what local affections of the urinary organs themselves may give rise to hæmorrhage; and how, under different circumstances, we are to interpret this symptom.

One very common source of hæmorrhage from the urinary passages, is the presence within them of calculous matter. The pressure occasioned by the aggregation of the earthy mass, when it is formed in the kidney, or by its accidental change of position, lacerates, or lays open by ulceration, some of the smaller vessels with which it is in contact. And in those cases in which a calculus descends into the bladder, and is ultimately voided, it may, in succession, give rise to hæmorrhage, first from the kidney from which it is separated; secondly, from the narrow tube of the ureter through which it is forced; thirdly, from the bladder which it enters, and wounds, or irritates; and fourthly, from the urethra in the last stage of its progress out of the body.

There will be the same liability to hæmaturia, if the concretion, instead of coming down from the kidney, is formed originally in the bladder. The appearance of blood in the urine suggests therefore, in many cases, the fearful suspicion, that there is, or is likely to be, a stone in the bladder. Dr. Heberden, in his *Commentaries*, says, "urine made of a deep coffee colour, or manifestly mixed with a large quantity of blood, has within my experience been very rarely the effect of any thing but a stone in the urinary passages. I therefore suppose a strong probability of this cause, whenever I see this appearance."

Again, blood may proceed from the kidney, or from the bladder, in consequence of malignant fungous growths, to which those parts are liable· a disease which, though more surely fatal than the stone, is scarcely, to the unhappy subject of it, so appalling.

Hæmorrhage may take place from the surface of the bladder from chronic disease, not cancerous, of that membrane. Mr. Howship has recorded an instance of this kind which occurred in Mr. Heaviside's practice. An old East Indian, who had long been subject to nephritic complaints, was suddenly seized with what was thought to be retention of urine. A catheter was passed, but as no water flowed it was supposed that it had not entered the bladder, in the situation of which there was a manifest tumour. The patient died the next day; and the bladder was found distended by a very large coagulum of blood which had come from the diseased mucous membrane. There was no trace of hæmorrhage in the kidneys, nor in the ureters.

I hold in my hand a preparation showing disease of the prostate gland, which had been accompanied by hæmaturia.

Now we judge of the exact seat of the hæmorrhage, and of its cause, partly by the nature and appearance of the effused blood, and partly by the symptoms that precede or accompany the bleeding.

Dr. Prout states that "when blood is derived from the *kidney*, it is in general equally diffused throughout the whole urine: on the contrary, when derived from the bladder, the blood for the most part comes away in greater or less quantity at the termination of the discharge, the urine having previously flowed off nearly pure."

There are also certain modifications of the sensible qualities of the excreted blood, by means of which the same distinguished physician thinks he can pronounce, with considerable confidence, that the hæmorrhage is owing to malignant disease. "The red particles of the blood (he says) discharged in the earlier stages of fungoid disease, have often a remarkable appearance, and appear to the eye larger than natural; so that after they have subsided to the bottom of the urine, they at first sight somewhat resemble grains of lithic

acid gravel, and, like that substance, when the vessel is inclined, may be distinctly seen to roll along the bottom. From this peculiar appearance of the red particles of the blood, the presence of malignant disease may be often suspected before the symptoms assume a decided character." In a more advanced stage of the disease, there is often a dark-coloured offensive bloody sanies in the urine, and more or less of mechanical impediment in passing it. I should conceive that the microscope might aid the diagnosis of such cases.

There is one phenomenon which, whenever it occurs, is very characteristic of hæmorrhage from the kidney, or the commencement of the ureter. I mean the expulsion, with the urine, of slender cylindrical pieces of fibrin, which have evidently been moulded in the ureter, and subsequently washed down into the bladder by the descending urine. These little coagula are commonly of a whitish colour, the red particles of the blood having been removed; and they look like slim maggots, or small worms. They denote, with much certainty, that the hæmorrhage which they accompany is renal.

Such, then, are some of the points of diagnosis furnished by the qualities of the excreted fluid itself.

The bleeding may be presumed to come from the kidney, or from the upper part of the ureter, when it is accompanied or preceded by a sensation of heat, or of weight, or by some degree of pain, in the situation of the kidney; especially if these uneasy feelings are confined to one side of the body. This presumption will of course be strengthened if calculi have been known to descend from the kidney; and converted into certainty if the patient suffers, together with the hæmaturia, a fit of the gravel; and if there be no symptom of stone, or of disease, in the bladder.

On the other hand, when no symptoms referable to the kidney or the ureter are present, while there are signs of stone, or of disease of the bladder, or of a diseased prostate—a mixture of mucus with the blood; occasional retention, or a sudden stop in the stream, of urine; pain referred to the glans penis immediately after the bladder is emptied—then we conclude that the blood proceeds originally from that receptacle.

When pure blood comes away, either *guttatim*, or in a stream, unmixed with urine, and neither preceded nor accompanied by any desire to make water, it is probable that the *urethra* is the locus of the hæmorrhage.

Bleeding from the surface of the urethra doubtless may, and commonly does, proceed from some mechanical injury done to that channel: as in the passage outwards of a fragment of stone, or inwards of a surgical instrument. But it is probable that blood is sometimes exhaled from the same membrane in considerable quantity, under circumstances which favour or produce a strong determination of blood to the genital organs. A young man came to the Middlesex Hospital with hæmorrhage from the urethra, and said that he had lost a considerable quantity of blood in this way, within a few hours. The hæmorrhage appeared to have been the consequence of excessive indulgence in sexual intercourse. His own account of the matter was that he had passed the night with a female, in whom the monthly period had just returned; and he ignorantly fancied that the hæmorrhage from his own person was the result of a sort of contagion. However, the bleeding was permanently arrested by the introduction of a bougie, which was allowed to remain for a short time in the urethra. This was the solitary instance to which I alluded just now, of (perhaps) idiopathic hæmorrhage, occurring within my own knowledge. When the hæmorrhage comes originally from the urethra, the blood may regurgitate into the bladder, and coagulate there; and mislead an observer into the belief that the hæmorrhage was vesical.

It appears, then, that, in many instances, certain local symptoms are associated with hæmaturia, and point distinctly to the part of the urinary apparatus whence the blood proceeds.

But many cases are very obscure. Blood sometimes appears, mixed in greater or less quantity with the urine, when there is no pain, nor any other sign which would lead us to fix upon one part rather than another as the source of the hæmorrhage. Now I believe that hæmaturia, bearing this indeterminate character, will generally turn out to be *renal*, and to depend upon earthy concretions in the kidney. It is true that the hæmorrhage which results from cancerous disorganization, whether of the kidneys or of the bladder, may also be painless. But cancerous disease of these organs (unless it extends

from parts in the neighbourhood, as from the rectum, or from the uterus, to the bladder) is very rare; and when it does occur, the nature of the case may usually be ascertained from those peculiar qualities of the effused blood which I have mentioned as being characteristic of malignant growths.

A calculus can seldom remain long in the *bladder*, at any rate will seldom cause bloody urine, without giving some other notice of its presence there: but concretions form in the kidney, sometimes in great numbers, and reach a considerable size, and remain there long, without furnishing any signal from which we might suspect their existence; except (perhaps) the occurrence of hæmaturia. We know this, because calculi are frequently met with in the kidneys of persons who had never suffered any pain or obvious derangement of the urinary organs during life; and because, in other persons, in whom such calculi pass down from the kidney towards the bladder, the first notice of their existence is often given by the acute suffering they inflict during their transit through the narrow ureter.

Yet though calculi may lodge in the infundibula, or in the pelvis, of the kidney, without manifesting their presence by exciting pain, it is very conceivable that, by progressive enlargement they may lay open, or, by accidental change of position they may wound, some of the smaller blood-vessels of the part, and so give rise to painless hæmaturia. It will strengthen the presumption that such is the source of the bleeding, if it has succeeded (as hæmorrhage from the urinary passage often does succeed) to a fall; a shock, or jar of the body; or jolting on horseback or in a carriage. Similar movements may occasion bloody urine when there is stone in the bladder; but then the irritation will be *felt* in that sensible part; the hæmaturia will *not* be painless; the bleeding will not be the only symptom.

It is, then, I say, my belief that very many of the obscure cases of hæmaturia may be referred to renal calculi: and if this view of the subject be correct, it will render it probable that the alleged instances of idiopathic hæmorrhage from the kidneys ought thereby to be reduced in number.

The expulsion of the blood in hæmaturia, whether it be painful or not, is sometimes attended with severe rigors. I mentioned before, that, in some persons, almost any irritation of the urethra, the passing a bougie, for example, will bring on a shivering fit. I had some time ago a patient under my care in the hospital, who had hæmaturia of an obscure kind, and the discharge of blood was always marked by a smart rigor. Dr. Prout speaks of an instance of obstinate hematuria in which a shaking fit constantly preceded the hæmorrhage. Dr. Elliotson, too, in one of his lectures, gives an account of a case of intermittent hæmaturia. The patient was under his care in St. Thomas's Hospital, and had formerly had the Walcheren fever. He was admitted for ague, and every time that the cold stage of his attack came on, he voided a quantity of pure blood from the urethra. He was cured, by quina, both of his ague, and of his hæmorrhage.

If we may trust to the records of physic, instances of periodic hæmaturia are not uncommon.

One circumstance yet remains, worth noticing, in respect to hæmaturia; and it depends upon the hæmorrhage itself rather than upon the disease of which the bleeding is a sign. I allude to coagulation of the effused blood in the bladder, however it may have got there. This circumstance is sometimes the source of much inconvenience, and suffering, and even of danger, to the patient. It may cause retention of urine, and all its evil consequences; and a still worse event is, that the coagulum sometimes supplies a nucleus, around which calculous matter is deposited, and thus lays the foundation of that horrible malady, "the stone."

Treatment.—You will perceive, from what I have said, that the *treatment* of hæmaturia resolves itself, in most cases, into the treatment of the disorder, or bodily condition, with which the hæmorrhage is associated, and of which it is merely a symptom.

Sometimes, however, the bleeding itself is so profuse, or so long-continued, as to require direct efforts on our part towards its restraint.

"When (says Dr. Prout) the bladder becomes distended with blood, and complete retention of urine in consequence takes place, recourse must be had to a large-eyed catheter, and an exhausting syringe, by the aid of which, and the occasional injection of cold water, the coagula may be broken down and removed. If the hæmorrhage be so profuse that

the bladder becomes again distended with blood in a very short time, the injection of cold water into the rectum or bladder is sometimes of great use; and should these means fail, from twenty to forty grains of alum may be dissolved in each pint of water injected into the bladder; a remedy that seldom fails to check the bleeding, even when the cause is malignant disease. I have never known any unpleasant consequences follow the use of this expedient, and have seen it immediately arrest the most formidable hæmorrhage, when all other means had failed; and when the bladder had repeatedly become distended with blood, almost immediately after its removal."

Among remedies given by the mouth, the same physician thinks highly of the acetate of lead. I have mentioned before, in these lectures, a nostrum called, after the name of its inventor, *Ruspini's styptic*. This has often been known to put a stop to hæmorrhage which had resisted other remedies. I will read you one example of this from Sir Benjamin Brodie's published lectures. Speaking of hæmaturia, dependent upon disease of the prostate gland, he says: "Those medicines which operate as styptics when taken internally, and which are useful in cases of hæmorrhage from the lungs, are also useful in hæmorrhage from the prostate. I had a patient with very diseased prostate. A frightful hæmorrhage took place. The usual methods of treatment were adopted, but were of no avail. The skin became pale, the pulse became weak, and the patient was exhausted; yet the bleeding continued. Large quantities of blood were drawn off with the catheter: nevertheless the bladder continued to become more and more distended with blood, and was felt prominent in the belly as high as the navel. All other remedies having failed, I gave the patient a dose of the nostrum known by the name of Ruspini's styptic, and repeated the dose two or three times in the course of the next twelve hours. In about half an hour after the first dose was taken the hæmorrhage ceased; and it never returned. The patient lived a year and a half afterwards, and there was no reason to believe that any ultimate harm arose from the bleeding."

For a long while this nostrum seems to have baffled analysis. The late Dr. Maton told me that Dr. Wollaston had examined it, and arrived at the negative conclusion, that it contained no metallic substance. Dr. A. T. Thomson has since announced that it mainly consists of a solution of *gallic acid* in alcohol diluted with rose-water.

There is no substance more highly spoken of as a remedy for internal hæmorrhages by foreigners, and especially by the French, than the extract of *Rhatany* root, the *Krameria* of our Pharmacopœia. A female was sent to me by my colleague, Mr. Arnott, complaining that for some weeks she had been passing bloody urine. She had gone through the ordinary routine of treatment without benefit. There were no symptoms present which threw any light on the precise source or cause of the hæmorrhage. I recommended a trial of the rhatany, and she began to take a scruple of the extract, mixed with water, three times a day. As in Sir Benjamin Brodie's case, the hæmaturia ceased after the first dose, and it did not return for many months. I mention this instance the rather, because the *gallic acid* enters into the composition of this vegetable extract also.

Now the gallic acid is one of those substances which, when introduced from the digestive organs into the blood, passes through the round of the circulation unchanged, and reappears in the urine. We may conceive, therefore, that it stays internal hæmorrhage by exerting its astringent property upon the ultimate capillary blood-vessels in its passage through them. It certainly is applied, in solution, after its elimination from the blood, to the urinary passages: and thus, in hæmaturia, it may be presumed to produce its styptic effect upon the bleeding surface.

To the same principle are owing, I believe, the astringent and styptic virtues of the uva ursi, bistort, tormentil, the pomegranate, kino, catechu, and the several preparations of gall nuts. I am sorry that I have not had much opportunity of trying the gallic acid itself, in its separate state; for I have heard of several instances of its successful employment in cases of internal hæmorrhage. If, as I believe, it really has this power, it is very desirable that it should be prescribed in a more definite and precise manner than is afforded by its natural combinations with so many different vegetable matters.

Abdominal tumours.—I am unwilling to take leave of the cavity of the abdomen, without saying a few words (very few they must be) respecting the various kinds of

tumour to which it is obnoxious. It may seem strange that the diagnosis of abdominal tumours, which manifest themselves to the touch, and to the sight, should be so difficult and puzzling as it often is. I mentioned some reasons for this before: the loose and shifting manner in which some of the viscera of the belly are packed and fastened; their liability to enlarge beyond their natural limits; their accidental dislocations under disease. It would be vain to attempt even a sketch of the infinite variety of these deviations from the healthy state. Every case of abdominal tumour forms a separate object of study, and must be judged of by its proper circumstances. All that I can profess to do, is to offer you some rough hints on this interesting subject.

Some kinds of tumour result from morbid growths; such are all the varieties of cancer: some from the presence and multiplication of parasites; of which we have examples in collections of hydatids: some are produced by the distension of hollow organs; as when concretions, or fæcal matters, or gases, lodge in the intestines; or when urine accumulates and is pent up in the bladder: some consist in the mere enlargement of parts.

Let us enumerate the principal of these; that you may know what chiefly to expect.

1. There are, I say, tumours from lodgements in the *bowels;* and these are more hopeful than most kinds of abdominal tumours. Sometimes the stomach, or some part of the intestinal canal, is distended in consequence of a mechanical impediment to the course of its contents: and this impediment may be invincible.

2. *Ovarian* tumours are very common. Of these I spoke at some length in a former lecture.

3. The *liver* is very liable to enlargement: either from simple congestion of blood; or from the interstitial deposit of adipous matter; or from the intrusion of malignant growths; or from colonies of hydatids.

4. So also the *spleen* swells, from fulness of blood, or from specific deposits in its substance.

5. The *kidneys* sometimes attain a vast size; being occupied by malignant disease, or swollen by pus that finds no vent.

6. Enlargements of the *mesenteric glands;* cancerous degeneration of the peritoneum, especially where it forms the *omentum;* tumours connected with the *uterus;* aneurisms of the *aorta;* vast distention of the gall-bladder; constitute other species of abdominal swelling, which I simply mention without further comment.

Now our judgment of the character of a given tumour is naturally influenced by its *place.* In the right hypochondrium, we suspect the liver; in the left, the spleen; in the epigastric region, the stomach; in the hypogastric, the womb, or the bladder; in either flank, an ovary, or perhaps a kidney; in the track of the colon, we guess at fæcal collections.

But sometimes the situation of the tumour fits more than one, or than two, suppositions. Between the ribs and the ilium on the right side we may have an enlarged ovary, a tumid kidney, a distended cæcum. A prominence in the epigastrium may be due to cancer of the stomach, to an infarcted transverse colon, to a ventral aneurism. Above the pubes, the distended bladder, or the enlarged uterus, may equally project. The sigmoid flexure of the colon loaded with fæces, the left kidney exaggerated by disease, a bulky ovary, may either of them occupy the same sinistral space.

Moreover, the colon deviates strangely, and not seldom, from its natural course and position: and the magnified viscera may invade, by their displacement, or by their irregular expansion, the regions that are proper to other organs.

Our conjectures are assisted by the associated symptoms, and by observation of the regular performance, or of the disturbance, of particular functions. Yet here, also, we meet with continual sources of fallacy. Pressure from a tumour without may, as well as infarction within, impede the passage of alimentary matters through the bowels, of urine through the ureters; and cause, in the one case, flatulence and tormina, in the other, retention or suppression of urine. Growths foreign to the liver may, nevertheless, press upon its excretory ducts, and occasion jaundice. And so of other parts and functions. I mean, that the functions prominently deranged are not always the functions of the part occupied by the tumour, but of organs which are secondarily and accidentally subjected to its disturbing influence. Your sagacity will be abundantly tried in balancing the evidence of

different symptoms in these obscure, yet palpable, forms of disease: and after all you will often doubt; and often, when you do not doubt, you will mistake.

Enlargement of the liver may usually be distinguished from other tumours of the right hypochondrium, by *percussion*. Try from the clavicle downwards. At first, you get a hollow sound. Then, a little below the nipple perhaps, (for the spot varies much in different subjects) the sound begins to grow dull. If this dulness be traceable, without change or interruption, to the tumour, the inference is strong that the tumour is hepatic. Any other tumour there situate leaves, most commonly, when the patient is recumbent, a palpable sulcus above it; or a space in which the sound, upon percussion, is different from that which is yielded by the liver.

Percussion helps us to discriminate an ovarian from a renal tumour. When the swelling is large, the intestines lie behind the one, in front of the other: and the sound is affected accordingly.

Tumours are readily *movable*, are generally intestinal, omental, or ovarian.

A pulsating tumour is not necessarily an aneurism. The healthy artery will lift almost any sort of hard swelling that happens to lie directly over it.

The occurrence of hæmatemesis or melæna would corroborate your belief that a tumour in the right hypochondrium was hepatic—in the left, was splenic.

Even when you are satisfied as to the organ affected, there comes another question, scarcely, in some cases, less difficult than the first—What is the *nature* of the tumour?

Suppose, for the sake of illustration, that your inquiry relates to the liver. If the tumour be large, smooth, roundish, of slow growth, and the general health be not materially deranged, it is, most likely, an *hydatid* tumour. If along the edge and upon the surface of the augmented liver, you can feel large inequalities and projections, and if the complexion and general state of the patient are expressive of failing health, the enlargement is, in all probability, *cancerous:* and if there be other traces of carcinoma in the system, this conclusion becomes almost certain. Small hard irregularities betoken the *hobnail* liver; which is, sooner or later, accompanied by ascites. When, without pain or jaundice, the liver of a phthisical patient transgresses its natural boundaries, it is, presumably, a *fatty* liver.

By applying a similar method of investigation to other ventral enlargements, you may frequently hit the right scent, and trace the mischief to its true source. To treat the subject in detail would require a volume. I may refer you to a series of papers by Dr. Bright, in the *Guy's Hospital Reports;* where you will find a host of examples, and much valuable information, concerning the most common and the most important kinds of "abdominal tumours and intumescence."

LECTURE LXXXI.

Acute Rheumatism; Symptoms; Varieties; Treatment. Chronic Rheumatism; Phenomena; Plan of Cure.

Gout: Description of a Paroxysm; Progress of the Disease; general state of the Health in Gouty Persons; Causes of the Disease; Diagnosis between Gout and Rheumatism.

I proceed, this afternoon, to the consideration of that very common, very painful, and sometimes very perilous disease, *rheumatism*. There are two species of it, the acute and the chronic. They graduate, however, insensibly into each other; and the chronic is often a sequel of the acute form. Yet this is not necessarily so. Chronic rheumatism occurs in persons who have had no preceding attack of the disorder in its acute stage or degree.

Rheumatism implies inflammation: but, as I mentioned in an early part of the course, it is inflammation of a peculiar or specific kind. In the first place, it is inflammation of a particular tissue—the *fibrous tissue:* and it may therefore manifest itself wherever that

tissue is employed in the fabric of the body. No doubt the inflammation does involve other tissues also: but it is always, probably, by extending to them through what has been called *contiguous sympathy*. Thus we have the *synovial* membrane of a joint inflamed in many cases, the inflammatory action having spread from the fibrous textures around the joint: or, as I formerly pointed out to you more in detail, the serous surface of the pericardium, and the serous surface (or what is analogous to a serous surface) of the inside of the heart, and especially that part of it which is carried over the valves—each and all of these serous membranes are extremely liable to be affected with inflammation in the acute form of rheumatism: but in all of them it is probable that the *fibrous* tissue was the first to suffer. The pericardium is, as you know, a fibro-serous membrane; and fibrous tissue is interposed between the folds of the serous membrane, in the cardiac valves.

Rheumatism, therefore, is essentially inflammation of the fibrous tissue: and it most commonly seizes upon the fibrous parts that lie round the *larger joints:* the ligaments and the tendons: and in this respect you may almost consider the perpetually moving *heart* as one of the large joints. Yet this inflammation, when confined to the fibrous tissues, is not *common* inflammation. At any rate, it does not reckon among its events (as common inflammation does) either suppuration or gangrene. If suppuration sometimes occurs (and it certainly occurs very rarely) it is because the rheumatismal inflammation has extended to contiguous textures, and then has run the ordinary course of inflammation. Inflammation of the *cellular* tissue around a joint may thus inflame and suppurate. The inflammation of the synovial membrane *may* be of sufficient intensity to give rise to the formation of pus. When, however, the inflammation extends to the serous tissues within and around the heart, the products of the inflammation are just the same as when inflammation of the same textures, of the common kind, is anyhow produced.

Acute rheumatism, then, consists in redness, heat, pain, and swelling (that is to say, in inflammation) of the parts lying around, or entering into the composition of, one or more of the larger joints of the body; generally of several at the same time, or in succession; with a disposition to shift from one joint to another, or to certain internal organs, and especially to the membranes of the heart; and with fever.

This tendency to shift its place—to what is usually called metastasis—is a very remarkable feature of the disease. The inflammation will appear, in one joint, suddenly, and as suddenly subside in another which is previously occupied: and then, perhaps, it will jump back again to its old quarters. In many instances, however, it invades fresh joints without wholly ceasing, and sometimes even without diminishing at all, in those formerly affected. It may visit in this way every large joint in the body, and even seize upon some of the smaller ones; or it may possess nearly all of them at once. It is most commonly seen to affect the ankles and knees, the knuckles, wrists, and elbows. It is often seated in the shoulders also; and in the hips. The joints of the fingers frequently suffer; and I have seen one instance in which the joints of the jaws were manifestly implicated in the rheumatic inflammation. But by far the most serious and dangerous leap which the diseased action is apt to take, is to the membranes of the heart.

But I pointed out to you, in a former lecture, the symptoms of rheumatic carditis its consequences, the various ways in which it connects itself with the joint affection, and the treatment by which we are to attempt to moderate it. I dismiss, therefore, this, the most momentous complication of acute rheumatism, from the present discussion; and shall confine myself solely to the disease as it manifests itself externally.

The shifting and migratory inflammation of the textures lying round or composing the larger joints, is attended with high inflammatory fever; with a remarkably full, bounding pulse; with flushed cheeks; headache; profuse, drenching, sour-smelling perspirations, which distress and weaken the patient, but bring no relief to his pain; with a white-coloured, dirtyish, thick fur on the tongue, which is red, however, at its tip and edges; with turbid and acid urine. But this severe and inflammatory fever (*synocha* Cullen calls it) has no tendency to generate into a typhoid form: and *that* is a striking feature in the complaint. Neither is the intellect affected, except when carditis takes place: and then, as I stated formerly, violent delirium is apt to ensue, misleading the practitioner, drawing his attention away from the chest, where fatal changes are in progress, and fixing it upon

the head, where no inflammation at all exists, but which is disturbed through sympathy with the cardiac disorder. With this exception, we do not find patients in acute rheumatism *delirious*. Throughout all this febrile disturbance there is no coma, no marked trouble of the stomach or of the bowels, no vomiting, no diarrhœa, no petechiæ, no aphthæ, no sordes about the mouth, all which are of ordinary occurrence in the course of common continued fevers.

The joints are exquisitely *tender*, as well as painful. The fibrous tissues, which are endowed with but little sensibility in their sound and healthy state, become acutely painful when occupied by inflammation. The pain is increased by pressure; and therefore by whatever implies pressure; by movements of the joints consequently. The patients are reduced to perfect helplessness by the pain. Their common phrase is, that they have entirely lost the use of their limbs: and this is true enough in fact, but not true in the medical sense of those words. They have not lost the *power* of moving them; there is no *palsy;* but they *dare not* move them, because the effort gives them so much torment.

Pain in the affected joints is more constant than swelling; and swelling more constant than redness. The swelling differs also in different cases in rather a remarkable manner. In fact, there are two varieties of acute rheumatism: a circumstance first noticed by Dr. Chambers, at St. George's Hospital: and afterwards made public by Dr. Francis Hawkins in his Gulstonian lectures. The varieties are spoken of under the names of *fibrous or diffused* rheumatism; and *synovial* rheumatism. I apprehend, however, that in both of them the inflammation has its starting point in some fibrous texture: but that in the one, a considerable extent of that texture is implicated, while the inflammation does not involve the neighbouring synovial or serous tissue; and in the other, the extent of fibrous tissue affected in the outset is comparatively small, while the local symptoms are more expressive of the secondary synovial disease.

I will briefly state the distinctive characters of these two varieties of what is primarily and essentially the same malady.

In the one, then, the inflammation commences in the immediate neighbourhood of one of the larger joints: not *in* the joint, but *near* it. It attacks the tendons, fasciæ, ligaments, and possibly also the muscles. There is not at first much redness, or swelling; but after the pain has been of some duration, there is a puffiness around the parts affected, caused apparently by turgescence of the blood-vessels, and at length slight pitting, or *œdema*, may supervene, from effusion into the surrounding cellular tissue: and what redness is present is disposed in streaks, following the course of the tendons.

On the other hand, in the synovial variety, which shows itself more frequently and more plainly in the knee than anywhere else, the pain which marks the onset of the complaint does not last long before some degree of swelling is perceptible, together, in most instances, with slight redness of the skin: and this swelling is not due so much to turgescence of the blood-vessels, or to œdema of the cellular tissue, as to fluid poured into the *cavity* of the joint. And the form and character of the swelling indicate that it is the result of the fulness and distension of the synovial membrane; it is tight and elastic, and protrudes, as it were, through the spaces that intervene between the tendons and ligaments by which it is in other parts bound down and restrained: and *fluctuation* is often distinctly perceptible in the superficial joints, when both hands are applied to them.

These are the *local* differences between the two forms of the disease. And there are differences equally well marked between the constitutional symptoms that attend them.

It is in that form which κατ' ἐξοχήν is called *fibrous* rheumatism, that the inflammatory fever runs so high; that the tongue is so thickly furred; that the round, full, bounding pulse occurs; that the profuse, spontaneous acid perspirations break out, which exhaust the patient's strength without alleviating his sufferings; that the urine is high coloured, and deposits a copious sediment like brick-dust.

In the *synovial* form, the fever is either less intense from the beginning, or soon moderates after the joints begin to swell; the tongue is less foul; the patient sweats much less. It is to this form that the term *rheumatic gout* is often applied.

It is said that the tendency of the inflammation to fix upon the cardiac membranes is less noticeable in this than in the fibrous form; an important difference if it be real, as I am inclined to believe it is.

Now, of these two varieties the fibrous is infinitely the most common: and in the majority of cases, if the disease runs on, you will find the characters of each variety blending and mingling together: sometimes the synovial symptoms predominating, but much more frequently the others. And this is just what might be expected, supposing the notion to be correct that in both cases the fibrous tissue is primarily affected; only differently in degree and in extent.

I know of no other exciting cause of acute rheumatism than exposure to cold, and especially cold combined with moisture. And this is the reason why the disease is very common among the poorer classes of society, who are more in the way of that cause and cannot guard against it so effectually as their wealthier brethren; among whom it is comparatively rare.

Acute rheumatism is principally a disease of youth; prevailing most, I believe, from the age of puberty to that of thirty-five or forty. I have repeatedly, however, seen it in children; sometimes as early as the third or fourth year: and I stated to you some time ago, that the chance of the joint affection being complicated with rheumatic carditis is the greater in proportion as the patient is the younger. With, perhaps, one exception, I never knew the disease occur in an unequivocal form before puberty, without its being attended with inflammation of the lining or investing membranes of the heart.

Treatment.—I have already told you what I believe to be the proper plan of treatment to pursue when rheumatic carditis is present: in that case the affection of the limbs is of secondary consequence. But what are we to do when there is no complication of the joint disease; no invasion of any of the viscera?

Why, if you seek for instruction upon this matter in books, or even among practical men, you will meet with a very perplexing diversity of opinion. Apart from the cardiac affection, acute rheumatism has no *danger* about it; and the articular inflammation terminates, sooner or later, in recovery in most cases, whether the heart be implicated or not. And most persons who have been for any considerable time in practice have their own favourite method of conducting the disorder to its termination. While many employ free blood-letting, and other active antiphlogistic remedies, some, on the contrary, even in this present day, put their trust in bark. Some give large doses of calomel in the outset of the disease, such as half a scruple or a scruple, with or without a grain or two of opium; and they repeat the dose daily, or oftener, till the urgent symptoms give way; and in the manner I have seen the disease apparently cut short. But I have also known many instances in which the disease was painful, and protracted, and obstinate, although this practice was adopted early, and fairly carried out. Some physicians, again, give smaller and more frequent doses of calomel and opium; and some think opium alone to be as useful as this combination. Others depend mainly upon colchicum: others, upon large and repeated doses of conium: and some attempt the cure of acute rheumatism by sweating the patient by means of guaiacum and similar stimulant medicines, and a profusion of bedclothes.

Now you may be sure—when men's opinions concerning the treatment of a disease which is of common occurrence and easy recognition, are thus unsettled and diverse—you may be sure, first, that no specific for that disease has yet been discovered; and secondly, that the disease is not very obedient, or not steadily obedient, to any remedial plan. When I first began to practice, I pleased myself, now and then, with the belief that I had ascertained the best cure for acute rheumatism: so rapidly and decidedly did the disorder recede and cease upon the administration of such or such a remedy. But, on the next trial of it perhaps, my expectations have been miserably disappointed. This marked improvement has happened under the use of colchicum, of conium, of calomel with opium, of alkalies. I did not, in the prosperous cases, mistake spontaneous recovery for cure. The change was too great and immediate, and the instances of success too numerous, to admit of that explanation. Whether it be that slighter diagnostic marks have been overlooked, and that sometimes *gout* has in reality been cured under the semblance of rheumatism—whether bodily idiosyncrasies have withstood the influence of remedies—or whether atmospheric agencies have kept up the disorder in spite of proper treatment—I cannot tell; certain it is that we are occasionally baffled, and the patient continues to suffer, notwithstanding the diligent enforcement of all the approved remedies and plans of treatment, one after the other. I

am far, however, from thinking that remedies are useless: and I do by no means assent to the dictum of the first Dr. Warren, who, when asked what was good for acute rheumatism, answered "six weeks."

In the fibrous form, with the bounding pulse, the flushed face, the high inflammatory fever, you may bleed your patients from the arm; especially if they are young and robust. They will bear to lose a large quantity of blood without fainting; and you will always find the blood drawn to be remarkably buffed and cupped. The pain and inflammation are local; but the case is not adapted to local remedies. We may, by leeches, or cupping, or cold applications, be able perhaps to subdue the inflammation in a knee or an elbow: but from the migratory character of the disorder, we incur the risk, by such topical measures, of giving the inflammation a new and a more serious location.

Venesection will, almost always, afford a marked degree of relief to the sufferings of the patient: but the relief will seldom be complete or permanent: and we may sometimes with propriety repeat the bleeding. In very few instances, however, can we hope to extinguish the disease by blood-letting. And this is one of the cases in which you must not take the state of the blood already drawn as a criterion of the propriety of abstracting more. The blood will remain ready to show the buffy coat long after the use of the lancet has ceased to be available or safe.

The advantages of blood-letting in this complaint are, first, the partial relief which it bestows. It is seldom that the pain is not sensibly mitigated, and the fever calmed, provided the bleeding has been carried to a proper extent, sufficiently soon; secondly, free and early venesection may sometimes perchance (but not often) cut short the attack. This, however, is a favourable effect which you must not reckon upon, and which I would not advise you to aim at. But, in the third place, blood-letting may be serviceable, by preparing the body for other remedies: for calomel, opium, purgatives, colchicum.

Yet, looking to my own practice in acute rheumatism, I find that although I am in the almost daily habit of treating it—for it is a disorder from which our wards are never perhaps entirely free—I rarely prescribe phlebotomy. Many of the patients come under our care at an advanced period of the disease; when the time for bleeding, with any prospect of benefit, has gone by. Some have been bled before their admission. Others, in whom the rheumatic diathesis has become inveterate, are suffering recurrences of the malady: and neither does the recurrent disease require, nor would the system bear, active and repeated depletion. In many instances, again, the constitution has been battered and broken by a London life: while in others there is no great intensity of febrile disturbance from the first. So that the cases in which I am tempted to employ the lancet are really few and far between. At the same time I can well believe—what I have often heard—that in the country, in young persons of unimpaired vigour, in the first attacks of the disorder, and when the inflammatory symptoms run high, venesection may be of much service in mitigating the sufferings of the patient, and in facilitating his cure.

I believe few persons now adopt the plan of forced perspiration for the cure of acute rheumatism. Formerly it was the fashion to give powerful sudorifics: Dover's powder, or antimony, in large doses: and the patient was "accinctus ad sudorem;" covered up in bed between thick blankets, with a hot bottle or brick at his feet. But in the severe fibrous variety of rheumatism the perspiration is profuse without any artificial means being used to excite it, and it is not accompanied by the smallest alleviation of the pain: nay, sometimes the patients will tell you that they are worse, in that respect, while the sweating continues.

The preparations of colchicum have sometimes, whether venesection has been premised or not, an almost magical effect in quelling the disease. Frequently, when most successful, (though that is by no means a necessary condition of their success) they exercise some marked influence upon the stomach and bowels. Colchicum is very apt to occasion deadly nausea and vomiting, griping, and diarrhœa, and when these consequences ensue from its use, the inflammation of the joints often subsides entirely. At any rate, if the rheumatism does not give way when the stomach and bowels become thus affected, you may be certain that to push the colchicum further would be useless.

Our wishes, however, and our expectations, from colchicum, are often doomed to be defeated. I believe that in proportion as the synovial symptoms predominate, or mix

themselves distinctly with the fibrous—in proportion as the disease approaches in its characters to gout—you may expect to be successful with colchicum. Large doses are not requisite. Twenty minims of the wine or of the tincture may be given every six hours, until the result is obtained. Or a grain of the inspissated juice, or of the acetous extract of colchicum, every four hours. Under this treatment the disease sometimes vanishes within three or four days, the medicine producing sickness and purging, and the rheumatism rapidly declining. Occasionally the same favourable event takes place, although there has been no disturbance of the stomach or bowels.

There are some cases which yield readily to calomel and opium: and in the fibrous variety I think that calomel and opium are the remedies to which, after sufficient bleeding, you will do well to trust. For it is in this variety that the extension of the disease to the membranes of the heart is so liable to happen: and if any thing can protect the patient against this fearful complication of a malady which, previously attended by no danger, becomes by this addition almost necessarily fatal—or if any thing can arrest the carditis before it leaves indelible mischief imprinted on the heart—it is (in my opinion) *mercury*.

Dr. Hope has lately (1837) published an account of a particular mode of using calomel and opium in this disease, which mode he learned from Dr. Chambers, and which is followed, more or less closely, I fancy, by many other physicians. It is said to be so successful, that even the details of it ought to be known. The plan is described as follows:—After a full venesection, or even two, in the robust, but without any bleeding in the feeble and delicate, eight or ten grains of calomel, with a grain and a half of opium, according to the age of the patient and the severity of the case, are administered every night; and followed every morning by a strong black dose, sufficient to insure four or five stools at least. With this treatment is combined, thrice a day, a saline draught containing from fifteen to twenty minims of the *vinum colchici*, and five grains of Dover's powder. When the pain and swelling are greatly abated, if not almost gone (which Dr. Hope affirms to happen often within two days, and almost always within four), the calomel is omitted: or it is omitted sooner if the gums become at all tender. The opium, however, is continued to the amount of a grain or a grain and a half at bed-time; and in severe cases a grain also at noon is added; and the colchicum, and the black dose, are still given as at first. Dr. Hope considers it a case of exception if the patient be not well in a week.

The following are stated by Dr. Hope as being the great advantages of this plan of procedure:—1. That the patient is generally sound, well, and fit for work, in a week or ten days after the pains have ceased. 2. That the gums are rarely affected, especially if you previously ascertain that the patient has not a morbid susceptibility of mercury. 3. That it is rare to see inflammation of the heart if the treatment is early begun; not oftener, he thinks, than in one of a dozen cases. 4. If the slightest symptom of endo- or peri-carditis *does* supervene, a few extra doses of calomel and opium, given every four or six hours, will generally affect the constitution in twenty or thirty hours, which, with two or three cuppings or leechings on the region of the heart, almost always places the patient in a state of safety.

Now believing that some such plan as this, made up of blood-letting in some cases, purging, calomel and opium, and colchicum, is as good as can be pursued; and indeed that, with individual modifications, it *is* very much pursued, especially in London at present; still I cannot help feeling great doubt—doubt which I would willingly not entertain—about all the advantages just enumerated. Too often we do not see the patient till there is such disease of the heart present as is manifest to the ear; and then, as I stated in a former lecture, *perfect* recovery and repair seem scarcely within the scope of possibility. Too often, when the patient *is* seen early, the system refuses to admit of the specific protection of the full mercurial action. There is a practical objection also to the exaction of four or five motions from the bowels of these rheumatic patients every day; I mean the inconvenience, the increase of pain, and the fresh exposure to cold, occasioned by frequently going to stool; and this is a disadvantage likely to be particularly felt in *hospitals*.

Bark used to be highly thought of for the cure of acute rheumatism; but it is only beneficial, or safe, during the convalescence. Where the system has been much reduced or broken by the complaint, or by the discipline the patient has undergone, I think that

the decoction of cinchona is of service in fortifying the body against the renewed agency of those causes on which the original attack depended.

Cases frequently occur which are neither absolutely acute, nor absolutely chronic. The inflammation, without being intense in any one joint, lingers in many; and the fever, though not entirely absent, is moderate. The joints are hot and painful, and the skin is dry, and there is some thirst. To this intermediate character the term *subacute* is sometimes applied; and it will serve our purpose of distinction.

In this form of the disease, as well as in the more active form, the urine is almost always loaded with lateritious matters, and strongly acid. Looking upon this as an indication of treatment, I have prescribed alkalies, and with much success: the liquor potassæ, for instance, to the amount of a drachm daily, for several days together; keeping the bowels free by laxative medicine. Under this plan, more surely in my experience than under any other, the urine clears, the pain abates, and the joints are liberated. Nitre has been praised of late as being eminently successful against rheumatism: I have not had much opportunity of trying it, but I should think it likely to do good in these subacute cases.

Chronic rheumatism.—There are two kinds of *chronic* rheumatism; one attended with local heat and swelling, although the constitution at large sympathizes very little or not at all with the topical inflammation; the other characterized rather by coldness and stiffness of the painful joints. In the former of these the pains are increased by pressure, and by movements of the limbs, and by external warmth; the warmth of a bed, for example; and there may be even some slight degree of pyrexia at night. In truth this form of chronic rheumatism claims a near relationship with the acute, *into* which it sometimes passes, and *of* which it is frequently the sequel. It accordingly requires antiphlogistic remedies, only less vigorously applied. It is important for you to know that, in these cases, you may, with less hesitation, less fear I mean of driving the inflammation to some more vital part, apply leeches, and cold washes, to the painful joints. Otherwise, the principle of treatment remains unchanged. The complaint is, however, often obstinate, and lingering, and prone to recur. It frequently involves and cripples the smaller joints, especially those of the knuckles and fingers; rendering them knobby, and distorting their form and position. The fingers take a permanently oblique direction, slanting outwards towards the ulna: and Dr. William Budd has drawn attention to the curious fact that the corresponding joints of the two sides of the body are always affected exactly in the same manner. To use a paradoxical expression, the deformity is symmetrical. One crooked joint is just the copy of its fellow. Surely this indicates the *constitutional* origin of the disorder.

In the other form of chronic rheumatism, what some call *passive*, the remedies that answer best are of a different kind. The pain is alleviated by friction of the joint, and the patients are most comfortable when they are warm in bed, and especially when moderate perspiration is present. They are singularly benefited also by summer weather. Persons who are much troubled by this wearing complaint, and who can afford to live where they please, would do well to take up their residence in a warm climate. Wherever they may be, such patients should be protected against atmospheric vicissitudes by warm clothing: they should be cased in flannel from the neck downwards. Warm bathing is of great service; and especially baths of salt water, of a temperature not less than 100°, that they may act as a stimulus to the cutaneous circulation: warm douches; the vapour bath; or the hot-air bath, of which, as I said before, the patient may receive the benefit lying in bed. And to warm clothing, and warm bathing, may be added friction, with some stimulating liniment, and what is called shampooing. It is in these cases that stimulating internal medicines are often of use. Turpentine; some of the animal oils, the cod-liver oil for instance; guaiacum. Opiates, too, are frequently remedial of the pain; and there can be no better form for their administration than that presented to us in the celebrated Dover's powder; the *pulvis ipecacuanhæ compositus* of the Pharmacopœia.

There is a remedy which has recently come into extensive use, for the relief of chronic rheumatism; I mean the iodide of potassium. It is *most* certain of acting beneficially when that fibrous part, the *periosteum*, is principally affected. Its virtues in the case of venereal nodes (*i. e.* in venereal inflammation of the periosteum) were first distinctly pointed out by Dr. Williams, of St. Thomas's Hospital. I believe it is equally effectual

upon whatever cause chronic inflammation of the same part, with nodes and thickenings, may depend.

Some of you probably saw a woman who was lately my patient in the hospital, and who had been worn down to a skeleton by the pain she had endured from chronic periostitis giving rise to nodes, which did not appear to be traceable to syphilis. She had been in the habit of lulling the pain by large opiates at night, and begged to have them after her admission. I gave only the iodide in the ordinary dose (five grains thrice daily), and she slept without opium; and in a week or two lost her nodes, and was perfectly well.

Gout.—Closely allied to acute *rheumatism*, and yet distinct from it, is the singular disease which in this country is popularly called the *gout:* which Cullen, in the first instance, was disposed to term arthritis; but as arthritis would imply inflammation of all or any of the joints, he afterwards adopted the ancient name of *podagra* (foot-pain).

The same author has given, in his *First Lines*, an excellent account of the phenomena which constitute a paroxysm of gout. It is copied from Sydenham, who drew from nature; for he had himself suffered frequent and severe visitations of the disease during a period of thirty-four years.

The attack begins, most commonly, an hour or two after midnight. The patient, who had gone to bed and to sleep in his usual health, and without suspecting what was about to happen, is awakened by a pain in one of his feet, mostly in the first joint or *ball* of the great toe; but sometimes in other parts of the foot—the heel, the instep, the ankle. With the coming on of this pain there is generally more or less of a cold shivering, which gradually ceases as the pain gets worse, and is succeeded by heat. The pain grows more and more violent and intolerable; and is spoken of by those who suffer it as amounting to torture. It is a grinding, crushing, wrenching pain; or a burning sensation as if a hot iron were pressed into the joint. Some humorous Frenchman described it in this way. "Place (said he) your joint in a vice, and screw the vice up until you can endure it no longer. That may represent rheumatism. Then give the instrument another twist, and you will obtain a notion of the gout." The pain is attended with great restlessness and misery, and exquisite tenderness. The patient cannot bear the weight of the bed-clothes upon the affected limb; nor the jar of a heavy foot-fall in his chamber. In a vain search after comfort he is perpetually shifting his foot from place to place, and from posture to posture. At length, about the ensuing midnight, the pain remits; sometimes gradually, sometimes so suddenly that the patient attributes the relief to his having at last found an easy position. He falls asleep in a gentle perspiration, and when he wakes the next morning he finds the part, which had been so painful, to be red, swelled, tense and shining, surrounded by more or less œdema, and by turgid veins. The same series of symptoms recur, in a mitigated degree, for some days and nights; and then the disease often goes entirely off, not to return till after a long interval.

As the œdema subsides, and the redness fades, the cuticle of the part that has been inflamed peels off; and this process of desquamation is generally attended with troublesome itching.

Such is a picture of an attack of gout, occurring in an adult subject, for the first time, and in its most regular and genuine form.

Attacks of this kind are preceded, in most instances, by some marked disorder of the functions of the stomach; diminished appetite, flatulence, heartburn, nausea perhaps. And during the paroxysm the urine is very high coloured, and acid, and turbid; depositing a copious pink, or brick-dust sediment. The stools, also, are unnatural; pale, or of a dark green, and very offensive. After the fit, when the complaint has ceased entirely, it generally (says Cullen) "leaves the person in very perfect health; enjoying greater ease and alacrity in the functions of both body and mind, than he had for a long time before experienced.

Progress.—But the disorder, which has thus departed, is very apt—nay unless extreme care be taken to prevent it, and even in spite of all care, it is almost sure—to return. At first, perhaps, it recurs not oftener than once in every three or four years; but after some time the intervals are shorter, and the attacks become annual, happening about the same time of the year: afterwards they come twice every year; and at length they return seve-

ral times during the course of the autumn, winter, and spring. And as the fits are more frequent, so also they are they more protracted, till, in the advanced state of the disease, the patient is hardly ever free from it, except perhaps for two or three months in summer. I do not mean that all this occurs invariably in all cases alike; but this is a sketch of the general course of the complaint.

There are other phenomena also to be noticed as time advances, and as the disease is repeated. At first, I say, it commonly appears in one foot only; afterwards every fit includes both feet, the one after the other: and as the disease continues to recur, it not only attacks both feet in succession, but after having ceased in the foot which was secondly visited, it will return again into the foot first affected, and perhaps a second time also into the other. It passes, too, into other joints, both of the upper and lower extremities, large as well as small; so that there is scarcely a joint that may not, at one time or another, be seized upon. But as the disease proceeds, and the fits get to be more numerous, the pains are commonly less violent than they were at first; the patient is, however, more affected with sickness, and suffers more in his general health.

Again, after the earlier attacks, the joints usually recover entirely their former strength and pliancy; but when the disorder has returned again and again, they are not so readily nor so completely restored to their previous condition, but remain weak and stiff: and sometimes they lose at length their capacity of motion altogether.

Also, in many gouty persons, but not in all, after the disease has frequently recurred, what are called *chalk stones* form; concretions that look exactly like chalk take place around and outside the joint, filling up the cellular tissue, and lying, in general, immediately beneath the skin. The material of these curious concretions is deposited at first in a half fluid state, and resembles soft mortar; but the more watery ingredients being afterwards absorbed, it becomes dry and hard. Of course when this stuff is deposited in any quantity on the outside of a joint, it must limit in a great measure, or entirely prevent, the motion of that joint. And sometimes matter of the same kind is effused into the joint itself; as I have once seen in a gouty patient who died in the Middlesex Hospital.

Gout is a disease that was well known, and well observed, by the ancients. In its genuine form it could neither be overlooked nor mistaken. Many very interesting facts relative to this painful disorder have accordingly been ascertained: and I proceed to notice the chief of these; but I must do so with as much brevity as I can.

First, then, gout is an *hereditary* disease. I do not mean to say that the disposition to it is always a transmitted disposition; but that the complaint is much more likely to occur in persons in whose pedigree it can be traced, than it is in other persons. It may, I believe, be generated by certain habits of life; and, on the other hand, in spite of an inherited predisposition, the disease may be staved off and averted. Let the son of a rich and gouty nobleman change places with the son of a farm servant, and earn his temperate meal by the daily sweat of his brow, and the chance of his being visited with gout will be very small. Granting this, we see reason, independent of the general analogy of hereditary disorders, why the gout may be expected sometimes to leap over a generation, just as family likenesses are known to intermit; while yet the *disposition* may descend to the children of those who, in their own persons, have never suffered the *disease*. Among 522 gouty persons, concerning whom Sir Charles Scudamore had collected information, 332 could trace their disease to the father, mother, grandfather, grandmother, uncle, or aunt. In the remaining 190 the disease was not known to have existed on either side of the family tree.

2. There is a pattern of body which is believed to be favourable to the acquisition of gout. "It attacks (says Cullen) especially men of robust and large bodies, men of large heads, of full and corpulent habits, and men whose skins are covered with a thicker *rete mucosum*, which gives a coarser surface."

3. Whether, in a given individual, there be an inherited tendency to the disorder or not, its access is promoted in a remarkable manner by a full and luxurious mode of life, and by sedentary or inactive habits.

4. It is observed of gouty persons, that they are usually subject to nephritic complaints also, to fits of the gravel, to renal and vesical calculi. These disorders of the urinary organs commonly begin to manifest themselves after the gout has plagued the patient for some

time. They do not coincide with the paroxysms of gout, but the two happen alternately: or (what is equally expressive of the connection between the two forms of disease) the children of gouty and nephritic parents inherit often the one or the other of these maladies; but "whichever may have been the principal disease of the parent, some of the children have the one, and some the other. In some of them the nephritic affection occurs alone, without any gout; and this frequently happens in the *female* offspring of gouty ancestors."

The urinary concretions to which gouty people are so subject, and the morbid states of their urine generally, belong to the *lithic* diathesis. Dr. Prout holds that "the lithic acid, developed principally during the mal-assimilation of the albuminous textures, may be considered as the characteristic feature in gout." And the chemical composition of the *chalk-stones* which sometimes accompany gout, is in accordance with this statement; and illustrates strongly the connection between gout and gravel. The so-called chalk-stones consist mainly of lithic acid combined with soda; of the lithate or superlithate of soda. Sometimes this very lithate of soda, perfectly white, is deposited in large quantities in the *urine*. Dr. Prout says that he has seen it copiously secreted of the consistence of mortar, so as to block up the urethra in its passage outwards. Now this is just the stuff which is deposited around, and sometimes within, the joints, and which hardens as it collects. I may mention here again that many persons have the gout long, and severely, without having any of these concretions. They are incidental to the more chronic forms of the disorder, in which the pain and the fever, though of long duration and frequent recurrence, are slight in degree. The cuticle at length gives way, and the earthy matter lies bare. A namesake of mine, Mr. Henry Watson, describes, in the first volume of the *Medical Communications*, the case of a Mr. Middleton, who was accustomed, when playing at cards, to chalk or score the game upon the table with his gouty knuckles.

5. Gout attacks especially the male *sex*. Some few women, however, suffer it, in its regular and decided form; and generally these women are robust and plethoric. Cullen noticed its occurrence in "several females whose menstrual evacuations were more abundant than usual." But the disease chiefly happens in women after the catamenia have ceased to appear. Heberden knew a female who had numerous sores from chalk-stones.

6. Cullen observes that the gout does not usually come on till after the *age* of five-and-thirty. Heberden, who in his long and extensive practice among the higher classes of society in this town saw as much, perhaps, of this disease as any physician ever did, says that he never met with a case which he could decidedly pronounce to be gout, before the age of puberty. Sir Charles Scudamore has collected a statistical account of 515 examples of gout, in which the period of the first assault had been noted. Of these, 142 began between the ages of 20 and 30; 194 between 30 and 40; and 118 between 40 and 50. The greater number, you will observe, was between 30 and 40.

However, I believe that where the inherited disposition is strong, and the habits of living are such as to foster that disposition, gout may show itself, occasionally, even prior to the age of puberty: but this is, certainly, the exception to a very general rule.

7. Gouty persons are subject to various ailments, which spring from the same fountain as the well-marked paroxysm: derangements in the functions of the digestive organs, of the heart and lungs, of the brain and nerves.

The most familiar of these ailments is indigestion, with its various circumstances of impaired appetite, sickness, vomiting, flatulency, heartburn, acid eructations, gastrodynia. Pains and cramps occur in several parts of the trunk, and shoot thence into the upper extremities, and are relieved by the extrication of wind from the stomach. The bowels are irregular; colicky diarrhœa being sometimes the prevailing fault, but more commonly costiveness. With all this the patient is apt to be excessively dejected and hypochondriacal, morbidly attentive to every bodily feeling, disposed to exaggerate his sufferings, and apprehensive of the worst event.

When the viscera of the thorax are affected, the patient has palpitations, fits of dyspnœa, faintings, or even pangs like those of angina.

In the head occur pain, giddiness, transient affections of the vision and of the hearing, threatenings of palsy and apoplexy.

All these, you may say, are feelings and ailments to which any and all persons are liable.

True: but the remarkable peculiarity which connects them, in some men, with gout is this;—that they often all clear away and disappear upon the breaking out of a paroxysm of that disease in the foot.

Hence such symptoms are regarded as indicating one variety of *irregular* gout. Cullen, led by a questionable theory, classes them under the head of *atonic* gout. Sometimes the patient so affected is said to have *lurking* gout; or *masked* gout.

In another variety of irregular gout, the complaint commences, in the ordinary way, in a joint; but the pain and inflammation do not reach the ordinary degree of intensity, or at any rate, do not continue for the usual time and then recede gradually in the accustomed manner, but they disappear abruptly and entirely, while symptoms of severe and alarming disorder arise, as suddenly, in some internal part. This Cullen names *retrocedent* gout. It affords an example, as I conceive, of true metastasis. The internal part most commonly attacked is the stomach. It becomes affected with a peculiar feeling of anxiety and distress; with sickness, vomiting, or violent pain which the patient calls spasm, and which probably is of a spasmodic character. More rarely the retrocession is to the heart, when syncope or urgent dyspnœa ensue; or to the head, when it may terminate in a stroke of apoplexy, or of paralysis.

In a few cases the disorder, thus alighting on some other part than a joint, is plainly inflammatory. The most common example of this is gouty inflammation, with scalding and a puriform discharge, of the urethra; simulating very exactly an attack of gonorrhœa. So also there is a gouty form of ophthalmia, or gout in the eye; gout in the testicle; and a year or two ago an eminent physician of my acquaintance suffered a violent and dangerous attack of what was considered to be gout in the throat. Dr. Cullen speaks of these inflammatory affections under the title of *misplaced* gout: but they may well enough be ranked under one of the preceding heads of masked, or retrocedent gout.

The disposition to gout may be engendered, and when inherited will infallibly be strengthened and developed, as I have told you already, by certain habits of life: by sensual indulgences, and (but in a less degree, I believe) by want of bodily exercise. Of this we have the strongest negative evidence in the remarkable immunity from the disease enjoyed by the working poor in our rural districts. One never hears of the gout among agricultural labourers. Sir Gilbert Blane states that, during ten years in which he was physician to St. Thomas's Hospital, although in his private practice he reckoned one hundred and thirty patients who had gout, being about one in twenty-six of the whole number, he had not a single case of it among 2406 patients in the hospital. This I think strange, for in the London hospitals it is not very uncommon for us to meet with gout; but then it is in persons who have lived fully and inactively: in the servants of wealthy families, for instance, butlers, coachmen, porters—men who often live more luxuriously, and more idly a great deal, than their masters. And among the rich, those who are most subject to gout are notoriously those who indulge most in what are called the pleasures of the table; who eat largely of animal food, and drink much wine, especially if they are indolent withal. Such men generate for themselves the lithic acid diathesis; and if the gouty tendency happens to have been born with them, they incur the disease, under these habits, with more or less readiness, according to the degree of that innate disposition. Strong exercise certainly *remedies*, in some measure, the evil effects of this mode of life, by promoting the excretions of the body: but gout used to be exceedingly common in the old-fashioned fox-hunter, who "rode hard," while he also "lived hard." Mere sedentary habits do not produce gout, as we learn from the comparative exemption of females, and of the poor, who, following sedentary employments, are yet compelled by their poverty, which is so far a blessing to them, to be temperate. Men who eat much meat generally indulge themselves in drinking also: the two causes go together, and it is difficult to estimate their separate influence. Butchers, who live fully upon animal diet, are said to be rarely affected with gout, but then they necessarily take a great deal of exercise. It appears that the use of wine, and of malt liquors, fosters the disposition to gout much more than the abuse of distilled spirits. The paucity of gouty patients among the lower classes in this gin-drinking town suffices to show this. I have been told that gout is very little known in Glasgow, where the commercial men live richly, and lead sedentary lives, but do not drink much

wine, their favourite beverage being rum-punch, of which they are not at all sparing. Dr. William Budd says that the disease is common among the "ballasters" on the Thames; that, although they are not a numerous body, many are admitted with gout every year into the *Dreadnought*. Now these men being much exposed to inclemencies of weather, and using great bodily exertion, which is attended with profuse sweating and much exhaustion, think themselves warranted in drinking (besides spirits) two or three gallons of porter daily. This shows the effect of malt liquor in producing the gouty habit of body.

On the other hand, the inbred gouty tendency may be so strong, as to be scarcely kept in check by the most abstemious regimen.

A fit of the gout may be *brought on* by various circumstances: in other words, the possible *exciting* causes of gout are many. A paroxysm has been frequently known to follow immediately upon an unusually severe debauch. Strong mental emotion has sometimes the same consequence, especially emotion of a depressing kind. Excessive fatigue—more particularly fatigue produced by too much walking exercise on any one day—is another exciting cause. And this is unlucky, for it often discourages a patient from again making use of a proper and even a necessary amount of exercise of that kind. Another exciting cause which frequently operates is external injury. The first attack of gout often fixes upon the seat of an old hurt: and a very slight recent injury is sometimes enough to determine a paroxysm—a trifling bruise, or sprain, the pressure of a tight shoe; nay, Dr. Heberden tells us that he verily believes he has seen an attack of gout brought on by the bite of a flea; showing how easily the disease may be excited, when there is a strong predisposition to it. This it is which makes us so often doubt the accuracy of gouty persons, when they tell us that they are lame from a sprain.

Dr. Cullen enumerates sundry debilitating circumstances, which, as such, appear to operate in calling into action the gouty disposition. And there can be no doubt that a state of weakness does often favour the eruption of the malady. A friend of my own had lately a most serious attack of continued fever, in the course of which he became hemiplegic, and his life was despaired of. Soon after the fever had left him, and while he was yet extremely feeble, he had three attacks of gout in quick succession.

Diagnosis.—Pains have been taken by several writers, especially by Heberden, to lay down the distinguishing characters between gout and rheumatism. A first assault of gout can scarcely be confounded with an attack of acute rheumatism. The limitation of the inflammatory redness to one foot, and the restless distress of the gouty patient, contrast strongly with the helpless and motionless cendition of the rheumatic, who is pinioned, so to speak, in many limbs. There may be more room for doubt and mistake in the advanced state of gout, when many joints have at length become involved; but even then you may generally decide by inquiring into the history of the patient, and learning the circumstances of his early attacks.

The main points of distinction may be broadly and generally stated thus.

In gout the small joints are first and chiefly affected, especially the point of the great toe: in rheumatism, the large. The redness of the gouty inflammation is more bright and vivid than that of the rheumatic; and the fluctuations between agony and ease are greater and more frequent. Gout usually affects one joint only at a time: rheumatism often many at once. The inflammation in gout is attended with more œdema than in rheumatism; and is followed, in the majority of instances, by desquamation and itching, phenomena which we do not notice at the close of rheumatic inflammation. Gout is not attended with those drenching acid sweats which are so characteristic of acute fibrous rheumatism. The gout is decidedly hereditary: rheumatism, if hereditary at all, is much less distinctly so. The gout occurs rarely or never, whereas rheumatism is not very uncommon, before the age of puberty. In gout, though many functions suffer, there is no tendency to carditis: in rheumatism, with far less general disturbance, that tendency is very marked. Gout is the punishment (some have thought it the privilege) of the rich, of persons who live fully, luxuriously, and idly: rheumatism is most frequently the appanage of the poor, and of those who toil.

LECTURE LXXXII.

PATHOLOGY OF GOUT. PROGNOSIS. PREJUDICES RESPECTING THE DISEASE. TREATMENT: DURING THE PAROXYSMS; DURING THE INTERVALS. CUTANEOUS DISEASES.

I YESTERDAY described the phenomena of gout, from its primary outbreak to its crippling consummation. I told you what observation has collected concerning its causes; and I pointed out the circumstances which distinguish it from rheumatism. Let us look a little closer into the essence of this curious malady.

Pathology.—The pathology of gout has been the theme of endless controversy. Humoralists and solidists contend alike for the triumph of including the disease within the pale of their respective theories. The very name, *gout*, derived through the French *goutte* from *gutta*, expresses summarily the doctrine of those who imposed it: and we trace the same, or a similar idea, in the appellation of the kindred disorder, *rheuma*tism.

"The opinion (says Cullen) which has generally prevailed, is, that gout depends upon a certain morbific matter, always present in the body; and that this matter, by certain causes thrown upon the joints or other parts, produces the several phenomena of the disease."

You will find this doctrine at the bottom of all Sydenham's speculations on the subject. But Cullen doubted it, and even endeavoured, in an elaborate argument which you may read in his *First Lines*, to disprove it. He held gout to be an affection of the nervous system. I shall not trouble you by detailing his argument, for I consider it an utter failure. I am satisfied that the ancient doctrine, which asserts the *humoral* origin of the disease, is the true one. "Morbific matter" (it may well be called a *poison*) is generated, or detained, under certain circumstances, within the body, and silently collects in the blood; until, after obscure threats, perhaps, and prelusive mutterings, it explodes in the foot; and then the bodily economy, like the atmosphere after a thunder-storm, is, for a while, unusually pure and tranquil. To some such conclusion as this the result of all modern research seems clearly and unfailingly to tend. Dr. Holland, for example, in his thoughtful and thought-exciting volume, recently published, expresses his belief in "a *materies morbi*, which, whatever its nature, is capable of accumulation in the system, of change of place within the body, and of removal from it." In this, and in several other propositions relative to gout, enunciated in distinct terms by this learned writer, I fully concur. Some speculations still more lately put forth by Dr. William Budd, in a communication to the Medical and Chirurgical Society, throw a strong light upon this perplexed subject; and bring the phenomena, not only of gout, but also of many other important complaints, within the operation of one general, comprehensive, and intelligible law. I shall take leave to refer, in a very cursory manner, to some of Dr. Budd's positions.

I need not remind you of the various ways in which extraneous matters find entrance into the blood. Poisons, under their proper shape and name; medicines, which misapplied become poisons; our natural food and drink, which the folly of man converts into poison; the products or dregs of the secondary assimilative process: these are common sources of impurities, more or less hurtful, which mix and circulate with the vital fluid. Some of these extraneous matters escape harmlessly by one or more of the waste-pipes and emunctories of the body. Some are entangled in its solids: but not indiscriminately; for different substances have their special or favourite resting-places. All this is well known to persons conversant with toxicological researches.

Now this doctrine, of the elective affinity between certain tissues or parts of the body, and certain morbific principles conveyed to them by the blood, is applied by Dr. Budd to elucidate the very curious fact of the symmetrical local manifestations of many disorders; which disorders are themselves so far general that they derive their origin from the circulating fluids. This symmetry he finds the most exact in chronic constitutional complaints, wherein the local morbid changes are effected in a manner which approximates closely to the process of healthy nutrition. He shows good reason for believing (what, if the whole theory be true, we should expect) that the same symmetrical phenomena are modified by the *amount* of the poison collected in the system. If there be a certain quantity only, it may settle in some favourite or congenial spot, on one side of the body. If there be more than enough to saturate that part, it goes next to the corresponding spot upon the opposite side; or, perhaps, to an analogous part of the other limb of the same side. If there be more still of the poisonous material, it flies to, and occupies, other parts also. He further shows that the elective affinity is more exclusive, and the bond of union stronger, in respect to some morbid principles, than to others: and in proportion as the affinity is weak, so is the local manifestation of the disease apt to shift, by metastasis, from place to place. When the matter which has thus entered, or combined with, a certain tissue or organ, is anyhow loosened and released from that union, or repelled from the part, it is again set afloat in the blood, to "break out" elsewhere; to tease various organs, perhaps, or to derange the whole economy. The alteration so often to be noticed between certain cutaneous eruptions and internal disorders of function, is a striking and familiar example of this. The eruption affords presumptive evidence of the detention of some peculiar morbid principle in that part; and the internal affection which succeeds the disappearance of the eruption, denotes that the morbid principle has re-entered the blood. Some of these peccant or poisonous matters fix permanently in the affected spot or spots; and some of them may even be recovered in substance from the dead tissue by chemical means: the poison of lead, for example, from the symmetrically palsied muscles. Others appear to be expended gradually in the part, and so eliminated from the system. Dr. Budd observes, that the regular arrangement of these local tokens, whether they be outward or internal, is disturbed by the presence of fever. Probably the febrile tumult may itself be owing to the quantity of the noxious matter in circulation within the body. He states, also, that *cæteris paribus*, this morbific matter is most apt to pounce, in the first instance, upon parts which have been previously hurt, or which are mechanically irritated at the time. For which reason a part that has once been affected by it is more likely than other parts to suffer again.

Now, see how thoroughly the ascertained phenomena of gout accord with this theory. Certain habits of life produce fulness, and richness, and impurity of blood; the same habits which breed the lithic acid diathesis. We may even conjecture this acid, or some of its compounds, to be the actual *materies morbi*. Lithates are poured forth with the urine, and sometimes deposited in vast masses around and within the gouty joints. At first, after obscurer intimations of the presence of the poison in the system, it thunders in the foot; and there, perhaps, is all discharged and spent. The chemist, Berthollet, found that the skin of a part affected with gouty inflammation communicated instantly to litmus paper a deep red colour: a large quantity of acid was evidently passing off by exhalation from the inflamed surface. If the poison be too copious to find sufficient vent in one joint, it attacks another, or more than one. "Quandoque etiam primis morbi diebus, cum materia peccans adeo exuberat ut ei capiendæ pes unus impar sit, utrosque simul pari vehementiâ fatigat: sed ut plurimum pedes successivè, uti diximus, adgreditur." These are Sydenham's words. A chain of repeated paroxysms at length purifies the blood: "donec tandem materiâ peccante prorsus absumtâ, æger pristinam obtinuerit sanitatem." The descent of the disorder upon a particular joint is often determined by a recent blow or sprain, or by the chronic weakness consequent upon an ancient hurt. If the inflammation, after thus settling, be repelled from the foot, the poison, being driven again into the blood, may light upon some vital organ, and place the patient's life in immediate jeopardy. The late Dr. Parry, of Bath, had at one time under his care two patients who had attempted to cut short or to ease a paroxysm of gout by plunging the affected foot

into cold water. This gave instant relief to the pain, and in both instances the inflammation presently abated; but in both, also, *hemiplegia* occurred a few hours afterwards.

If these views respecting the pathology of gout be true, it can scarcely be doubted that they are applicable, *mutatis mutandis*, to the cognate disorder—acute rheumatism. Dr. Prout, indeed, considers the *lactic* acid, developed chiefly during the secondary mal-assimilation of the gelatinous tissues of the body, to have the same relation to rheumatism which the *lithic* acid, derived from the mal-assimilation of the albuminous textures, has to gout.

Prognosis.—The prognosis of gout may be gathered, without much farther suggestion on my part, from what has already been said. The inflammation which befalls the joints has no worse *event* than the thickening, or, perhaps, the chalk-like deposit, which it produces, so that *gout in the extremities* is not a mortal disease. But as it is not always confined to the extremities, the life of a gouty person is justly held to be insecure. You will find that all insurance companies exact, *cæteris paribus*, a larger premium from those who have had the gout. When it proves fatal, it is by translation of the disease, or rather of the gouty virus, to some vital part; to the stomach, the heart, the lungs, the brain.

As the early visits of gout are generally followed by a striking change for the better in the health and feelings of the patient, it is not to be wondered at that the disease, in its genuine and decided form, should have sometimes been wished for, and even courted. It is commonly thought that a fit of the gout clears the system of all other disorders. It does, indeed, clear it, for the time, of those disorders which resulted from the poison of gout. But this fact has led to great practical mistakes. First, to the error of looking on inertly, and doing nothing to remedy the ailments which are supposed (often very wrongly) to depend upon lurking gout, and to require a fit of the gout for their cure; and, secondly, the more dangerous experiment of endeavouring to force on such a fit by excess and intemperance. Men forget, or do not know, that the enemy thus reinforced, instead of evacuating the fortress by its outports, may retreat triumphant into the citadel. To drop metaphor, such a course of living may, indeed, determine an attack of the disease in the extremities, but involves the fearful peril of some internal seizure. Besides, the benefits expected from external gout belong to its earlier returns alone. The more numerous the fits, the faster does the general health break, and the more stubbornly do the associated symptoms cling to the patient: and many persons linger on, martyrs (as they say) to the disease, long after they have ceased to be fit for any of the business of life, or capable of any of its pleasures. Nevertheless, as Heberden observes, "people are neither ashamed nor afraid of it; but solace themselves with the hope that they shall one day have the gout; or, if they have already suffered it, impute all their other ails, not to having had too much of that disease, but to wanting more. The gout, far from being blamed as the cause, is looked up to as the expected deliverer from these evils."

And this mistaken ambition is heightened, no doubt, by the notion, still more absurd and ridiculous, yet very generally prevalent, that it is a *creditable* thing to have the gout: a notion which evidently originated in the fact of its being peculiarly incidental to the wealthy and the great, to men of cultivated minds, and intellectual distinction. Nothing can show more strongly the power of fashion than this desire to be thought to possess, not only the tone and manners of the higher orders of society, not their follies merely and pleasant vices, but their very pains and aches, their bodily imperfections and infirmities. All this is more than sufficiently ludicrous and lamentable: but so it is. Even the philosophic Sydenham consoles himself, under the sufferings of the gout, with the reflection that it destroys more rich men than poor—more wise men than fools. "At vero (quod mihi aliisque licet, tam fortunæ quam ingenii dotibus mediocriter instructis, hoc morbo laborantibus solatio esse possit) ita vixerunt atque ita tandem mortem obierunt magni Reges, Dynastæ, exercituum classiumque Duces, Philosophi, aliique his similes haud pauci. Verbo dicam, articularis hicce morbus (quod vix de quovis alio adfirmaveris) divites plures interemit quam pauperes, plures sapientes quam fatuos."

Treatment.—The treatment of a gouty patient naturally divides itself into that which is proper during the paroxysms, and that which is proper during the intervals between the paroxysms.

It was maintained by the great physician, whose words I have just been quoting, that all artificial evacuations during a fit of the gout are useless or hurtful. He therefore discountenanced blood-letting, purging, and the use of diaphoretic medicines. It was nature's prerogative, he said, to exterminate the peccant matter in her own way; namely, by depositing it in the joints, whence it might be dispersed by insensible transpiration. Evacuant remedies had no other effect than that of recalling into the blood this peccant matter, which nature had already thrust forth to the extremities of the body; whereby it happened that the virus, which should have been eliminated through the joints, fell upon some of the viscera; and so the patient, who was in no danger before, became in peril of his life. I mention all this to show you how entirely identical was Sydenham's theory of the gout with that which is now rapidly regaining its lost ground in this country, and which I firmly believe to be the true one. After his time, and upon his authority, the treatment of gout lapsed into an inert expectancy. Even Cullen came to the conclusion that the best thing to be done is to commit the sick man to "patience and flannel alone." Here and there, indeed, an advocate of more active measures sprung up. Dr. Rush thought that venesection was always safe, and generally serviceable: and some persons, following the bad example of the illustrious Harvey, were for extinguishing the inflammation by immersing the affected joint in cold water. Heberden, however, had clearer and juster views upon the subject. He perceived that one reason why physicians did nothing to check the paroxysm was, that they did not know what would check it. He agreed with Cullen in thinking "that no medicine for curing the gout had yet been found;" but he did not partake of his belief in "the impossibility of a cure by medicines."

"The itch (he observes) is supposed to be wholesome in some countries, where it is endemical; and an ague has been considered as a minister of health, whose presence and stay ought by all means to be courted. These opinions are now pretty generally exploded in England; and I hope the time will come when a specific for the gout, as certain as those which have been discovered for these two disorders, will ascertain the equal safety and advantage of immediately stopping its career, and preventing its return."

That time has come: for the colchicum, judiciously employed, may fairly be accounted a specific for the gouty paroxysm. And it is remarkable how long this truth has been seen, though not distinctly or steadily. The hermodactyl of the ancients is the modern colchicum, and was in high estimation among them for its efficacy in the same forms of disease as are benefited by the colchicum now. It bore, with some, the name of *unima articulorum*, the soul of the joints, because (as Quincy states) it prevented "the lodgment of such gritty matter as occasions the gout and arthritic complaints." And I think there can be no doubt that the active principle of the quack medicine so much in vogue for the cure of gout some years ago—I mean the *eau medicinale*—was either the same with that of the meadow saffron, or derived from the same family of plants which Decandolle has associated together under the title of "Colchicaceæ."

This drug has certainly the property of easing, in an almost magical manner, the pain of gout. How it operates is not so clear. It is apt to produce nausea, faintness, and diarrhœa; but its curative influence is not conditional upon the occurrence of these symptoms. Sometimes the rapid disappearance of the gouty inflammation is its only perceptible effect. The patient may be in helpless agony, with a tumified red joint to-day; and walking about, quite well, to-morrow. The colchicum is therefore plainly an anodyne. It also sensibly modifies the condition of the urine, rendering it less acid, and even alkaline; and increasing its quantity. These effects are consequent, I presume, upon changes in the blood wrought by this substance, which thus, and there, proves somehow an antidote to the poison of gout.

There are, as you are aware, various preparations of colchicum in use: the wine of the bulb; the wine of the seeds; the vinegar of colchicum; the acetous extract, made by evaporating that vinegar; the inspissated juice of the plant itself. These are all of them active and valuable medicines; and I should pretend to more knowledge than I possess, if I undertook to tell you which of them is the best.

The mode of administering the remedy, in a regular fit of the gout, is simple enough. For example, you may give forty or sixty minims of the *vinum colchici*, in a saline draught, at bed-time; and half a drachm more, in a warm black dose, the next morning; and you

may repeat this sequence if the gout continues. Some persons give twenty minims every six hours, with a drachm of Epsom salts, and a drachm of syrup of poppies in the draught, till the symptoms yield; but I prefer the other plan. In this way the pain is usually calmed, and the swelling reduced in a few days; or even, as if by a charm, in a few hours.

But you must not be satisfied with thus quelling the pain and inflammation. A strong prejudice at one time existed, and still exists among some practitioners, against the colchicum. It was said that it had indeed the power of cutting short the paroxysms, but that it cut short the patient's life also: that they who trusted to it for getting rid of the gout, very seldom lived more than two or three years afterwards. How far this was true I cannot tell: but even admitting it to be true, it was not, I conceive, so much the fault of the medicine as of the patient, or of the medical man who did not properly admonish the patient. Men were very glad to get rid of their gout on such easy terms; and they will sometimes say to us now: "I have, as you see, got the gout. This is Monday. I must be in the House of Commons, or attend such and such a meeting, or be at the head of my regiment, on Wednesday: and I expect that you will enable me to do so." Or even sometimes the reason may be that they are engaged to some good dinner two or three days afterwards. Now if patients are content, or are suffered to be content, with expelling the gout from their toe, without observing abstinence more than a day or two, and without any depletion or further medication at the time, we can easily perceive the probability of their being soon attacked by some formidable internal complaint. I apprehend that the proper way to eradicate the lurking residue of the mischief is to continue to give small doses of the colchicum; five minims of the wine, for instance, two or three times a day, for a while. Moreover, purgatives must be employed, if that remedy does not prove aperient. No violent purgatives, however, which, by weakening the patient, seem to strengthen the power of the gouty virus. With mild cathartics, moderate doses of mercury will generally be advisable to correct the subsisting disorder of the hepatic functions; and the patient must adopt and pursue abstinent, or at any rate strictly temperate habits, in respect to meat and wine.

And as I think that the dregs, if I may so speak, left behind it by a gouty paroxysm, may be dispersed by the continued use of what, in the usual acceptation of that word, I may call *alterative* doses of colchicum, (doses, that is, which produce the desired purpose gradually, and by insensible operation,) so I think it probable that many a fit of the gout might be averted, if the remedy were given in the same way upon the first occurrence of the ordinary premonitory troubles. Many of those troubles never appear to reach the crisis of a fit. There are headaches, attacks of asthma, derangements of the digestive organs, which, occurring in a gouty person, are presumed to be fainter intimations of the presence of the gouty poison in the blood; and if such symptoms yield (as unquestionably they often do) to colchicum, the presumption draws near to proof. Dr. Holland has well remarked that the meadow saffron, by its curative effects, may bring sundry maladies, hitherto thought anomalous, under the same law of morbid *gouty* action; just as the Peruvian bark has reduced many complaints, that were previously vague in their nature, within the same category of *agueish* distempers. The same author conjectures that as hypochondriasis is certainly often symptomatic of the gouty poison in the male, so may sometimes the kindred disease, hysteria, be in the female.

Strange stories are recorded—strange, but I believe true—of instantaneous cures of the gout by strong mental emotion; by sudden terror, by violent wrath. Dr. Rush relates an instance of this. An old man who for several years had suffered an annual attack of gout, was lying in one of these paroxysms, when his son, by some accident, drove the shaft of a wagon through the window of his room, with vast noise, and a great smashing and destruction of glass. The old man leaped out of bed, forgetting his crutches; and his wife, on entering the apartment, was surprised to see him walking up and down, and exclaiming angrily against the author of the mischief. The late Professor Gregory, of Edinburgh, was in the habit of mentioning another example to the same effect, authenticated to him by a naval surgeon. It occurred in the person of an officer who was freed from an attack of gout, when at sea, by an alarm of fire. Whether this influence of certain states of the mind be rightly alleged or not, it is clear that we never can hope to make any prac-

tical use of such a remedy. Indeed, a fit of the gout has been sometimes *brought on* by a mental shock.

The treatment of a gouty patient in the intervals between his attacks of gout, whether regular or irregular, must be chiefly regimenal. The instances are not few of men of good sense, and masters of themselves, who, being warned by one visitation of the gout, have thenceforward resolutely abstained from rich living, and from wine and strong drinks of all kinds, and who have been rewarded for their prudence and self-denial by complete immunity from any return of the disease; or upon whom, at any rate, its future assaults have been few and feeble. On the other hand, many who are liable to gout are taught by sharp experience that a single debauch, a casual glass or two of champaigne, even an unusual indulgence in the use of animal food, may suffice to bring their enemy suddenly upon them. I am sure it is worth any *young* man's while, who has had the gout, to become a teetotaller. But the case is different with the *old*, and with those whose health has been broken by the inveterate disease. They must be allowed a certain quantity of their accustomed good cheer, or they become an easier prey to the disease. In such cases you must trim, as well as you can, between opposite dangers; between the Scylla of excess, and the Charybdis of debility.

It is the same with respect to exercise. The young and the hearty can scarcely take too much: the old and the dilapidated, by one act of over exertion, may incur the penalty of an attack. Although I can do little more than point out general principles for your guidance, I may remark, in reference to exercise, that it should never be *violent*, lest it excite a paroxysm by straining any part, or by causing great fatigue: that it should be *habitual*, daily—not used by fits and starts, and interrupted by long periods of indolence or inaction: and that it should be *active* muscular exercise, as distinguished from passive exercise or gestation. No mode of exercise is so good as that of walking; and with this may be agreeably and beneficially conjoined riding on horseback.

Early and regular hours are also of much importance; and the avoidance of severe mental application. Sydenham relates that one of the most atrocious attacks of gout he ever underwent was induced by intense thought and study, in the composition of his medical works.

The regimen which I have been recommending may require some power of self-control; yet in reality it implies no severity of mortification. It is perfectly compatible with life's best enjoyments: but to be effectual it must be adopted early, as soon as the disease threatens; and steadily persevered in. Gouty persons, however, do not like these restraints. They are ready to believe that an attack of gout will do them good, or if they are disabused of that error, they are desirous that some medicine may be found which will avert the disease, without their being obliged to forego their accustomed indulgences. "To gratify this desire (says Cullen) physicians have proposed, and to take advantage of its empirics, have feigned, many remedies." One of these was the famous Portland powder, of which Heberden remarks: "Unum est ex multis quæ vocantur remediis specificis, quorum ortum, et splendorem, et occasum vidi." It consisted chiefly of bitters and aromatics, and had descended, with some slight variations in its composition, from the time of Galen. Another preventive has recently been praised by Dr. Graves, of Dublin, as being highly serviceable, although (what is a suspicious circumstance) it had, like the Portland powder, fallen out of fashion. These are its ingredients:—Two ounces of orange peel, an ounce of powdered rhubarb, and two ounces of the *pulvis aloës cum canellâ* of the Dublin Pharmacopœia, steeped for a week in a quart of brandy. A table-spoonful of the strained infusion is to be taken, mixed with two or three spoonfuls of water, night and morning. Sir Henry Halford recommends what I think a better form of prophylactic remedy; viz. a few grains of rhubarb, with double the quantity of magnesia, every day: or some light bitter infusion, with tincture of rhubarb, and about fifteen grains of the bicarbonate of potass.

Now what has been observed respecting preventive remedies of this kind is, not so much that they are inefficacious, as that, *when exclusively trusted to*, they are unsafe. I believe that they are often useful by improving the digestive process; but they are dangerous substitutes for a course of temperance and exercise.

When gout attacks the stomach, either by retrocession or primarily, it often proves

rapidly fatal. The gastric affection is not, in general, inflammatory; so we judge, at least, from the *juvantia*. The attack, which consists of violent pain, and a sense of weight or of constriction, in the epigastrium, with sickness, vomiting, and a disposition to faint, is often relieved by the employment of stimulants. But such remedies would be likely to aggravate inflammation. It will always be well, when symptoms like these occur, to inquire whether any indigestible food has been lately taken; for *gout* (so called) in the stomach has sometimes turned out, under the test of an emetic, to have been nothing more than *pork* in the stomach. In the true gouty seizure, antacids will frequently remove the pain; magnesia, in full doses, with rhubarb. If this does not succeed, opium may be resorted to; and if it should be vomited, opiate enemata may be injected. Dr. Heberden thought that opium, and hot spices, were more efficacious and less inconvenient, in these cases, than wine and spirits; but when they fail, a glass of brandy will often allay the pain completely. The mustard poultice, or the turpentine stupe, applied over the epigastrium, has been followed by strikingly good effects. And it is in these emergencies, contingent upon retrocedent or misplaced gout, that we are justified in the endeavour to induce gout in the extremities; not however by internal stimuli, but by enveloping the feet in a mustard poultice, and so enticing or provoking the foe to quit his hold of the interior, and to appear in the outposts. And this expedient should be practised, whatever may be the internal organ upon which the gouty disorder has settled.

Sometimes, but much less commonly, actual gastritis does seem to ensue; and therefore all these cases are anxious and alarming cases. I do not know how the inflammatory affection can be discriminated from the non-inflammatory, unless it be by the occurrence of *tenderness* with the pain, and of fever. You must treat such cases as you would treat an ordinary case of gastritis, taking no further heed of the gout, except by the application of stimulating cataplasms to the feet.

This concludes what I proposed to say respecting gout and rheumatism; diseases of which the local seat is not exactly external, nor yet do they belong strictly to the interior of the body, except in their accidental complications. They form a link of connection between the internal and external disorders which fall to the care of the physician; and I proceed, in the next and last place, to speak of those complaints which either concern the integuments alone; or which, at any rate, are attended with some notable affection of the skin.

Cutaneous diseases.—Under the general head of *cutaneous* diseases, are included maladies of very different kinds, and of very different degrees of importance. Some are attended with fever, and run a definite course, and are often dangerous to life. Others are chronic, irregular in their progress, troublesome perhaps, and obstinate, and disfiguring, yet implying no peril to the existence of the patient. Some again are contagious, while many are not so. But before I enter upon any farther account of these diseases, I wish to make you acquainted with the names by which the various morbid appearances presented by the skin have been known, since the time of Dr. Willan.

That author—whose works have been augmented by Dr. Bateman, so that perhaps I ought to say *those* authors—divides cutaneous diseases into eight orders, distinguished from each other solely by the appearances upon the skin. I shall omit the last of these orders, the order of *maculæ*, such as freckles and congenital spots and discolorations, because in fact these are not diseases at all.

The first, then, of the appearances described by Dr. Willan are *papulæ;* pimples. These are little elevations of the cuticle, of a red colour, and solid; not containing, I mean, any fluid. They are of uncertain duration, and often terminate in scurf. They are supposed to denote inflammation of the papillæ of the skin. If you wish for an example of a papular eruption, look at that of small-pox, at its very earliest outbreak.

The second are *squamæ;* scales. These are small, hard, thickened, opaque, whitish patches of unhealthy cuticle. The subjacent surface is red. They are well seen in lepra and psoriasis; and are very common in syphilitic eruptions.

The third are *exanthemata;* rashes. They consist of superficial red patches on the skin, variously figured, and irregularly diffused, and of all sizes. We have examples of

them in some of the most important febrile cutaneous diseases; scarlet fever, measles, and others.

It is a pity that some other technical denomination was not chosen to express these rashes; for the term *exanthemata* has long been familiar in the profession as the title of an order of diseases in Cullen's Nosology.

The fourth are *bullæ;* blebs, miniature blisters. Large portions of cuticle are detached from the subjacent skin, by the interposition of a thin transparent liquid; with inflammation beneath them. Such occur in erysipelas sometimes, and in pemphigus.

The fifth are *pustulæ;* pustules. Circumscribed elevations of the cuticle, containing pus, and having red inflamed bases. We have instances of these in common boils; and in the eruption of small-pox when at its height and maturity.

The sixth are *vesiculæ;* vesicles. Small elevations of the cuticle, covering a fluid which is generally clear and colourless at first, but becomes afterwards whitish and opaque, or pearly. These are exemplified in the eruption of cow-pox, and in the chicken-pox. You will observe that these vesiculæ differ very little, except in size, from the bullæ or blebs. They often terminate in small scabs.

The seventh are *tuberculæ;* tubercles. This also is an unlucky appellation, since the word tubercle is almost appropriated, in the present day, to the scrofulous deposits which infest the lungs, and other parts of the body, in pulmonary phthisis. However, these cutaneous tubercles are small, hard, superficial tumours, circumscribed and permanent; or if they suppurate at all, the suppuration in them is partial. Sometimes they slowly ulcerate at the summit. The imperfectly suppurating pustules of the modified small-pox, and certain red spots which are apt to haunt the face, particularly of young persons, furnish examples.

Now it is very convenient, for the purpose of distinguishing different diseases, and of describing them, to know these outward marks when you see them, and to use these names. But they form a very unfit basis for the *classification* of diseases. Maladies may usefully be classed according to their causes; according to their intimate nature; according to the general plan of treatment they may require. But the superficial markings of disease have no definite relation to any of these heads. Besides, a complaint which is papular to-day may be vesicular to-morrow, and pustular next Saturday. Yet the classification most commonly followed in this country, and in France, is that of Willan and Bateman. Here we find collected under one and the same division maladies which nature has stamped with broad and obvious marks of distinction; the febrile with the non-febrile; contagious complaints with those which have not that property; ailments that are local and trivial, with diseases of grave import, and deeply-rooted in the system at large. And, on the other hand, distempers which nature has plainly brought together, and connected by striking analogies and resemblances, this methodical arrangement puts widely asunder. I point out, without professing to remedy, these imperfections. I cannot even undertake to give you any full or systematic account of the many disorders comprised in this classification. There is, however, one group, so remarkable, so important, and so highly interesting, that I shall consider it as much in detail as I can. I allude to the group which Cullen comprehends under the title *exanthemata.* With this exception, the advancing year warns me that I must contract what I have to say respecting diseases of the skin within very narrow limits.

LECTURE LXXXIII.

EXANTHEMATA. THEY ARE CONTAGIOUS; SOMETIMES EPIDEMIC. PERIOD OF THE ERUPTION; PERIOD OF INCUBATION. THEORY OF CONTAGIOUS FEBRILE DISEASES. CONTINUED FEVER.

Of the numerous complaints which are ranked among the diseases of the skin, some, I observed in my last lecture, are attended with fever, and some are not. Among the former there is a highly interesting group, distinguished by other and more important characters than the mere presence of fever, or peculiar marks upon the skin; characters that enabled Cullen to collect these diseases into a separate order, to which he gave the name of *exanthemata*. This is his description of them. "Morbi contagiosi, semel tantum in decursu vitæ aliquem afficientes; cum febre incipientes; definito tempore apparent phlogoses, sæpe plures, exiguæ, per cutem sparsæ." Contagious diseases; attacking a person once only in his life; beginning with fever. At a definite period small eruptions appear, often numerous, scattered over the skin. These, you will allow, are very remarkable characters. They are not all strictly and universally true, perhaps, of all the forms of disease which I propose to bring now under your notice; but they apply with more or less exactness to continued fever, to the plague, to small-pox, chicken-pox, measles, scarlet fever, and erysipelas.

Hooping-cough, and the mumps, might be placed in the same catalogue, although there is no specific eruption on the skin in them: but I have already spoken of these two disorders.

Before I take up the consideration of any one of these diseases in particular, I shall premise a brief survey of certain circumstances that are more or less common to them all. This preliminary examination of the exanthemata as a class, will give you, I trust, clearer ideas respecting them: at any rate it will enable me to dispense with much needless repetition afterwards, and so to save both your time and my own; a matter of some consequence at this advanced period of the session.

In the first place, then, the diseases comprehended in this group are *contagious* diseases. You will hear persons disputing about the *term* contagion; but such disputes can only arise from the want of a distinct definition of the sense in which it is employed. I understand a disorder to be contagious, when it is in any way *communicable* from one person to another. Some would restrict the word contagion to the cases in which there must be absolute *contact* of the healthy body with the sick body, or with its visible off-scourings. When the disease can be conveyed through the medium of the atmosphere, or by means of other intermediate substances called fomites, they would call it *infectious*. And there is no objection to such a distinction, provided it is understood by the reader, or hearer, as well as by the writer or speaker. But since in all cases the disease is conveyed to the person of the recipient by particles of matter proceeding from the person of the sick, and since it seems very unimportant whether those particles are in a solid or in a gaseous form, whether they are imparted by direct contact of the two human bodies, or by being wafted through the air, or carried upon articles of clothing, I shall include both and all these modes of communication under the single term, contagion. This, in fact, is what is done in common discourse: all disorders that are "catching," I shall take leave to consider *contagious*.

In this sense I believe that all the diseases just now enumerated *are* contagious; some, no doubt, much more strongly and distinctly so than others. Some of them, indeed, are

undeniably contagious. For example, we are privy to, and sometimes willing agents in, the communication of small-pox from one individual to another. There are others concerning the contagious nature of which medical opinion is less settled and unanimous. Many persons deny that continued fever is communicable from person to person. The evidence from which I conclude that it is so, I will lay before you when I have described that disorder. Even they who admit that it is contagious, are of opinion, many of them, that it sometimes breaks out spontaneously, without the intervention of any specific virus. No one questions, I fancy, the contagious properties of measles; or of scarlet fever. Whether the plague, and whether erysipelas, be always or ever so produced, has been thought more doubtful. There is every reason for believing that the small-pox, at least, has now no other source than contagion. How it first arose it may be difficult to conjecture; but it is never known to originate spontaneously now-a-days.

Small-pox may in truth be regarded as the παραδειγμα, or type, of this group of diseases. I shall therefore take, by anticipation, some well-ascertained facts in its history, for the sake of illustrating the general subject. It is a malady which could scarcely be mistaken for any other; and of which the horrible aspect and fatal tendency are so strongly marked, that its appearance has always been watched with affright by mankind in general, and with intense interest by the philosophic physician.

In the acme of this disease, when it is severe, the whole surface of the body is covered with innumerable little pustules. A minute portion of the matter contained in any one of these pustules, just so much as may suffice to moisten the point of a lancet, is inserted, we will suppose, beneath the cuticle of a healthy man, who has not been near the sick man. What follows this engrafting? Nothing, apparently, for several days: but then febrile symptoms burst forth: and by and by a crop of papulæ appear sprinkled over the skin; and these gradually ripen into pustules precisely resembling that from which the engrafted drop was taken.

The very same phenomena ensue, if a healthy man enters the chamber of a small-pox patient, and breathes, for a certain time, an atmosphere tainted with the emanations from his body.

The points to be noticed here are—1, the manifest introduction of the virus into the system: 2, its dormancy, for a while, in other words the occurrence of a period of incubation: 3, the breaking out, at length, of a disease identical in its symptoms and in its character with that of the first patient: and 4 (most surprising of all), the enormous increase and multiplication of the poisonous matter.

I say the *history* of small-pox leads to the settled belief that this disorder, of which few persons are not readily susceptible, never occurs, except from contagion. It was quite unknown in Europe till the beginning of the eighth century. No mention of any such malady is to be found in the Greek or Roman authors of antiquity. Now whatever may have been the deficiencies of the ancient physicians, they were excellent observers, and capital describers of disease: and it is impossible that a disorder so diffusive, and marked by characters so definite and conspicuous, should have escaped their notice, or have been *obscurely* portrayed (if known) in their writings.

On the other hand, Mr. Moore, in his learned and interesting *History of Small-pox*, has shown that it prevailed in China and Hindostan from a very early period; even more than 1000 years before the time of our Saviour. That it did not sooner extend westward into Persia, and thence into Greece, may be attributed partly to the horror which the complaint everywhere inspired, and the attempts that were consequently made to check its progress by prohibiting all communication with the sick, partly to the limited intercourse which then took place among the eastern nations, but principally to the peculiar situation of the regions through which the infection was diffused; separated as they were from the rest of the world by immense deserts and by the ocean.

The disease is said to have broken out in *Arabia* at the siege of Mecca, in the year in which Mahomet was born; *i. e.*, in the sixth century. It was widely propagated by his wars, and by those of the Arabs afterwards; and, as I said before, it is generally believed to have first found entrance into Europe at the time of the overthrow of the Gothic monarchy in Spain by the Moors; when to avenge the well-known outrage upon his daugh-

ter "Count Julian called the invaders." Whensoever and wheresoever it came, it spread with fearful rapidity and havoc.

What I wish you to remark is this: that while almost all men are prone to take the disorder, large portions of the world have remained for centuries entirely exempt from it, until at length it was imported; and that then it infallibly diffused and established itself in those parts.

Of the more modern history of the disease our knowledge is more precise and sure. It tends uniformly to the same conclusion.

There was no small-pox in the New World before its discovery by Columbus in 1492. In 1517 the disease was imported into St. Domingo. Three years later, in one of the Spanish expeditions from Cuba to Mexico, a negro covered with the pustules of small-pox was landed on the Mexican coast. From him the disease spread with such desolation, that within a very short time, according to Robertson, three millions and a half of people were destroyed in that kingdom alone. Small-pox was introduced into Iceland in 1707, when 16,000 persons were carried off by its ravages; more than a fourth part of the whole population of the island. It reached Greenland still later, appearing there for the first time in 1733, and spreading so fatally as almost to depopulate the country.

Evidence to the same effect is furnished by the results of vaccination in some countries. In Mr. Cross's *History of a Variolous Epidemic* which occurred at Norwich in 1819, it is stated, upon good authority, that vaccination was adopted in Denmark, and made compulsory, in 1800. After the year 1808, small-pox no longer existed, and was a thing totally unknown. Whereas during the twelve years preceding the introduction of the preventive disease, 5,500 persons died of the small-pox in Copenhagen alone. Statements corroborative of this account have been made to me in the present year (1838) by Dr. Blick, an intelligent Danish physician, who was on a visit to London.

Now it is a very instructive fact respecting this disease, thus rankly contagious, and arising from no other source than contagion, that when it is epidemic in any place, many instances of it occur which we can by no means trace to contagion. Dr. Gregory tells us that of the numerous cases received into the Small-pox Hospital (to which he has long been physician) not one in twenty is capable of being referred to any known source of infection; the disease being ascribed by the patient to cold, fatigue, change of air, or some other innocent circumstance. A prisoner shut up in solitary confinement in the Penitentiary at Milbank was seized with small-pox. Surely this should warn us against inferring of analogous disorders (of continued fevers, for example) that they are necessarily not contagious, because we often fail to discover any way in which the poison could have been applied. If small-pox be produced by contagion alone, and yet the mode in which the contagious matter has been communicated eludes sometimes our closest scrutiny, then we must conclude that the same thing may happen in *other* contagious diseases, of which the contagious property may not be so strong or so obvious. Nay, the argument from analogy will lead us a step further. If once a disorder of this kind is decidedly proved to be sometimes the effect of contagion (and this I think I shall be able to prove to you of continued fever)—we cannot help entertaining a doubt whether the disorder in question really ever has any other cause. It is chiefly with a view to the light which they throw upon the obscurer subject of continued fever, that I am thus anticipating some points in the history of the contagious nature of small-pox.

Again, it is noticed of small-pox—and it is the same with the other diseases in this group—that the human body is not always equally susceptible of its contagious influence. Some individuals are more readily affected by it than others: and the same individual more so at one time than at another. There are even some who seem to be incapable of taking the small-pox—just as some, who are quite as much perhaps in the way of it as their neighbours, never become infected with the great pox. Of two hundred and fifteen persons who had not been vaccinated, nor had the small-pox, and who were living at Norwich in the same houses with persons ill of that disease, fifteen did not become affected with it; and of these fifteen it was ascertained that ten had escaped under similar circumstances of exposure before. I mentioned, on a former occasion, the fact that a certain dog, in Paris, could not be made to take the contagion of rabies.

It is not at all uncommon for persons to resist the influence of contagion at one period,

and to yield to it at another, even when the exposure has appeared to be less complete. Mr. Cross gives a striking example of this. A man, who believed that he had had the small-pox, lived for twelve years as a nurse in an establishment for the reception of persons inoculated with that disorder. At the end of that time he caught the small-pox, which proved fatal to him. Now this might have been, and probably was, as the man supposed, a *second* attack. The late Mr. Lockley told me an instance still more remarkable, as being free from that ambiguity. Nearly the first patient he ever attended, if not the very first, was an old woman, who for years had been in the habit of going from village to village as a nurse; and of nursing a great number of persons labouring under small-pox, which she had never had, and against which she (naturally enough) believed herself proof. At length she was taken ill, and died of small-pox, under Mr. Lockley's observation, at the age of eighty-four.

In many cases we can assign no reason for these variations and differences. Age seems to have something to do with them. Infants are but little susceptible of the operation of contagions. Debility, howsoever produced, certainly augments the disposition to be affected by this, as by other causes of disease. The dose and strength of the poison must also be taken into account. As some men can drink a much larger quantity of wine (which is an alcoholic poison) than others, without being intoxicated, and are differently influenced by the same quantity at different times, so is it also with the animal poisons we are now considering; so is it, as I showed you before, with the mineral poison of mercury.

This fluctuating power to resist contagion is most conspicuous, perhaps, when viewed in reference to scarlet fever. After the very earliest periods of life, children catch infectious disorders of all kinds readily enough; more readily than in mature age. The poison of scarlet fever operates with less certainty upon adults than the poisons of small-pox, or of measles. Some medical men escape scarlet fever altogether, although brought much into contact with it by their vocation. I do not know that I ever had scarlet fever.

Another fact, well worthy of notice, is, that small-pox, which is so rankly contagious, and which has at present no other source besides contagion, has its alternate periods of slumber and of activity. This metropolis, and most of our large towns, are never entirely free from it. Scattered cases occur, here and there; and when thus thinly disseminated, the disease is said to be *sporadic*. But there are seasons in which it spreads rapidly and extensively, and assumes the form of an *epidemic* distemper. We are now living (1838) in the midst of one of these epidemics of small-pox. The same is equally true of the other complaints included in this group. Sometimes they are confined to single families; sometimes they pervade a whole district.

Hence you can never infer that any febrile disorder is not contagious, merely because it prevails epidemically. Many epidemic diseases are not contagious. But the two properties may and do meet in the same malady. They are not to be set in opposition to each other, or regarded as incompatible properties, as they have been by some ingenious writers.

With respect to these epidemic visitations of the exanthemata, certain general facts have been ascertained, very useful and necessary to be known.

1. The strength of the contagion, and the severity and fatality of the disease, vary at different periods of an epidemic. In general the contagion is the most active, and the disorder the most fierce, at the outset of the epidemic. By degrees its violence slackens, and it ceases to spread. This is to be partly explained by the circumstance that the number of persons who are susceptible of the disease, and who have not yet been attacked, are fewer and fewer as the epidemic proceeds. The fire languishes for lack of fuel. But this does not seem to be all. The disease dies out before it has affected all those who are capable of receiving it. We might I think expect, prior to experience, that the earlier cases would usually be the severer; for the weak, who are less liable to struggle with the complaint, and those who, by peculiarity of constitution, are most susceptible of the morbific influence, are likely to be the first to suffer.

2. There are great varieties also in the general character of the symptoms that occur in different epidemics of the same disorder. At one time, or in one place, inflammatory symptoms run high; in another place, or at another time, there is an early tendency to debility and sinking. One epidemic is more malign than another. And the practice

varies accordingly: so that these are facts of the greatest importance. The prevailing character of the malady is attributed to what is called the *epidemic constitution of the season*. And when we have made out, by observation, what this epidemic constitution is, we have obtained a clue to the proper management of the disorder. Thus continued fever, as it has appeared in London during the last ten years, has required and borne far less depletion than it did for the preceding ten years or more.

You may learn from this how dangerous it is to apply indiscriminately in one epidemic the remedies that may have been found useful in another: and also how foolish and unfair it is to censure the practice employed and recommended by others, merely because it differs from that which we, in other epidemic visitations of the same disorder, have considered fitting and beneficial.

These differences in the prevalency of the disease, and in the character of its symptoms, are not to be explained by any variation in the exciting cause, which is a definite animal poison; nor can they be reasonably ascribed to any appreciable quality or agency of the weather *at the time*. They must depend upon changes that have been slowly wrought upon the human body: and those changes, constituting an acquired predisposition, are probably due to *previous* conditions of the atmosphere, which have exercised a long and gradual influence upon all the individuals of a community.

After stating, in the first clause of his definition, that the exanthemata are contagious diseases, Cullen announces, in the next place, the very curious fact, that they occur but once in a person's life. "Semel tantum in decursu vitæ aliquem afficientes." In this they offer a remarkable contrast to inflammations, which, having happened once, are, for that very reason, more apt to happen again.

You will take care to observe, that it is not the mere circumstance of the disease being *contagious* that makes the difference. Those disorders which shield the system against their own future recurrence are, all of them probably, contagious; but the converse does not hold. It is not true that all contagious disorders protect the constitution from their own return. Syphilis, purulent ophthalmia, the itch: these not only do not secure a patient from a repetition of the disease, but perhaps they even render him more liable to it in future.

Neither is the proposition absolutely and invariably true of any disease. Like most general rules, it admits of occasional exceptions. There is not one of the group enumerated in the beginning of this lecture, which has not been known to occur more than once in the same person. Small-pox has, in many instances, affected the same individual twice; even when the first attack had been so severe as to have engraved deep traces of its visit upon the skin. There are a few instances recorded of its *third* occurrence. It was believed, at one time, that whenever the disease was thus repeated, it was *always*, in the first instance, severe, which, taken in conjunction with the repetition, was thought to indicate a strong natural susceptibility of the disorder. But it has since been noticed that the primary visit is sometimes unusually mild: and this fact (so powerful is the love of theory) has led to the supposition that the first attack was not sufficiently intense to affect the whole mass of blood, and to destroy the inborn susceptibility. I believe that the two attacks have always been separated by a considerable interval of time. I have myself known one very striking instance, about which there could be no mistake, of the recurrence of measles in several children of the same parents. This proclivity to be again affected by the specific poison seems to run in families. It is less uncommon for scarlet fever to happen a second time in the same individual. No contagious disease therefore furnishes complete future protection against itself. But that this privilege belongs, as a general rule, to small-pox, to measles, and to scarlet fever, there can be no doubt: and the validity of the rule is applicable in the order in which I have here mentioned them. It applies also, though less uniformly, to the other exanthemata. A person who has suffered a well-marked attack of continued fever is far less liable than another, according to my experience, to have that disease again. The plague is said to afford a temporary safeguard against itself. Very few persons have it twice in the same season. During one epidemic Dr. Russell found that, among 4400 individuals who underwent the disease, only twenty-eight contracted it a second time. The singular property we are considering is less plainly visible in erysipelas than in any other malady of the group.

The next clause in Cullen's definition asserts the supervention of the cutaneous marks, in technical language of the *eruption*, at fixed times (*definito tempore*) after the commencement of the general fever. It is clear, therefore, that the cutaneous inflammation cannot be the cause of the fever, but is itself an effect of the contagious poison. Here again we have a mark of distinction between febrile exanthemata, and inflammatory fevers, or what Cullen calls the phlegmasiæ, in which the local inflammation commonly precedes the pyrexia.

In point of fact, the circumstance to which I have just referred shows the impropriety of ranking these diseases under the head of *cutaneous* diseases. They would more rightly be called *blood* diseases. The disseminated cutaneous inflammation is a curious and an important circumstance; and it is the symptom which, in the majority of cases, is most *distinctive* of the disorder: but it is not an essential circumstance. Thus, although there often is a manifest eruption (quite independent of common petechiæ) in the earlier stage of continued fever, a lenticular mottling, somewhat like that of measles, yet it is often absent altogether. And the very same thing happens in other diseases of the same group—diseases which all the world considers and calls cutaneous. A febrile affection, often a fatal one, but attended with no rash, proceeds sometimes from the contagion of scarlet fever. The fever—or rather the patient—is *not* scarlet. The worst form of *cynanche maligna* is of this kind. It is just the same in measles. Authors speak of morbilli sine morbillis; of scarlatina sine scarlatinâ; and even of variolæ sine variolis. Catarrhal symptoms certainly occur sometimes in weakly children who have been fully exposed to the contagion of measles; and the complaint has proved fatal, without there having been any eruption at all. So in the plague: certain cases occur in which there are no buboes nor carbuncles, yet which undoubtedly originated from the common contagion of the prevailing epidemic.

The time at which the eruption comes out differs in the different diseases; and even in the same disease it is subject to occasional variation. The rule with respect to small-pox, is, that the spots begin to be visible on the third day; that on which the sickness and fever commenced being reckoned the first. As far as I have observed, this rule is a very constant one. It has been noticed, however, that when the disease is confluent, and therefore severe, it occasionally shows itself in eruption on the second day; and when quite distinct and mild, sometimes not till the fourth.

The regular period for the outbreak of the cutaneous affection in measles is the fourth day: it scarcely ever begins sooner; but it is often later—on the fifth or the sixth day, or even later than that.

Cullen assigns the fourth day for the ordinary appearance of the rash in scarlet fever also. But in this he is decidedly wrong. Sometimes it is, I believe, perceptible on the first day; but its most general period is the second day. In severe and unfavourable cases it may be postponed till the fourth day, or longer.

The eruption, which is tolerably constant in some varieties of continued fever, observes less regularity in the time of its arrival.

The period which intervenes between the reception of the poison, and the supervention of distinct symptoms—the period during which the virus, though doubtless at work, seems to lie dormant in the system—the period (in one word) of *incubation*, differs also in the different diseases of this group, and varies even in different cases of the same disease. Although no marked changes occur during this period, I believe that some slight deviation from the usual condition and feelings of the patient might often be observed, if they were expected and looked for.

The period of incubation in continued fever is very uncertain. In a paper upon this curious subject, published in the ninth volume of the *Medical Gazette*, Dr. Gregory states it as his opinion, derived from much inquiry, that ten days is the *average* period. Dr. Haygarth reckoned the minimum period at seven; the maximum at seventy-two days. Sir William Burnett, in his *Account of a Contagious Fever at Chatham*, gives the history of a party of men belonging to the *St. George*, lying at Spithead, who were sent, on the 3d of January, 1811, to assist in navigating the *Dolphin* troop-ship; the crew of which were affected with typhus fever. On the 10th (seven days after exposure) fourteen of these men were sent to the Hospital-ship from the St. George, ill with the fever;

and many subsequently, up to the 21st of January (the eighteenth from exposure), after which period no cases occurred.

The period of dormancy is more definite, yet still liable to some variation, in most of the other exanthemata. "At the Small-pox Hospital," says Dr. Gregory, "abundant evidence has been afforded that the period of incubation is usually about twelve days." It is a remarkable fact that, "when the small-pox is received into the system by inoculation, seven days only elapse between the insertion of the virus and the establishment of the fever."

Dr. Bateman puts the period of incubation in measles at "from ten to fifteen days." I have known several instances in which the date of a single short exposure was exactly ascertained, and in which the disease commenced precisely a fortnight afterwards. In scarlet fever the average period is shorter; not more than from four to six days. In the plague it is, I believe, more variable; but generally not exceeding a few days. Dr. Russell tells us that, among those inhabitants of Aleppo who shut themselves up after having been in the way of the contagion, no instance occurred of the appearance of the malady later than the ninth or tenth day.

To say that a febrile disorder is contagious, is the same thing as to say that it is produced by an *animal* poison. Now there are many poisons, very deadly poisons too, which cause diseases that are not communicable from person to person. That particular poison, the *malaria*, is of this kind.

Of the inorganic poisons some are taken into the blood, and emerge again from the body, unaltered, with one or more of the ordinary secretions; chiefly with the urine. They may induce changes in the body as they pass; and if these changes be salutary, the substances so inducing them become medicaments. If the changes be destructive or injurious, they are strictly poisons.

Other of the inorganic poisonous substances do not find so ready an exit from the body. They enter into permanent chemical union with the constituent tissues of particular organs. In this way, to use the words of Liebig, they deprive the organs of the principal property which appertains to their vital condition, viz., that of suffering and of effecting transformations. If the organs of which the functions are thus destroyed, be vital organs, these poisons are fatal.

But the animal poisons, those at least with which we are now concerned, act in a totally different manner. They affect changes in the blood, whereby they are themselves abundantly multiplied or reproduced; and the eruptive disease that ensues seems to be the mode provided by nature for the escape or the expulsion of this newly-formed morbid matter from the system. This is the old-fashioned humoral pathology; founded on bold, unproven speculation: and it is most curious to see these very doctrines, which had sunk into universal discredit and contempt, now again assuming their places, as scientific truths, upon the secure basis of organic chemistry. A wonderful specimen this of the sagacity of the older physicians—of the despised wisdom of our forefathers.

The ancients attributed various disorders to a fermentation of the animal fluids. The cause of fever, according to Hippocrates, was some morbid matter in the blood. This matter, by a process of concoction, was brought, in a certain number of days, into a state in which it was ready for expulsion from the body. It was then thrown off by hæmorrhage, by sweat, by alvine discharges; or deposited upon the surface in the form of abscess, or cutaneous eruption: and these eruptions or evacuations constituted the crisis of each fever.

The doctrine thus enunciated by the father of physic is very nearly the same with that which Liebig is teaching in the nineteenth century. This distinguished chemist ascribes the phenomena which succeed the introduction of certain animal poisons into the blood, to a process exactly resembling fermentation. Let me try, in a few sentences, to expound to you his views on this deeply interesting subject.

You know that the brewer excites the fermentation of his *sweet-wort*, by adding to it a small quantity of *yeast*. Wort is an infusion of malt, and contains sugar and gluten, with other vegetable matters, in solution. Yeast is putrefying gluten; and its component particles are, therefore, in a state of intestine motion or transposition. When placed in contact

with sugar in solution, it has the property of communicating a similar intestine motion to the elements of the sugar, whereby they arrange themselves into new and simpler forms; namely, into alcohol and carbonic acid. If there were no gluten in the wort, this would be the whole of the process: during which the added yeast disappears.

But the decomposition or *fermentation* of the sugar reacts upon the gluten in the wort, and converts it gradually into yeast, which, mingling with the liberated carbonic acid, rises and floats upon the surface of the fermenting liquid. So that, when the process is completed, there has been produced thirty times as much yeast as was originally added to the wort.

Now this is but a type of what happens in other fluids under analogous circumstances: and it may be laid down as an abstract proposition in Liebig's, or rather his translator's words, that "a substance in the act of decomposition, added to a mixed fluid in which its constituents are contained, can reproduce itself in that fluid, exactly in the same manner as new yeast is produced when yeast is added to liquids containing gluten."

Thus the virus of small-pox (which virus is formed out of the blood) causes such a change in the blood as gives rise to the reproduction of the poison from the constituents of that fluid: and whilst this process is going on, the natural working of the animal economy is disturbed: the person is ill. The transformation is not arrested until all the particles of the blood which are susceptible of the decomposition have undergone the metamorphosis.

Liebig shows that similar processes may take place in mixed fluids (and, therefore, in the blood) without the regeneration of the added substance: just as the fermentation of a solution of sugar is effected by the addition of yeast, without any reproduction of the yeast, if there be no gluten in the saccharine solution. In such cases, the disease, which accompanies, or results from, the transformations that occur in the blood, is not contagious: the poison is not renewed. It is thus, apparently, that certain *miasms* produce disorders which are not communicable from person to person.

In order that a specific animal poison should effect its own reproduction in the blood, and excite that commotion in the system which results from the formation and expulsion of the new virus, it is requisite that a certain ingredient (analogous to the gluten in the brewer's sweet-wort) should be present in the blood: and this ingredient must have a definite relation to the given poison.

If this ingredient be indispensably necessary to life, the poison, which transforms and destroys it, is inevitably a fatal poison. May not this be the *modus operandi* of the poison of hydrophobia?

Again, if this ingredient be wanting, no reproduction of the poison takes place; nor, of course, any of those symptoms which are consequent upon such reproduction. The poisonous qualities of the animal substance are not developed. It ceases to be a poison.

And this ingredient, if naturally present, is exhausted and destroyed, for a while at least, by the operation of the poison. Hence, for a while at least, the same disease cannot be again produced by the agency of that poison.

Supposing the ingredient to be one which is not essential to the composition of the blood, and to have been thus destroyed or exhausted, it may never be replaced. Or it may be replaced only after a long interval. In some persons it may never exist at all; or it may exist at certain periods only of their lives. It may even be acquired by unnatural or peculiar modes of living.

All this is not only very possible, but probable. A certain number of peculiar substances do certainly exist in the blood of some men which are absent from the blood of others. In childhood and in youth the blood of the same individual contains variable quantities of substances, which are not to be found in it at other periods of life.

This theory of Liebig's offers, then, a reasonable explanation, the only explanation, indeed, that I have ever met with—of the curious facts, that certain contagious disorders furnish a protection, temporary or permanent, against their own return; that they have a tolerably definite period of incubation, and run, for the most part, a determinate course; that some persons are less susceptible than others of the influence of these animal poisons, or not susceptible at all; and that the same individual may be capable of taking a contagious disease at one time, and not at another.

Moreover, the light supplied by this theory gives distinctness to our conceptions respect-

ing certain deviations from the regular course and type of these diseases; which deviations are not uncommon.

Thus the symptoms which precede and usher in the eruption are sometimes slow, halting, and irregular in their progress: appear, and then recede, and reappear, so that we are in doubt what is about to happen, until at length the disease declares itself in its decided and authentic form.

We may suppose this to depend upon some tardiness or interruption of the process, whereby the virus is (to use the ancient term) concocted.

Again, the series or combination of symptoms that mark the specific disease is sometimes, as I stated before, *incomplete*. We have the eruption of measles without the catarrhal symptoms: the sore throat without the rash of scarlet fever. And experience has found that, where the malady is thus imperfectly developed, the protection it confers against its own recurrence is also incomplete. To explain this double failure we may reasonably infer a corresponding defect in the series of changes which the poison tends to produce in the mass of the blood.

Glandular enlargements and chronic abscesses are frequent *sequelæ* of these exanthematous disorders. They may be considered to represent the dregs of the reproduced virus, which has been imperfectly eliminated from the system by the usual channels.

Such is a brief exposition of Liebig's ingenious theory. Do you ask whether I adopt it, with implicit credence in its truth? I answer, "By no means." Respecting points so curious, it is scarcely possible to refrain from speculation altogether. These views come recommended by the authority of a consummate chemist. They furnish a plausible explanation of the main facts of the case: namely, that the disease is produced by an animal poison; that the specific virus increases prodigiously in quantity within the body during the progress of the disease; and that the susceptibility of its influence in that individual, is thereby somehow exhausted. I entertain the theory, therefore, until a better one is propounded. It has this incidental merit, that it involves no risk of practical error.

The subtle contaminating effluvia which proceed from the bodies of the sick enter the blood of those who catch the disorder, chiefly, I imagine, by being inhaled into their lungs in breathing. The poison may, perhaps, be capable of being spontaneously absorbed through the skin: and upon this supposition oil has been smeared over the surface with the view of shutting out the contagion of the plague. The virus may gain direct entrance into the blood; we know that it sometimes does so, for we ourselves insert it, in inoculation of the small-pox. Dr. Francis Home imparted measles by engrafting some blood of a person ill of that complaint; and subsequent attempts to excite the disease in that way have been equally successful. Some rash and unfortunate trials have proved that the plague is communicable by inoculation with matter from the buboes.

Endeavours have been made to estimate the distance to which the influence of different contagious emanations extends. The effluvia in small-pox, measles, and scarlet fever, are the most active; operate, I mean, at the greatest distance. In continued fever they have a less range; and in the plague the diameter of the infectious circle is probably very small. Some have even supposed that the plague is communicable only by actual contact; but the opposite opinion seems the more likely, namely, that you may *touch* plague patients with impunity (as Bonaparte is known to have done on a memorable occasion) if you avoid inhaling their breath, or the effluvia proceeding from their bodies.

The most important practical result of the experiments made by Dr. Haygarth and others, for determining the absolute distances to which the power of the contagion extends in different disorders, was, that *where ventilation is complete*, in other words, where the gaseous poison is freely diluted with atmospheric air, the sphere of its operation is very limited.

It is an interesting subject of inquiry, worth glancing at for a moment, how far the power of different contagions is modified by differences of temperature. Small-pox is readily propagated either in hot or in cold regions: in Mexico near the equator, in Greenland towards the pole. The plague does not spread when the temperature is below 60° or above 90° Fahrenheit. The vaccine matter loses its property of producing the cow-pox if it be exposed for a certain time to extreme cold, or to a heat of 95°. Typhus fever,

measles, and scarlet fever, are said to be of extremely rare occurrence in the intertropical regions. Dr. Henry has turned these facts to useful account by proposing to decompose and destroy certain contagions lurking in fomites, by the operation of artificial heat.

Having thus pointed out many circumstances of interest, which are common to all, or nearly all, the diseases grouped together under the title of exanthemata, I may now proceed to a more particular account of those diseases in succession. And I shall begin with *continued fever;* because, although it does not afford the best-marked example of the collection of symptoms that compose Cullen's definition of the order, yet a right understanding of the practical points concerned in the management of this febrile disease will assist us materially towards a just conception of the modifications of treatment that may be required by the rest.

Continued fever.—We hear continually, both in and out of the profession, different species of fever spoken of. By the public, typhus fever, brain fever, bilious, putrid, low, nervous. And systematic writers are to the full as particular: mucous fever, ataxic, adynamic, gastro-enteric, and so forth. Now, admitting that fever shows itself under various forms, I am persuaded that the effect upon the mind of all this subdivision is bad and hurtful. It encourages a disposition, already too prevalent, to prescribe for a disease according to its *name*. There is *no* line of genuine distinction between continued fevers, that can be relied on. They run insensibly into each other, even the most dissimilar of them; and are traceable often to the same contagion. I shall in the first place, therefore, attempt to describe this disease of continued fever as it occurs in its most ordinary and simple form; and then point out its principal *varieties*.

Symptoms.—Continued fever does not always commence in the same way. It often happens, that for several days before the disease assumes its distinct and proper aspect, and before the patient is rendered unable to pursue his usual occupations, he is affected with certain morbid symptoms, which may be considered premonitory of the fever; so that it is sometimes difficult to mark the precise beginning of the disease. These preliminary symptoms result apparently from an altered condition of the *nervous system*. The poison in the blood disturbs the functions of animal life before it causes any palpable derangement in the mechanism of the circulation. The expression of the patient's countenance alters; he becomes pale, languid, and abstracted. Those about him observe that he is looking very ill. He is feeble, and easily tired; reluctant to make any exertion of mind or body; listless, and apprehensive often of some impending evil. He loses his appetite; his tongue becomes white and inclined to tremble; the bowels are irregular, often confined, sometimes affected with diarrhœa; his senses lose their natural delicacy. He has uneasiness or wandering pains in various parts of the body; and occasionally there is some giddiness: drowsiness, perhaps, during the day, and unsound and unrefreshing sleep at night. To collect all this into one expressive word, the patient evidently *droops*.

In other cases these preliminary movements are altogether wanting. Chomel gives the following comparative account, deduced from the exact observation of 112 patients in this particular. In 73 of these cases the invasion of the disease was sudden, without any warning, in the midst of apparent good health. In 39 there were prelusive circumstances.

The regular onset of the fever is, very frequently indeed, marked by a *shivering* fit. Another common phenomenon at the period of the invasion is *severe headache;* pain or aching across the forehead. According to Chomel, the headache is usually first experienced by the patient when he gets up in the morning. But this certainly is not a constant symptom. There is sometimes a sense of heaviness and vertigo rather than headache. Another symptom which sometimes, but not so often, sets in with the fever, is *diarrhœa:* and this is an important circumstance when it does occur. It is attended, perhaps, with pain or uneasiness in the abdomen. You will also perceive, even when there have been no premonitory circumstances, that symptoms arise, even thus early, which belong to the nervous system, and denote some disturbance and alteration in the functions of sensation, thought, and voluntary motion. They are comprised under the general phrase, "*febrile oppression*," and they are different from what we notice when pyrexia supervenes upon inflammation. You will obtain a clearer notion of what this term, febrile oppression, means, by watching at the bed-side of *one* patient in this disease, than by any description

I can give you. There is great inaptitude for exertion of the power of thought, or of motion. The expression of the face is dull and heavy, absent, puzzled. The patient presents very much the appearance of a person made stupid by drink: and he staggers a little if he attempts to walk. The muscular power is sensibly enfeebled: sometimes the patient will struggle against this; but in a few hours, or in a day or two at furthest, he takes to his bed.

These are the symptoms which mark the outset of the disease we are about to consider. They occur sometimes in other disorders of the class in which I have placed continued fever. In the plague, for example. The patients appear like people who are drunk. Now these symptoms result, no doubt, from changes which are going on in the blood, and which make an early and a strong impression upon the nervous system. And there is another circumstance which, when it is observable, denotes a depressed state of the nervous power. Practitioners, sometimes, are in doubt whether the case may not be one of some visceral inflammation: or, perhaps, knowing it to be continued fever, they still think it expedient to *bleed* the patient. Now faintness or actual syncope is much more easily produced by the abstraction of blood, in *continued fever*, than it is in *inflammation:* and this fact may occasionally be the means of distinguishing between incipient continued fever, attended, for instance, with catarrhal symptoms, and pure incipient pneumonia.

In order the more clearly to describe the course of continued fever, I shall divide it, as others have done, into periods; weekly periods. Not that there is any such period of seven days allotted to particular symptoms; but that in the simplest forms of the disease, when it runs its course most evenly and favourably, and therefore, we may suppose, the most *regularly* also, there is a succession of different *sets* of symptoms, which occupy each *about* that space of time; nearly enough to allow of my taking it, I say, as a help to the better *describing* the disease.

Many of the symptoms which occur during the first stage of the disorder—during the first week, we will say—are such as belong to the sanguiferous system. The pulse becomes more frequent than in health, there is increased heat of skin, and thirst; headache, and throbbing of the temples. The pulse varies considerably in different cases. Generally, I say, its frequency augments; but sometimes it is even slower than natural. The acceleration of the pulse is greatest (*cæteris paribus*) in those constitutions which are the most irritable. In young persons, in females, and in weak or delicate males, it will often rise, soon, to 120: while in stronger adults, it does not so early attain its maximum of frequency, and perhaps does not exceed 100 throughout the whole course of the disease. Should the pulse in any instance reach 130, or 140, the disease is severe: and the majority of such patients die. The absolute frequency of the pulse is not, however, of so much importance in this disorder, as its steadiness. If it shifts from one number to another, *that* affords a worse prognostic even than its being very frequent; provided it keeps at the same standard. The skin, during this period, is generally hot and dry, and it *feels* to a bystander very hot and *pungent*. The actual heat, however, is not so great as the sensation given to the hand might persuade us it was. Dr. Bateman found that, in a majority of the cases treated by him, the heat, as ascertained by the thermometer, was about 100°; and he never observed it higher than 104°. The thirst is usually troublesome for the first few days. The tongue becomes clammy or dry; sometimes it is clean and smooth; more often furred; its edges and tip will, perhaps, be red, then a white fur will begin, which either covers the central part of the tongue, or is divided by a straight brown streak which occupies its middle portion. This brown streak is often the first step to dryness and blackness of the tongue.

During the same period a careful examination of the abdomen will detect indications of diseased action *there*. Sometimes diarrhœa is an early symptom; generally it is postponed, I think, to the next period; or to the latter part of the first. When it does occur, the stools are, for the most part, loose and frequent; and either of a dark colour, and fœtid, or of a yellow-ochre appearance, like pea-soup somewhat. If you make pressure upon the abdomen, you will find it unnaturally hard and resisting, as though its walls were made of pasteboard; slightly tympanitic, as you learn by making percussion. Frequently, uneasiness is manifested when pressure is made on the belly, particularly over

the cæcal region; and Chomel remarks that another symptom, not commonly met with in other diseases, is usually noticeable in the first stage of this, viz., a little gurgling movement, evidently from the intermixture of liquid and gas within the bowel, which movement becomes audible, or palpable to the hand, upon pressing the same region. This symptom is still more common in the more advanced stages of the disorder.

There is evidence, frequently, of a slight affection of the membrane lining the air-passages, from nearly the first: some notable quickness of respiration, and some diffused rhonchus and sibilus, audible through the stethoscope.

And among all the indications of increased action in the circulating system, the symptoms that relate to the nervous centres remain perceptible. The aspect of the patient is peculiar: the features fixed and inexpressive; or expressive merely of apathy and indifference. If he is spoken to briskly, he responds; and although his sensibility seems blunted, his answers are, as yet, rational, and to the purpose. Delirium does not come on, in general, till towards the end of the first week. The muscular power is greatly depressed. The patient lies on his back, motionless; he sleeps but little, waking often; and the short snatches of repose which he seems to get, are disturbed, apparently, by uneasy dreams: and he *fancies*, and says perhaps, that he does not sleep at all. Sometimes, even during the first stage of the disorder, when the bowels are relaxed, the prostration of strength is so great, or the tendency to stupor and indifference is so marked, that the stools are passed under him as he lies in bed, without any apparent endeavour on the part of the patient to prevent it; and without any notice of his wants being made to his nurse. The urine, during the same stage, is scanty, and high-coloured, and ill-smelling often. Towards the very end of the first weekly period, the eruption which is peculiar to continued fever sometimes begins to show itself: but this is commonly postponed to the next stage; and I shall describe it in connection with the other symptoms that are apt to occur in the second week of the disorder.

It is seldom, except in very malignant forms of continued fever, that death takes place during its primary stage. Of forty-two cases treated by Chomel, one alone was fatal in that period.

LECTURE LXXXIV.

CONTINUED FEVER, CONTINUED. PHENOMENA OF THE SECOND WEEK; DELIRIUM, AN ERUPTION, DIARRHŒA: OF THE THIRD WEEK; RECOVERY, OR DEATH IN THE WAY OF COMA, OF APNŒA, OF ASTHENIA. SYMPTOMS THAT USHER IN THOSE MODES OF DEATH; MORBID CHANGES FOUND AFTER THEM.

IN the last lecture I commenced the consideration of that important disease, which is best known in this country, under the name of continued fever. I told you my opinion that there is but one species, although there are many varieties, of continued fever. Sometimes this disease is preceded by symptoms of a slighter disturbance of the system; and sometimes it sets in suddenly, in the midst of apparent health. Whatever premonitory symptoms may take place, they indicate some alteration in the functions of the *nervous system*, upon which many pathologists have supposed that the first and most direct impression is made, by the exciting cause of the fever. But the exciting cause, in many cases, probably in all, is a specific poison received into the blood: and all analogy is in favour of the belief that the primary change is wrought upon the *blood* itself. The whole mass of the blood is gradually vitiated; and the first evidence of the circulation of this altered fluid, is depression of the powers and functions of animal life. Among the earlier symptoms of the declared disease, shivering, headache, and occasionally diarrhœa, take the lead.

For the convenience of description I divided the course of the disorder into three weekly stages: not that it necessarily *runs* its course in three weeks, but because the sets of

symptoms which succeed each other while the disease is in progress, occupy, in the cases which seem to proceed the most regularly, *about* the space of seven days each.

Now the symptoms present during the first week are expressive of disorder both of the sanguiferous and of the nervous system. The patient is hot, flushed perhaps, and thirsty, and he has a frequent and hard pulse. Besides this he manifests indifference, and stupor; his senses are blunted; his intelligence is diminished. His muscular strength is reduced in a remarkable manner; so that he cannot sit up: in many cases he cannot even lie on his side, or turn himself about well in bed, but remains in the supine position: and if he be purged by medicine, or spontaneously, the stools are apt to pass from him into the bed, without his knowing it, or without his taking any care to prevent it. This, however, is more common, and more marked, in the second period or week; the phenomena of which I next go on to sketch.

In the first place, in mild cases, and in some epidemics, the patients begin to *improve* soon after the first week. Dr. Welsh, in his Account of Fever as it occurred some years since in the Queensbury-House Establishment in Edinburgh, says that of 743 patients, 373 (that is, fully one-half) had passed the worst, or had begun to get better, by the ninth day; and several of the physicians who witnessed the fever that prevailed in Ireland in the earlier part of the present century, concur in declaring that, in a vast majority of cases, the disease had "taken the turn," and the patient begun to mend, by that time. But the epidemics, in these cases, were undoubtedly mild. In general, no change for the better takes place at the end of the first seven or eight days; but what are called the *typhoid* symptoms develope themselves more distinctly.

The changes that occur are usually the following:—the pulse becomes more frequent, weaker, and more compressible. The tongue grows drier and browner. More sordes, and of a darker colour, accumulate on the teeth and lips: and it is in this period that delirium is most apt to ensue; and that certain *eruptions* are most often observed. But the symptoms that relate to the nervous system are often still the most prominent. The patient generally loses his *headache*. His voluntary movements, however, become very much weakened, and are sometimes exercised irregularly. The posture which the patient in this stage almost always assumes is, I say, indicative of this weakness; he lies on his back, and he *sinks down in the bed*, slips towards the foot of the bed. He is unable to make or bear that degree of voluntary exertion which would be necessary to place him upon his side. Hence we hail it as a good omen—because it is an indication that the patient still retains some strength—if we find him on his side, or even on his back with his knees drawn up. Other proofs of muscular debility, approaching to palsy, are apt to present themselves. The voice becomes feeble; the patient can scarcely utter an audible sound. Perhaps he is unable to swallow. This is a very bad symptom, though it is one that has been recovered from. Sometimes it seems that the power of deglutition is not lost, but the sick man is too listless to try to swallow; or the dry and parched state of his tongue and throat render it difficult and painful for him to attempt to do so. The patient is apt to lie with his mouth open: and breathing thus through the mouth tends to dry the tongue. Hence it is well to desire the sick person to swallow a mouthful or two of water, and so to moisten his tongue, before you decide upon the state of that organ, or upon his facility of deglutition. Often, in bad cases especially, there are little convulsive startings of the tendons, (*subsultus tendinum* is the technical name of the symptom,) and other irregular and involuntary actions of the muscles: tremulous movements, especially of the tongue and of the hands; and sometimes the patient is unable to put out his tongue at all. There are two symptoms which present themselves in the majority of instances in the second week of the fever, and which deserve your particular attention: I mean delirium, and the eruption which belongs to the disease.

The delirium is peculiar. The patient wanders, at first, in the night only; and the delirium commonly appears on his awaking from disturbed sleep. Sometimes he is desirous of getting up, and talks incessantly and earnestly in a loud voice, and can only be kept in bed by the imposition of some restraint. Usually, however, his rambling is of a tranquil kind, and without agitation. His mind seems elsewhere: he is inattentive to all that passes around him; but he lies still, muttering disjointed words or sentences, like a man talking in his dreams. From this state of *typhomania* the patient may sometimes

be roused by loud speaking addressed to him, or by the sight of a strange face; so that, though incoherent and delirious just before, he may become collected when his medical attendant enters the room. But he presently relapses. During the delirious state there is a great deficiency of sensation, and insensibility to impressions. The patient is deaf. This deafness you may hear spoken of as being a good omen, or favourable sign; but it is so only by comparison: it indicates a condition of brain less perilous than its opposite, in which the sense of hearing is morbidly acute. Imperfection or loss of vision is much rarer, and much more dangerous, than deafness; yet the eye is generally dull—unlike the brilliant eye of acute phrenitis; it corresponds with the expression of the countenance, which is perplexed rather than wild. Sometimes, however, as the disease advances, black spots, like flies on the wing, *muscæ volitantes*, appear before the patient's eyes: in consequence, it is presumed, of partial insensibility of the retina. The patient attempts to grasp or catch these in the air, or to pick them from the bed-clothes. This is called *floccitatio*. After these symptoms recovery is not common. The mouth and tongue are dry; yet the patient no longer complains of thirst. The taste, the smell, the sense of touch, are all impaired: even external inflammation may take place, especially about the hips and sacrum, and go on to gangrene without any complaint of pain from the patient. He seems altogether careless about the issue of his disorder. If, at this period of the fever, you ask him how he does, he will probably declare that he is quite well. I have already alluded to the involuntary passage of the fæces; this may depend, in part, especially in the advanced stage of the disorder, upon debility or paralysis of the sphincter muscles. The urine also dribbles away frequently: and these are points which must always be looked after; first, for the sake of keeping the patient as clean and dry as possible, the irritation of the urine and fæcal matters tending to produce sloughing ulceration; and, secondly, with the view of preventing the bladder from becoming unduly distended. Retention of urine, and all its bad consequences, may otherwise occur. It is a good general rule, therefore, to examine the hypogastric region every day with the hand; and also to ask *to see* the urine, not for any purposes of prognosis, but to ascertain that it is regularly discharged.

It is also, I repeat, in this stage of the disorder that the rash or eruption, which so often accompanies it, very frequently shows itself. Sometimes it is noticed earlier. It consists of small rosy blotches, of a roundish or lenticular shape; scarcely raised, if raised at all, above the general surface of the skin on which they appear. Chomel states that they vanish under pressure; but it is not so. I have again and again observed that they diminish or become fainter under the pressure of the finger; but they are not effaced even for an instant. They are sometimes few; sometimes so numerous as to dapple the whole surface of the abdomen, or of the thorax, or of both. Upon the limbs they are less common, and less closely set. I believe that they often besprinkle the back, although they are seldom looked for there. They vary in intensity of colour, and, therefore, in distinctness. The whiter the skin, the more obvious do the spots become. In brunettes they may easily escape notice. In this form of disease the entire skin is often unnaturally dusky.

The eruption does not come out all at once: nor is its duration always the same. Sometimes it disappears entirely after two or three days. Sometimes, on the other hand, it lasts a fortnight, or more. In the latter case it is probable that successive crops of the spots continue to arise.

The rash now described stamps continued fever with one of the most striking characters of the exanthematous group of febrile diseases; but it certainly is less constant than the cutaneous phenomena of small-pox, measles, or scarlet fever. It occurs much more regularly in some epidemics than in others. Fever is very rife in St. Giles's, and in other crowded parts of this town, just now (1838). Our wards at the Middlesex are full of it; and scarcely a case presents itself without these spots. We speak of it familiarly as the *spotted* fever; or (from the resemblance which the rash bears to that of measles, hereafter to be described) as the *rubeoloid* fever.

You cannot well confound this mottled rash with *petechiæ*, which are little specks, or dark circular spots, resulting from a minute extravasation of blood beneath the cuticle. The specific rash and these petechiæ are, however, sometimes mingled together.

It may not be superfluous to caution you against mistaking *flea-bites*, which are common to nearly all our hospital patients, for this specific eruption, which is peculiar to the fever patients. The round red stain, with a dark point for its centre, sufficiently distinguishes the mark of the insect from the rose-coloured blotches of the disease.

There is another eruption described by the French as *occurring* in this disease without being *peculiar* to it. In this country it is now very rare; but it used, when the hot plan of treatment was in vogue, to be very common indeed here, in various febrile complaints; and it was, and is, occasioned by profuse sweating. *Sudamina*, the vesicles composing the eruption are called. They are small, hemispherical, transparent elevations of the cuticle, containing a clear watery fluid. The vesicles are from a quarter of a line to half a line in diameter; they have no red bases; and they are so perfectly pellucid, that when you look upon them in a direction perpendicular to the skin on which they stand, they may readily elude observation. Viewed sideways, they present bright surfaces, and look like so many drops of water, and you may feel with your hand that they *roughen* the part affected with them. These sudamina are mostly met with on the thorax, along the sides of the neck, and about the axillæ. By degrees, the limpid fluid disappears, and they shrivel up; the cuticle becomes wrinkled, and dries into a whitish powder.

Diarrhœa is another marked symptom observed in many cases of continued fever, though not in all, at this period of the disease. When the stools are involuntary, when they are passed in bed without notice on the part of the patient, they add materially to his danger, by the irritation, and the sores, which are liable to result from their contact with the skin. There is seldom much pain of the abdomen complained of by the patient; but if you make pressure, especially about the situation of the cæcum, you may often remark that he winces, or that a transient expression of suffering passes across his features. The character of the evacuations remains the same, and is almost distinctive of the disease: thin, yellowish, ochrey, like pea-soup. When in fever, such stools persist day after day, and several of them every day, you may safely infer that there is ulceration of the bowels, although there should be no pain complained of, even when the abdomen is pressed.

And the same conclusion will become still more certain when *hæmorrhage* from the bowels occurs, as it is apt to do, in this stage of the fever. It often takes place unexpectedly, sometimes in considerable quantities, and rapidly exhausts the patient; or it recurs at intervals to a smaller amount, wasting his strength as effectually, though more slowly. The bleeding is probably owing, in general, to the division or opening of some of the mesenteric veins, by the ulcerating process which I shall more fully describe by and by. This is not, however, a necessary consequence of the ulceration; for the vessels are usually obliterated previously to their erosion. Sometimes blood may be thus poured into the bowel without being voided. Andral relates a case in which a man died suddenly, and unexpectedly, at an advanced period of continued fever. Large clots of black blood filled the lower two-thirds of the small intestines, which were crowded with patches of ulceration. No part of the blood had passed the valve of the cæcum.

Hæmorrhage from the bowels occurs also in continued fever sometimes in another way; in connection with other *putrid* symptoms, as they are not unaptly called: petechiæ, purple spots, bruiselike blotches, and extreme depression of the vital powers. In these cases the hæmorrhage is strictly of the *passive* kind, and it is a symptom of the worst omen. Like those effusions of blood from the same parts that happen in scurvy and purpura, it depends upon a morbid condition of the blood. This is no matter of speculation, for by this time the sensible qualities of the blood are manifestly changed: its natural tendency to coagulate when withdrawn from the body is diminished, the crassamentum is large and loose, and fills the cup, and sometimes is rather an incoherent sediment than a clot. When these putrid symptoms are strongly marked, a peculiar fœtor is exhaled by the patient's body; his tongue becomes dry, black, and fissured; his teeth are covered with dark sordes; sloughs form from the mere pressure of the bed on which he lies; in extreme cases the toes have mortified; and Dr. Roupell relates one terrible instance in which both legs rotted away to the bones, which it became necessary to saw through: yet this patient recovered.

Death may take place in this, the second period of continued fever. Of forty-two pa-

tients who died under his care, Chomel counted *nine* in which the fatal event occurred between the eighth and the fifteenth days.

The phenomena belonging to the *third* period of the fever vary considerably according as the disease is about to terminate in death, or in recovery. Among Chomel's forty-two fatal cases, I have already mentioned that one death took place in the first week, and nine in the second; the remaining thirty-two all occurred in the third period. And the tendency of the fever to terminate during this period is equally well marked in the cases which recovered. Of twenty-four such patients, one alone began to be convalescent in the first week; and of those whose symptoms were at all serious, not one showed marks of convalescence before the end of the second.

When the disorder is about to end favourably, the more formidable of the symptoms diminish and abate. The patient begins again to attend to questions that are put to him; the air of stupor which had hung over his countenance clears away; he once more shows an interest in what is going on around him; the temperature of his skin becomes more natural, the tongue moist and cleaner at its edges, and the frequency of the pulse is less. The evacuations from the bowels are less numerous, more consistent, and more healthy; and the patient is aware when the necessity for passing them arrives, and gives notice, or asks for assistance. Generally, at the same time with these tokens of improvement, the emaciation which has taken place becomes remarkably conspicuous; perhaps it is the more observable on account of the patient's resuming a more natural expression of countenance.

In many instances, the amendment is so gradual that we can scarcely say when it begins. In other cases the favourable crisis is preceded by an aggravation of most of the former symptoms, and a marked increase of the general distress. This is a very curious circumstance, and it did not escape the notice of our great dramatist.

> Before the curing of a strong disease,
> Even in the instant of repair and health,
> The fit is strongest. Evils that take leave,
> In their departure most of all show evil.

Certain evacuations are also sometimes observed to accompany or to be connected with the favourable change; and the most common of these is the evacuation of sweating.

On the other hand, when the disease is about to terminate in death, that event may take place in different ways; in either of those modes, in short, which I took some pains to distinguish in the earlier part of this course of lectures. I told you then that I had been taught the importance of studying the tendency to this or that mode of dying, in reference especially to fever, by Dr. Alison. My own experience has since sufficiently approved to me the wisdom of his teaching. Cullen inculcates the necessity of "obviating the tendency to death." To do so, we must ascertain the direction of that tendency. We do not so much *cure* these exanthematous maladies, as keep our patients alive while they recover. If we would prevent their dying we must know in what manner they are in danger of dying.

The most common mode of death in continued fever is certainly that of *coma*. The organic life survives the animal life. The muttering, half-conscious, dream-like stupor, from which the patient may be roused for a while, becomes, by degrees, more profound, and death begins in the head. This mode of death, occurring in the second or third week of the fever, is associated, frequently, with the symptoms of putrescency already described. But as the stupor deepens, the pulse generally grows weak, and the extremities become cold. So that death does not come purely in the way of coma; but we have a compound of coma and asthenia, in which the coma takes the lead.

Now coma may result from at least two different kinds of cause. One cause is pressure, which is mechanical. Another, which is probably chemical, is the circulation of some noxious or narcotic substance (such as opium) in the blood. And there are, doubtless, many physical conditions of the nervous mass itself which are capable of arresting the cerebral functions, and producing coma. To which kind of cause are we to ascribe the stupor that supervenes during the progress of fever? That is an interesting, and in reference to practice, an important, question.

Morbid appearances.—Physicians have diligently attempted its solution, by examining the dead brain. I cannot tell you how often I have looked, and looked in vain, for some palpable disorganization, or some effusion implying pressure. All who are familiar with the dead-house of a hospital are aware that this fruitless search for some physical explanation of the comatose state, after death by fever, is of very common occurrence.

Chomel—one of the latest, and a very able and accurate French writer on fever—gives the following statement in respect to 38 fatal cases, in which the brain was carefully and minutely inspected by him. In 15 of the 38, no morbid appearance at all was perceptible: in 12 there was some serous fluid, from a tea-spoonful to a table-spoonful, in the ventricles: in 7 there was what he calls *œdema of the meninges*, effusion, that is, in the meshes of the pia mater: in 6 general but slight diminution of consistence: in 2 some alteration of the density: in 5 a speckled appearance of the cerebral substance. Now to what conclusion do these facts lead us? Why, in the first place, to the conclusion that those pathologists are in error who maintain (as Dr. Clutterbuck does, for whose experience and talents I entertain a sincere respect), that the essence of continued fever is *inflammation of the brain*. Not only do we fail to discover, in many instances, any traces of inflammation, upon inspecting the dead brain, but we find that, during the life of the patient, measures which would be likely to aggravate any inflammatory mischief—strong stimulants, for example, wine or brandy,—do actually and obviously, in cases innumerable, relieve the comatose symptoms, and benefit the patient. The inference seems unavoidable, that the coma, in such cases, has some other cause than that mechanical pressure which arises sometimes from the effusion of fluid upon the surface of the brain, or within its ventricles; and that other cause is supplied by the poisoned blood. Here again we may adopt the pathology of Shakspeare:

> The life of all his blood
> Is touched corruptibly: and his pure brain
> (Which some suppose the soul's frail dwelling house)
> Doth by the idle comments that it makes
> Foretel the ending of mortality.

In some malign epidemics the nervous system is overwhelmed at once, in the very outset, by the force of the poison. The patient becomes stupid or bewildered; his surface is cold, clammy, purplish, and his pulse feeble: the coma rapidly augments, and death may ensue within twenty-four hours. We sometimes see this fearful train of symptoms in small-pox; and still more often and more strikingly in the worst forms of scarlet fever. I believe that in these cases there is no deviation, cognizable by our senses, from the healthy texture and appearance of the parts within the skull.

Nevertheless, there may be, and there often is, in these fevers, actual inflammation of the brain or its membranes: but this is an incidental complication. We conjecture that, in addition to the influence of the poison upon the nervous system, there may be a low degree of inflammation going on within the head, when we find it externally hot, when the patient has flushed cheeks, and a vascular eye, and complains of dull headache. And there are some instances in which we recognize more distinctly the outward signs of encephalitis—severe pain in the head, high and fierce delirium, intolerance of light and of sound, with much heat of skin, and a hard pulse. When coma succeeds such symptoms as these, we naturally ascribe it, in part at least, to the effects of the inflammation: and rightly, for we find traces of inflammation after death; serous effusion beneath the arachnoid and in the cerebral ventricles; shreds of coagulable lymph; and more rarely suppuration. I suspect that genuine encephalitis, which is of course attended with pyrexia, is sometimes mistaken for continued fever with intercurrent inflammation of the brain. Great attention, and some skill and judgment, are required for discriminating those cases of fever in which such inflammation occurs, and for directing the appropriate treatment.

The death in fever by *apnœa* is certainly much rarer than that by coma: yet it is not very unfrequent. It often mingles itself with the death by coma. From the earliest period of the fever we may, in most cases, notice some increased quickness of respiration, which is not entirely owing to the mere fever, or to acceleration of the circulation; for the

ear, when applied to the parietes of the chest, discovers rhonchus, and sibilus, at least. Frequently there is considerable dyspnœa for some hours, or for a day or two, before death: and this may be apparent only, in consequence of the stupor; or it may be real, and proceeding from a low degree of pneumonia, which, by interfering with the due arterialization of the blood, may aggravate, or even give rise to the coma; and such pneumonia is apt to be masked by the fever; declaring itself by none of the ordinary symptoms of cough, rust-coloured sputa, or pain of the thorax. The inflammation, thus latent, is discoverable, however, by the sense of hearing.

Now what does dissection teach us with respect to the condition of the lungs after death from continued fever? Why, the most common unnatural appearance met with in the pulmonary substance is engorgement: a state similar to that which occurs in the first stage of pneumonia. But here the engorgement is probably in a great measure mechanical, and takes place during the last few days of the patient's life. As the vital powers diminish, the laws which govern the physical world resume their empire. The fluids, and the blood especially, accumulate in the most depending parts of the viscera: and the lower and posterior portions of the lungs in particular become loaded; but besides this, it is not unusual to find large portions of the lungs in a state of hepatization, and even infiltrated with pus: and the bronchial tubes clogged up by viscid and frothy mucus.

The mode of death by *asthenia* or syncope is not very common *as existing by itself;* but it is *often* combined with one or both of the two other modes. In some cases of fever, however, death does appear to take place from *mere debility of the heart:* there having been no pulmonary embarrassment, and the head having remained clear. Death, in such cases, is preceded by those symptoms of debility which have been already described. The pulse becomes small and weak, and like a thread; the patient lies on his back, and sinks down in the bed; the features sharpen; the eyes are hollow and dim, as though glazed; the sphincter muscles fail to contract; the extremities grow cold; cold sweats appear on different parts of the body; and at length the heart ceases to beat, and the patient to exist.

Death occurring in this manner does not occur *early*. It happens at an advanced period of the disease. It is noticed sometimes in persons who have been largely bled, or too actively depleted at the commencement of the fever; and in those who have suffered a good deal from *diarrhœa*. In short, as the two previous modes of death are connected, often, with morbid conditions of the head and chest respectively, so death in fever occurring purely or *chiefly* by asthenia, connects itself with morbid conditions existing within the belly.

And we trace the vestiges of disease much oftener in the *abdomen* than either in the *brain* or in the *thorax;* and the mischief which we there discover is more constant and definite than elsewhere. It is proper, therefore, to inquire somewhat more closely into the morbid appearances observed in the abdomens of those who die of continued fever.

I need scarcely remind you that the intestinal canal is largely furnished, on its inner surface, with glands, or follicles, which consist of little more than crypts, and of which the precise office has not yet, I think, been accurately determined. Some of these glands are sprinkled (one may almost say at random) over the whole tract of mucous surface. These are accordingly called *solitary glands*. Till I was better instructed by Professor Todd, I used to speak of them as being the glands of *Brunner;* whereas the glands discovered and described by that anatomist are limited to the duodenum. Other glands or follicles are collected into groups, and are named *glandulæ agminatæ*, or often the glands of *Peyer*, who has given a capital description of them. Now the arrangement of these glands of Peyer is peculiar; and, in reference to the morbid anatomy of continued fever, very necessary to be known. They are met with in the ileum alone: they are, mostly, oblong in form: and they occupy that part of the bowel which is opposite its mesenteric attachment. They are largest, and most numerous, and consequently most thickly set, in the lower end of the ileum, which, in some instances, is almost entirely covered with them: they are found also upon the ileo-cæcal valve; but beyond that they do not go in that direction. Ascending from the cæcum towards the jejunum, these groups or patches become smaller, shorter, more circular, and less numerous: they are separated by longer and longer intervals, till at last they cease to be visible at all. Where there are valvulæ conniventes, there the

situation of these patches is very obvious; for the valvulæ conniventes are interrupted, and never run across them. This fact has sometimes led to curious mistakes. I have seen in the museum of the College of Surgeons a preparation put up by Mr. John Hunter (but not labelled I presume by him) professing to be an example of the destruction of portions of the valvulæ conniventes by ulceration. It is nothing more than one of these natural patches, rather more plainly developed than usual.

These glands, in their healthy state, are much more conspicuous in some bodies than in others. You may see, if you look at them attentively, that they are made up of a congeries of mucous follicles, of which the *orifices* are obscurely apparent. Now the main alterations met with in the abdomen after death, in continued fever, are alterations of these very glands: of the solitary glands, to wit; and still more constantly and remarkably of the agminate glands.

The changes which these glands undergo are of the following kind. In the first place they become enlarged, and more *perceptible* than they are in the natural state. They then present a grayish transparent surface, dotted over with black points; which black points mark, I conceive, the excretory mouths of the several follicles. Then, as the inflammation, for such it is, advances, the patch becomes reddish perhaps; and the follicles burst or ulcerate, or slough away: not altogether, but partially and by piecemeal; so that an irregular ragged ulcer is generally left, having thickened edges. Sometimes, however, the follicles disappear without there being much redness or thickening: the mucous membrane immediately adjacent, and even the remaining part of the patch of follicles, being pale, and level. Sometimes the patch puffs up into a sort of fungous swelling, in which all trace of the follicular structure is lost. The colour of the ulcerated surface is various, as well as its form and appearance. Sometimes it is pale and gray; sometimes red; oftentimes yellow, as if the exposed cellular and other tissues were stained by the ochrey fluid which had been poured from the bowels during life. What I have hitherto stated relates to Peyer's glands; but the solitary glands participate, usually, in the change. They become, in the first place, large and hard, and present a whitish coloured projection from the surface, which, by a mistaken analogy, has sometimes been called a pustule. At length a loss of substance takes place in these also; beginning at their summit, and producing a small, but sometimes a deep ulcer.

And of these changes it is further to be observed, that they are more common, more numerous, more extensive, more advanced, in proportion as we approach the cæcum. It is natural that we should meet with *more* ulcers near the cæcal valve, because there are more glands there; but undoubtedly the ulcers are (in general) *further advanced* there than higher up in the bowel.

Going along with this ulceration of the mucous glands of the intestines, and above all of the aggregate glands of the ilium, you will frequently find inflammation (*i. e* redness, hardness, and swelling) of the corresponding *mesenteric* glands. This would seem to be a consequence of the inflammation and ulceration of the mucous glands. The inflammation of the mesenteric glands is, I conceive, secondary; and bears the same relation to the ulcers seen in the bowel, as a bubo in the groin bears to a chancre on the glans penis.

These alterations are extremely interesting, because they afford a plausible explanation of many of the symptoms of continued fever. They account for the diarrhœa; they account for one form of occasional hæmorrhage from the bowels; they account for the uneasiness or pain which is experienced when the abdomen is pressed, and teach us why that uneasiness is greatest in the situation of the cæcum. Nay, we can even understand why, although these ulcerations exist, there may be *no* pain occasioned by them. This may be partly owing to the general insensibility to impressions and sensations of all kinds, produced by the stupor; but partly, also, it probably depends upon the depth to which the ulceration goes. The mucous tissues are possessed of but little sensibility even under inflammation; but if the muscular and peritoneal coats become involved in the inflammatory process, then pain begins to be felt. I remember, a few years ago, attending a young lady ill of fever, with a very well-informed practitioner, but one of the old school, for he had not turned his attention much to the state of the intestines in that disease. One day we learned that our patient had had hæmorrhage; from the uterus her friends supposed; but when I saw the discharge, I was certain, by its appearance, and by its odour, though it

was not mixed with any fæces, that it had come from the bowels; and I stated my conviction that there was ulceration in the lower part of the ileum. But she had no pain in the abdomen. You might press any part of it without exciting the smallest uneasiness. So, distrustful of my opinion, they called in an eminent accoucheur, who also pressed and examined the belly: but neither could he detect any tenderness or undue sensibility. He next examined the uterus *per vaginam;* but could discover nothing wrong there. Some few nights afterwards the general practitioner was called out of his bed to this patient. He was told that the hæmorrhage (or flooding as they called it, for they persisted in believing that it was uterine,) had returned; and before he arrived at the house the patient was dead. We next day opened the body together. The uterus was perfectly natural; there was no vestige of discharge or bleeding in the vagina; but the ileum, for about a foot above its entrance into the cæcum, was in a state of superficial but ragged ulceration, and universally red and smeared with blood. I mention this as a strong fact in illustration of the possibility of there being much disorganization of the inner surface of the intestines, without any pain to reveal it.

Now the alterations I have last been sketching—the thickening, redness, tumefaction, and ulceration or sloughing of the glands of Peyer, and also of the solitary glands—are so common in fever, particularly in some epidemics, that many pathologists are of opinion that fever is essentially inflammation of these glands, and nothing else. But this, I am sure, is an error. In the first place, if this doctrine were true, it would almost follow of necessity that the severity of the case, and the intensity of the symptoms, should be in proportion to the number, depth, and extent of the ulcerations. But this is far from being so. In cases in which the symptoms have been of the worst kind, there have been found very few ulcerations, and those small and apparently insignificant. On the other hand, when the complaint has run a moderate course, but at length has terminated fatally, it is not uncommon to discover a frightful amount of disorganization in the ileum.

But what is more conclusive is, that the occurrence of this inflammatory condition of the mucous follicles of the intestines is *not constant*, in continued fever. If *one* well-marked instance of the disease should occur, without any trace of a morbid action having been going on in the mucous follicles, that instance would, of course, suffice to overturn the theory. But *scores* of such cases have occurred. I have seen many such myself; and other persons have seen more. Since attention has been drawn to the subject, the patches of glands, and the whole tract of mucous membrane, from the stomach to the rectum, have been diligently explored: and the result seems to be that, at certain times and places (in other words, in certain epidemics), the ulceration of the inner surface of the intestines is far less common than in others. It was comparatively rare in an epidemic of which I witnessed some part in Edinburgh. Then I came to London; and for several years I never saw a body opened after death by continued fever, without finding ulcers in the bowels. More recently, however, and especially during the present epidemic (1838), I have looked for them, carefully, in many cases that have proved fatal in the Middlesex Hospital, and have discovered neither ulceration nor any other apparent change in the follicles of the intestines. Still, in my own experience, such ulcers have been vastly more often present than absent. Chomel, during five years' investigation of this matter in the Hotel Dieu, never met with an exception to the general rule of their occurrence; or of some degree or form of that kind of alteration in the mucous glands, of which the ulcer is the last stage.

We must conclude, upon the whole, that although an inflammatory state of the solitary and aggregate glands, which strew the surface of the mucous membrane of the alimentary canal, is not the essence of fever, yet that it is a very frequent companion of continued fever.

But when it does occur, is this state of things always, or necessarily fatal? By no means. It *may* be fatal, in more ways than one. It may lead to death, by exhausting diarrhœa, in the way of slow asthenia; it may kill by laying open a large mesenteric blood-vessel, and so producing copious hæmorrhage and mortal syncope; it may, and often does, destroy the patient, by *perforation* of the bowel: the ulcer penetrates the mucous and muscular coats, and reaches the peritoneum; and sometimes that membrane gives way, and sometimes it does not. The consequences of its rupture or perforation—the escape, I mean,

of the contents of the bowel, and the supervention of intense and incontrollable peritonitis,—I need not again dwell upon. But the ulcers may, and doubtless often do, *heal:* and the *scars* which they leave behind them are frequently to be seen. The ulcerated surface seems to clothe itself afresh, by degrees, with a new mucous membrane; which is thin, however, and adherent to the subjacent tissues, and does not slide over when pressed between the finger and thumb, as the healthy portions of the coats of the bowel will do upon each other. And in the place of the cicatrix there is usually to be seen a manifest puckering, and a number of little wrinkles or lines, radiating from a common centre.

The spleen—I may observe, before I conclude this lecture—is more frequently found altered in texture after death by continued fever, than any other part except the glands of which I was last speaking. It is usually enlarged, of a dark colour, and of soft, and sometimes almost rotten, consistence.

To-morrow I shall endeavour to point out some of the most important *varieties* that have been found to occur in this disease of continued fever.

LECTURE LXXXV.

Varieties of Continued Fever. Its Causes, Exciting and Predisposing. Prophylaxis.

Having traced, in the last lecture, as clear an outline as I could, of the ordinary course and the different terminations of continued fever, I wish to touch, briefly, to-day, on some of its principal varieties; and then to inquire into its causes.

Varieties of continued fever.—Although fever is, as I have stated, a specific disease, it assumes divers forms; and so dissimilar are some of its phases that they might seem to belong to totally different maladies. These variations relate not only to individual cases, but to whole epidemics. In some places and seasons, the inflammatory type predominates, marked by excitement of the sanguiferous system; in others, depression of the nervous system, characteristic of the typhoid type, is the prominent feature of the disease. Most generally of all, the disorder commences with inflammatory fever, and ends with typhoid symptoms. The distinctions drawn by Cullen, now well nigh obsolete, were founded in nature. To the inflammatory form he gives the name of *synocha*, which he thus defines: "Calor plurimum auctus; pulsus frequens, validus, et durus; urina rubra; sensorii functiones parum turbatæ." With this he contrasts his *typhus*. "Calor parum auctus; pulsus parvus, debilis, plerumque frequens; urina parum mutata; sensorii functiones plurimum turbatæ; vires multum imminutæ." These forms I would have you bear in mind: not that you are likely to meet with many instances of pure synocha, nor of pure typhus, but because they furnish standards of comparison, towards which, in opposite directions, the fevers of different epidemics approach. The most usual variety of continued fever is represented by the *synochus* of the same author, which is a compound of the two others. "Febris ex synochâ et typho composita: initio synocha, progressu et versus finem typhus." The unsoundness of many of Cullen's theories, and the conceit of later and far inferior writers, have thrown his *First Lines* into undeserved neglect; but his clear, succinct, and faithful pictures of disease, will not easily be surpassed; and are worthy of your attentive study.

The difference is very striking between the kind of fever that I witnessed in London for ten years before the arrival of the spasmodic cholera in this country, and the kind of fever that has since prevailed, and is now (1838) so rife around us. During the first of these periods, the antiphlogistic regimen was indispensable in the outset of the disease; in most instances, bleeding, either general or topical, was required, and well borne; there was no eruption to be seen upon the skin; the glands of Peyer, according to my own ex-

perience of the fatal cases, were almost invariably affected; and the mortality was very moderate. This was an inflammatory phase. The present epidemic offers a marked contrast in all these points. A large per-centage of those who contract the fever die; after death we seldom detect any disease of the agminate glands of the intestine; the peculiar rash scarcely ever fails to show itself; we are taught by experience to refrain as much as possible from blood-letting; and almost from the beginning, or quite, we find it necessary to sustain our patients by a liberal allowance of strong animal broths. The typhoid is now the prevailing type. You might, I say, almost suppose that I have been speaking of two distinct maladies. But during each of the periods in question, some scattered cases have occurred, bearing most of the characters proper to the other period. Moreover, all acute diseases have assumed, within the last ten years, in this town at least, an unusually *asthenic* character. So that the differences observed in the aspect and phenomena of continued fever, depend more, I conceive, upon an acquired disposition of the human body, produced by some obscure general influence, and therefore affecting the entire London community, than upon any change in the essential nature of the disease, or in the virus which (as I believe) occasions it.

The inverse relation between the rash, and the intestinal ulceration, is remarkable. When the one is prevalent, the other is rare. It would seem, in conformity with Dr. William Budd's views, that the specific poison displays its elective affinities by settling sometimes upon the mucous glands, sometimes upon the cutaneous tissues, and sometimes by sharing itself, though unequally, between the two.

Duration.—I have incidentally alluded to the duration of the fever. In this particular, also, there is much variety: a fact which is apparent even to the vulgar, and expressed in their ordinary discourse. They talk of the one-and-twenty-day fever, and of the fourteen-day fever, according as the disorder "takes the turn" in three weeks or in a fortnight. It appears from the report made some years ago to the government, upon the state of fever in Ireland, that the fever generally began to depart on the fifth day; wherefore it was called, in that part of the kingdom, the five-day fever. Of the fever patients whom I saw in the Edinburgh Infirmary, almost all were fairly convalescent, or dead, before the end of the third week. In this place, the disease runs out longer; lasting three, four, and five weeks. I observed also in Edinburgh that many of those who recovered had a tedious convalescence, in consequence of very troublesome sloughs and ulcers which formed upon the sacrum and hips. In London these bed-sores are comparatively uncommon.

The symptoms attending the fever seem to vary also, *cæteris paribus*, according to the *season of the year;* and the *situation* of the patient: whether, I mean, he is surrounded with pure and cool air, or with a foul and hot atmosphere. In the colder months there is greater risk of inflammatory complications, and especially of pectoral affections. in the autumn we look more for diarrhœa, or for dysenteric complaints, engrafting themselves on the disorder. Where the air is close and foul, the symptoms show a much greater tendency to the typhoid type, and the deaths are more numerous, than where it is pure.

Exciting cause.—It is of great importance to hold correct notions as to the *exciting cause* of continued fever; respecting which there has been, and there still is, a perplexing contrariety of opinion among medical men. You are aware, from what has already been stated, that I consider the disorder to originate in an animal poison, and to be contagious; communicable, I mean, from one who is labouring under the complaint, to another who is not.

When the same disease attacks many persons in the same house or neighbourhood, at about the same time, the popular suspicion soon arises that the disease is catching. Yet you know that disorders may be widely prevalent without being contagious. Agues, for example, engendered by malaria: ordinary catarrhs and sore throats, produced by vicissitudes of the weather. When an epidemic malady affects large masses of the people suddenly and at once, it is presumably not contagious; at any rate it must have some other source besides contagion. When, on the contrary, it begins in a certain spot, and gradually spreads thence as from a centre, the presumption is in favour of its propagation from person to person. In investigating this subject, if we trace the fever among persons who have had intercourse with the sick, and more frequently in proportion as that intercourse

has been close and continued; and if we find that other persons, living in the same place, and under precisely the same circumstances, except that they have had no known communication with the sick, do escape the fever; we have in these facts convincing evidence that the disease has been spread by such intercourse; in one word, that it is contagious.

Have we, then, facts of this kind? We have, in the amplest abundance.

We find, even in hospitals, where cleanliness and ventilation are prized and enforced, that fever attacks many of the persons who come most often and most intimately in contact with those already ill of that disease; chiefly the nurses, next the clinical assistants, and the most assiduous of the students, and the medical officers; rarely the other patients, even in the same ward. The separation of a few feet, if due regard be had to ventilation, is sufficient to render the poison inoperative, by diluting and diffusing it in the surrounding purer atmosphere. Three of our nurses in the Middlesex Hospital have fallen ill of fever during the epidemic now prevailing; and two of the three have died. I am now attending a pupil of the Charing Cross Hospital, who is persuaded that he caught the fever while watching some bad cases of it there. It is only when our wards are unusually full of fever patients that these disasters occur: but they happen very often indeed, constituting the rule rather than the exception, wherever many fever-patients are collected together: as in hospitals which are exclusively appropriated to their reception, or in the fever *wards* of certain general hospitals. In such places the effluvia which proceed from the bodies of the sick are, in spite of all care, the most abundant and the most concentrated.

Dr. Welch, whose book I have referred to before, has the following statement in point. "In this hospital (that is, Queensbury-House in Edinburgh, which was opened for the *sole* use of fever-patients during the prevalence of a severe epidemic in that city in the years 1817, 18, and 19), since it was opened (which was the year before the time when Dr. Welch was writing), my friends, Messrs. Stephenson and Christison, the matron, two apothecaries in succession, the shop-boy, washerwoman, and thirty-eight nurses, have been infected; and four of the nurses have died. With the exception of but two or three nurses, who have been but a short time in the hospital, I am now the only person who has *not* caught the disease, either here, or at the infirmary, within the last eight or ten months."

I may quote a passage from Dr. Alison, in illustration of the same thing. He is speaking of a more recent epidemic, which occurred in Edinburgh in 1827 and 1828. He says: "During this epidemic, as well as in that of 1817–19, many of the clerks and nurses employed in the Royal Infirmary had taken fever. Since November last, six of the clerks employed in the clinical wards only, four of those employed in the ordinary wards, and twenty-five nurses or servants, have taken fever. All these persons had necessarily frequent and close intercourse with the fever patients in the house, having been employed more or less constantly in the fever wards, excepting only four of the servants. Of these four two had been employed in the laundry where the linen from the fever wards was washed; one was a porter employed at the gate, who would of course have communication with the fever patients at their entrance or dismissal, as well as with their relations coming to visit them; and one was a nurse employed in the servants' ward, but who was in the habit of visiting the fever wards." Now mark the contrast. He adds, that "in this very place and season, those of its inhabitants who have *not* had intercourse with fever patients have almost uniformly escaped the disease. Of the inhabitants of the *ground floor* of the house, (including patients in the lock-ward,) none but those already mentioned as having washed the linen from the fever wards, and the barber who shaved the heads of the fever patients, have taken the disease. Yet in the case of malaria" (to which I must apprise you that many medical men of great authority ascribe the occurrence of continued fever) "it is the ground floor of the house that is generally found the most dangerous. No one of the nurses, whose duty has confined them to the medical or surgical wards where no fever patients were admitted, has taken fever, with the single exception of the woman in the servants' ward above mentioned. And of the numerous *patients* in these ordinary wards, the only one who has taken the fever, within my knowledge, during the present year, was a patient in the men's general clinical ward, who lay in the bed next the door that communicates with the clinical fever ward. If there be *malaria* in this

house, therefore, it would seem to restrict itself *in point of space*, as at Queensbury-House *in point of time*, to the immediate vicinity of fever patients." To understand this last remark, you should know that, in ordinary years, the inmates of Queensbury-House escaped fever.

We have similar testimony nearer home; in this metropolis. "Every physician connected with the London Fever House, with one exception (writes Dr. Tweedie), has been attacked with fever; and three out of eight have died of it. Also the resident medical officers, matrons, porters, laundresses, domestic servants not connected with the wards, and every female who has performed the duties of nurse, have, one and all, invariably, been the subjects of fever. And to show that the disease is capable of being engendered by fomites, or clothes, the laundresses, whose duty it is to wash the patients' clothes, are so invariably attacked with fever, that few women will undertake the loathsome and disgusting office."

Now it is in vain to attempt to escape from this kind of evidence, by saying that the situation of the Fever Hospital is infested with some local miasm: for the inmates of the Small-pox Hospital, which is immediately adjacent to it, are not affected in this way with continued fever, but remarkably exempt from it, as Dr. Gregory testifies.

Evidence of a somewhat different kind, but leading to the same conclusion, is to be found in the fact, that when persons, having the fever upon them, are transferred to some distant spot that was previously free from fever, they frequently form centres from which the disease begins, thenceforward, to spread. It is imparted, in this way, even in the country, from family to family, and from village to village.

I was summoned home from Edinburgh on account of my mother's serious illness with continued fever. She was living in a village where there was little or no fever prevailing. While she was recovering my sister took the disease, and had it severely. It had evidently been introduced into the house by a man-servant, whose family lived in a neighbouring village. This family had the disease raging in their house, and he was in the habit of going there occasionally, and always of sending his linen to be washed there. He first, in our house, had the disorder, then two of the maid-servants, and next my mother.

The cook of Trinity College, Cambridge, living in a street called the Petty Cury, had a daughter in London who fell ill with continued fever, and who insisted upon going home. At that time there probably was no case of fever in Cambridge: certainly none in the Petty Cury, as Dr. Haviland (who gave me the account) satisfied himself by inquiry. The girl was very ill indeed after she reached her father's house; but she ultimately recovered. Every inhabitant of that house, except an old, seasoned nurse, became affected with the fever; and three or four of them died. But no fever existed in the other houses of the same street. When one of the sick persons was convalescent, it was thought her recovery might be accelerated if she were put into a lodging at Trumpington, a small village two or three miles from Cambridge, in which there certainly was then no fever. Here she and the old nurse were waited on by a servant belonging to the Trumpington House. That servant soon sickened of the fever, and was sent to Addenbrook's Hospital, where she died.

Take one more instance to the same effect, related by Dr. Alison. "Some years ago, at a time when there was no great number of fever cases in Edinburgh, I met with a case in the son of a shoemaker, who was lying in a room in which his father and two apprentices were at work. I could not prevail upon the father to remove his son to the hospital, although I stated the danger of the apprentices being affected. Within two or three weeks after, I found that the two apprentices were lying ill of fever in their own houses: one of them two hundred yards, the other half a mile distant from the workshop, and widely distant from each other. These young men likewise lay at home during the fever; and each of their cases was speedily followed by a succession of others in the inhabitants of the rooms which they occupied, and of those immediately adjoining, who had never been at the workshop. In one of these houses seven, and in the other twelve, were thus affected. Now on the supposition of the fever being contagious, all this was to be expected, and all corresponded to the predictions which were hazarded on that belief. But on the supposition of such succession of fever cases depending on miasmata, there must have

been at least two, more probably three, separate and accidentally concurring miasmata to explain the phenomena here observed; one at the workshop, and one at each of the houses of the apprentices: and there must have been this extraordinary coincidence, that at each of these last the malaria sprung up just at a time when a patient was lying ill there of fever, which he had apparently contracted elsewhere. Further, the three houses in which these successions of fever cases were observed, are in situations very different from one another; and all of them have been, to my knowledge, perfectly free from fever for years together, both before and since that time, notwithstanding that fever has been much more generally prevalent, and that they have been inhabited by successive families. What probability is there (continues Dr. Alison) that three separate miasmata should have arisen in these three houses, just at the time when their presence was required in each to produce an effect which had been foretold as the consequence of another cause undeniably operating on all?"

If we contrast facts such as I have been advancing, with other cases, in which all the circumstances appear to have been precisely the same, *except* the presence of the alleged cause of the disease—making our observations always upon as large a scale as possible—we approach, as nearly as the subject will admit of, to a demonstrative proof that continued fever spreads by contagion.

The reports which were made by the accredited physicians, to a committee of the House of Commons, respecting epidemic fever in Ireland, contain abundant and valuable evidence on this point also. Dr. Cheyne states that the farmers and householders in some parts of Wicklow, who would not harbour or admit into their houses strolling persons, nor go to wakes or funerals, remained free from the disease. In Ballytore a committee was formed of persons who took pains to instruct the inhabitants as to the precautions to be observed against infection: such as refusing admission to wandering beggars, absenting themselves from wakes and other assemblies; and even, under certain circumstances, from places of worship. These precautions were so effectual that not a case of fever occurred. Four villages in the neighbourhood of Lismore are stated by Dr. Barker to have been preserved from the fever, chiefly by the exertions of some Roman Catholic clergymen, who persuaded the inhabitants to avoid all communication with Lismore, and with another town in its vicinity, where the fever was rife.

It was observed also that bodies of persons collected together, and fenced about by barriers which precluded intercourse between them and places infected with the fever, remained exempt from it; children in charity schools, soldiers in barracks, and even prisoners in the jails. In the same parliamentary report you may see a letter from Dr. M'Donald, who had established a fever hospital at Belfast, which hospital alone, at one period, contained 190 cases of typhus. Very near the hospital was a school, containing 700 or 800 young persons; a poorhouse with 300 inmates; and a barrack with 1000 soldiers. These places were never more free from fever than at that time.

Facts to the same purpose abound in the medical reports of the army and navy. Those of the navy are especially valuable and instructive in this matter, because the whole of the circumstances in which the patients are placed come under the certain and immediate cognizance of the medical officers superintending them. I cannot go into particulars here, but must content myself with referring you to the writings of Dr. Lind, Sir Gilbert Blane, and Dr. Trotter. You will there find that ships which had, for a great length of time, been quite free from fever, have had that disease spread rapidly from one individual, recently imported, so as to affect almost all the crew. This was often the case when raw recruits were drafted from the receiving ships. It appears, from reports made to Dr. Trotter by different naval surgeons, and published in his *Medicina Nautica*, that the fever was conveyed to a great number of vessels forming the Channel Fleet, from the receiving ship called the *Cambridge*.

It is, moreover, found, that when persons ill of fever are taken away from their own close and crowded houses, and when means of purification are employed, the fever ceases to spread in those houses. This well-ascertained fact it is which gives to fever hospitals their greatest, nay almost their only value. They would otherwise, as we have already seen, be detrimental or dangerous to all concerned with them, by concentrating the poison that produces the fever; without equivalent benefit. As it is, they cause, indeed, a certain

amount of disease and of death; but by affording opportunities for clearing an infected neighbourhood of the seeds of the fever, and by so preventing its diffusion among a large and healthy community, they save many more lives than they sacrifice.

You may be surprised that I should appear to labour this point; and should take so much pains to prove what may seem to you, as I confess it seems to me, to be an undeniable proposition. But I do so because many have denied, and do still deny it; and plausible reasons are given, by men of name and character, for discarding altogether the notion of continued fever being propagated by contagion. Some even, who have begun by expressing their belief that the disorder was contagious, have ended by becoming strong, aye violent anti-contagionists. The late Dr. Armstrong was one of these; and there are living men, of high and deserved repute, both in this country and abroad, who hold what I cannot help thinking erroneous—and if erroneous, then manifestly dangerous—opinions on the subject. Chomel informs us that not one in a hundred of the profession in Paris believes that typhoid fever is a communicable disease. Upon a subject so important it is right that you should be furnished with data for forming a correct and settled judgment. But I scarcely expect that the facts I have now brought forward will carry the same conviction to you all; for I agree with Dr. Christison in believing that the discrepancy which exists upon this, as upon some other controverted questions, depends, in part, upon the different constitution of men's minds, and their peculiar habits of thought. Most of the anti-contagionists whom I have known have belonged to that party in this country which advocates what are called liberal opinions in politics and in religion. If this should prove to be generally true, it must be regarded as a curious psychological fact.

I have laid before you some of the positive evidence which goes to prove the contagiousness of continued fever. You may naturally ask, and it is but fair that I should tell you, what are the arguments on the other side of the question.

First, then, it is asserted that continued fever cannot be contagious, because some, nay many persons, who *have* intercourse with the sick, do not contract the disease.

But the force of this reasoning is completely broken by the well known fact that, in respect to diseases which are on all hands acknowledged to be contagious, and which are even propagable by inoculation, small-pox for example, the same kind of exemption notoriously happens. Some of those who so escape may have had the fever before; and have thereby become less capable of being re-infected by the poison. I believe it to be uncommon for a person to have genuine continued fever a second time, unless he is exposed to the contagious matter in a very concentrated state, or for a long time together. Some may, by original peculiarity of constitution, be proof against its power; but probably in most cases, the immunity arises from the circumstance that the person has been fortunate enough, or careful enough, to escape imbibing an effective dose of the poison. You may breathe, for a good while, without much hazard, an atmosphere but faintly imbued with the contagious effluvia; and you may, though with somewhat more of hazard, breathe, for a short time, air which is strongly tainted by them, and yet go free.

Allowance must also be made, in some cases, for the effect of *habit* in fortifying the system against contagion. Persons who are much and often exposed to these effluvia, are thereby *seasoned*, in some degree, to the noxious atmosphere: just as drunkards and opium eaters become at length impassive under such a dose of their customary stimulus as would intoxicate or stupefy a novice. Upon this principle has been explained the comparative immunity from contagious diseases, (under like circumstances of exposure,) of medical practitioners, and nurses; of the keepers of filthy lodging houses, while the new-coming inmates suffer; and even of prisoners, who, without having had the disease themselves, may nevertheless carry forth and communicate the infection: as is said to have happened at the celebrated "black assizes" in Oxford; and again at the Old Bailey in the year 1750.

This argument, that the fever cannot be contagious because many of those who come near the sick are not affected with it, has been happily ridiculed by comparing an epidemic to a battle. A man might say, "I was in the battle of Waterloo, and saw many men around me fall down, and die, and it was said that they were struck down by musket balls; but I know better than that, for I was there all the time, and so were many of my friends, and we never were hit by any musket balls. Musket balls, therefore, could not have been the cause of the deaths we witnessed." And if, like contagion, they were not

palpable to the senses, such a person might go on to affirm that no proof existed of there being any such things as musket balls.

It must not be forgotten that this same argument, whatever may be its validity, is equally potent against any other general cause, against local miasms; against an undefined something in the air.

Again, the disorder does not spread, say they, in the houses of the rich. But why should it not do so, if it proceeds from any cause which strict attention to cleanliness and ventilation does not disarm? The fact, when rightly considered, is a strong circumstance in favour of the theory of contagion.

But then the disorder often occurs when we can trace no contagion: and the full answer to this argument is to be found in the fact that the same difficulty applies to diseases which are not only confessedly contagious, but which are believed to have, now, no other source than contagion; even, as I showed you before, to small-pox. All that the argument can amount to, is a presumption that the distemper may sometimes originate independently of contagion. A person may be exposed to the agency of the contagious matter quite unconsciously, by coming into contact with fomites, or by approaching another person who is just convalescent from the disease. A nobleman died here, in the prime of life, last year, of scarlet fever. The story goes that he had just come to town, and was dining out, and by his side sat a young lady who had just recovered from that complaint; and that from her he unwittingly received it. In such a place as this there are a thousand unsuspected ways in which contagion may be disseminated. It may lurk in a hackney coach; you may catch the complaint from your neighbour in an omnibus, or at the theatre, or at church; your linen may be impregnated with the subtle poison in the house of your laundress; or your coat may convey it from the workshop of your tailor. So that, when continued fever occurs, it is very difficult indeed to be *sure* that it has not arisen from contagion; and it becomes a very interesting question whether the disease ever proceeds from any other cause. It is, however, of primary importance to determine whether it is, or is not, communicable. By our belief in this matter the lives of our neighbours and clients may be saved, or lost. Even if we *doubt* about the contagiousness of the disease, we are bound to *act* as if we had no doubt upon the subject; and I would admonish you (although that is a lower and poorer motive) that your own reputation and success may be endangered by mistakes in this very thing. I have been told of one instance, which may serve as a warning. A lady came from the country to London, to see a sister, who was ill of fever. She asked the medical attendant if there was any danger of infection. He happened to be a staunch non-contagionist; and he said "no danger whatever." Upon the faith of that assurance, the lady resolved to sit up with, and to nurse her sister; but she fell sick herself of the disease, and died. I believe this did not shake the medical man's opinion. He attributed the mischance to some local miasm. But he could not persuade the lady's friends of this: they maintained that her death lay at his door; and whether they were right or wrong, the affair was so much talked of, and was so injurious to him, that he was obliged to leave that part of the town, and to seek for practice elsewhere.

Many sporadic cases occur of febrile disorder, which do not belong to the category of continued fevers, and yet are liable to be confounded with them. All the phlegmasiæ are attended with pyrexia, and the local inflammation may be slight, or very obscurely marked, and easily overlooked: but the febrile symptoms are much the same as those which constitute the more *inflammatory* forms of continued fever. In like manner severe bodily injuries, compound fractures for example, sometimes give rise to constitutional symptoms precisely resembling the most prominent phenomena of its *typhoid* forms. Excluding all such spurious resemblances of the genuine disease, I confess my own opinion to be, that continued fever, like small-pox and measles, originates, in all cases, from a specific poison. With respect to all these disorders, I conceive that, in every large community, there is constantly kept up a sufficient stock of contagious matter, to act as a sort of leaven, whenever circumstances favourable to the development of the disease, as an epidemic, may chance to arise.

Alleged causes.—Continued fever has been ascribed, by those who deny its origin in contagion, to certain other causes; some of which demand attention, not merely on account of

the interest that belongs to the whole inquiry, but also because they really are important agents in the extension of the disease.

First, then, it was, and is, a common belief, that continued fever may be *generated* by the effluvia constantly proceeding from the human body, even when healthy, if these effluvia are accumulated and condensed by the crowding together of many persons in close, dirty, and ill-ventilated places. It is unquestionable that, when once introduced, the disease spreads, under such circumstances, with fearful rapidity: but the point at issue is whether it is ever so caused *de novo*. I must refer you, for good evidence that it is not so engendered, to Dr. Bancroft's book on the yellow fever. He shows that typhus fever does not spring up in places where it could scarcely fail to appear if that theory were true. He instances the natives of the arctic regions, who, in order to shelter themselves against the extreme cold of their climate, live during the greater part of the year in close subterraneous dwellings, from which the fresh air is studiously excluded, and of which the atmosphere becomes so offensively foul as to be scarcely supportable by a stranger: yet continued fever is not known among them. A similar exemption from that disease is observed within the tropics, in the African slave-ships, where "the poor wretches are crowded together below the deck, as close as they can possibly lie, in a sultry climate, barred down with iron to prevent insurrection." Although many of them die from suffocation, and from fluxes, yet Dr. Trotter, who was himself, at one time, surgeon to a slave-ship, declares that "contagious fevers are not their diseases." Dr. Bancroft quotes also a narrative of the sufferings of 193 Europeans, who, during the time of the French revolution, were "deported" to Cayenne, in the Decade frigate. They were crowded, and even squeezed together, in so small a space, and for so long a time, that the sentinels, who were placed at the hatchways to guard them, and who were thus exposed to the hot and fœtid air which came from their hole of confinement, demanded that their period of offensive duty might be shortened. Yet none of these miserable persons perished, nor did fever, properly so called, arise among them. Neither did that distemper present itself in any of the survivors of the Black-hole at Calcutta; the frightful account of which by one of those survivors, Mr. Howell, is abstracted in an appendix to Dr. Bancroft's work.

Fever used to infest our English jails; but that it was always imported, and never engendered there by filth and defective ventilation, and by the accumulation of human effluvia, may be concluded from the fact that the benevolent Howard, when he visited the prisons on the continent, found, to his great surprise, that they were free from fever, although they were no less close, crowded, and impure than our own. He brings the result of his observations and inquiries concerning the cause of the jail-fever to this pointed conclusion;—"If it were asked," says he, "what is the cause of the jail-fever, it would in general be readily replied, the want of fresh air and cleanliness; but as I have found in some prisons abroad, cells and dungeons as offensive and dirty as any I have observed in this country, where, however, this distemper was unknown, I am obliged to look out for some *additional cause* for its production"—which additional cause can be no other than the contagious poison emanating from the bodies of those who have the fever. It is true that fever is most frequently met with, and most rapidly propagated, where men are crowded together, in jails, or in close and ill ventilated places: but this affords no reason for supposing that it is ever *generated* there, any more (to use a homely illustration of Dr. Bancroft's) than the general prevalence of lice and other parasitic vermin in such places, proves that these vermin are *generated* by filth, by pent-up human effluvia, and want of ventilation, instead of being merely *fostered* thereby.

Again, continued fever has been attributed, with great confidence, to a vitiated state of the air, from the putrefaction of *dead* animal and vegetable substances. Dr. Bancroft deals with and demolishes this error also; showing that neither the putrid atmosphere of dissecting-rooms (respecting which *you* must have some personal experience), nor the noisome effluvia from full and ill-conducted burial-grounds, nor those to which tallow-chandlers, soap-boilers, glue and cat-gut makers, and the melters of whale-blubber, are exposed, nor the foul air of sewers and privies, have ever been known to produce any thing like continued fever. In some parts of Essex, near the coast, where the farmers are in the habit of manuring their fields with shoals of sprats, I have seen large tracts covered with these

fish in a state of putrefaction. The stench they occasion is horrible; but no disease results. Dr. Chisholm, in a paper to which I can only refer, but which I would recommend you to look at, in the sixth volume of the *Edinburgh Medical and Surgical Journal*, brings forward other, and very satisfactory, instances, to the same purpose; from a bone manufactory, near Bitton, in Gloucestershire; from an establishment (now relinquished) on the banks of the Avon, for converting the flesh of dead animals into adipocire; from manufactories for refining sugar, where the blood of slaughtered animals is kept for that use by butchers; from the leather-dressing business;—all tending, I say, to the conclusion, that air, contaminated by the decomposition of animal substances, is not necessarily noxious to life; still less productive of that specific disease which we are now considering. The old belief, therefore, was unfounded, that the exhalation from the dead and putrefying bodies of men and horses, lying unburied on the field of battle, are capable of producing a pestilence. Many instances to the contrary are on record: one, of an early date, is thus stated by *Diemerbroek:*—"Anno 1642, in agro Juliacensi maxima strages facta est, et ad minimum 8000 militum occisi fuerunt, præter majorem adhuc famulorum, rusticorum, aurigarum, puerorum et mulierum numerum, atque equorum copiam innumerabilem: corpora inhumata sub dio computruerunt, nulla tamen pestis insecuta est."

It has been said that fever is produced by some unknown condition of the air, imperceptible by our senses, but distinct from contagion. I believe this is a very popular notion; but it is perfectly gratuitous and untenable, and inconsistent with observed facts. You may have fever raging in certain parts of a town, and yet the parts in the immediate vicinity of these be quite exempt from it. Now this could hardly be, if there were some general property diffused through the atmosphere capable of engendering continued fever: and, as I stated before, this notion is severely pressed by one of the arguments which the non-contagionists themselves are fond of employing. If the disease depends upon contagion, some persons in the community may come less near the sick, or imbibe a less dose of the contagious poison, than others, and so escape: but if the cause of fever were spread abroad throughout the whole atmosphere, *all* must be exposed to its influence, and few, we may suppose, could avoid suffering from its operation.

Exposure to cold is another cause to which fever has been sometimes attributed. But this is an agency so widely prevalent, that if it alone could excite fever, that complaint would be far more general than it really is, and we should be able to trace it oftener and more distinctly than we can, to the alleged cause. That, by its debilitating effect, cold will predispose the body to be affected by the contagion of fever, I well believe: and the same remark is applicable to another cause that has been assigned; namely, deficiency of nourishment. Dr. Alison, in his admirable essay *On the Management of the Poor in Scotland*, has clearly proved that the prevalence of contagious fever amonst the lower orders is always in direct proportion to their state of physical destitution. The association of pestilence with famine is proverbial. But we do not find that continued fever is ever created by the mere want of nutriment. In persons who have sought to starve themselves to death—among sailors who have of necessity been kept upon very short allowances of food—in cases of insulation by snow storms, or by the accidental closure of a mine—we find, indeed, that *disease* is produced by the privation of nourishment; but it is not continued fever. The condition into which the sufferers are brought is more like scurvy. So that although a want of sufficient aliment may be, and doubtless is, a powerful auxiliary in promoting the effect of the contagious poison, there is no ground for supposing that it ever primarily or solely occasions fever.

In fact, all the circumstances which I have now been considering act as *predisposing* causes. They render the human body an easier prey to the true *exciting* cause, which is a specific animal poison. You will remark that they are all *debilitating* circumstances; and where several of them coexist, their joint influence in subduing the system, and bringing it into subjection to the contagion, is very great. For the same reason, depression of spirits, however produced, has a strong predisposing effect; as strong, perhaps, as any other single cause. Of this I have seen, if I mistake not, some remarkable examples. Upon this principle we may explain the fact, that continued fever is especially apt to attack those of the lower classes who have recently come to reside in the metropolis; who are

often living anxiously, and with persons to whom they were not previously known. We have had numerous opportunities, in the hospital, of noticing this curious circumstance: and it is mentioned by almost all the French writers on fever. To the same principle also we must attribute the tendency to fall down in fever, observed in young soldiers and sailors. You will find statements to that effect in Dr. Trotter's works, and in those of Dr. Lind and of Sir Gilbert Blane. When fever appears in a ship, the raw sailors are always the soonest attacked by it; and it is the same with the recruits in the land service. In a defeated, dispirited, or retreating army, its ravages are often frightful. Here we have, combined, fatigue, a deficiency of wholesome food, and mental depression.

However paradoxical the assertion may seem, a predisposing cause may even be applied, and operate, *after* the exposure to the exciting cause—and so render the latter effective when it might not, otherwise, have been so. Dr. Russell observed the plague sometimes to "hang ambiguously" about persons for several days. In this state, an overheated bath, or a sudden impression of fear, especially *fear of the disease*, has roused the lurking poison into activity. It is a suggestion of Dr. Henry that atmospheric variations may call into action contagious poisons already admitted into the system, but not yet manifested by the usual phenomena; and, operating thus over a wide space, and upon numbers at once, may occasion those sudden and violent outbursts of epidemic disease, of which numerous examples are on record.

You must, I think, perceive the importance of distinguishing between the one exciting cause and the many predisposing causes of fever. The latter generally are beyond our control. We cannot hope to remedy extensive destitution; nor to separate it effectually from its concomitants of filth, and despondency of mind. But we may, by timely diligence, root out the specific contagion, or confine it within narrow bounds. When the sick can be at once removed from their crowded homes to a fever hospital, and their impure apartments ventilated, cleansed, and whitewashed, the disease may often be kept in check, if not entirely stopped; and the yet healthy persons of the infected district be preserved from its grasp. For, as I stated before, there is reason to believe that the poison, unless pent up, does not remain active at any great distance from the person from whom it proceeds; not even many yards, or feet. It is very rare, I apprehend, to meet with instances of the disease being communicated in the open air. It is almost always caught, if at all, in the interior of houses. It is extremely uncommon for it to extend from one bed to another in our general hospitals, where great attention is paid to cleanliness and ventilation. The noxious qualities of the poison are diminished, and at length destroyed, by its dilution with common air, just as those of other gaseous poisons are: and hence, in private houses, in the better ranks of society, where the rooms are spacious and airy, and proper precautions are taken, the disease hardly ever spreads.

What are those proper precautions? They are simple, and may be stated in a very few words.

Where choice can be made, a large apartment should be selected for the sick person. Unless the weather be very hot, there should be a fire in the room, for it acts as a ventilator. The air of the chamber should be kept fresh by having a window, or a door, (according to the weather) always open; or both window and door. Bed and window curtains, carpets, and all superfluous articles of furniture, should be removed. Great diligence should be used in keeping the patient clean, by the requisite ablutions, and by frequently changing his sheets and his body linen; and these should be immersed at once in water: and all discharges from the sick person's body should be instantly carried out of the room.

All unnecessary intercouse with the patient, by his family and friends, should, for his sake, as well as for theirs, be forbidden. As life advances, the susceptibility of the disease appears to diminish; for which reason the nurses and personal attendants of the patient should not be very young: and all who do approach the sick bed should take care to avoid, as much as possible, inhaling the patient's breath, or the emanations which proceed from his person. Friends who visit the apartment at intervals only, should never enter it *fasting*.

And you may, if you please, employ the chloride of lime as a disinfecting agent, *in aid of* the simple measures I have just recommended; but by no means *instead of* them. People hang a bag of camphor round their necks, and think themselves safe against in-

fection. The mental confidence which that expedient is calculated to inspire, may perhaps afford some degree of protection: but camphor has, in reality, no prophylactic virtue; and all these artificial scents are objectionable, inasmuch as they tend to conceal offensive odours which might otherwise reveal the actual danger.

I shall speak of the *treatment* of continued fever when we next meet.

LECTURE LXXXVI.

CONTINUED FEVER, CONCLUDED. TREATMENT. SMALL-POX. ITS ESSENTIAL SYMPTOMS. DISTINCTION INTO DISCRETE AND CONFLUENT. PERIODS AND MODES IN WHICH IT PROVES FATAL.

Treatment.—The treatment of continued fever has been, at all times, a stumbling block to young practitioners; and a subject of dispute even among physicians who have built it upon their own experience. Before I attempt to trace out any plan, or to lay down any principles for your guidance, it may be useful to inquire how it happens that the practice in this disease has been so fluctuating and unsettled.

In the first place, then, it is very difficult to estimate the value and efficacy of any particular plan of treatment, and still more of any particular remedial substances, in this disease. Continued fever, like other disorders which run a definite course, and have no direct or necessary operation in spoiling the structure of vital organs, has a strong natural tendency to terminate in health. We see this tendency when the disease is left entirely to itself, and it equally exists when remedies are employed to regulate its course, or to abbreviate its duration. No one can doubt, who has had much experience in fever, that this tendency is sometimes thwarted by the *nimia cura medici;* and that patients get well in *spite* of the well-meant but mischievous interference of the Doctor. This tendency to recovery is a constant source, therefore, of fallacy in our observations upon the behaviour of this disease under different plans of treatment; and upon the effects and utility of remedies. It leads us, too often, into the danger of ascribing to drugs what is really due to the workings of nature; of confounding antecedents and sequences with causes and effects; of counting recoveries as cures. And this danger is increased by the circumstance that continued fever, although it observes a certain definite course, is nevertheless liable, even when left altogether to itself, to sudden and remarkable *changes in the symptoms*, sometimes for the worse, and sometimes for the better; and often we cannot perceive any obvious reason for these fluctuations. But if this happens when no medicine is given, so also will it happen when the disease is submitted to treatment; and it requires more than a little care and discretion to avoid attributing the changes which so occur to the remedy which was last employed. For example, the abatement or cessation of *headache*, after a few days have elapsed, is a natural phenomenon: whereas an inexperienced or a careless person might easily persuade himself that it had yielded to his method of treatment, and that it was a favourable omen: neither of which conclusions would, however, be warranted by the circumstance upon which it was founded.

There is yet another source of difficulty connected with this subject. I have shown you that not only individual cases of fever, but different epidemics, vary much in their character; so that a plan of treatment which was well suited to one epidemic, may be improper and even hurtful if indiscriminately applied in another.

These considerations may serve in some measure to teach us how it has happened that so many *different*, and sometimes *opposite*, remedies and modes of treatment have been recommended by different practitioners for the cure of this disorder. The tendency to a termination in health was very plainly visible in the epidemic fever in Ireland, to which I have more than once referred. The mortality among the patients who were placed in sheds upon straw, with very little medical care, and even without any great personal

attention from others, was very small indeed. No one can form even an approximate judgment of this tendency, who has not seen the disease under several varieties of practice. Doubtless one rule which we derive from a clear perception of the same tendency is, that we should *not* interfere *unnecessarily*. Ασκειν περι τα νουσηματα δυο (says Hippocrates) ωφελεειν, ἡ μὴ βλαπτειν. There are two things to be considered in the treatment of disease: first, that we do the patient *good;* secondly, that *at least* we do him *no harm*. In all these exanthemata, he must be reckoned the safest and the best practitioner who knows when to abstain from acting, as well as when to act; in other words, who has learned when, and to what extent, the case may be left to the salutary processes of nature.

However, there is an opposite error to that of mischievous activity. The tendency to recovery which manifests itself under different modes of treatment, and even in spite of opposite modes, has induced, in some minds, a degree of scepticism as to the utility of any remedies, that may be easily carried too far. It does not follow, because the majority of patients under continued fever would at length emerge into health, although no remedial measures were employed, that the disease ought therefore to be abandoned to what Cullen calls the *vis medicatrix naturæ*. It is not quite correct to say, with the older pathologists, that the whole disorder is merely an effort of nature to throw off something noxious to the system, and *therefore* is not to be interfered with. The true view of the matter, I apprehend to be that which a toxicologist might take. The disease is produced by a poison of which the injurious impression upon the animal economy at length ceases, or passes off, of itself; in the same manner, only more slowly, as the influence of a dose of opium will spontaneously pass away. But during the natural course of the fever, as in many other cases of poisoning, morbid processes are apt to be set up, which, if suffered to proceed unchecked, would inflict irreparable injury upon important organs, and which are fairly within the scope of remedial management. Our object must be, when the fever is once established, to conduct it to a favourable close: to "obviate the tendency to death." Upon this point I ageee most entirely with Pitcairn, who, being asked what he thought of a certain treatise on fevers, declared, "I do not like fever curers. You may *guide* a fever; you cannot *cure* it. What would you think of a pilot who attempted to quell a storm? either position is equally absurd. In the storm you steer the ship as well as you can; and in fever you can only employ patience and judicious measures to meet the difficulties of the case."

When some immediate change ensues in the symptoms or in the feelings of the patient upon the administration of remedies that are generally followed by sensible effects, we are warranted in ascribing the change to those remedies. But even here comes in the fallacy already noticed, arising from the sudden and *spontaneous* changes that are apt to occur in fever: and this fallacy is to be got over only by multiplying our observations.

After all, the best guide that you can have in determining upon the general principle of treatment in a given epidemic, or even in an individual case, is that which Dr. Alison has ably enforced in his lectures and in his writings upon this subject. I mean the observed tendency to this or that mode of dying. The manner and circumstances of the deaths, are of more practical importance than of the recoveries.

After briefly passing in review some of the principal remedies that have been employed and recommended for this disease, I propose to sketch the plan which I am myself in the habit of pursuing in the management of fever patients.

It was once a favourite practice with physicians to attempt to *cut short* the fever at its outset: and the two expedients which were chiefly relied upon for that purpose were *emetics*, and the *cold affusion*. They have both of them, in this country, gone very much out of fashion. In truth, neither reason nor experience encourages us to look for such a result from such measures. If fever depends (as I believe it does) upon a poison in the blood, it is not to be dislodged by the act of vomiting, nor washed out by the forcible descent of cold water upon the skin: and in the few instances in which the one or the other of these remedies may have *seemed* to arrest a fever, or to check its progress, that effect has always occurred at the very commencement of the complaint; so that we cannot be sure (and the probability lies the other way) that these were really cases of fever at all, or that they would not have ceased even if nothing had been done for them. Perhaps emetics may, in the present day, be too much neglected. I have no notion of their

stopping the fever: but when given early, especially if gastric disturbance is a prominent symptom, they are sometimes followed by a marked abatement of many morbid sensations. "It is astonishing," says the observant Sydenham, "how it happens, that a vomit, which does not produce either a large or a morbid discharge from the stomach, should so materially relieve the nausea, restlessness, anxiety, and furred tongue of the patient."

The cold affusion is not more effectual in cutting fever short than the treatment by emetics; and it has these great disadvantages, that it fatigues and alarms the patient: and when the vital powers are naturally feeble, or are much depressed by the disease, the very shock of the affusion may be attended with injurious consequences.

A modification of this expedient is, however, often of great use in abating the morbid heat, and soothing the uneasy feelings of the patient. I mean the practice of cold or tepid *sponging* of the surface. This is one of the remedies which, when the symptoms appear to indicate it, deserves to be tried; and the propriety of continuing or of discontinuing it may be determined by a very simple test; namely, the feelings and wishes of the patient himself respecting it. It is most adapted to the more inflammatory, and least adapted to the more typhoid types of the malady.

Great controversies have been maintained in regard to the effect of *blood-letting* in fever. They who hold that the fever consists in a general disturbance of the system, growing out of some local inflammation, and they especially who believe that continued fever is nothing else than inflammation of the brain and its membranes, would naturally seek to cure it by the remedies of inflammation. But although local inflammation, and even inflammation of the brain, is very apt to spring up in the course of continued fever, there is no reason for thinking that inflammation *anywhere* is *essential* to the fever; but very much reason for the opposite opinion. The active use of blood-letting has been in favour and out of favour, with the medical world, again and again: and this very circumstance would of itself make us doubt the propriety of its indiscriminate adoption.

The late Dr. Armstrong gave a strong and unfortunate impulse to the practice of free blood-letting in continued fever, by the publication of his well-known and ingenious treatise on the disease. I have no doubt that great mischief was done by that work. I may say so without scruple, since Dr. Armstrong is gone, and neither his feelings, nor his success, can be hurt by the expression of such an opinion: and I do so the rather, because it is well known here that Dr. Armstrong saw reason, as his experience increased, to qualify those views respecting the nature and management of fever, which his earlier observation of it in the country had led him to form. This change in his sentiments was probably justified and produced by a change in the character of the fevers that he witnessed; but it does not appear in his book. You have heard me state already that whereas the fevers which occurred in London for some time previously to the year 1831 or 1832, not only bore, but required the abstraction of blood, in some way or another,—since that period, and especially since the epidemic cholera visited us, it has been necessary to abstain, whenever we could with safety, from taking blood at all; and still more necessary, even if we take away blood with one hand, to uphold the patient with the other: while, in the former period, wine, and stimulants of all kinds seemed generally superfluous, if not pernicious.

Dr. Williams, of St. Thomas's Hospital, has shown, very clearly I think, in his recent publication on "Morbid Poisons," that the evidence against the efficacy and the safety of bleeding in continued fever—of bleeding largely, I mean, from the arm—far outweighs that in its favour: and I venture to advise you, as the result of all that I have seen of the disease in London and elsewhere, *not* to draw blood from a vein, even early, *merely* because the disease is or appears to be *fever;* not to order venesection unless there be some other manifest reason for it—unless, *i. e.*, the febrile symptoms run unusually high, or unless some local inflammation is unequivocally present: and, when you do bleed, do not take a drop of blood more than seems absolutely necessary to answer the desired end. Bleed your patient, therefore, if at all, in the upright posture.

Purgatives.—What are we to say in general with respect to them? *This*, I believe—that the intestines should be cleared by an active aperient at the outset: and that laxatives should be continued if the bowels do not act every day without them. When the

typhoid type is strongly marked, and when the symptoms indicate ulceration of the intestinal glands, purgatives are not to be pressed.

Much contrariety of opinion has prevailed also among practitioners, and does prevail, about the administration of *mercury* in this disorder. Without attempting to strike the balance between these conflicting judgments, it is my business and duty to state my own belief, to tell you what is the result of my own observation, upon this and other disputed points. I must repeat, then, that my practice has altered, in several particulars, within the last few years. In the fevers which I treated, or saw others treat, in London, prior to the breaking out of the present epidemic, mercury, in one shape or another, was almost constantly prescribed; and a great number of the patients were brought, sooner or later, under the specific operation of that mineral: and in these patients (with one exception only, where the mercury appeared to do neither good nor harm) a *decided improvement* was almost immediately apparent upon the supervention of soreness of the mouth; and all such patients ultimately recovered. I am aware, however, and I wish you to be aware, of an alleged source of fallacy in this matter. The gums in that variety of fever to which I am at present referring, did not readily take on the mercurial action; and it *might be* (though such is not my own impression), it might be that the affection of the mouth by mercury was attributable to the mildness or to the cessation of the disease, rather than the cessation of the disease to the effects of the mercury upon the system: that the improvement was the cause, and not the consequence, of the mercurial action.

In the form of fever that is *now* epidemic (1838), I do not think mercury so proper. Last year the spotted fever broke out in a patient of mine while he was in the hospital, and while his gums were sore. I found, upon inquiry at that time, that of twelve fever patients who had been recently treated in the hospital, by my colleagues and myself, with mercurials in greater or less quantity, four had died, in all of whom the gums were affected: whereas, of sixteen others, who took no mercury, three only died; and of these three, one was pulseless at her admission, from uterine hæmorrhage, and the other two were so feeble and exhausted that they were scarcely able to swallow. I should give mercury very cautiously, therefore, if at all, in this typhoid variety of the fever.

Besides a strict enforcement of the antiphlogistic regimen, my own mode of treating continued fever is somewhat of this kind. I am always desirous that the patient's *hair* should be cut off. The mere removal of it is often attended with benefit; the headache and confusion of thought are relieved, and the patient is calmed. We can then also, with much greater convenience and effect, apply cold washes to the head. Patients sometimes demur to this shaving of their heads: but they generally consent if you explain to them that their hair will at length fall off, in consequence of the fever; and that the head, if on that account only, had better be shaved at once. The head and shoulders should be somewhat raised, and thin strips of linen, kept constantly wet with some cold lotion, should be continually applied upon the forehead and scalp. It should be the business of one person to attend to this. You would be surprised at the rapidity with which the cloths sometimes dry.

Now with regard to this remedial measure you need not have any difficulty. It will do good, and should be steadily employed. so long as it is *grateful to the feelings* of the patient; and it *will*, generally, be pleasant and agreeable to him so long as the head remains morbidly hot. If the temperature of the scalp is above the natural standard, and especially if the cold application makes him shivery or uneasy, and gives him annoyance instead of comfort, then it must be at once discontinued.

If the patient suffered intense headache, and his face was flushed, and the heat of the surface great, and he was wildly delirious, and his pulse was full and hard, I might perhaps deem it right to take blood from his arm, while he sat up. But even under these circumstances I should generally think it better to apply leeches to his temples, or behind his ears, or to remove a few ounces of blood from his neck by means of cupping-glasses, and at the same time to ply assiduously the cold lotion.

If the bowels have not been already purged by nature, or art, it is right to give three or four grains of calomel at once, and to follow this up by a black dose. After that I was formerly in the habit of prescribing, in the early periods of the fever, unless the bowels were irritable, a pill composed of two grains of calomel with two grains of James's powder,

every six hours; washing it down with a common saline draught. In hospital practice I frequently omitted the saline draught; allowing the patient to drink toast and water, or barley water, as often as he wished. If there was diarrhœa, or early prostration, I gave, instead of the combination just mentioned, five grains of the *hydrargyrum cum cretâ*. At the same time I carefully investigated the state of the abdomen; and if I found tenderness at the epigastrium, or in the cæcal region, in both of which situations tenderness was common, I applied, over the tender spot, from six to ten leeches, and covered the leech-bites with a light poultice. If the diarrhœa ran on profusely, I added to the hydrargyrum cum cretâ a certain quantity of Dover's powder, or of the extract of poppy.

In those forms of fever (whereof the prevailing epidemic affords you a well-defined specimen) in which there is a strong and early tendency to typhoid symptoms, a signal loss of strength, a confused and dusky countenance, a mottled state of the skin simulating the eruption of measles, a dark dry brown tongue, a feeble pulse—in these forms I begin very early to give the patient a full allowance of beef tea; and if the typhoid and nervous symptoms become more pronounced, I add ammonia, Hoffman's æther, and, what is better still, wine; and I omit the mercury.

Under this kind of management the patients will often go on, in a doubtful state, for some days, and at length begin to recover. Many of them, especially in the more typhoid varieties, sleep heavily, as the disorder passes slowly off.

There is one point in the treatment of fever, of exceeding importance, and of some nicety: I mean the use of *opiates*. If they are given inopportunely, they are apt to puzzle and perplex the case. You do not know how much of the disposition to coma is owing to the disease, and how much is the consequence of the remedy. Again, you may easily *augment* the natural tendency to coma, and lull your patient into a fatal stupor. But, when judiciously administered, opium will often save a patient who would inevitably sink without it.

It is in that form of fever which the French call the *fievre ataxique*,—when the patient is affected with delirium, restlessness, wakefulness, and spasm, and the disturbance of the nervous system outruns the disturbance of the sanguiferous system,—that opium is so beneficial. The condition of the patient resembles that of a person in *delirium tremens*. It is said that these symptoms occur most commonly among patients in those ranks of life that are above the lowest rank; and it probably is so: but they are apt to take place in any patients, high or low, rich or poor, who have had the mind overwrought, and the nervous system unstrung, whether by dissipation and intemperance, or by anxiety of any kind. Sydenham was quite aware of the existence of this particular set of symptoms, and of the remedy for them. Of all these symptoms, *sleeplessness* is the most urgent. Dr. Grattan and Dr. Latham have both written in praise of the same opiate treatment, under such circumstances, as was recommended by Sydenham. Dr. Grattan observes, with great truth, that two or three nights, spent in restless delirium, are followed by the worst consequences; and that patients who pass *three* nights in succession in that way, almost invariably die. If the symptoms be well marked, the best mode of proceeding is to give a tolerably full dose of opium in the evening; one-third of a grain of acetate of morphia is a common prescription with me. The amendment of the patient, on the following day, is often very striking. Unless the same symptoms recur, it is better, I think, not to *repeat* the anodyne. But, as Dr. Latham cautions us, "there are cases, where the indications for the employment of opium are *doubtful*. Wild delirium, and long wakefulness, and a circulation weak and fluttering, seem to call for a considerable dose of opium. Yet, withal, there is a certain jerk in the pulse, so that we cannot help *suspecting* that the blood-vessels have something to do with the sensorial excitement. Under such circumstances, I have certainly seen (says he) twenty minims of laudanum produce tranquil sleep, from which the patient has awoke quite a new man. But I have also seen the same quantity produce a fatal coma, from which he has never been roused. Now (continues Dr. Latham) since it is a fearful thing to strike a heavy blow in the dark, where the alternative is of such magnitude, it is the safest and the best method to administer a small dose, at intervals of an hour or two: so as to stop short of actual mischief at the first glimpse of its approach, or to be led, by a plain earnest of benefit, to push the remedy to its full and consummate effect. Many doses may

be required for this purpose; but we shall see, after the first or second, whether to go on or to desist."

When, as is sometimes the case, the stress of the disorder falls upon the thorax, and there is much dyspnœa, with the sounds that denote inflammation of the bronchial membrane, or of the pulmonary substance, leeches or cupping-glasses may be applied to the chest; and, in milder cases, a blister, or a mustard cataplasm.

These remedies —cold to the shaven head; the local abstraction of blood wherever there happens to arise evidence of local inflammation; an active purge at first, and mild aperients afterwards, if the bowels are confined or sluggish; moderate astringents, if there is much or urgent diarrhœa, a few grains of Dover's powder, for example, or of the extract of catechu; opium in a more efficient dose, when the nervous symptoms are prominent, particularly sleepless delirium and restlessness; in certain cases small and repeated doses of some preparation of mercury; and in certain cases, *early* support by animal broths, and even by wine—these remedies, adapted to the particular circumstances of individual patients, form the staple of the treatment of continued fever, according to the best of my judgment and experience.

After what has been said, I need scarcely again admonish you to study carefully, not merely the symptoms of any particular case to which you may be called, but the general character of the fevers that are at the same time prevalent, and the manner of dying in the fatal cases. If you find that they who die, die chiefly in the way of asthenia, that will be a strong reason for caution in respect to the removal of blood, and for the early employment of beef-tea, and other means of support. In the form of fever which has of late years been common in the most crowded and unhealthy parts of London, I am sure that the risk of beginning this sustaining treatment a little too early is much less than the risk of commencing it a little too late. If plenty of beef-tea does not suffice, you must give the patient wine, and that sometimes to a considerable amount, or even brandy; the egg-flip of the Pharmacopœia for instance, the *mistura vini gallici*. The object is to keep him alive, to keep the heart in motion, until the depressing influence of the exciting cause of the disease shall have passed by. If the wine should flush or excite him, or render the pulse hard, it must be diminished in quantity, given less frequently, or omitted altogether. If there be indications of local inflammation—pain (for example) in the cæcal region, increased by pressure—leeches may be used, and wine given at the same time. There is nothing inconsistent in such mixed practice. We seek to remove the local inflammation by unloading the capillary blood-vessels of the part, while we uphold the general powers of the constitution which are ready to sink. The great art of getting a fever patient through a bad attack, is to have him judiciously and perpetually watched, by night and by day. The remedy that is proper one hour may do harm if pushed during the next. And there is another reason, which I may mention *en passant*, why the sick person should never be left alone, even for a moment. I have heard of more than one or two instances, of patients, in the delirium which attends the disease, getting out of bed, and out at the window, during the temporary absence of their nurse, and perishing from the fall.

If the patient relishes and wishes for the beef-tea, or the wine, that is no small warrant of the propriety and usefulness of its administration.

A word or two more may be proper, even in this cursory sketch, respecting certain incidental points of practice that are apt to arise.

Often, when there is no longer any detectible uneasiness produced by pressing the belly, the diarrhœa will persist, and *meteorismus* will come on; a tympanitic distension of the intestines with gas. When this happens, especially in the latter periods of the fever, a large *blister*, laid over the abdomen, has often very happy effects, both upon the diarrhœa and the meteorismus. The diarrhœa, when very urgent and obstinate, may often be checked by opiate *enemata*. In some cases I have recently found catechu of great use, in these long-drawing forms of diarrhœa during fever.

Again, if the patient sinks into profound coma, a blister should be applied to the shaven scalp. The sick man will sometimes awake from deep stupor while the blister is still rising.

It is always a matter of importance, as I stated before, to inquire carefully, every day,

into the condition of the bladder of these fever patients. In the state of stupor and indifference in which they often lie, they appear not to be sensible to the want of passing the urine, which collects in the bladder, and distends it enormously; not only increasing the present hazard of the patient, but laying the foundation, sometimes, of future disease of the kidneys, in case he recovers from the fever. You must not take the assurances of the nurses upon this point. They will often tell you that the patient has made plenty of water, when in fact the urine has been dribbling away from him, overflowing, while the bladder is stretched to the utmost. Feel, therefore, with your hand, and percuss the hypogastric region, as well as that of the cæcum, at every visit.

It is requisite, too, that the under surface of the patient's body should not only be kept scrupulously dry and clean, but be *looked at* every day, or twice a day. If the projecting points, the hip bones, the sacrum, the shoulders, the elbows, should become *red*, that is a sign that they are likely to slough or ulcerate. This evil consequence of continued pressure upon parts of which the vital power and healthy tone are lowered, may often be prevented by washing the erythematous spots with brandy. Should the skin be already broken, the place may be covered with soap-plaster. An adjustment of pillows and of posture—or the water-bed—may sometimes supersede the necessity of these local expedients.

When ulceration of the mucous follicles perforates the bowel, that catastrophe does not *always* so distinctly declare itself in fever, as in other cases. I have seen such perforation, and its resulting peritonitis, when no complaint of pain had been made by the patient, so great was his insensibility. In general, however, the accident is denoted by symptoms which cannot be mistaken. When it does occur, there is but little chance of the patient's recovery: and that little will be best husbanded by the adoption of the plan of treatment which I formerly laid down: opiates, to check the peristaltic movements of the intestine; and a rigid adherence, for some days, to the horizontal posture.

I have seen a few instances, in which an œdematous swelling of one leg and thigh has occurred in the advanced stage of fever, like that which is incidental to parturient women, and dependent upon the same cause—inflammation and obstruction of the great vein that returns the blood from the limb towards the heart. I believe that the inflammation extends itself, in such cases, from some of the smaller veins of the mesentery, and is excited in them by the intestinal ulceration. Fomentation of the affected limb comprises all the remedial treatment which this accident requires or admits of.

The management of the patient during convalescence is scarcely of less importance than during the progress of the fever. The chief danger is, that his desire to be allowed to get up, and his wish to eat animal food, should be too soon indulged. The latter of these errors is more frequently the cause of a relapse than any other circumstance; and relapses are often more perilous and difficult to remedy than the original malady. You must be prepared, therefore, to withstand the solicitations of the patient and of his friends, who think that if strength be wanting, strong drinks, and plenty of meat, are the things to impart it. Until the tongue is quite clean and moist, and of its natural colour, and the pulse has lost all its hardness, and the skin its excess of heat, the patient must be kept to broth, jellies, puddings, and preparations of the well-known farinaceous articles of food. Then he may begin with some boiled white fish, and so gradually eat his way, through chicken, and a mutton-chop, to his ordinary diet again.

Such, I say, is the general plan of treatment which some observation of this disorder has persuaded me is the best. Summarily expressed, it consists in the exercise of incessant vigilance, and the adoption of the proper remedy at the proper moment. It lies between a timid or sceptical abandonment of all known resources, and a meddlesome rashness in applying them. The flame of life may be suffered to expire for want of timely succour, by the practitioner who folds his arms, and looks on; as it may be rudely extinguished by a restless or routine interference which has no definite or intelligible purpose. Boerhaave, in the preface to his *Aphorisms*, professes that he knows of nothing which can be fitly termed a *remedy*, "quin solo tempestivo usu tale fiat." In fevers the wisdom of this maxim is eminently conspicuous. The rational objects of treatment are, to mitigate the urgency of symptoms that cannot be wholly subdued; to redress (as far as art may redress) those dangerous complications which are incidental but not essential to the disease; and to aid the conservative efforts of nature, when these manifestly languish and fail.

Dr. Williams holds that enemata of warm water, and syrup of poppies, will do all that can be done beneficially. For some time he lost only one patient out of sixty-three thus treated. This was a most encouraging result. But then, when the fever changed its type, as it did about the period of the arrival of the epidemic cholera, he lost one in every four or five: a very large mortality. These facts illustrate, in a strong manner, the necessity, which I have so often endeavoured to inculcate, of taking into the account, when we would estimate the value of a particular remedy or plan, the difference which obtains in different epidemics, whether bad, good, or no treatment at all be adopted. A far surer method is to compare (if you would experiment at all) two or more different modes of practice in different cases of the *same* epidemic. Thus Dr. Latham, finding during one season that his wards were full of fever, while yet its type was so mild that scarcely any died, thought this a favourable opportunity for trying whether mercury had any beneficial operation upon the disease. Accordingly he treated half his cases with small doses of the *hydrargyrum cum cretâ;* and the other half with the *liquor ammoniæ acetatis*, and so forth, and no mercury; and he found that the patients in the first of these classes were, on the average, convalescent sooner than in the last. Chomel fancies, from some trials, that the chlorate of soda is a useful remedy, in addition to the general plan of management; but further experience seems wanting to settle this question.

Small-pox.—I pass, without pausing, from the consideration of continued fever, to that of *small-pox* or *variola:* a disease, fortunately, less common in this country than it used to be, yet still sufficiently frequent and formidable to require that we should acquaint ourselves with the phenomena it is accustomed to present; and very prevalent here, as it happens, at present (1838). I have already mentioned, by anticipation, several points in its history.

Symptoms.—This frightful disease sets in with smart febrile symptoms: rigors, followed by heat and dryness of skin, a hard and frequent pulse, pain in the epigastrium, with nausea and vomiting, and headache. Sometimes wild delirium, sometimes convulsions, mark its outset. Then, to use the words of Cullen's definition, "tertio die incipit, et quinto finitur, eruptio papularum phlegmonodearum, quæ spatio octo dierum, in suppurationem et in crustas demum abeunt, sæpe cicatrices depressas, sive foveolas, in cute relinquentes."

When small-pox is fully formed, it cannot be mistaken for any other complaint: but it is of some importance to recognize it at its very commencement, for the force of the impending disorder may sometimes be lessened by judicious measures adopted at that early stage. The symptoms, however, that mark the outset of all febrile diseases are necessarily very much the same. If the pyrexia sets in when small-pox is prevalent in the neighbourhood, if the person in whom it occurs is an "unprotected" person (*i. e.*, one who has neither been inoculated nor vaccinated previously), and especially if he is known to have been exposed, within from nine or ten days to a fortnight, to the contagion of variola, we may well suspect that the disease will turn out to be small-pox, and act upon that suspicion.

Nevertheless, there are some symptoms which, being common in the commencement of variola, and not common at the beginning of continued fever, or of the other exanthemata, may assist the early diagnosis. Vomiting is one of these; pain of the back another. When these symptoms are violent, they usually usher in a severe form of the disease. The same may be inferred from a continuance of the nausea and vomiting, after the coming out of the eruption; which is very unusual. Heberden noticed that acute pain *in the loins* was almost always followed by a severe disorder; that pain higher up, between the shoulders, was of better augury; and that it was to be reckoned in all cases a good sign, if there was no pain of the back at all. Early delirium, stupor, or convulsions, announce severity in the subsequent course of the malady. Yet not always, especially in children. Within the last month I was asked to see a child which had been suddenly attacked with convulsions, followed by coma. In due time the eruption of variola appeared, and the disease ran a mild course, with little aid from medicine, although the child was previously unprotected.

The peculiar eruption almost always begins to show itself on the third day of the fever.

The earlier it comes, the severer generally does the disorder prove. In judging of the date of the eruption, you must bear in mind that parents and servants are apt to state its accession to have been later than it was in reality: for the spots are at first so minute that they often escape observation. They also frequently begin to come out in the night; and the morning of the second day of the eruption is called the first day.

The eruption comes out first on the face, then on the neck and wrists, and on the trunk of the body, and lastly on the lower extremities. Such is the rule; so that (as is specified in the definition) it does not cease to come out till the fifth day: and it keeps ahead, in that order, throughout the disease. There are, indeed, some exceptions to this rule. Occasionally the spots appear first upon the extremities, but this is very rare. In some instances straggling papulæ continue to spring up after the main crop is fairly completed; but these stragglers seldom arrive at the same size with the others.

The pimples, or papulæ, ripen gradually into *pustules*, the suppuration being complete by their eighth day; and on that day the pustules generally begin to break, and crusts or scabs to form. In four or five days more the scabs are falling off. There are some variations in all this also. In children, the crusts are sometimes visible on the seventh day; and in adults, when the disease is severe, they sometimes do not begin to form till the ninth day. In all cases some of the pustules are liable to be prematurely broken, by accident, or by the patient's scratching; and these will crust over earlier than they otherwise would have done. So that in fixing the period of incrustation, you are to regard those pustules only of which the natural progress has not been interfered with.

All that I have hitherto been saying, applies, with more or less exactness, to the disease in all its varieties. But its severity differs exceedingly, as I have already hinted, in different cases. Its severity, in truth, is almost always in direct relation to the *quantity of the eruption*. The number of pustules indicates, in the first place, the quantity of the variolous poison which has been reproduced in the blood. In the second place, it is also a direct measure of the extent to which the skin suffers inflammation. Sometimes there are not more than half a dozen pustules; sometimes there are many thousands. If all these were collected into one, it would be an enormous phlegmon. For both these reasons the system suffers commotion, distress, and peril, in proportion to the quantity of the eruption.

Distinction into discrete and confluent.—When the pustules are very many, they run together; when they are few, they are separate from each other. And this affords a broad line of distinction, which can neither be overlooked nor mistaken, into the variola *discreta*, and the variola *confluens*. In the one, the pustules are distinct, and of a regularly circumscribed circular form. In the other they coalesce, and their common outline becomes irregular. Now the discrete form of the disease is scarcely ever dangerous; the confluent form is never free from danger. The distinction therefore is of the greatest importance and interest. To be fully estimated each form must be considered separately.

In the discrete variety, in which the disorder may be presumed to run its most natural course, the eruption is at first, according to the phraseology of Willan, *papular*. The pimples gradually increase in magnitude, but it is not till the third day of their appearance that they begin to contain a little fluid on their summits. For two days after this they increase in breadth only, and a depression is observable in the centre of many of them. The cuticle is bound down there somehow, for a time, to the cutis vera. It is the *eighth* day *of the disease*, or the *fifth* day *of the eruption*, before the pustules become perfectly turgid and hemispheröidal. During the time in which they are thus filling up, the face swells; often to so great a degree that the eyelids are closed, and the natural aspect is completely changed or concealed. The skin between the pustules on the face assumes a damask red colour. About the eighth day of the eruption a dark spot makes its appearance on the top of each turgid pustule, and at that spot the cuticle breaks, a portion of the matter oozes out, and the pustule dries into a scab. When this crust at length falls off it leaves behind it either a purplish red stain, which is still very characteristic of the disease, and which very slowly fades; or a depressed scar, which is indelible. In the latter case the patient, or more properly his skin, is said to be *pitted* with the small-pox, or pock-marked. The swelling of the face begins gradually to diminish after the eruption has become thoroughly pustular.

This is the course which the eruption pursues on the face, where the pustules, even in the discrete form of the disease, are usually thicker set than on any other part of the surface. And it pursues the same course, only two or three days later, upon the extremities, where it also begins later. The face and hands swell just as the face swelled, but they begin to tumefy as the features begin to subside. Some of the pustules, especially on the extremities, do not burst at all, but shrivel up.

In this, the distinct variety of the disorder, the fever generally ceases entirely upon the coming out of the eruption: the headache, the pain of the back, the vomiting, the restlessness, abate and disappear, the pulse resumes its natural force and frequency, and the skin its natural temperature. About the seventh or eighth day of the eruption there is commonly for a day or two a recurrence of fever. This is called the fever of maturation.

You are to observe that we judge of the eruption as it appears on the *face*. The disease is of the confluent kind, when the pustules are confluent there, whether they be so or not upon the trunk and extremities. Sometimes they are neither confluent nor strictly separate, but stand just thick enough to touch each other, without absolutely coalescing; every pustule preserving its circular outline. In that case the disease is said to be of the *cohering* form. When the pustules are confluent over the whole body, their number is often prodigiously great, and their progress is less regular than in the discrete and milder variety of the complaint.

In the first place the eruptive fever itself is usually more violent and tumultuous in the confluent disease: the disturbance of the sensorial functions is more common and more decided, the sickness more distressing, the pain of the back and loins more severe. The eruption comes out earlier, and more confusedly; the pimples being at first very minute, and crowded together in patches, and not seldom accompanied by a rash like that of scarlet fever, or erysipelas: whereby the diagnosis, in so far as it depends upon the appearance of the skin, is rendered for a while uncertain. I have at present in the Middlesex Hospital a patient in whom the papulæ of small-pox were, at the outset, so intermingled with the appearances and sensations of urticaria, that I doubted, for twenty-four hours, what the true character of the eruption might be. It is sometimes like that of measles; but the similarity and the uncertainty are soon at an end, for the pimples soon begin to exhibit a fluid on their summits. They do not, however, as they advance, and pass into pustules, fill up so completely as in the distinct form; they are flatter, less plump, more irregularly depressed, and even of a different colour; being at first whitish, and then of a brown tint, and seldom of the yellow purulent hue which is seen in the variola discreta. Sometimes they are even bluish, or purple. In the confluent form there is commonly some abatement of the febrile distress upon the coming out of the eruption, but the remission is much less marked than in the discrete. About the fifth or sixth day fresh rigors are apt to occur, marking the fever of maturation. Most of these points of distinction between the two varieties of the disease are well marked in Cullen's definitions. The distinct form he defines thus: "Variola (discreta) pustulis paucis, discretis, circumscriptione circularibus, turgidis; febre, eruptione factâ, protinus cessante." And of the confluent kind his definition is "Variola (confluens) pustulis numerosis, confluentibus, circumscriptione irregularibus, flaccidis, parum elevatis; febre post eruptionem perstante."

But the most important difference between the two forms is in what is called the *secondary fever*, which sets in about the eleventh day of the disease, or the eighth of the eruption, just when the maturation of the pustules is complete, and they begin to desiccate. This secondary fever is slightly marked in the distinct small-pox, and very intense and perilous in most instances of the confluent. It is at this period of the disorder, that death, in the fatal cases, oftenest occurs. Of 168 such cases, recorded by Dr. Gregory, the deaths happened in twenty-seven (nearly one-sixth of the whole) upon the eighth day of the eruption. That, therefore, is the most perilous *day*, as the second is the most perilous *week*. Thirty-two died in the first week, ninety-nine in the second, twenty-one in the third. The early occurrence of death denotes a peculiar *malignancy* in the disease. The nervous system appears to be overwhelmed by the force of the poison. During the second week the disorder proves fatal chiefly in the way of apnœa; from some affection of the respiratory passages. After that period the characters of asthenia commonly predominate. The patient sinks under some casual complication, or the powers of life are

gradually worn out by so much irritation of the surface, and so large an amount of suppuration.

So much for the ordinary course of small-pox, and of the symptoms that are essential to that disease. There are, however, other concomitant circumstances, with which you ought to be acquainted; and these I will endeavour to specify at our next assembling.

LECTURE LXXXVII.

SMALL-POX, CONTINUED. INOCULATION. VACCINATION. THEIR COMPARATIVE ADVANTAGES. TREATMENT OF SMALL-POX.

IN the last lecture I brought before you, in a rapid sketch, the ordinary course, and the essential symptoms of small-pox; both in its distinct and in its confluent form. I have yet to mention some other circumstances that are very frequently to be noticed in connection with that disease.

Both kinds are accompanied by *sore throat;* the tonsils and fauces are tumid and red: and with this sore throat there is associated, about the period when the face swells, sometimes in the discrete variety, and almost always in the confluent, more or less *salivation*, which lasts for several days. At first the discharge is thin and plentiful; but, towards the period of maturation, it often becomes viscid and ropy, and is with difficulty got rid of by the patient. This salivation is of some importance as a *prognostic* symptom. If it ceases abruptly, and especially if at the same time the swelling of the face suddenly and prematurely subsides, the peril is great. Besides this, Sydenham regarded the ptyalism as a *diagnostic* circumstance; as a mark which identified with true small-pox a fever called by him the variolous fever, the *variolæ sine variolis* of De Haen and others. "The resemblance (says he) which this fever bore, in its symptoms, to small-pox, induced me to give it the title of variolous fever, which seemed indeed so much the more appropriate, as the fever raged at the same time with small-pox, and got well under the same treatment. The two diseases belonged evidently to one family, and there was no difference between them, saving that in small-pox the morbific matter was directed towards the skin, in the shape of an eruption; while in the variolous fever this matter was expelled from the system by the salivary glands." Notwithstanding this statement, it is difficult to believe that any such disorder as variolæ sine variolis ever proceeds from the contagion of small-pox.

This affection of the salivary glands does not so often occur in children; but diarrhœa appears sometimes to take its place.

The soreness of the fauces often depends, in great measure, upon pustules there situate. You may see that the tongue, the roof of the mouth, the inside of the cheeks, the uvula, and the velum palati, are thickly studded with them. It is affirmed by some writers that the pustules of small-pox occur in various internal parts of the body, and especially upon the mucous membrane of the intestinal canal. I believe this to be a mistake. The enlarged solitary follicles often put on very much the appearance of pustules. Cotunnius, who has written a good book *De sedibus variolarum*, asserts that pustules appear only upon the skin, and upon those parts of the mucous membranes which are freely exposed to the air. In one solitary instance he met with them in the trachea so low as its third ring. He fancied that previous desiccation of the part was necessary to their formation. He says that none appear on the cornea, while it is kept moist. He denies that they are seen in the interior of the body, or upon the fœtus in utero: but in this last point he is certainly wrong: and this is a strong objection to his theory. The most striking facts which he alleges in support of his opinion of the necessary presence of air—besides the fact of the eruption being more copious on parts usually exposed to the atmosphere, as the face and hands—are, that pustules do not form on the inner surface of the eyelids,

except in cases of *ectropium;* that they appear on hæmorrhoidal tumours only when these project beyond the margin of the anus; and that that portion alone of the glans penis is ever affected by them which is uncovered by the prepuce.

Dr. Gregory, however, states that true variolous pustules do not form upon the conjunctival membrane: and that the blindness of one or both eyes which is so common a result of small-pox, especially in children, is produced by an intense kind of ophthalmia, which sets in at the period of the secondary fever, and rapidly involves and spoils the transparent tissues of the organ.

During the period of maturation, a peculiar greasy, disagreeable odour, quite *sui generis*, proceeds from the body of the patient. If taken into the sick chamber blindfolded, one might name the disease at once by this smell. About the same time also many patients are tormented by itching of the surface; so that they are provoked to scratch off the heads of the pustules; and by so doing they often insure the formation of pits. In many cases of confluent small-pox this itching appears to constitute the chief part of the patient's suffering.

There are various troublesome complications to which, in severe cases of the confluent form, the patients are liable during the secondary fever; erysipelatous inflammation involving the subcutaneous cellular tissue in various parts of the body, and leading to the formation of abscesses; glandular swellings in the groins and axillæ, going on sometimes to suppuration; sloughing sores on the hips and sacrum; phlebitis; and in two or three instances I have seen the large joints, after death, full of matter. One of the most serious symptoms, at this period of the disease, is dyspnœa. The air-passages, and especially the larynx, become clogged by viscid mucus, the arterialization of the blood is interfered with, and the patient is in danger of suffocation. Occasionally life is suddenly extinguished by œdema of the glottis, supervening upon that affection of the fauces which I mentioned just now.

In one most fearful phase of this always formidable disorder, symptoms indicative of what is called the putrid diathesis manifest themselves—petechiæ, vibices, hæmorrhages from various parts of the body. The pustules, instead of being plump and yellow, are flat, red, purple, or blue; that is, they contain blood, or a sanious ichor, in the place of pus, constituting the *variolæ nigræ* of Sydenham. the *bloody* small-pox of Mead. I believe that these appearances augur in all cases a fatal result. Hæmorrhage from the uterus is not uncommon; and in pregnant women abortion, and then, most commonly, death. Heberden says that he examined, in many instances, the fœtuses so parted with, but never could perceive upon them any traces of small-pox. His experience, therefore, agreed with that of Cotunnius; and we may conclude that infection of the fœtus in utero is very rare. Yet, unquestionably, it sometimes happens; and the circumstances under which it has been noticed are various and interesting. In one instance, related by Mr. Flinders, the disorder was eight or ten days later in the fœtus than in the mother. A woman, near her full time, took small-pox. The pustles were mature about the 10th or 11th of June. On the 18th she gave birth to a full-grown boy, upon whose face and body there were many pustles, discrete, and nearly ripe. The child died the same night. It is a very curious fact that the fœtus has caught the disorder, doubtless through the medium of the mother, although she, having had it previously, was unaffected by the contagion. Dr. Mead relates that "a certain woman, who had formerly had the small-pox, and was now near her reckoning, attended her husband in this distemper. She went her full time, and was delivered of a dead child. It may be needless to add, that she did not catch it on this occasion; but the dead body of the infant was a horrid sight, being covered all over with pustules. In the first volume of the *Medico-Chirurgical Transactions*, Dr. Jenner gives an account of an infant which, upon the fifth day of its age, became indisposed, and on the seventh exhibited the eruption of small-pox; so that the contagion must have been communicated to it while yet in the womb. A few days before her confinement the mother of this child had seen, in the street, a person covered with small-pox pustules, the smell and sight of whose body had sensibly affected her. I see no reason, therefore, for doubting that the unborn being may pass safely through the disease while in the womb, and derive from that attack the customary immunity for the future. My namesake, Sir William Watson, describes, in the *Philosophical Transactions*, an instance in which the

scars left by the pustules were visible upon an infant at its birth. This child was afterwards inoculated without taking the disease. Its mother, who had formerly had it, nursed, when far advanced in pregnancy, a servant ill of small-pox. Dr. Pearson met with a similar example. Mary Spooner was inoculated by him in her sixth month of utero-gestation, and had the disease severely. Her child was twice inoculated with small-pox matter, but without effect.

Like all these contagious exanthemata, small-pox has its periods of dormancy and of activity. Every now and then, at irregular intervals—and, as it would seem to our ignorance of the cause, capriciously—it overspreads a district or country as an epidemic. At this moment (1838) it is more prevalent in London, and in many parts of England, than it has been known to be for many years past. When epidemic, it is also, in general, more than ordinarily severe; although different epidemics vary much in that respect.

There is no contagion so strong and sure as that of small-pox: none that operates at so great a distance. Dr. Haygarth states, "that during his long attention to this subject, not a single instance had occurred to prove that persons liable to the small-pox could associate in the same chamber with a patient in the distemper, without receiving the infection." It is readily communicable in every way; by inoculation, by breathing a contaminated atmosphere, by the contact or vicinity of fomites. Nay, it may be caught from the dead body. Mr. Cæsar Hawkins has recorded an interesting example of this. The body of a man, who died of small-pox, was brought into his dissecting-room in Windmill Street; and four students took the disease from that source. Of these, one only had *touched* the body.

There is one appearance which I think curious, although perhaps it has not any great practical interest; and which I omitted to notice in the last lecture, when describing the course of the eruption. Without going minutely into the anatomy of the pustules, you may distinctly see, if you closely examine them when they are about five or six days old —you may see, at least in many of them—two colours, viz. a central whitish disc of lymph, set in, or surrounded by, a circle of yellower puriform matter. In truth, there is, in the centre, a *vesicle*, which is distinct from the pus. You may puncture the vesicle, and empty it of its contents, without letting out any of the pus; or you may puncture the part containing the pus, and let *that* out, without evacuating the contents of the vesicle. The vesicles have even, by careful dissection, been taken out entire; and they are said to consist of several little cells. It is most probable that the lymph contained in this separate vesicle is the purest part of the variolous poison.

Before I say any thing of the measures to be adopted during the progress of small-pox, I have to bring under your notice two expedients of still greater interest and importance; the one of them contemplating a mitigation of the disorder, the other its total prevention. You anticipate that I am about to speak of *inoculated* small-pox in the first instance, and of the *vaccine disease* in the second.

Inoculation.—I have many times stated, and all the world knows, that small-pox may be imparted to a healthy person by inserting beneath his cuticle a minute quantity of the matter taken from a variolous pustule. This, perhaps, is not very surprising; but it is surprising that the disease, so received, should be much milder than if it had been contracted in what is called "the natural way," by breathing an atmosphere charged with the contagious poison. Why it should be so it is difficult to conjecture. The fact is sometimes expressed by saying that the disease is milder when the virus is admitted through the cutaneous, than when through the mucous tissues. But I am not at all sure that the hypothesis involved in this proposition is true. No attempts have been made, that I know of, to introduce the poison artificially through a wound in any mucous surface. I should rather guess that the small quantity of the poison conveyed by inoculation into the blood may make the difference. But whatever the explanation, the fact is unquestionable, and obviously of the highest importance. By what accident it was first learned (for it evidently could not have been reasoned out) we do not know. The Chinese claim to have been in the habit, for many centuries, of *sowing* the disorder, by putting some of the crusts into the nostrils. But this is a different thing from inoculation, the surface being entire, and the effluvia from the crusts being drawn into the lungs by the act of inspiration. It is said that a true engrafting of the virus has been in use by the Brahmins in India, time out

of mind. It certainly was practised in Turkey at the very beginning of the last century, and perhaps somewhat earlier. In 1713, Dr. Emanuel Timoni, an Oxford graduate, who had settled at Constantinople, wrote to Dr. Woodward, in London, giving him an account of the new process, and testifying to its success. This account was communicated to the Royal Society, and published in its Transactions the following year. In 1715, Mr. Kennedy, an English surgeon who had travelled in Turkey, gave similar information to the English public in his *Essay on External Remedies*. And in the *Philosophical Transactions* for 1716 you may see a notice of the same process, as described by M. Pylarini, the Venetian consul at Smyrna. But these statements were neglected, or had no practical result. We owe the actual introduction of the practice of inoculation into Great Britain to the good sense and courage of an English lady, whose lively epistles have taken their permanent place in our country's literature. Lady Mary Wortley Montagu, the wife of our ambassador at the Ottoman Court, writes thus, from Adrianople, in the year 1718: "The small-pox, so fatal and so general amongst us, is here entirely harmless by the invention of *engrafting*, which is the term they give it. Every year thousands undergo the operation; and the French ambassador says, pleasantly, that they take the small-pox here by way of diversion, as they take the waters in other countries. There is no example of any one who has died in it; and you may believe I am well satisfied of the safety of this experiment, since I intend to try it on my dear little son. I am patriot enough to take pains to bring this useful invention into fashion in England." In fact, she recommended it by her own example. The first person inoculated with the small-pox in England was her daughter. Then a child of a physician, Dr. Keith, who had visited Miss Wortley; afterwards some condemned felons, who were pardoned on condition of their submitting to the experiment; and at length, some of the royal family. But the practice was not thoroughly established, nor properly appreciated, by the English public, until the middle of that century.

Its efficacy in mitigating the severity and danger of the disease, in saving life and preventing deformity, was signally great. The mortality in the natural small-pox was estimated at one in five. But Baron Dimsdale, a great inoculator, declared that not one in fifteen hundred died of the engrafted disease. Two brothers, named Sutton, who had introduced, or rather revived, a very improved method of treating the disorder, professed to have inoculated 20,000 persons, without fairly losing one. But these, doubtless, were exaggerated statements. Among 5964 individuals, inoculated at the Small-pox Hospital in 1797, 1798, and 1799, there were nine deaths; *i. e.*, one in six hundred and sixty-two. We may take Dr. Gregory's estimate of one death in five hundred cases, as being probably within the mark.

In the inoculated disease the period of incubation is comparatively short; the pustules are seldom numerous, and still more seldom confluent; and the secondary fever is generally slight or awanting.

I may mention here, also, that the eruption is not unfrequently preceded by a rash, something like that of scarlet fever, and called by Willan the *roseola variolosa*. It fades in the course of a day or two, and then the small-pox pustules are seen emerging just in the same state that they would have been in, at the same period, if no such rash had appeared. The efflorescence happens oftener in the inoculated than in the casual disease. In the former it is looked upon as rather a favourable sign; in the latter, especially if the rash be of a dark red colour, it is considered unfavourable, and as the herald of a severe confluent disorder.

Vaccination.—A far superior expedient has since been discovered, in the practice of *vaccination*, which has rendered the inoculation of small-pox not merely unnecessary, but, in most cases, perfectly unjustifiable. Yet circumstances do sometimes arise, even now, in which it may be allowable and right to engraft the matter of small-pox; as when an unprotected person is unavoidably exposed, or has recently been exposed, to the contagion of that disease, and there is no vaccine matter at hand. The advantage of inoculating in such a case is, that the inoculated or milder form gets the start of the natural and severer; the fever commencing sooner than it would otherwise do. To show you the value of the practice in such cases, and the degree of protection which it affords to individuals whom we cannot vaccinate, I may mention a fact which Professor Gregory, of Edinburgh, was

in the habit of relating, and which was told him by a naval surgeon. The small-pox was introduced among the crew of a man-of-war, in a tropical climate, where no vaccine matter was to be procured. The men were almost all unprotected. Sixteen of them took the disease in the natural way; and of these, nine, or more than one-half, died. Of 363 who were inoculated, under the disadvantages of a hot climate, and no preparation, not one perished.

That a disorder communicated to the human animal by one of the brutes should protect the former against the contagion of small-pox, is one of the most interesting facts in the whole history of medicine. How glimpses of a truth so remarkable were first revealed to the casual observation of certain peasants, and how the result of this chance observation was gradually "matured into a rational and scientific form by a mind deeply imbued with the best principles of sound philosophy," I have not leisure to tell you in detail. And it is the less necessary that I should do so, as you may find the whole subject thoroughly narrated and discussed by Dr. Baron, in his interesting biography of Edward Jenner.

Dr. Jenner found among the great dairy farms in Gloucestershire a popular belief that no person who had had the *cow-pox* (an eruptive vesicular complaint communicated from the udder of the cow to the hands of the milkers) could "take the small-pox." Satisfied, by inoculating with small-pox matter several individuals who had had the vaccine eruption, that this was not an unfounded notion, he at length conceived the great and happy idea of propagating the cow-pox from one human being to another, and so preventing, in all cases, the perilous distemper of small-pox, which he hoped might thus be finally expelled from the earth.

By degrees, Dr. Jenner ascertained that some persons, who had had sore hands from milking, were not thereby rendered proof against the contagion of small-pox; but this difficulty was soon cleared up by the discovery that the teats of cows were liable to different kinds of eruption, and he learned, by close observation, which of these was the peculiar eruption that produced in the human frame the protecting disorder.

Dr. Jenner set himself to trace, if possible, the origin of the disease of the cow. First, he found that it was peculiar to certain dairies; then, that in those dairies *men* were employed in milking. Following up this clue, he further found out that those men had also the charge of the farm-horses. Next, he learned that the teats of the cows generally began to exhibit the specific eruption at that time of the year when a complaint called "the grease" chiefly prevailed among the horses. Hence he concluded, that the malady was conveyed to the cows by the hands of the men who had been dressing the heels of horses affected with the grease. Subsequent inquiries have, however, shown that this conclusion was not strictly correct.

Another difficulty which lay in Dr. Jenner's way, and which his patience and sagacity surmounted, was this. He found that some who were casually infected from the true complaint in the cow were not protected. This depended, as he afterwards ascertained, upon the period of the disease in the cow, at which the virus was communicated to the milker. The thick matter proceeding from the vesicle late in its progress produced indeed a severer local sore than the thinner matter of its earlier state, but it did not confer the desired protection. The same thing is observed in respect to small-pox. If the matter used for inoculating be taken from a fully matured pustule, it does not so surely excite the disease as when taken from a more crude one.

The next important step in this most interesting investigation was to determine whether the vaccine disease could be transmitted, by engrafting, from one human being to another, and whether, if so transmitted, it retained its protecting power. The 14th of May, 1796, was the birthday of vaccination. On that day, "matter was taken from the hand of Sarah Nelmes, who had been infected by her master's cows, and inserted by two superficial incisions into the arms of James Phipps, a healthy boy of about eight years old. He went through the disease apparently in a regular and satisfactory manner; but the most agitating part of the trial still remained to be performed. It was needful to ascertain whether he was secure from the contagion of small-pox. This point, so full of anxiety to Dr. Jenner, was fairly put to issue on the 1st of the following July. Variolous matter, immediately taken from a pustule, was carefully inserted by several incisions—but no disease followed."

It is scarcely necessary for me to notice the objections which were made to the practice of vaccination. Some of them were merely foolish—as, that it was unnatural and impious to engraft the diseases of a brute upon a Christian. Others were untrue—as, that it introduced into the system new disorders, distinct from the cow-pox. It triumphed over all these cavils; and in six years from its first promulgation the discovery was known in every region of the world.

It was soon found, however, that some, who had apparently had the cow-pox by inoculation, were nevertheless not incapable of taking the small-pox; and that these failures were, many of them at least, attributable to the mistakes that were made in the time or manner of performing the operation. It became necessary, therefore, to ascertain precisely the conditions requisite for the production of the genuine disease. And these conditions have been successfully investigated by Dr. Jenner and by subsequent observers.

You will learn to recognize the true vaccine vesicle only by repeatedly examining it for yourselves. Yet a brief description of its characters and progressive changes may be useful to you.

On the second or third day after the insertion of the vaccine matter into the arm, the punctures look red and inflamed, and on the fourth or fifth day the vesicle becomes perceptible; a pearl-coloured elevation of the cuticle enclosing a minute quantity of a thin transparent liquid. It gradually increases in magnitude till the eighth day, when it should measure from a quarter to half an inch across. Like the pustule of small-pox, it is more prominent at its circumference than at its centre, and it consists of small cells, from ten to fourteen in number. By puncturing carefully one of these cells, a drop of the virus may be let out, the other cells remaining full. Up to the seventh, or eighth, or even to the beginning of the ninth day, the inflammation around the vesicle should extend to only a very small distance from it. After this, it spreads, and what is called the *areola* is formed; a circular red border, which continues to increase during the ninth and tenth days, and begins to fade on the eleventh, passing through shades of blue as it declines, and leaving a degree of hardness behind for two or three days more. By this time, a brown or mahogany-coloured crust has formed over the vesicle, of a nearly circular shape; this becomes gradually harder and darker, and finally detaches itself about the twentieth day. The cicatrix which it leaves should be somewhat less than half an inch broad, circular, slightly depressed, marked by radiating lines, and dotted with little pits which seem to correspond to the cells of the vesicle.

About the eighth day there is usually some slight febrile excitement manifested, which soon subsides. This is analogous to the secondary fever of small-pox: and it appears to furnish the condition of the desired protection.

Of course it is of much moment to determine whether the cow-pox has run its proper course or not; and it is not always easy to say how far the progress of the vesicle may deviate from that which has just been described, without failing of its protecting influence. A very ingenious *test* of this, free from all ambiguity, has been devised by Mr. Bryce. His plan is this. He vaccinates the other arm, or some other part of the body, four or five days after the first vaccination. If the constitution has been properly affected by the first operation, the inflammation of the second vesicle will proceed so much more rapidly than usual, that it will be at its height, and will decline and disappear, as early as that of the first: only the vesicle and its areola will be smaller. In fact, from the time of the formation of the areola, the second vesicle is an exact miniature of the first. If the system has not been duly influenced by the first vesicle, the second will run its own course, increasing up to its eighth day, and so on. Should this be the case, the second vesicle should be tested by a third.

We find the germ of this criterion in the early history of vaccination. Dr. Jenner vaccinated the children of his friend Mr. Hicks, the first *gentleman* who consented to adopt the practice. This Mr. Hicks became afterwards an expert vaccinator himself, and it was his custom, in a doubtful case, to perform a second vaccination a few days after the first; and he remarked that the second vesicle made "immense strides to overtake the first."

After some time it became apparent that Dr. Jenner's estimate of the protecting power of the vaccine disease had been set too high. He had hoped and believed, as others also had, that the cow-pox would in all cases prove a perfect and permanent protection against

the small-pox; but those hopes have been disappointed. Doubtless complete protection is the rule; but—how thoroughly and regularly soever the vaccine malady may have proceeded—it is too certain that very many exceptions to this rule have taken place, and are daily taking place around us.

And this fact, which has become too glaring to be denied or explained away, has depreciated the value of the process of vaccination, in the public esteem, far more than, if rightly considered, it should have done. For it is a remarkable and most important truth that the disease which, in some duly vaccinated persons, follows exposure to the contagion of small-pox, is much milder and shorter even than the inoculated, and *à fortiori* than the natural small-pox. The disorder thus occurring is, therefore, denominated the *varioloid* disease, or (more conveniently, in my opinion,) the *modified* small-pox.

The constitutional symptoms of this modified disease are, in general, at the outset, and for several days, much the same with those of the regular small-pox. The eruptive fever is of equal length and intensity. There is frequently much headache, and sickness, and sometimes even delirium. The eruption begins about the third day: it is often copious, and sometimes confluent; and in the confluent cases the eruptive fever does not entirely subside as soon as the crop of pimples has come out.

It is in its subsequent progress that the complaint is modified: in respect both to the appearances presented by the skin, and to the constitutional symptoms.

Three distinct kinds of eruption have been observed—

1. The eruption sometimes approaches in its character and course very nearly to that of the ordinary small-pox. The pustules fill up, have the central depression, and ultimately crust over, and the face swells. But this course is performed in a shorter time than that of the ordinary disease, and the pustules are usually smaller. This is the severest and the least common form of the modified small-pox.

2. Sometimes the papulæ show a little fluid on their tops only, but never fairly suppurate, nor break; but the vesicles dry up, and hard prominences, with livid bases and horny summits, remain.

3. There are other cases in which a great part of the eruption consists of red pimples, which soon become livid, but contain from first to last, no fluid whatever.

In the majority of instances of modified small-pox, all these forms of eruption coexist. Some of the papulæ go on to suppuration, others become crowned with a horny summit, and others never exhibit any fluid at all.

But the most important characteristic of the modified disease, is the total absence of secondary fever. The constitutional disturbance which, for the first week, may have been as severe as in the ordinary small-pox, generally subsides entirely when the eruption has reached its acme. The patient is convalescent just when, in the unchecked and regular form of the malady, his danger is beginning to be most urgent.

These two circumstances, then—the short duration of the eruption, and especially the absence of secondary fever—furnish the broad distinctions between the regular and the modified small-pox: and almost always, when vaccination has been thoroughly effected, and small-pox occurs afterwards, it occurs in this modified form; and the modified form of small-pox is very seldom indeed fatal, though some instances of death resulting from it have certainly happened.

Several questions of the greatest practical moment and interest here present themselves: but it is impossible that I should discuss them. I will *state* some of them, however, that you may bear them in mind in your future opportunities of observation; especially as they are yet, for the most part, undecided questions; and questions which can be answered only after repeated and careful observation.

The first is, whether the protecting influence of cow-pox upon the human frame diminishes by lapse of time, and at length wears out. There seems reason for suspecting that such may, sometimes, at least, be the case. Certainly in many, but not in all, of those who have gone through the vaccine disease, revaccination at a distant period reproduces, in a greater or less degree, its primary effects. A friend of mine, who was vaccinated in 1799, has a son nine or ten years old, who was vaccinated at the age of three weeks. Both of them have lately been revaccinated. The boy was somewhat affected by the renewal of the operation; the father not at all. It yet remains to be determined

whether all those who are susceptible of some impression from a second vaccination, are liable to be affected by the contagion of small-pox: and whether a repetition of the operation of engrafting the cow-pox renews, or adds to, their security against small-pox. At any rate, the practice of revaccination is a safe and advisable precaution. Dr. Gregory says of it, "we have sufficient facts before us to state with confidence that it need never to be recommended prior to the tenth year of life; and that the age best fitted for it is from the period of puberty to that of confirmed manhood."

But, secondly, is there any ground for supposing that the wished-for protection ever fails to be conferred, because the operation is performed too early? It has been suspected that it is less likely to produce the requisite, or an enduring, effect upon the constitution when it is done while the child is at the breast. But most children are vaccinated within that period. We know that this is a time when they are but little susceptible of contagious disorders in general. If this suspicion be well founded, Dr. Gregory's first proposition requires correction.

A third question is, how far the frequent failure, in late years, of complete protection can be ascribed to the circumstance that the vaccine virus has been repeatedly transmitted from one human being to another, and its supply thus kept up, without any fresh recurrence to the cow, the original source of the disorder. Dr. Jenner was, himself, not without apprehension that this might prove a cause of failure. But the analogy of other animal poisons supplies no warrant for such a belief. For one year I had a seat, as the Senior Censor of the College of Physicians, at the National Vaccine Board, and I then had opportunities of satisfying myself that lymph which had been transmitted without interruption from person to person ever since the time of Jenner, continued to generate as perfect a cow-pox vesicle as at first. If, as Dr. Heim asserts, there are no less than five kinds of spurious cow-pox, all communicable by inoculation from the teats of the animal to the human body, I cannot help thinking that recourse should not be had rashly or needlessly to lymph recently obtained from the cow.

In the fourth place there are yet moot points, respecting the number of vesicles, and the degree of constitutional disturbance, which are requisite to insure, and to prolong, the protective power of vaccination. The constitutional effect will bear some proportion to the number of vesicles; and of these, it would seem, there should be several; and one or two of them, at least, should be suffered to pursue their entire course untouched.

Comparative advantages of inoculation and vaccination.—With regard to a fifth question, the most important of all, we may speak very decidedly; and it is a question concerning which it is of the utmost consequence that medical men should form, and disseminate among the public, correct opinions: I allude to the comparative merits and advantages of *inoculation with small-pox* and *vaccination*.

The advantages of the practice of inoculation to the individual, supposing him doomed to have small-pox, were great and obvious; to the community at large they were very doubtful. It gave the undoomed individual, for certain, an ugly disease, which was comparatively free from danger, in exchange for the chances, on the one hand, of contracting a very hazardous form, and on the other, of escaping altogether from any form, of variola. We need not inquire which is the most eligible branch of this alternative; we know which was by most men actually chosen. But the practice of inoculation, by carrying the virus and the disease into every village throughout the length and breadth of the land, filled the country with contagion; insured the disease to all who were subjected to the operation; and diminished to all who were not, the chances of escaping it. No doubt the distemper was produced artificially in many more persons than would have caught it naturally, had inoculation never been thought of. So that while the relative mortality, the per-centage of deaths from small-pox, was lessened by this practice, the absolute mortality was fearfully increased. Such at least is the judgment expressed by most who have thought and written on the subject. Dr. Heberden compared the number of deaths ascribed in the London bills of mortality to small-pox during the first thirty years of the last century, with the number during the same period of years at the close of the century, and he found that they had increased from 7·4 per cent. to 9·5 per cent. To be sure, some allowance must be made for the increase in the whole population of London during that interval; but on the other hand we must take into account the deaths (not noted in those bills) which fol-

lowed the inoculation of small-pox in secluded villages, where but for that practice, the poison might seldom have been found. It is right, I say, that this matter should be steadily contemplated, in all its lights, and with all its shadows, in order that the inestimable blessing conferred upon mankind by the researches of Dr. Jenner may be fairly set forth, and adequately appreciated. The vaccine virus produces a slight disorder, which is attended with no risk, and which (unluckily I may say) is not communicable except by direct engrafting. It not only does not disseminate a dangerous and deadly poison, but if rightly used, it affords the means of eradicating from a well regulated community, or at least of confining within narrow limits, the most loathsome pestilence which the world has known. Where vaccination is, the contagion of small-pox need never come. In Denmark, as I told you, variola had at one time disappeared, before the defensive influence of compelled vaccination. Chance, and a careless security, engendered by the absence of the pest, have led to its reintroduction there. It is much to be regretted that the boasted liberty of this country renders it almost impossible to enforce by law a practice which would be so conducive to the public weal. Some good might be done by enacting that no person should be eligible to even any parochial office of trust or profit who could not produce a certificate that he had been duly vaccinated. And the benefits which this safeguard confers on the individual are scarcely inferior to those which it is calculated to bestow upon society. It unfortunately does not give complete protection against small-pox to all, but it gives complete protection to many. And you must recollect that small-pox itself is not a universal and absolute assurance against its own return. But the cow-pox relieves all from the necessity, imposed by inoculation, of coming within the sphere of the variolous contagion. It renders many, I repeat, impregnable to that poison, if they do chance to be within its range; and its advantage to the comparative few who suffer the double misfortune of being exposed to the contagion of small-pox, and of being affected by it, is this, that it gives safety, though not exemption; that it takes away the sting and peril of the variolous disease, by curtailing it of the secondary fever. At the very worst, it leaves the individual liable, by a twofold ill luck, to contract a form of small-pox very much less dangerous than that which he would voluntarily accept by submitting to the operation of inoculation.

It is difficult to adduce exact numerical statements in illustration of this reasoning; but I may quote two short series of facts as samples.

During an epidemic in Scotland, Dr. John Thomson saw from June, 1818, to December, 1819, 556 cases. Of these 205 had previously had neither small-pox nor cow-pox, and 50 of them died; nearly 1 in 4. Forty-one took the small-pox for the second time, and Dr. Thomson knew of 30 other such cases, making 71 in all, whereof 3 died; or 1 in 23. Three hundred and ten had been previously vaccinated, and among these there was but one death.

The population of Marseilles at the time of an epidemic there, in 1828, was estimated at 40,000; that is to say, of 30,000 vaccinated, 2000 variolated, and 8000 unprotected. Among the 30,000 vaccinated, about 2000 were attacked with small-pox, and 20 perished; 1, namely, in 100. Of the 8000 unprotected, 4000 were attacked; and 1000, or 1 in every 4, died. And out of the 200 variolated, 20 took the disease a second time, and 4 died; or 1 in 5.

There yet remains a highly interesting but a less practical question. Dr. Jenner, as I stated before, believed that he had traced the cow-pox to its origin in the heels of the horse afflicted with *the grease*. It has since been made out that the disease which, in the horse, corresponds with and produces the specific malady of the cow, is a vesicular eruption, having no necessary connection with the grease, but extending sometimes all over the animal's body. Now the question is, whether these two distempers, occurring in the cow and in the horse, are identical in their essence and nature with the small-pox of man. If so (as Dr. Jenner believed, and Dr. Baron strongly maintains), a part of the mystery attending the whole subject vanishes. The protection furnished by the cow-pox resolves itself into the more familiar law, that certain diseases engendered by animal poisons, happen to the same individual but once, and shield the body against their own recurrence. In conformity with this theory, Dr. Baron names the disorders respectively, variolæ, variolæ vaccinæ, and variolæ equinæ.

The notion, you see, is this, that the vaccine disease is in truth small-pox, rendered mild by passing through the system of the cow. The great object of inoculating the small-pox is to produce a benignant form of that disease, by diminishing the number of pustules. The cow-pox diminishes the number to one; and while it reduces the severity of the disorder to a *minimum*, it absolutely takes away its power of propagating itself, except by a direct engrafting of the visible virus. The disease is not sufficiently intense to taint the air with poisonous effluvia. At the same time it affords (but less surely and less permanently) the customary protection. Such is the theory, which is intelligible and plausible, and supported by strong facts and persuasive reasoning; for all which I must needs refer you to Dr. Baron's book.

To avoid breaking the thread which connects the different parts of the main subject, I have postponed to the last what I have to say respecting the *treatment* of small-pox.

Treatment.—This, for a long time, was conducted upon an erroneous principle, and eminently disastrous. The older physicians attempted to force out, through the skin, the morbid matter existing in the blood. The eruption they considered to be the natural and only cure: and adopting the vulgar maxim, that "it was better out than in," they did all they could to promote a copious eruption, by a hot regimen, by covering the patient with bed-clothes, by keeping the doors and windows jealously closed, and excluding every breath of fresh air, and sometimes by administering wine and cordials. The celebrated John of Gaddesden, the author of that curious book the *Rosa Anglica*, improved even upon this. He surrounded the half-suffocated patient with red curtains, red walls, red furniture of all kinds: every thing he saw was to be red: for in that colour there was, he pretended, a peculiar virtue. This John of Gaddesden, by the way, was a very sad knave, and the first Englishman, I believe, who had the luck to be made Court physician. He had one medicine so good as to be fit for the rich only; and he recommended a double dose for the wealthy. "Duplum sit, si pro divite." He flourished in the fourteenth century.

Sydenham was the first, in this country, to employ the opposite or cool regimen in small-pox; and although his prejudiced contemporaries refused to follow his example, and adopt his practice, he confidently predicted its final triumph—"obtinebit demum me vitâ functo."

But it was subsequently to the introduction of the method of inoculation that the cooling treatment was fairly established, by the Suttons—two brothers, one of whom, Robert, lived at Bury St. Edmunds; the other, Daniel, at Ingatestone, in Essex. These men, wiser in their generation than the regular physicians, had the good sense to pursue the same plan of general management which had been so prosperous in the East, whence the practice of engrafting was originally imported. Daniel, in particular, became famous for his successful inoculations: and the great secret of his success seems to have consisted in his making one puncture only; exposing his patients much and often to a cool atmosphere; supplying them freely with refrigerant drinks; and restricting them to a spare diet. Under this course, Cullen, who adopted it from the Suttons, declares that ninety-nine times in the hundred, inoculation imparts a distinct small-pox, and very generally of the mildest form.

Now the same principle applies to the casual disease when we have reason to suspect that it is impending, or have the opportunity of treating it at its commencement. The object is to prevent, if possible, a copious eruption; upon which, as we have seen, the severity and peril of the disorder entirely depend. It has been thought that venesection, by its antiphlogistic power, and, perhaps, by letting out, with the blood, some portion of the regenerated virus, might lessen the number of the forthcoming pustules. But you cannot insure this effect by blood-letting: and you must bear in mind that, should the eruption prove confluent, suppuration, to a large amount, is inevitable, and—like that of an extensive burn—will require, in order to go on favourably, a certain degree of constitutional vigour.

You may abate the force of the eruptive fever, and keep down, it is believed, the number of pustules, by saline purgatives, so exhibited as to produce two or three loose stools every day, and by free ventilation of the surface of the body. The skin may even be sponged with tepid water, if its temperature be very high.

When the eruption is all come out, if the pimples on the face be very few and distinct, the danger is over, and there is no more to be done. At this period Cullen dissuades the further use of purgatives, as being sometimes hurtful.

But if the pimples on the face be many, and confluent, the patient will still require a great deal of attention. Our business is to look out for, and to meet, untoward symptoms.

About the eighth or ninth day, wakefulness, and restlessness, and sometimes tremors, are apt to come on; and the proper remedies for this set of symptoms, in small pox as well as in continued fever, are opiates. In variola, when given in full doses at bed-time, their good effects are often very conspicuous the next day.

If the maturation of the pustules should proceed tardily, if they should not fill up properly nor their contents become purulent, then strong broths may be of use, or even wine. But the effects of these must be carefully watched, and their amount adjusted to the necessities of the case.

When the pustules are livid, and intermixed with petechiæ, and typhoid symptoms occur, the disorder generally proves fatal. In such cases it is customary to prescribe bark and acids, in addition to the wine and opiates.

The proper plan of managing the patient during the continuance of the secondary fever, is to keep his bowels moderately open by gentle laxatives, or by enemata; and to give opiates once or twice a day. These are the more necessary on account of the irritation of the skin. The cooling regimen must now be given up; and the strength must be supported by a nourishing diet. Wine and cordials are indicated if the pulse be feeble; but the swelling of the hands and wrists often makes it difficult to feel the pulse.

Various external applications have been tried, with the view of relieving the intolerable itching; which often induces the patients to scratch and tear their faces, and to insure the formation of scars. Cold cream is used for this purpose: or a solution of common salt, applied lukewarm; or a liniment composed of equal parts of olive oil and lime-water. This may be smeared, from time to time, over the itching surface, by means of a soft camel's hair pencil.

The dyspnœa which sometimes comes on late in the disease, is a very ugly symptom. I know of nothing that can be done for it beyond blistering the throat and chest.

LECTURE LXXXVIII.

Chicken-Pox. Measles. Scarlet Fever.

I must not omit a short notice of the disorder called *chicken-pox;* for although a very unimportant complaint, it has given rise to many disputes. Other names which it has borne are *varicella, crystalli, variolæ pusillæ.*

Connected with the small-pox, and arising from the same contagion, there are several forms of eruptive disease. I mentioned the chief of them in the last lecture, as varieties of *modified* small-pox. Now these mild and irregular forms of variola, both parents and medical men, wishing, I suppose, to believe nothing in disparagement of the protecting power of vaccination, are very apt to consider, and to call, chicken-pox: and this error having been discovered, some persons have rushed to, or rather revived, the opposite opinion—equally erroneous in my judgment—that there is no such substantial disorder as chicken-pox; but that all the eruptions which have passed under that name have really been forms of modified small-pox. Dr. John Thomson, of Edinburgh, is one of the stoutest maintainers of this doctrine. No doubt an eruption of short duration, and vesicular through the greater part of its progress, is often caused, especially in persons who have been vaccinated, by the contagion of small-pox: but a similar eruption proceeds also from another distinct contagion, that, namely, of chicken-pox.

The best description of the true chicken-pox that I am acquainted with has been given

by Dr. Gregory. The disorder is almost peculiar to infants, and children of tender years. Willan has, however, described one unambiguous example of it, in a gentleman thirty years old; and another genuine instance was seen by Dr. Gregory, at the Small-pox Hospital, in the person of an adult female. The eruption is preceded by little or no premonitory fever, commencing usually on the shoulders, neck, and breast, affecting almost always the scalp, but sparing very much the face—which, in small-pox, never escapes.

The eruption is composed, from the very first, of perfectly transparent vesicles, surrounded by a very slight degree of superficial redness. They are usually numerous, but distinct. Dr. Gregory says that when the eruption is very copious, the body has the appearance of having been exposed to a momentary shower of boiling water, each drop of which had occasioned a minute blister. Crops of vesicles appear in succession for two or three days; and while new ones are forming, the first are beginning to shrivel. The vesicles that remain after the second or third day become slightly opaque, and like pearls. When irritated by friction, they sometimes take on so much inflammation as to be converted into pustules. The scabs are small and gummy, dry quickly, and crumble off, instead of being detached in one mass. In a few instances, shallow cicatrices are left by the vesicles. During the short progress of this eruptive disease there is no constitutional disturbance of any consequence.

It has been ascertained of this genuine chicken-pox, or varicella lymphatica, that it occurs once only to the same person; that it spreads by contagion; that, nevertheless, it is not communicable by inoculation—whereas the matter of modified small-pox, when engrafted, produces genuine variola; that it occurs equally among those who have, and those who have not, been vaccinated; that its course is affected by antecedent vaccination; and that the vaccine vesicle and disease proceed with perfect regularity after the occurrence of chicken-pox. Now this never happens after small-pox.

It appears from Möhl's work *De Varioloidibus et Varicellis*, that from the year 1809 to 1823, chicken-pox was annually observed at Copenhagen without concomitant small-pox; and that both diseases have since prevailed at intervals epidemically, but always under circumstances which satisfied the physicians of the town that their sources were distinct.

It must therefore, I think, be admitted, that there is a separate disease, called chicken-pox, which springs from a specific poison; produces a vesicular eruption; runs a definite course; has no tendency, when undisturbed, to suppuration; occurs but once; and affords no protection against small-pox, while, on the other hand, small-pox affords no protection against it.

The main point of practical importance is, however, this; that it if we meet with any eruption which is at all equivocal, we should use the same precautionary measures for preventing the extension of the disease as if we were sure that it was modified small-pox. But this salutary rule is often, I say, neglected or infringed, to the danger and detriment of those unprotected persons who happen to be in the vicinity of the sick child.

The *treatment* required in chicken-pox is abundantly simple; it is the same, in fact, which has been already recommended for the mildest cases of the discrete small-pox.

Measles.—Another of these blood diseases is *the measles;* called, also, by nosologists, *rubeola*, and *morbilli.*

Like different human faces, all the complaints belonging to this group have the same set of features, and therefore a mutual resemblance, while the separate lineaments differ so much in their character and relative circumstances, as to give to each disease its distinctive aspect. There are also minor shades of difference between individual cases of the same specific malady.

Measles, accordingly, has its introductory fever, its period of eruption, its peculiar kind of eruption, its course by stages. It is communicable from person to person, and it generally occurs but once to the same person. On some of these points I spoke before.

The introductory fever is sometimes severe, and nearer in its type to synocha than to typhus. Like all fevers, it begins with lassitude, and shivering which is soon followed by heat of skin, acceleration of the pulse, anorexia, and thirst. But the peculiarity in the fever which precedes the eruption of measles is, that it is very constantly attended with an inflammatory condition of the mucous membranes; especially of those which are proper

to the air-passages. The eyes become vascular and watery, the eyelids heavy, turgid, and red. The membrane which lines the nasal cavities, the fauces, the larynx, trachea, and bronchial tubes, is affected. Hence we have, generally, as symptoms, much sneezing, as well as lachrymation, a copious defluxion from the nostrils, soreness of the throat, and an obvious redness of the fauces, and most commonly a dry, hoarse, peculiar cough. In short, the symptoms which usher in an attack of measles are the symptoms of coryza and catarrh. In some instances there is diarrhœa also, indicating a simultaneous affection of the mucous membrane of the intestines; and not unfrequently vomiting: but the vomiting, as in small-pox, ceases upon the coming out of the eruption.

The regular period for the appearance of the eruption is the fourth day of the disease; seldom earlier, frequently later: sometimes as late as the eighth or tenth day from the commencement of the catarrh. The eruption itself is a rash, consisting, at first, of minute papulæ, which, as they multiply, coalesce into blotches that have, more or less, a horse-shoe or crescentic shape, and leave the intermediate portions of skin of their natural colour. It is two or three days in coming out, beginning on the face, neck, and arms, then reaching the trunk of the body, and so travelling down to the lower extremities. In this course it resembles the eruption of small-pox. It fades in the same order, standing out three days at least on the face before it begins to decline; so that its whole duration comprises a space of six or seven days. It becomes browner as it fades. You may feel that it is slightly elevated above the general surface of the skin, especially upon the face, which is somewhat bloated and swollen. The parts which the rash has recently occupied are left covered with a dry, small scurf. The cuticle does not peel off in large flakes, as I shall have to tell you that it oftentimes does in scarlet fever, but a great part of it crumbles away in a fine branny powder. Occasionally, yet very seldom I believe, the rash is intermixed with a few small and short-lived vesicles.

This termination of the papulæ is very unlike what happens in variola: and connected with the eruption there are two other important particulars in which the measles differs essentially from the small-pox. In the first place, the fever does not cease, nor even abate, upon the emergence of the eruption; but sometimes increases in intensity. And in the second place, the disorder is not more severe, nor more dangerous, because the eruption is plentiful, or early. So far from it, indeed, that in some of the worst and most perilous cases the eruption is apt to be partial, and to appear late and irregularly.

The eruption is the *distinguishing* feature of measles, but the catarrhal affection is, in every way, the *most important*. Indeed the rash may, and sometimes does, happen without the fever and the catarrh; and nosologists recognize a variety of the disorder under the title of *rubeola sine catarrho*. But it is observed of this variety, that it confers no protection whatever against the recurrence of the malady: in truth, it is most commonly succeeded in a few days by an attack of measles in its regular and complete form.

I need not stop to repeat what I told you in a former lecture about the other general features of this eruptive complaint. The period of incubation is from ten days to a fortnight. The contagion is active enough, though certainly it is less strong and diffusive than that of small-pox. When once introduced into a family or school, the disease rapidly spreads to those individuals who have not already had it. It is capable, though with much less readiness and certainty than small-pox, of being propagated by inoculation; but as the disorder is not rendered milder by being so introduced into the system, this process has no utility or interest, and is never resorted to. Occasionally rubeola visits the same individual twice; but this is the exception to the general rule. Perhaps, in some reputed instances of its recurrence, the first accession may have been without fever and catarrh, and therefore an ineffectual safeguard for the future. I myself know, however, two large families in which most of the children have suffered a repetition of the genuine unmitigated disease.

The measles resembles the other diseases of the group in this also, that at times it pervades a community as an epidemic; at times occurs here and there only, sporadically. The general character of the symptoms varies considerably in different epidemics. Morton and Sydenham, and after them Sir William Watson, have described visitations of what they call *putrid* measles. Sir William Watson was physician to the Foundling Hospital, and he witnessed two epidemics of this putrid kind among the children in that

institution. He states that the eruption appeared unusually early, so early as the second day of the disease; and that, besides cough and dyspnœa, the complaint was marked by extreme debility, and attended with dysenteric diarrhœa. More seemed to die of the intestinal affection than of the pectoral. He lost, in one of these epidemics, nineteen out of one hundred and eighty-three patients. The malignant character of the disorder was manifested by the frequent occurrence of gangrene, both externally and internally. In this more typhoid variety of measles, the rash is often irregularly and imperfectly developed, and of a livid colour.

Sydenham found that measles of an unusually bad kind prevailed in London, in the years 1670 and 1674; the very same years in which small-pox was also remarkably malignant and fatal. This illustrates what I have stated before; viz., that the typhoid tendencies of these and other febrile disorders depend less upon any peculiar virulence in their *exciting* causes, than upon some change previously effected in the human body by the silent and gradual influence of certain *predisposing* causes.

The diagnosis of measles is seldom difficult. In the outset of the fever you may guess what is coming by the coryza, catarrh, and hoarse cough; especially if the disease be about. On the very first day of the eruption, the small, red, and hitherto separate spots are very like the incipient pimples of small-pox. Do not, therefore, at this period, express too confidently your opinion respecting the nature of the complaint. Parents and nurses might be uncharitable enough to attribute your mistake to inexperience or ignorance. The progress of the disease will soon remove all doubt. The eruption of small-pox presently exhibits some fluid, while that of measles has none—unless, indeed (what is uncommon), a few miliary vesicles mix themselves with it. But these make no advance in twenty-four hours. Ordinarily the isolated pimples visible upon the first day soon augment in number, and collect themselves into semicircular groups; and if any question at all arises, it is whether the disease be measles or scarlet fever. I shall presently describe the latter disorder; and then I will point out the marks of distinction between the two.

The prognosis in measles is governed chiefly by the mildness or the severity of the pectoral symptoms. The most common cause of death, in the fatal cases, is inflammation of some one or more of the textures that compose the lungs. And even when this immediate danger has passed by, the disease too often leaves chronic pulmonary mischief behind it. In scrofulous children, and young persons, it frequently awakens the slumbering germs of consumption. And when that specific effect is not produced, it is apt, in adults, to inflict upon the constitution a blow which is never throughly recovered from; the patient becoming, from that time forwards, delicate and valetudinary. The prognosis is always unfavourable when the eruption does not stand out well, is of a livid colour, and accompanied with typhoid symptoms, or with a disposition to gangrene.

We augur favourably of the case when the thoracic symptoms are not severe; when the fever moderates upon the coming out of the rash; and when the rash is steadily persistent, and there is no excessive prostration of the strength.

Being contagious, and occurring for the most part but once, measles is principally seen in children; although no period of life is exempt from its attacks. In many children the disorder is so slight as to require little more than judicious domestic attentions. The free application of cold air to the surface, which is so beneficial in small-pox, would in measles be unsafe, on account of the pectoral symptoms. For this reason the patient should be kept in bed; with no more clothes, however, or warmth of the apartment, than he is accustomed to in health. The antiphlogistic regimen must be adopted; and if the bowels are not quite open naturally, gentle laxatives should be given. It may be well, also, to prescribe some diaphoretic medicine; a draught, for example, containing two or three drachms of the *liquor ammoniæ acetatis*, with half a drachm of the *spiritus ætheris nitrici*, and an ounce of camphor julep, to be taken three or four times in the twenty-four hours.

The most important part, however, of the treatment relates to the remedies to be employed for the pulmonary symptoms, which in the outset depend, almost always, upon bronchitis. But the inflammation is apt, in severe cases, to spread insidiously from the mucous to the other tissues—the bronchitis becomes pneumonia—and we find, after death, some portions of the lungs hepatized; usually small portions. For the most part, however,

it is extensive inflammation of the bronchial mucous membrane that we have to dread. And really I cannot give you any better or fuller directions in respect to the management of these inflammatory affections, than I endeavoured to lay down when I was speaking of *bronchitis* and *pneumonia*, as they occur idiopathically. You will judge of the extent and severity of the inflammation, partly by the common symptoms, partly by the help of your ear; and you must apportion your remedies to that intensity, so judged of. You will take blood from the arm, or from the chest, apply a blister, and give tartar emetic. And it is of importance that whatever kind or amount of depletion is adopted, should be resorted to *early*.

When the rash is about to decline, a spontaneous diarrhœa often sets in, and appears to have a beneficial effect in abating the febrile symptoms. If this natural curative process should fail to occur, it may be imitated by the exhibition of gentle aperients.

In weakly children blisters are apt to cause troublesome sores; and in some epidemics of measles, the sores thus produced show a disposition to become gangrenous. When any such tendency is noticed, blisters had better be avoided altogether. At other times, the inconvenience to be apprehended from a blister may be prevented by one of two plans; either by interposing a piece of silver paper between the blistering plaster and the skin; or by suffering the blister to remain upon the part three or four hours only, then taking it off, and applying a poultice. The cuticle will rise under the poultice, and the sore will not, in general, be a troublesome one.

If the eruption disappears prematurely, it may sometimes be restored by putting the patient into a warm bath. And if he is, at the same time, in a low state, especially if typhoid symptoms threaten or show themselves, you must treat the case upon that indication, just as you would in continued fever; giving wine and support, with great caution and watching of their effects.

It is of considerable importance to protect the patient from danger *after* the disease has subsided; by warm clothing, by preventing him from going out of doors too early, or being in any way exposed to cold. Pneumonic inflammation, and dysenteric purging, are frequent consequences of the want of prudence in this respect.

Scarlet fever.—I proceed, in the next place, to the consideration of *scarlet fever*.

This also is a contagious febrile disease, attended almost always, during a part of its course, by a rash, and by *sore throat*. It seldom comes on a second time.

There are some distinct varieties of this disorder, concerning which it is necessary that I should say a few words.

The two striking and important features of the disease are the *affection of the throat*, and the *affection of the skin*. They may both be well marked; or only one of them may be well marked: and this circumstance has led nosologists to divide one and the same complaint into two independent maladies; to which Cullen and others have assigned the respective names of *cynanche maligna*, and *scarlatina*. When, in an earlier part of the course, I was treating of the diseases of the throat, I purposely omitted the cynanche maligna; because that is only another name for a particular form of scarlet fever. If you look to Cullen's definitions of these complaints, you will see how very much alike they are. They both specify inflammation of the fauces, a cutaneous rash, and fever. But in the definition of scarlatina, the rash is dwelt upon and described, and the fever is called synocha; while in that of cynanche maligna, the ulceration of the throat is more insisted on, and the fever is said to be typhoid. The truth is, that these two kinds of disorder both spring from the same contagious poison. The malignant sore throat may be caught from a patient who has mild scarlet fever; and mild scarlet fever may, in like manner, be contracted from one who is labouring under the malignant sore throat. The two forms graduate insensibly, in different cases, towards each other; and it would be impossible, even if it were desirable, to draw any strict line of separation between them.

For convenience, however, of description, and for the better direction of the treatment, authors generally make three *varieties* of scarlatina. Scarlatina *simplex*, in which there is a florid rash, and little or no affection of the throat; scarlatina *anginosa*, in which both the skin and the throat are decidedly implicated; and scarlatina *maligna*, in which the

stress of the disease falls upon the throat. The epithet *maligna* marks truly the fearful character of this form of the malady.

I need scarcely remind you of a sort of mystification which prevails among the public about this complaint, and which many practitioners, for no good reason that I can see, seem disposed to encourage. Mistaking the Latin and scientific name of the disorder for a mere *diminutive*, you will hear mammas say, "Oh, my children have not got the *scarlet fever*, but only the *scarlatina*." I always disabuse them of this absurd error, when the opportunity of doing so occurs. It can produce nothing but confusion, and a disregard of requisite precautions.

Like measles, and for the same reasons, scarlet fever, though persons of all ages are susceptible of it, is eminently a disease of children; but it is much more to be dreaded than the measles.

It is somewhat strange that scarlet fever was not recognized, in this country at least, as a distinct disease, till about two centuries ago. In all probability it had long existed, and had been always confounded with measles. Morton speaks of it under the name of *morbilli confluentes;* and Hoffman calls it, by a similar mistake, *rubeola rossalia*. The febris scarlatina described by Sydenham must have been of a very mild kind; for he does not mention any ulceration of the throat. Dr. Fothergill, in 1748, was the first to describe, as a new and separate disorder, that perilous form of the complaint which Cullen designates cynanche maligna; and it was long called the Fothergill sore-throat. The identity of this affection with genuine scarlet fever has been slowly established by subsequent observers. The characteristic differences between scarlet fever and measles were first fully specified by Dr. Withering.

The disease begins, as the exanthemata in general begin, and as continued fever which I have grouped with them is apt to begin, with shivering; lassitude, and rapidly augmenting debility; headache, frequently severe, sometimes with delirium, occasionally with nausea and vomiting. Then, generally on the second day (and Cullen is wrong when he says it is generally on the fourth), the eruption begins to come out. In some of the worst forms of the disease it may, indeed, be deferred till the fourth day.

Although scarlet fever and measles were so long confounded together, the differences between them are well pronounced, and, when once pointed out, are easily enough recognized.

Rubeola is distinguishable, then, from scarlatina—

1. By the presence, at the outset, of catarrhal symptoms—by the sneezing, the cough, the defluxion from the eyes and nose, which precede the rash. There is, doubtless, in many cases of scarlatina, a running from the eyes and nose, but not till late in the disease; at any rate not prior to the eruption.

2. By the absence of severe inflammation and ulceration of the throat; symptoms which always accompany severe cases, at least, of scarlet fever.

3. By the characters of the eruption itself. The rash in measles is more elevated above the surface than in scarlatina, and of a darker colour. In measles it is said to present somewhat the tint of a raspberry, and in scarlet fever to be that of a boiled lobster. In measles the papulæ are collected into semi-lunar clusters, leaving interstices between them of healthy skin. The redness of scarlatina commences in minute points, which speedily become so numerous and crowded, that the surface appears universally red. They begin on the face, neck, and breast, and extend to the extremities, pervading at last every part of the skin. The scarlet colour is deeper, in general, about the groins, and in the flexures of the joints, than elsewhere. Lastly, the rash of measles, in its most regular form, appears on the fourth day of the disease; that of scarlet fever on the second.

On the arms and legs the eruption of scarlatina occasionally differs somewhat from that which is visible on the trunk; is more spotty, more papular, and the papulæ are somewhat prominent, while over the body there is a general diffused blush.

In some cases of scarlet fever (probably in some epidemics, for I observed the phenomena I am about to mention in four or five cases in succession which were brought into the Middlesex Hospital within the space of a month or six weeks), some parts of the red surface are closely studded with little transparent vesicles, containing a thin colourless liquid, and resembling what I described to you before as *sudamina*. In all the instances in

which I have seen them, these minute vesicles have been most thickly set on the thorax, and on the front and sides of the neck. The liquid is soon re-absorbed, and the cuticle under which it had been enclosed shrivels up, turns white, and comes off in a thick white scurf: so that the part from which it separates looks at first sight as if it had been powdered. I have recently seen two cases of this vesicular form of scarlatina in private practice. I show you Rayer's delineation of the vesicles.

The eruption, in the most regular and favourable cases, stands out for three or four days, and then begins to fade and decline, becoming by degrees indistinct, and disappearing altogether, in the majority of instances, before the end of the seventh day. About this time desquamation of the cuticle begins to take place, in smaller scurf or scales from the face and body, in large flakes frequently from the extremities. The scarf-skin of the hands and of the feet sometimes separates almost entire. A glove or a slipper of cuticle comes away at once. You may see such things in most museums.

In that variety of the disorder which we call scarlatina *maligna*, the rash is apt to come out late, and imperfectly, and sometimes not at all: and instead of being bright and florid, to present a bluish or livid tint. Sometimes it suddenly recedes; and then, perhaps, appears again: and occasionally it is diversified by purple spots.

Willan and Bateman have given the name of *roseola* to an eruption which is also attended with inflammation of the throat, and between which and scarlatina it is certainly difficult, if not impossible, at first to discriminate. The roseola, however, is not contagious, and has more of a chronic character than scarlatina. It comes and goes, and has no settled or definite course. Dr. A. T. Thomson lays down *this* distinction between them; but I do not know that we can trust to it:—"In scarlatina (he says) the rash first attacks the face, and then extends to the trunk of the body, passing off by the extremities; whereas in roseola the extremities are *first* affected."

The appearances of the *tongue* in scarlet fever are also peculiar and characteristic. In the scarlatina *simplex*, and *anginosa*, it is often covered, at the outset, with a thick, white, cream-like fur, through which are seen projecting the red and exaggerated papillæ; the edges of the tongue being likewise of a bright red colour. The red points gradually multiply, and the white fur clears away, and at length the whole surface of the tongue becomes preternaturally red, and clean, and raw-looking: and after becoming thus clean, as well as red and rough, and like a strawberry, it will sometimes, when the disease goes on unpromisingly, get dry, and hard, and brown—as you know it is apt to be in certain forms and stages of continued fever.

The first thing of which the feverish patient usually complains is sore throat, with some stiffness of the neck: and if you inspect the fauces, you will see, without in general so much swelling of the tonsils as occurs in common quinsy, a diffused redness, sometimes of a dark-claret colour, including a large part of the palate. In a short time you may perceive that the tonsils and velum are covered irregularly with whitish exudations, or gray aphthous crusts: or, perhaps, you see a sloughy kind of ulceration left by the separation of these crusts.

The progress of the distemper, and its degree of severity and of danger, differ very greatly in different cases. Sometimes the deviation from the feelings and condition of health is so very slight as scarcely to deserve the name of a disease; sometimes the disorder defies all treatment, and the deadliest forms of plague are not more fatal.

In these malignant and terrible cases, the eruption, if it appears at all, is livid and partial, and fades early, and is attended with a feeble pulse, a cold skin, and typhoid depression. Sometimes the patient sinks at once, and irretrievably, under the virulence of the poison, and life is extinguished in a few hours. A gentleman called one day at my house, and not finding me there, followed me between twelve and one o'clock to the hospital. He wished me to visit his wife, four or five miles out of town, who had been taken ill that morning. He feared that she was about to have scarlet fever, but he was not much alarmed for her safety; for when he found that I could not be at his house before six, he said that that hour would not suit the general practitioner in attendance upon her, and he begged me to fix some time for seeing her *the next day*. I did so; but the same afternoon rapid sinking came on, and the patient was dead very soon after the hour at which I had first proposed to visit her.

In other cases of scarlatina maligna, the typhoid symptoms rapidly deepen; and death, in children, is apt to occur on the fifth day of the complaint; and not uncommonly as soon as the third. The pulse becomes frequent and feeble; the tongue dry, brown, and tremulous; the debility extreme; the throat is ulcerated and gangrenous; and the respiration is impeded by viscid mucus which collects about the fauces. Over this variety of the disease, medicine has comparatively little control.

The chance of recovery is much greater in the scarlatina anginòsa, when the eruption is florid, and stands well out. But even in this form of the disorder there are many sources of danger, and various ways in which it may prove fatal.

In the first place many of the patients die, apparently from inflammation or effusion within the head. They have violent headache, with furious delirium, which is followed by coma, and death.

And, secondly, the state of the throat is full of peril. As the disease proceeds, although the rash may be steadily persistent, the throat becomes foul and sloughy, an acrid discharge from the nostrils, which are so stuffed and swollen internally that the patient can scarcely breathe through them, runs over and frets the upper lip; the parotid and submaxillary glands swell, sometimes enormously; and fever is lighted up afresh. In this way many cases prove fatal in the second week of the disorder. The cervical swellings cause constriction of the fauces and stiffness of the neck; and sometimes, doubtless by interfering with the free return of the blood from the head through the jugular veins, they produce a tendency to coma. With these symptoms there is often also purging, and an excoriated anus.

The acrid matters furnished by the ulcerating and gangrenous throat irritate the nasal membrane in the one direction, and that of the alimentary canal in the other. We thus account for the running from the nose, the soreness of the alæ nasi and upper lip, and the smarting diarrhœa: and the swelling of the parotids and neighbouring glands is evidently caused by absorption of the irritating and poisonous matter from the ulcerated throat. There is just the same relation and dependency between these different local alterations, as between the enlarged mesenteric glands, and ulceration of the follicles of Peyer in continued fever; between a bubo in the groin, and a chancre on the glans penis. It is the condition of the throat that gives rise, in these cases, to the most formidable symptoms. The system is reinoculated from that source. Whenever I see the glands much enlarged at the angle of the jaw, and beneath the jaw, in a child labouring under scarlet fever, I augur ill of the case. Sometimes the mischief extends into the larynx, and so destroys the patient. But this is probably a very rare event. There is, however, still another, and a very common consequence of the throat affection—I mean inflammation of the eustachian tube, reaching sometimes the tympanum itself, and causing permanent deafness, either by closing up the tube, or by the destruction of the membrana tympani, and of the little bones belonging to it. In one case, which was under my own care, I observed that, for a short time before death, every time the child swallowed, a part of the fluid food ran out immediately at one of its ears. I had no opportunity of examining the state of the part after death, but the disorganization arising from the sloughing ulceration of the throat must have been frightful.

Scarlet fever sometimes befals parturient women; and then it almost always proves fatal. I have seen two instances only of recovery from this perilous complication.

Scarlatina simplex is scarcely, I repeat, a disease. Sydenham has said of it that it is "fatal only through the officiousness of the Doctor."

Even when the patient has escaped from the complaint itself, he is often exposed to great hazard and distress from its *consequences*. Children who have suffered a severe attack of scarlet fever are liable to fall into a state of permanent bad health, and to become a prey to some of the many chronic forms of scrofula: boils, strumous ulcers, diseases of the scalp, sores behind the ears, scrofulous swellings of the cervical glands and of the upper lip, chronic inflammation of the eyes and eyelids. The same afflicting results are very common after small-pox also, and measles.

I have several times, when the rash of scarlet fever was disappearing, known pain and swelling of the larger joints to supervene, simulating very closely the local phenomena of subacute rheumatism: and I have noticed that the painful joints were eased and benefited

by friction; a circumstance which may help to distinguish this articular affection from true rheumatism. Another distinctive circumstance is that, although all these patients were children, the heart in no instance became implicated, in connection with the tumid joints.

But the most common, and the most serious of the sequelæ of scarlatina, is *anasarca*, serous infiltration of the subcutaneous cellular tissue, accompanied often with dropsy of the larger serous cavities. So common is this, that Cullen has even introduced the circumstance as a part of his definition of scarlet fever. He found the dropsy a very manageable complaint; but it really is, in many—nay, in most cases, if we look to its probable ultimate consequences—a most formidable one. This affection belongs to the class of *febrile dropsies*. It appears to have no relation, or, if any, an inverse relation, to the violence and danger of the preceding exanthem. It is much more common after a mild than after a severe disease. This, in all probability, is owing to the circumstance that less care and caution are observed in the milder cases during the dangerous period of desquamation and convalescence; a period more dangerous, in that variety of scarlatina, than any other. In the graver cases the convalescence is slower, and more doubtful; and accidental, or careless exposure to cold is more guarded against, or takes place later: whereas, in the slighter kinds of the disorder, the patients are apt to go out while the new cuticle is still forming. If you carefully trace the histories of dropsy succeeding to scarlet fever, you will almost always find that the fever had been trifling; that the patient, considering himself well or nearly so, had heedlessly encountered a cold or damp atmosphere so soon as he felt himself strong enough to leave the sick chamber. Plenciz, who has written well on this subject, and who was quite aware of its importance, remarks that those patients who have had much desquamation of the cuticle are the most liable to the dropsy; that it is more frequent in winter than in summer; and in such as are early exposed to the open air after having passed through the fever, than in those who remain longer at home. When the desquamation is over, and the new surface has become in some degree hardened, the peril is past. According to the observations of Dr. Wells, the dropsical symptoms commonly show themselves on the twenty-second or twenty-third day after the commencement of the preceding fever. They have been known to begin as early as the sixteenth, and as late as the twenty-fifth day. When no dropsy took place before the end of the fourth week, Dr. Wells always ventured to state that it was no longer to be dreaded.

This anasarca is seldom observed except in children and young persons. The age of the oldest patient that Dr. Wells had known to be so affected was seventeen. Of ten instances of the disease seen by Dr. Blackall, six occurred in children not exceeding the age of ten, and two others in persons who were respectively ten and sixteen years old.

We cannot infer, from this, that the susceptibility of this dropsical condition lessens as years increase. The great prevalence of this variety of dropsy in early life has no direct relation to age as a predisposing cause. The fact is explained by the accidental peculiarities of the antecedent disease. The contagion of scarlet fever is active and widely diffused. Few children escape its agency. Few are capable of taking the disorder a second time. It follows that scarlet fever is rare in adult life: and as dropsy succeeds that disease in a very limited number of instances only, dropsy arising in connection with scarlet fever must, at the adult age, be still more uncommon.

Yet it is not unknown. One of Dr. Blackall's ten patients was thirty, another forty-two years old. Both of these were women.

In this, as in other species of febrile dropsy, the urine is very constantly troubled, bloody, albuminous; and it is an interesting fact, that the chronic form of renal dropsy, manifesting itself at some distance of time, has been distinctly traced back to its source in the acute anasarca immediately consequeut upon scarlet fever. The sequence has occurred, in all probability, much oftener than it has been noticed. There is scarcely room for doubting that the series of organic changes in the kidney, described by Dr. Bright, do frequently date their origin from an attack of febrile anasarca: and in proportion as facts, accurately observed, accumulate on this subject, the chain of connection becomes more clearly visible between acute febrile dropsy, dropsy succeeding scarlet fever, and chronic renal dropsy. It is evident, indeed, that the two first of these three are, in their characters and exciting causes, identical, the only difference between them consisting in the remarka-

ble predisposition towards the second, impressed upon the body by the preceding exanthem. Both of them are, in many instances, initiative of the third.

It is natural therefore to expect that in the variety of febrile dropsy now under consideration, as well as in those which I formerly described, *inflammation* should be common, and evidenced by its unequivocal effects. And it is so. But the dropsy, I am persuaded, has no essential connection with common inflammation of any part, unless the state of the kidney be of that kind. I have examined the body very carefully in fatal cases, and found the serous cavities full of clear liquid, without a trace of redness or of any of the unmistakeable products, or events, of inflammatory action.

The earliest threatenings of this formidable complaint demand attention. It is usually preceded for a day or two, or longer, by languor and peevishness; frequently by nausea and vomiting, and a costive state of the bowels. The pulse, in the outset, has been found slow, and beating with irregular intervals; but it afterwards becomes frequent. The urine, at first, is scanty as well as altered in appearance. The face becomes pale, and chuffy. Sometimes, as the disease proceeds, violent headache, dilatation of the pupils, convulsions, or palsy, denote effusion within the head. Much more frequently the pleuræ are the seat of the internal dropsical accumulation, and dyspnœa is a prominent symptom. Ascites, to any considerable amount, is rare.

The contagion of scarlet fever is active, but uncertain. It is not so strong, nor so uniform in its action, as that of small-pox; but it seems to be peculiarly subtle and tenacious. Fomites infected with the variolous poison soon lose their power to excite the disease if they are freely exposed to fresh air. But the contagion of scarlet fever lurks about an apartment, or clings to furniture and clothes, for a very long time, even after some care has been taken to purify them. Of this I have known several remarkable examples. You will be asked at what period the danger of imparting the disease on the one hand, or of catching it on the other, is over; and I would recommend you to answer that you do not know. I am sure I do not: and therefore I always decline the responsibility of giving an oracular opinion on the matter.

I may arrange what I have to say of the *treatment* of scarlet fever, according to the three varieties of it already mentioned, the scarlatina *simplex*—*anginosa*—and *maligna*.

The first of these requires nothing more than confinement to the house; and the observance of the antiphlogistic regimen in regard to diet; and regulation of the bowels.

With respect to the management of the severer forms of scarlet fever, great differences of opinion have prevailed. I should recommend you to look into Dr. Williams's book on *Morbid Poisons*, for some interesting and satisfactory *information* on this head. Satisfactory to me at least it is, because the result of it goes to justify that kind of practice which I have always considered to be the safest and the best in this disorder.

In the scarlatina anginosa, the treatment I employ is very much the same as that which I consider proper for many cases of continued fever. If the heat of the surface be very great and distressing, I should certainly not recommend the cold *affusion*, but cold or tepid *sponging* will be very refreshing and beneficial. If delirium should come on, I would shave the scalp, and apply cold to it, and take away some blood by leeches: and the leeches I would apply to the *throat* rather than to the *temples;* for the tonsils, in this form of the disorder, are more swelled and inflamed, and probably a part of the head affection may arise from disturbance of the balance of the cerebral circulation, produced by the tumefaction around the great veins that return the blood from the head. By leeching the throat you relieve that part, and at the same time the *head also*. If the fever were extreme and the delirium violent, I would take blood cautiously from the arm, while the patient was sitting up, and carefully watch the effect.

When none of these untoward head symptoms declare themselves, all that we have to do is to keep the bowels open by moderate laxatives. The patient may take saline draughts, which are grateful and cooling. The citrate of ammonia thus administered is what I frequently prescribe: and if the pulse be without hardness and feeble, I order an excess of the carbonate of ammonia, so that four or five grains of it in each dose may remain unsaturated by the lemon-juice.

With respect, then, to this form of the complaint, the principles of treatment are, not to interfere unnecessarily; to take blood when certain symptoms require it, but to take no

more than seems likely to be sufficient for the purpose in view; to bear in mind that the system is labouring under a morbid poison, which we cannot eliminate from the blood, but the dangerous effects of which we are to watch and obviate.

In that worst form of scarlet fever, the scarlatina maligna, all our care will too often be in vain. There appear to me two main sources of danger. The one arises from the primary impression of the contagious poison upon the body, and particularly upon the nervous system, which is overwhelmed by its influence. The patients sink often at a very early period, with but little affection either of the throat or of the skin. If we can save such patients at all, it must be by the liberal administration of wine and bark, to sustain the flagging powers until the deadly agency of the poison in some measure passes by. But another source of danger arises from the gangrenous ulceration which is apt to ensue in the throat, when the patient is not killed by the first violence of the contagion. The system is *re*-inoculated, I believe, with the poisonous secretion from the throat. Now under these circumstances also, quina, or wine, and upon the whole I should give the preference to wine, are to be diligently, though watchfully given. And something may be done, by way of gargles, to correct the state of the throat, and to prevent the distressing and perilous consequences which would otherwise be likely to flow from it. A weak solution of the chloride of soda may be employed for this purpose: and if the disease occurs in a child that is not able to gargle, this solution may be injected into the nostrils, and against the fauces, by means of a syringe or elastic bottle. The effect of this application is sometimes most encouraging. A quantity of offensive sloughy matter is brought away; the acrid discharge is rendered harmless; the running from the nose, and diarrhœa, cease; and the disease is converted into a form which approximates to the scarlatina anginosa. This is a great improvement upon the old plan of ordering capsicum gargles.

Of late I have been in the habit of directing a solution of the chlorate of potass in water (a drachm to a pint) as a *drink* for patients in scarlet fever, and in the typhoid forms of continued fever. This practice was suggested to me by Dr. Hunt, who tells me he has long employed it with advantage. Under the use of a pint, or pint and a half, of this solution daily, I have remarked, in many instances, a speedy improvement of the tongue, which, from being furred, or brown and dry, has become cleaner, and moist.

From several distinct and highly respectable sources, *chlorine* itself has been strongly pressed upon my notice, as a most valuable remedy in the severest forms of scarlet fever. My informants have stated, that whereas they formerly dreaded to be summoned to cases of that disease, they now, having had experience of the virtues of chlorine, felt no misgivings in undertaking its treatment. Since these representations were made to me, I have not had opportunities enough of trying this drug to enable me to speak confidently of its sanative power; but I shall certainly employ it in future. I presume that its disinfecting properties may, in part, account for the good it does. It probably deprives the foul secretions of their noxious quality.

In the fourth volume of the *Medical Gazette*, Messrs. Taynton and Williams, of Bromley, write in high praise of this remedy. I will give you the formula for its preparation.

Two drachms of the chlorate of potass are to be dissolved in two ounces of hydrochloric acid, previously diluted with two ounces of distilled water. The solution must be put immediately into a stoppered bottle, and kept in a dark place.

Two drachms of this solution, mixed with a pint of distilled water, constitute the chlorine mixture; of which a tablespoonful, or two, according to the age of the patient, may be given for a dose, frequently.

We must not omit, in this, as well as in the other forms of the complaint, to pay attention to the state of the bowels, and by no means to allow them to remain costive.

I have seldom used blisters in this disease; but an experienced physician has lately told me that, when applied *early* to the neck and throat, they seem to render the affection of the fauces mild.

When the patient is at length convalescent, he will require careful watching till that period has gone by at which the dropsical symptoms are apt to appear. It is by neglect or imprudence that these symptoms are brought on. The patient should be sedulously protected from all exposure to cold, and wet, and fatigue; indeed he ought not to be permitted to go out of the house until the progress of desquamation is fairly over; and I

would not willingly let a patient go out till some little time *after* this. When dropsical symptoms *do* occur, if they are very slight, they may be removed in general by purgatives, and by digitalis. The *supertartrate of potass* is a good remedy too in such cases, and the use of the *warm bath*, which may be repeated every night.

But if there be any indication of *inflammatory* disease within, we must adopt more active measures. We have not, *now*, to contend with the depressing influence of a morbid poison, but we have to dread the consequences of acute inflammation; or of the sudden effusion of fluid, the mere presence and pressure of which may fatally oppress vital organs. We should have for our object to arrest the inflammation—or to promote the removal of the effused fluid—by blood-letting, and by the exhibition of purgative medicines, and of *mercury*. The worst case of this kind that I ever witnessed occurred in a boy of fifteen, the son of a tradesman in my neighbourhood. He had scarlet fever, *mildly*, and had got well, or nearly well of it, as he believed; and he went, one evening, into his father's stable, and staid there some time in the cold, during the period of desquamation. A day or two afterwards he began to have headache, and in a few hours more was seized with convulsions of one side of the body, coma, and at length hemiplegia; and his face and extremities became at the same time anasarcous. A large quantity of blood was taken from his arm, he was cupped on the temples, and took mercury, till in a short space of time he was profusely salivated. Under this treatment the coma and dropsy rapidly disappeared, and he presently recovered the use of his palsied limbs, and got quite well. I make no doubt that some effusion took place within the cranium, as well as into the subcutaneous cellular tissue. The plan of treatment followed in this case, modified according to particular circumstances, is that which I should again pursue, and therefore what I should recommend you to pursue, in similar emergencies.

You are probably aware that *belladonna* is believed by many to exert a preventive and protecting influence upon the body against the contagion of scarlet fever. Hahnemann, the author of the Homœopathic hypothesis (and thereby of much mischief to mankind), was the first to assert this. It is said that belladonna administered in small doses causes sometimes a rash resembling that of scarlatina. It certainly is apt to produce dryness and redness of the fauces. I know nothing, by my own experience, of the alleged conservative property of this vegetable, but in the small quantities recommended, there can be no harm in trying it, *provided that* its employment does not lead to a neglect of other precautions. Three grains of the extract of belladonna are dissolved in an ounce of distilled water; and three drops of the solution are given twice daily to a child under twelve months old, and one drop more for every year above that age. It is affirmed that if this remedy does not prevent the disease, it will render it mild; and that if it be taken four or five days before exposure to the contagion, the resulting scarlatina never proves fatal.

LECTURE LXXXIX.

The Plague. Erysipelas. Erythema nodosum. Urticaria. Prurigo. Scabies.

Of that group of *contagious* exanthemata of which I undertook to give you some general account, two only remain to be noticed; viz., the plague and erysipelas.

Concerning one of these, the plague—as I have never seen, and hope never to see it; and as, with Cullen, I "think it unfit for a person who has never seen the disease to attempt its particular history"—I shall not presume to offer you any observations in detail. It is a very malignant kind of contagious fever; prevailing, at certain times and places, epidemically; attended with a sort of eruption, namely with buboes and carbuncles; and not furnishing, apparently, any sure or permanent security against its future recurrence. In Dr. Forbes' *Select Medical Bibliography* you will find a long list of works on the plague. I would second Dr. Cullen's recommendation, that you should consult those

authors only who have themselves had personal experience of the disease. Among the publications that fall within this rule may be mentioned Dr. Russell's History of the Plague as he saw it in Aleppo: Sir James M'Grigor's Medical Sketches; Sir Arthur Brooke Faulkner's Account of the Plague which occurred at Malta in 1813; Desgenettes' Histoire Médicale de l'Armée d'Orient; and Assalini's description of the malady as he witnessed it when in attendance upon the French army in Egypt.

Erysipelas.—I proceed, therefore, to *erysipelas*. And I wish, in the first place, to fix and define that specific complaint of which *alone* I propose at present to speak. The term erysipelas has been employed by medical men in a very loose and vague manner. Any diffused redness and inflammation of the skin is apt to be set down as erysipelas; and hence we have disputes as to the distinction between *erysipelas* and *erythema*. But it would tend, in my humble opinion, to the formation of more settled opinions in respect to erysipelas, if the term were restricted to that disease in which the integuments of the *face* and *head* become diffusely inflamed. The phrase *erysipelatous inflammation* may properly enough be applied to other cases, similar to this, in so far as the condition of the *skin* is concerned; but in what I should consider true erysipelas, in the medical sense of the word, there are other characters belonging to the disorder quite as important as, and more distinctive than, the cutaneous affection. What is usually spoken of as erysipelas of the face and head, and what I would call simply erysipelas, falls naturally within that group of exanthematous diseases which includes small-pox, measles, scarlet fever, the plague, and continued fever. It is an idiopathic inflammatory disorder, running a *tolerably regular and definite course;* attended by inflammation of the integuments of the body, or in other words by an *eruption;* often *prevailing epidemically:* and capable of being *communicated*, under circumstances favourable to its propagation, from one person to another. Its power to protect the constitution from its own recurrence is less certain; but in truth, so many different affections have been lumped together under a common name, that the proper phenomena of true erysipelas have not been made sufficiently an object of separate study to enable us to speak with any confidence on this point. I recollect, however, a certain female who has been three or four times my patient in the hospital with erysipelas: and one of the night nurses there, whom I treated for that complaint some time ago, is now lying ill of the same disorder, under the care of one of my colleagues.

Erysipelas, in the sense now explained, called in Scotland the *rose*, and in this country *St. Anthony's fire*, resembles other disorders of the same group in these points also, that the fever *precedes* the local inflammation, that certain premonitory symptoms frequently go before the outbreak of the disease, and that *sore throat* is an early, and almost a constant, accompaniment of the complaint. The patient feels ill—shivery, feeble, languid, and often drowsy. The actual attack generally sets in with distinct rigors; and the pulse is often very frequent from the first, for many hours, perhaps, before the redness commences. Very commonly there is also manifest disturbance of the alimentary canal, marked by nausea and vomiting, and not unfrequently by diarrhœa. Then some part of the face, usually one side of the *nose*, or one *cheek*, or the rim of one of the *ears*, begins to feel hot, stiff, and tingling: and upon examining it you find it to be of a deep continuous red colour, and to be swelled and hard. The redness and swelling gradually, and sometimes rapidly, extend themselves: they are defind by a distinct elevated margin; which advances, and invades progressively the neighbouring healthy surface, until the whole of the face, or of the scalp, or of both, is occupied with the inflammation. The lips swell enormously, the checks enlarge, the eyes are sealed up by their œdematous and prominent lids, and all traces of the natural countenance are effaced. I know of no disease, except perhaps the confluent small-pox, which so completely and speedily deforms and disguises the visage of the patient. A stranger seeing a young female in the height of the disorder, and revisiting her after her recovery, is astonished at the change. It seems as if, by some magic process, such as we read of in our nursery tales, a hideous monster has been metamorphosed into a comely damsel.

The inflammation frequently spreads from the face and forehead, or ears, to the hairy scalp; and from the head it travels backwards, in some cases, to the neck and the shoul-

ders. Sometimes—and in this it exactly resembles a scald—the inflamed surface becomes covered with irregular bullæ, or blisters; but often there is *no* vesication. This circumstance, therefore, which has been mentioned by some as furnishing a point of distinction between erythema and erysipelas, fails of that purpose.

In many cases the inflammation is quite superficial: in others it dips, as it were, through the skin, and affects the subcutaneous cellular tissue; and then, and there, suppuration, and even sloughing of the tissue, are apt to take place. We find this to be the case often in the loose tissue of the eyelids; and it is more common on the scalp perhaps than on the face.

After the redness has lasted three or four days, it fades, the swollen surface subsides, and desquamation ensues; and as the inflammation creeps, perhaps, gradually from one part of the surface to another, you may find the face becoming pale, and covered with patches of dead cuticle, while the scalp, or the upper part of the neck, is becoming red. Sometimes those parts of the inflamed surface on which blisters had formed are covered with *crusts*, rather than with merely dead and dry cuticle. In almost all these cases of erysipelas of the head and face there will be found to be redness and soreness of the *throat* also; although this is not always inquired into, or complained of.

There is considerable variety in the intensity and complication of the symptoms. Sometimes the sufferer lies patiently, yet apparently conscious and rational, till the tumefaction diminishes, and he is again able to open his eyes. Generally there is some wandering of the mind, especially at night; and in bad cases there is much delirium, and at length complete coma, and the patient dies at the end of a few days. In some of these cases the inflammation has extended to the encephalon; in others it is probable that the functions of the brain are disturbed through the febrile derangement of the circulation. When death takes place, and the head is examined, serous fluid is usually discovered beneath the arachnoid, and in the cerebral ventricles; and the veins of the pia mater are urgid. I have stated before that I doubt whether such appearances are always to be attributed to inflammation. Sometimes there are no morbid appearances at all within the skull.

It is said that the erysipelas does, now and then, suddenly desert the surface; and that inflammation of some internal part, and particularly of the brain, is apt to follow such rapid subsidence of the external malady. I presume that this metastasis is rare. I do not recollect to have seen it. But the *extension* of the inflammation, the supervention of delirium and coma, while the external inflammation *continues*, is of common occurrence.

This, then, is one way in which erysipelas is accustomed to prove fatal; by effusion within the head, and *coma*.

And there is another mode in which death is not unfrequently brought about, and which has not been so much attended to; I mean by the affection of the throat. The patients die sometimes almost suddenly: unexpectedly; you cannot account for the unlooked-for dissolution. But if the throat be examined you may (sometimes at least) there discover the solution of the mystery. The sub-mucous tissue of the glottis and epiglottis is filled with serum, the chink of the larynx has been nearly or completely closed, and the patient has died of *apnœa*. This is just analogous to what takes place externally: the enormous swelling of the eyelids, and lips, and face, is owing, in a great degree, to serous fluid poured out into the sub-*cutaneous* cellular membrane.

Another way in which erysipelas may kill, is by gradual *asthenia*. Without any stupor or much wandering, without any marked affection of the breath, the pulse becomes weaker and weaker, the surface cold, and the heart at length ceases to pulsate. This mode of dying is less common in this disorder than the other two.

The causes of erysipelas are various, and often obscure. I have stated that it is communicable, by contagion, from person to person: yet this contagious property is so feebly marked, that it is denied by many. It is more active at certain times, at certain seasons, at certain places, than at others; which is the same thing as to say that there are *predisposing* causes of the disease; that there are influences which augment the susceptibility of the body to the agency of the poison.

I believe that on the continent they do not allow erysipelas to be contagious at all: but very satisfactory evidence of the fact has been collected by several of our own practitioners.

In the second volume of the *Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge*, Dr. Wells has brought together several examples in which the complaint appeared to be unequivocally propagated by contagion. "I visited (says he), on the 8th of August, 1796, in Vine Street, Clerkenwell, an elderly man, named Skelton, who had been attacked several days before with *erysipelas of the face*. In about a week afterwards he died. On the 19th of the following month, I saw a Mrs. Dyke, of about seventy years of age, the landlady of the house in which Skelton had been a lodger, and found *her* labouring under an erysipelas of her face. I inquired whether any other person in her house had been ill of the same disease since the death of Skelton, and was told that his wife had been seized with it a few days after his decease, and had died in about a week. During my attendance upon Mrs. Dyke, an old woman, her nurse, was attacked with the same disorder, and was sent to her parish workhouse, where *she* died. Mrs. Dyke has since informed me that a young man, a nephew of Skelton, was taken with the disease of which his uncle had died, shortly after visiting him, and survived the attack only a few days. That she herself had been several times with Skelton and his wife during their sickness, and after their death had removed some furniture from the room they had occupied to her own apartment." Dr. Wells relates other histories of the same kind, all occurring when there was no particular *epidemic* of erysipelas prevailing to account for them. Professor Arnott has given some other examples, still more striking, of the propagation of erysipelas from one person to another, not only under the same roof, and in the same locality, but also when the parties lived at a distance from each other, and the intercourse between them had been casual and temporary. These cases are stated, I think, in the fifty-seventh volume of the *London Medical and Physical Journal*. The following incident has been told me upon good authority. A man living somewhere in Westminster fell ill of idiopathic erysipelas. In that state, for some reason or other, he was removed thence; and his brother, who was a servant in or near Portland Place, received him clandestinely into his master's house, and allowed him (for two nights, I believe) to share his bed. That brother was soon attacked with erysipelas; and in the course of his illness was visited by his master. The master also was attacked; and it is worthy of remark, that in both master and servant, the disease showed itself just seven days after they had respectively come near another who was affected with it. Dr. Elliotson gives an account of having suffered the disease in his own person. It came on him in five days after the breath from one of his patients, over whom he was stooping to examine the skin, and who had erysipelas badly, and died of it, had come upon his face. "I turned away (he says) disgusted, and said, *I hope I have not caught it;* but five days afterwards, having forgotten the circumstance, I was seized with it." More than once I have had occasion to remark that successive tenants of the same bed in a hospital have been seized with erysipelas after their admission.

But allowing, as I think we cannot but allow, that contagion is one of the exciting causes of erysipelas, there are others which *more frequently* excite it. At least there are many instances of the disorder in which we can trace no exposure to contagion, and in which we can perceive some other probable reason for its occurrence. Sometimes, no doubt, it comes on without any obvious cause. The application of cold often gives rise to it. Irregularity of diet is said to do the same. Violent mental emotions are also accused of being occasionally its cause: it is said to have been brought on both by anger, which is an exciting, and by fear, which is a depressing, passion. Many cases, not to be distinguished in their appearances and effects from idiopathic erysipelas, result from local injury. I have already told you that there is no inconsistency or absurdity in supposing that a disorder which originates in some common cause, may be capable of spreading in the way of contagion. And the testimony of Mr. Lawrence (who is not prone to admit of contagion on light grounds) goes to the effect that such is the case with the disease in question. He mentions an instance in which erysipelas of the head and face, which commenced after the insertion of a seton in the neck, appeared to him to have affected two individuals by contagion.

I say that erysipelas further resembles the disorders of the group in which I have placed it, that it sometimes prevails *epidemically:* and on those occasions, like the rest of the group, its occurrence is promoted by all circumstances that tend to debilitate the body: by

intemperance; by previous disease; by low spirits and anxiety; by insufficient nourishment; and by foul air. It used to be much more common formerly in hospitals than it is at present; when less attention was paid to cleanliness and ventilation.

Erysipelas is another of the diseases concerning the *treatment* of which there has been, and there is, a most embarrassing difference of opinion. Some, prescribing according to a *name*, tell you that it is inflammation, and therefore that the remedies of inflammation, and especially blood-letting, are to be vigorously employed. Others declare that the safety of the patient lies in the early and liberal administration of wine and bark. You will not think me a trimmer, I hope, or one who is content with indecisive practice, when I state that neither of these plans, in my judgment, is universally either proper or safe; yet that each of them is the best, under certain circumstances.

I think, the more you see of this disease the more convinced you will be that it is not to be *cut short* by any particular mode of treatment: that it *will* run a certain course; and that it will *generally* terminate, sooner or later, by resolution, whether remedies be employed or not. It does not follow from this that remedies are of no use: but it does follow that we are to exhibit them, not with the view of *curing* the disorder, but with the view of *conducting* it safely to its termination.

If you look at the history of erysipelas, and of the notions which have *prevailed* respecting it, you will find that the opinions in favour of giving support, and of abstaining, as much as possible, from the abstraction of blood, greatly preponderate. In the outset of the complaint, if the pulse be *hard* as well as frequent, and there be much headache, and active delirium, you must take blood either from the arm, or from the neck by cupping, or by leeches from behind the ears. But (I speak of the disease as it occurs in *London*) the time for this kind of practice soon passes by: and when you do bleed from the arm, you must (in this town) bleed with caution, and in the upright posture; that the desired effect may be produced by the abstraction of the smallest quantity of blood.

In all cases it will be right to purge the patient briskly at the commencement. A full dose of neutral salts, or of rhubarb and magnesia, will answer better, I think, in these cases, than calomel and senna. If you see the patient very early, and if there be any nausea or oppression of the stomach, it will be proper to administer an emetic.

In most instances you will soon perceive evidence of great debility: a *feeble* as well as a *frequent* pulse; tremors; a dry and brown tongue often. And these symptoms increase, if you *then* persist in drawing blood. The carbonate of ammonia does great good sometimes in such a condition: and this I am very much in the habit of giving. The bad cases of erysipelas are apt to baffle us all. I am not aware that I lose more of them than my neighbours. Speaking generally, a large majority of my patients get well; but I do not bleed one patient in a hundred, from the arm.

I am bound, however, to set before you the kind of evidence which exists in favour of the bark; or rather of the sulphate of quina, which is what I mean when I speak of the bark.

Dr. Fordyce, Dr. Wells, Dr. Heberden—all men of great sagacity and experience—recommend the treatment by bark. Dr. Jackson, an American physician, advocates, I see, the same plan. He says, that after a purge, and if necessary, an emetic, the sulphate of quina should be given in as large doses as the patient will bear; that from twelve to twenty-five grains in the twenty-four hours will generally suffice; and that we may know when the dose is sufficient by a buzzing which comes on in the ears. Dr. Elliotson also —whom I here quote the more willingly because I think he is rather of an antiphlogistic turn than otherwise, in general—says, that he has never seen quina do harm, even in active tonic erysipelas; and that in doubtful cases, when you hesitate whether to bleed and put the antiphlogistic plan in force, or to stimulate and support, the quina is *always* a safe and *eligible* medicine. Dr. Williams, of St. Thomas's Hospital, thinks better still of *wine*, which he gives in *all* cases *from the very beginning*.

I have hitherto limited my observations to erysipelas of the face and head as it occurs in *London:* and I have done so because I really believe that in the purer air of the country, and in young, strong, vigorous country persons, bleeding may be proper and requisite. You will find country practitioners pretty well agreed on this point.

But taking the disease as I see it here, I should say that *many cases* do well with but

little care or interference from medicine; that *many also*, but a fewer number than the former, prove fatal under whatever plan of treatment may be adopted; and that many patients are to be saved, by judicious management, that would otherwise die.

The first requisite for rescuing these perilous, yet recoverable, cases, is that they should be perpetually watched and tended. The indications of treatment may alter from one hour to another, and it is only by great vigilance on the part of the medical attendant, and on the part of an intelligent and obedient nurse, that medicine obtains its full chance of bringing the patient through.

After clearing out the alimentary canal, then, I would not be active in either way, unless I saw some plain indication for activity. If the pulse become weaker, and I did not feel sure about the propriety of stimulating, I would give five or six grains of the carbonate of ammonia every four hours, and beef-tea. If the disease went on smoothly under that treatment, well and good. If the powers still continued to sink, I should have recourse to wine, or to both bark and wine; but of the two, I am more friendly to wine; and as the patients like it better, you may be more sure of their taking it. If, on the other hand, there was much headache, and the pulse was hard, and the febrile distress great, I should apply a few leeches, and prescribe the tartarized antimony in a saline draught. The bowels should not be suffered to become confined; but the mild aperients are better than the drastic in these cases.

Various external medicaments have been used and recommended for the inflamed part. Putting leeches upon it; puncturing it with needles, or lancets; covering, or surrounding it with a blister, or by the lunar caustic in substance or in strong solution; dusting the surface over with magnesia, or flour; smearing it with various unguents, and particularly with mercurial ointment; keeping it wet with some cold lotion; or fomenting it with hot flannels.

Now, of all these local appliances, that which, according to my own observation, is the most useful, and which affords the greatest comfort to the patient, is the last that I mentioned; fomentation by flannels wrung out of a hot decoction of poppyheads. But in order to give *this*, also, fair play, it should be *continual;* not used for half an hour, and then intermitted; but it should be one person's business to apply the fomentation assiduously, as long as it is soothing and grateful to the patient. In a few cases, and *but* a few, the warm applications have been disliked, and cold lotions preferred: and when such is the case, there is no risk incurred by using them, so far as I know; no hazard, I mean, such as you may read of, of the inflammation being repelled from the surface, and driven in upon some vital organ. The local treatment most in favour with our apothecary at the hospital is that of covering the inflamed face and head with flour, by means of a dredging box. The patients declare that the flour cools, soothes, and comforts them. Now this is a more convenient, and, in some respects, a more eligible, application than that of hot flannels. It is less likely to fail of its purpose through the negligence of the nurse.

So much for erysipelas, as it usually comes under the notice and the management of the physician. You are aware that an affection of the skin very similar to that which I have been describing, and called also by the same name of erysipelas, is very common in other parts of the body; on the extremities especially, and occasionally on the trunk: and it will travel sometimes from an extremity till it reaches the head. These varieties of cutaneous inflammation are, in most instances, the indirect consequences of some local injury: of punctured wounds; of the stings of insects, or the bites of venomous reptiles; of mere scratches sometimes. Or the cutaneous inflammation will spread from old sores; or supervene upon dropsical limbs. It is curious that these complaints also are much more apt to occur, and even to multiply by a sort of contagion, or in virtue of some epidemic atmospheric influence—at certain times and places than at others. You will find there are periods when the surgeons of hospitals dread to perform any operation, lest it should be followed by this spreading inflammation of the skin. There are many points of great interest connected with these diversified forms of what is called erysipelas: they are more liable to be attended with gangrene than erysipelas of the face and scalp: on the other hand, they are more liable also to be complicated with inflammation of the subcutaneous cellular tissue, and with suppuration; and to require incisions to relieve the great tension of the inflamed parts, and to facilitate the escape of the pus, or of sloughy dead portions

of cellular tissue; but all these matters belong rather to surgery, and have been discussed, I make no doubt, by the Professor of surgery.

Erythema.—Very closely connected with erysipelas, and continually confounded with it, is *erythema.* It also consists in superficial redness of some portion of the skin; but it is not attended with inflammation of the cellular texture under the skin; nor with vesication; nor, in general, with fever; nor is it peculiar to the face and head.

There are numerous varieties of erythema described by writers on cutaneous disorders, to whom I must refer you for an account of them. Willan and Bateman; Wilson; Willis; Rayer; Alibert; and Biett, as his practice and lessons are reported by two of his pupils.

The only variety on which I am disposed to say a word, is one which is attended with more or less febrile disturbance. I mean what is called erythema *nodosum.*

This curious affection occurs much more often in young women than in any other persons; sometimes in feeble boys. The eruption is commonly preceded for a few days by indisposition, and some slight degree of fever. Then red elevated spots come out, on the fore part of the legs, and occasionally, but very rarely, on the arms. The redness appears in oval patches, of which the long diameter is parallel to the axis of the limb. They are pretty large patches, an inch and a half long, and an inch broad perhaps, and they evidently project and form bumps upon the anterior surface of the leg. From their look, you would suppose abscesses were about to form; but after lasting a few days, the red colour fades, or rather changes to a blue, and the protuberances gradually subside. This eruption seems sometimes connected with disturbance of the menstrual functions. Rayer has seen it occur in connection with acute rheumatism. So have I. A patient of mine in the hospital was attacked with acute rheumatism of the joints immediately upon the cessation of erythema nodosum. In another this order was reversed.

Now I am persuaded that, after an aperient, *rest, the horizontal posture,* and *quina,* constitute the proper treatment of this affection. I had once a housemaid, in whom the disorder appeared, and was attended with unusually high fever, and much indisposition. I treated her, therefore, antiphlogistically; *i. e.,* I kept her on low diet, and gave purgatives; but the disease went on. Fresh knots came out as the old ones faded. At length, I do not remember why, I prescribed some quina for her; and the improvement was immediate and very striking. She relapsed, however, once or twice, upon leaving off the bark; but by persisting subsequently in its use for some days after she appeared to be well, a permanent cure was effected. Since that time—now ten or twelve years ago—I have seen a good many examples of erythema nodosum, and I have treated them all alike; viz., first with an aperient, and then with the sulphate of quina: and they have all rapidly got well Probably they would have recovered nearly as soon under some other tonic treatment; but I have been so well satisfied with this, since I began it, that I have felt no temptation to try any other.

Urticaria.—There is a rash which is well known, and very tormenting, and, therefore, not without interest, although it is almost always without danger: I mean *urticaria.* It is arranged by Cullen among the exanthemata; but it does not properly belong to the group to which I restrict that name, for it is not contagious, and it may happen to the same person a hundred times over. The eruption consists of what, from analogy, are called wheals; *i. e.,* of little solid eminences, of irregular outline, but generally roundish or oblong, and either white, or red, or (which is most common) both red and white; the whiteness occupying steadily the central and most projecting part of the spot, or becoming manifest there when the integuments are put upon the stretch. The rash is accompanied with intense heat, a burning and tingling in the affected spots, and great itching and irritation. In truth, both the appearances upon the skin, and the sensations that attend them, are very much like the appearances and feelings produced by the stinging of nettles. Hence its trivial name, *nettle-rash:* which is, indeed, the same, in meaning, with the scientific appellation, *urtica* being the Latin for a nettle. Similar appearances follow almost immediately upon a smart blow with a cane, or with the lash of a whip, on the

skin. Red stripes or *wheals* arise, and within the reddened surface one or more elevated spots of a white colour are visible.

There are two varieties of urticaria: one in which the complaint runs a short course, and soon subsides, and may be considered acute; another in which it is chronic, and either persistent or intermittent. The acute form is attended with feverishness, which sometimes begins two or three days, but commonly not more than a few hours, before the eruption appears; or the fever and the rash may commence together. In most cases, perhaps in all, the disorder is intimately linked with some derangement, manifest or latent, of the digestive organs, and it may often be traced to the use of particular articles of food. It is very curious that the contact of certain substances with the mucous membrane of the alimentary canal should affect the external tegument precisely in the same manner as the virus of the nettle, and some other irritants, when these are applied to the skin itself. The offending articles of food do not produce the cutaneous affection in all persons, nor even necessarily in the same person at all times. But there are some edible substances which are much more likely than others to be followed by nettle-rash. Certain vegetable matters are very apt to excite the disorder in some persons: oatmeal; almonds, especially the bitter almonds; any bitter kernels; particular species of strawberries; raw cucumbers; mushrooms. Some of the vegetable substances used in medicine are known to have, frequently, the same effect: capivi, for example; the cubebs pepper; valerian. Urticaria has been brought on by drinking porter, or, most probably, by some of the drugs with which our porter is sophisticated. These effects are not confined to vegetable substances. Shellfish is a common source of nettle-rash. I have known it to be occasoned by prawns; crabs sometimes have the same unpleasant consequence; and still more, muscles. An hour or two after some one of these substances has been swallowed, and perhaps much sooner, nausea is felt, and oppression about the epigastrium; the patient becomes giddy, his face and head sometimes swell, his skin begins here and there to burn and tingle, and presently the eruption, as I have already described it, breaks forth. It is attended with intolerable itching and pricking sensations, especially at night, when the patient is warm in bed, or when the affected surface is exposed to the air. Vomiting and diarrhœa often supervene, and prove the natural cure of the attack.

We read that this disorder has sometimes proved fatal; but this must be under very unusual circumstances of weakness in the patient, or of some peculiar virulence in the exciting cause.

The chronic form of the complaint is apt to be very obstinate and teasing. It comes and goes, and comes again. The evening is one of its favourite periods. In those who are subject to it, the itching and the wheals are readily brought on by scratching or rubbing the surface. This is the urticaria *evanida* of Willan. Dr. Heberden had known persons afflicted in this way for ten years together. I have observed nettle-rash to occur in connection with sudden and violent paroxysms of dyspnœa, resembling asthma fits; so that I could not help suspecting that the mucous membrane of the respiratory passages was irritated after the same fashion with the external skin.

Even the chronic variety of urticaria is, in some instances at least, produced by certain *ingesta:* and the peccant substance may often be detected, and the tiresome malady be cured, by following the simple and judicious plan recommended by Willan; namely, that of instructing the patient to abstain, for a while, from all his customary articles of diet, one by one, in their turn. This experiment does not, indeed, always answer. The urticaria will sometimes abide, notwithstanding: so that although it probably depends in all cases upon some disordered condition of the stomach or bowels, we cannot say that such disorder is *always* the consequence of something that has been swallowed.

The treatment to be adopted in the acute or febrile nettle-rash, when it depends upon something recently received into the stomach, is that which common sense would suggest, and which nature often plainly indicates. We seek to expel the offending material by an emetic, and by purgatives: and this being done, the cure is completed. In the more chronic and recurring varieties, we endeavour, in the first place, by making the experiment recommended by Dr. Willan, to detect, that we may thenceforth interdict, any article of diet which may have caused the disorder. If we fail in this attempt, our object must be to correct that faulty state of the digestive organs, or to neutralize that inbred

poison, upon which the cutaneous affection depends. Laxatives and antacids are found to be the most successful means of attaining these ends. They may be given together, or separately. Rhubarb and magnesia: the carbonate and sulphate of magnesia; castor-oil. The snake-root has obtained some repute as a remedy for urticaria. You may prescribe, therefore, if you please, a scruple each of the carbonates of magnesia and of soda, in the infusion of serpentaria.

External applications seem to be of but little avail in this disease; and those which do appear to be of service, act uncertainly, and produce different effects in different persons. The warm bath sometimes gives ease in the severer cases. In the more chronic form of the disorder, spirituous washes, vinegar, sea-bathing, are things to be tried. And cases are related in which, when every other expedient has failed to give permanent relief, removal to a warmer climate has been successful. Dusting the itching surface with flour has, in my experience, afforded much temporary comfort. Still more effectual perhaps is a lotion (first recommended by Wilkinson, in a little work on skin diseases) composed of a drachm of the carbonate of ammonia, a drachm of the acetate of lead, and eight ounces of rose water.

I should add, that Dr. Elliotson has found *bleeding* very efficacious in relieving patients affected with acute or febrile urticaria. And you may have recourse to the lancet if the patient be strong and plethoric, and his pulse warrants it; and if he be so impatient of the irritation as not to be willing to wait the effect of other treatment, which would probably be quite as effectual, though *not* quite so rapid.

Prurigo—itching—is a cutaneous affection bearing some analogy to urticaria, at least in the sensations which accompany it. And a most terrible and melancholy affection it often proves to be. Sometimes the parts of the skin which are the seat of the itching do not present any perceptible deviation from the condition of health; but in the majority of instances, you will find, upon close inspection, that they are covered with papulæ, which are nearly of the same colour with the skin itself. Willan therefore places prurigo in the order of Papulæ. He describes several varieties of this troublesome complaint: prurigo *mitis;* prurigo *formicans;* prurigo *senilis*. The torment experienced by patients suffering under the severer forms of the malady is scarcely describable: they scratch and tear themselves incessantly till the blood flows, their sleep is broken, and their lives are rendered perfectly miserable. Sometimes this itching is diffused irregularly here and there over the surface; sometimes it affects the extremities only; and frequently it has a still more limited habitat, occurring round the anus, when it is called prurigo *podicis;* or on the scrotum, prurigo *scroti;* or, worst form of all, the prurigo *pudendi muliebris*.

All these forms of prurigo are apt to be aggravated by heat, and by exposure to the air; they are, therefore, especially distressing when the patient undresses and goes to bed. The scratching tears away the summits of the papulæ, and some watery fluid mixed with blood escapes, and concretes into small, thin, black scabs. In the prurigo formicans, the itching is combined with other painful and disagreeable sensations, which different patients describe in different terms: the feeling is like the creeping of ants, or the stinging of insects, or as if hot needles were thrust into the skin. The prurigo senilis, occurring, as that name implies, in old persons, is usually very obstinate, and often effectually destroys all comfort for the rest of the patient's life.

In such cases as I have now been mentioning, great care should be taken thoroughly to cleanse the surface of the body: and the diet should be rigidly plain. All kinds of rich sauces, hot condiments, pickles, and indigestible substances, should be peremptorily forbidden. Various local applications have been praised, but they are, in most cases, used in vain: lime-water, decoction of dulcamara, lotions composed of prussic acid in an emulsion of bitter almonds, a dilute solution of creasote, decoction of stavesacre, and of digitalis, ointments containing mercury, tar ointment, and a hundred others. In one instance lately, where the ingenuity of another practitioner had been fruitlessly exhausted, I was fortunate enough to effect perfect relief by smearing the itching surface with an ointment containing a small quantity of aconitine. Of internal remedies, sarsaparilla, alkalies, arsenic, the iodide of potassium, dulcamara, are the most hopeful. When these means fail, opium is our best, and indeed our only resource.

The local forms of prurigo are frequently connected with local disease, and are most likely to be relieved by measures directed against the primary disorder. Prurigo podicis is sometimes dependent on the presence of ascarides in the rectum. The same troublesome affection is not an uncommon symptom of internal piles: and it sometimes accompanies stone in the bladder.

The prurigo pudendi muliebris—itching of the genitals in females—is sometimes so constant and tormenting, and the impulse to scratch the itching part so urgent, as to drive the unhappy patient from society. It even gives rise, in some severe cases, to nymphomania. It may proceed from leucorrhœa: it is frequently a sign of uterine disease. It most commonly affects women in whom the menstrual discharge has ceased to appear. I have never had an opportunity of trying the aconitine in such cases. The local application which has been found most serviceable is the *yellow wash*, which, as you probably know, is a solution of corrosive sublimate in lime water, in the proportion of a drachm to a pint.

You will sometimes be consulted—at least I have been, on more than one occasion—about itching of the pubes and scrotum, produced by the presence of the pediculi that are vulgarly called crab-lice. The patients are sometimes quite unaware of the cause of the itching. You may relieve them by the wash I have just mentioned; or by a more elegant lotion, made by dissolving corrosive sublimate in a little spirit, and adding rose water. A single washing with such a lotion will destroy the whole colony: and the vermin become much more visible after this violent death, turning black, and relaxing their hold upon the skin.

Scabies.—Prurigo is a convenient generic name for these cutaneous affections, of which the prominent feature is the teasing sensation that accompanies them. But, besides all these, there is a specific disorder, which, from the intensity of that sensation, is emphatically termed *the itch*, and which deserves a short notice; for it is exceedingly common, and exceedingly distressing, and (what is more interesting still) it is easy of cure. It is one of the very few complaints for which we possess a specific or infallible remedy.

Scabies, or the itch, is, as every body knows, contagious; but it is contagious only in that particular sense which implies contact. It is not producible by any effluvia which the atmosphere can convey: it requires, for its propagation, that the healthy person should touch the diseased person, or some substance which has been in contact with his unhealthy skin. Certain parts of the skin are more liable to it than others. It is most common at the roots of the fingers and thumbs, between them as it were; on the wrists; between the toes; in the flexures of the joints. It may spread to almost every part of the trunk or of the extremities; but all observers agree in stating that it is seldom or never seen upon the face and head; a curious but unaccountable exemption.

The eruption is at first papular, and then vesicular, presenting a number of pointed watery heads. When the inflammation is aggravated by intemperate habits, or by the scratching from which the patient is unable to refrain, the vesicles are liable to be converted into pustules: and this has needlessly been made a separate *species* of itch, scabies *purulenta*, pocky itch: you see large pustules, filled with a yellow viscid matter, standing on an inflamed base. If you are not aware of these varieties and changes, you may make unlucky errors of diagnosis: affront your patient by telling him he has the itch when he has it not; or suffer him unconsciously to betray himself by communicating it to others, when he has. You will easily understand how it has come to be considered a disgrace to have the itch; for it is fostered and propagated in most unfashionable places, amidst poverty, vulgarity, and filth. Yet the most delicate and high-bred lady may contract the distemper; and when once it is contracted, it will go on indefinitely, through life, unless proper means are adopted for its cure. It never gets well if left to itself.

One very curious point in this discreditable malady, is its connection with a peculiar insect, called accordingly the *acarus scabiei*. The existence of this ectozoon had long been affirmed and denied; but the vexed question has at length been set at rest by the public demonstration of the acarus, by a M. Renucci, to a number of medical practitioners in Paris. It has since been often detected and exhibited here. Dr. Nevinson has lately assured me that he furnished Shaw, the naturalist, Dr. Wollaston, and others, with living

itch-mites, for examination, forty years ago. One reason, probably, why it has often been searched for in vain is, that the acari are not equally numerous with the vesicles: there is not an insect for every vesicle. Another reason is, that persons have not known exactly where to look for the insect. It is not *in* the pustules or vesicles; but *near* them; at the extremity of a short, small, superficial tunnel or furrow which runs from them. A third reason why the insect had so long and so often escaped detection, is to be found in its minuteness. It is barely visible by the naked eye; but under the microscope it is seen to be a most formidable monster, in outline like a tortoise, and having eight legs. I show you here its portrait; not a little, but enormously magnified. I hope to procure for you the privilege of seeing the creature itself. The first that I can catch I will ask Professor Jones to show you by means of his microscope.

There is good reason for believing that this parasitic animal is, not merely a casual companion, but the veritable cause, of scabies. Various attempts have been made, and made in vain, to produce the disease by inoculation of the fluid from the vesicles. On the other hand, transportation of the acarus has always excited the eruption.

These facts explain how it is that the itch, though readily communicable by direct contact, or by fomites, is not communicable through the medium of the air; that fomites long retain the contagious property; and that the disease is curable by whatever destroys the acari. I believe that the complaint called *the mange*, in dogs, camels, and sheep, has the same, or a similar origin.

Now *sulphur* is as sure to cure the itch as quina is to stop an ague. I presume that it kills the acarus; but whatever may be its *modus operandi*, I have never known it fail to remove true scabies. It is applied externally; and the only objection to its use is its disagreeable smell, and the dirtiness that belongs to ointments: but these inconveniences are far outweighed by its certain efficacy. Although many substitutes for this substance have been recommended, I pretermit them all, and advise you to employ the sulphur ointment, of which you may disguise the smell by the addition of a little bergamot, and the colour by intermixing a small quantity of vermillion.

The ointment should be carefully rubbed all over the skin, at bed-time, and most especially on parts visibly affected with the eruption. The patient should sleep enveloped in a flannel dress. The rubbing should be repeated night and morning, and in two or three days the complaint will be subdued. Then, thorough ablution with soap and warm water, and the destruction of the contaminated clothes by fire, will complete the purifying process.

LECTURE XC.

HERPES; ECZEMA; POMPHOLIX; LEPRA; PSORIASIS; IMPETIGO; BOILS; CARBUNCLE; PURPURA; SCURVY. CONCLUSION OF THE COURSE.

IF you look at the lists of *genera* and *species* appended to the various works which treat exclusively of cutaneous diseases, you will find that they are exceedingly numerous. But these disorders differ widely in their relative importance; and the principles upon which their remedial management proceeds are not so greatly diversified as these "tables of contents" might lead you to suppose. I have spoken pretty fully of the most serious and interesting of these maladies—I mean of the febrile exanthemata; but I have no time left for pursuing in detail the host of chronic affections to which the human skin is subject. Nor do I much regret this. To become expert in the diagnosis of these blemishes, and in curing such as are curable by our art, you must see them with your own eyes. Verbal descriptions of their changeful characters are of comparatively little service or interest. They are among the things that require to be "oculis subjecta fidelibus." Even pictured representations convey but an inadequate notion of the morbid appearances they are de-

signed to portray. The lecturer on skin diseases should have actual patients before him, to whose bodies he could point.

In this, the final lecture of the course, I can do no more than offer you some very cursory remarks upon some of the genera into which nosologists have distributed this class of disorders; and I may premise, that the treatment of the genus includes for the most part that of the species.

Vesicles are, as you know, small transparent elevations formed by a drop of aqueous fluid effused beneath the cuticle. Sometimes they are thinly scattered over the surface; sometimes collected into clusters: sometimes situated on a red patch of skin; sometimes quite free from redness. The whole crop comes out at once in some cases; in others the vesicles appear in irregular succession. They terminate also in various ways: by the reabsorption of the liquid, and slight desquamation; by the giving way of the cuticle, and the formation of little scabs, under which new cuticle is generated; and sometimes, though rarely, by ulceration.

Of the vesicular class of cutaneous disorders I have already described the three most important; namely, cow-pox, chicken-pox, and the itch.

Another not uninteresting genus of this class is *herpes*. This is a transient non-contagious eruption, consisting of red patches of irregular form and variable size, upon each of which stands a crop of vesicles. The eruption runs a definite course; and its several periods—its beginning, its increase, its acme, and its decline—are completed, when its progress is not interfered with, in about ten days. These characters, once known, are easily recognized; and it is of importance that you should recognize them. Not that the disorder itself is of any great moment; nor that it is at all under the influence of remedies; but because it may be confounded with some graver malady. For example, *præputialis* is a very common and a very trifling species, affecting the foreskin; but it might readily be mistaken for the result of the poison of syphilis, and so cause much alarm and distress to the subject of it, and entail upon him perhaps a needless course of mercury, and bring unmerited suspicion upon the person with whom, whether lawfully or unlawfully, he may have been connected. It has nothing whatever to do with sexual intercourse; and it requires no treatment beyond cold ablution, and the interposition of a piece of lint between the prepuce and the glans penis. In like manner another species, Herpes *circinatus*, the vesicular ringworm, is liable, when it appears upon the hairy scalp, to be mistaken for that pest of schools and of school-boys, the *favus confertus*, or *porrigo scutulata* of Willan, the common scald-head; a complaint which is naturally shunned as filthy, stubborn, and contagious. Herpes *iris* is a mere curiosity. In this species each group of vesicles is surrounded by four concentric erythematous rings, of different shades of colour. The rings form and fade in succession, one after another, by a slight extension of the inflammation each time. The spots constituting the eruption have been likened to small particoloured cockades. But the most singular species of all is the Herpes *zoster*, in which the separate patches lie in the direction of a band that encircles half the circumference of the body. Hence its names, zoster, zona, zona ignea, and in our vernacular tongue, *the shingles;* and even this Dr. Johnson derives from the Latin, *cingulum*, a girdle.

Most commonly the zone is confined to the trunk of the body, has a somewhat oblique direction like a sword-belt, and occupies exactly one-half of the circle, lying between the linea alba and the spine on one side only. Sometimes, however, it extends from the trunk to the limbs. Thus it may begin in the loins, pass obliquely across the flank, and terminate at the inner part of the thigh. Or it may commence from the upper part of the spine, cross the shoulder, and end on the arm or forearm. In the year 1833 I saw a lady affected with this strange eruption, in whom the clusters of vesicles began near the spine in the neck, passed over the scapula, then to the shoulder and axilla, whence the main line ran along the outer side of the upper arm till it reached the elbow, where it turned inwards, followed the inner side of the forearm, went across the palm of the hand, and terminated by two or three patches upon the palmar and inner side of the ring finger. Very rarely indeed it appears on the limbs only. Once I have seen it limited to the thigh and leg, where it corresponded to the course of the sciatic nerve. Once I have known it spread from the neck, behind, up into the hairy scalp. The most common situation of

the demi-cincture is across the base of the thorax. It is a curious feature of this curious disorder, that, in nineteen cases out of twenty, according to Biett, it occupies the *right* half of the body. Of this singular preference of the right side, if indeed it be a general fact, I can give you no explanation. I have seen fifteen cases of the complaint since I began to attend to that circumstance, and in ten of these the eruption was on the right side. Rayer, in the first edition of his book, said that eight cases in ten would be found to be on that side; but a longer experience has reduced that proportion. Of fifty-three examples seen by himself, thirty-seven only were on the right, and sixteen on the left. Reil states that he has *always* observed it on the left half of the body; and Mehlis, among twenty-five patients, counted sixteen in whom the left side was affected. This statistical point remains, therefore, to be settled, if it be worth settling, by a large induction of particular cases. The zone seldom transgresses the median line at either extremity, unless perhaps the redness of the extreme patches may extend a few lines further. It is said, however, in some exceedingly rare instances, to complete the circuit of the body. There is a vulgar but erroneous notion, that the eruption proves fatal when it thus surrounds the whole of the trunk: and this notion is as old as the time of Pliny, who says, "Zoster appellatur, et enecat si cinxerit."

The most important, because the most distressful, of the symptoms, is an intense darting pain, described by the patients as being deep-seated, very acute, and shooting through the chest. Fortunately, however, this is by no means a common incident. I have met with it in two or three cases only. Sometimes the pain precedes the eruption; more often it accompanies it; and it is apt to last, in spite of remedies, for some time after the eruption has disappeared. Mr. North tells me that, in a female patient of his, this pain continued to be severe and intractable for eighteen weeks. That this is a rare complication of the disorder we gather from the experience of M. Biett, who never once witnessed this symptom in more than 500 cases of shingles. The severe and intermitting character of the pain, and the peculiar direction of the row of herpetic patches, lead to the belief that the whole malady may arise from some fault in the nervous system. Of its causes we have no certain knowledge. It is said to attack young persons more especially, and those who have fine and delicate skins, and the male more frequently than the female sex. But I suspect that these assertions rest on a very loose foundation. Of the fifteen cases already mentioned, ten occurred in females. One of the patients was a child two years and seven months old, another was an aged man of about seventy-five. In several instances I have found, upon inquiry, that the patients, being children, were in the nightly habit of wetting their beds. Whether this has been any thing more than a casual coincidence, I do not know; but my attention was first directed that way, some years ago, by Mr. Wheeler, the apothecary of St. Bartholomew's Hospital, who told me that he had often noticed the same circumstance. According to Bateman, the disorder "seems occasionally to arise from exposure to cold after violent exercise. Sometimes it has appeared critical, when supervening on bowel complaints. Like erysipelas, it has been ascribed by some authors to paroxysms of anger." Schwartz saw three cases which followed violent fits of passion; and Plenck affirms that he had known it occur twice after furious anger—*and* a copious potation of beer.

The duration of the eruption is from ten days to a fortnight; but is liable to be considerably prolonged by troublesome ulceration, whenever the vesicles and crusts are prematurely chafed off by friction or pressure.

Very little, as you must perceive, can be done, or is requisite, in the way of treatment. The patient is to be cautioned against rubbing off the heads of the vesicles. Attention should of course be paid to the state of the stomach and bowels; and the diet should be regulated. Our main business is to look on, and to endeavour to set right whatever may be manifestly wrong.

Should the eruption be attended or followed by the intense shooting pain which sometimes, but not often, harasses the patient, it will be right to apply opiates, by friction, over the affected region. I would use the aconitine ointment in such a case. Warm baths will also be proper; and as the pain is probably neuralgic, the carbonate of iron is a remedy which ought to be tried.

Eczema.—*Eczema* is another genus of the vesicular class of diseases. It is characterized, in its commencement, by an eruption of very minute vesicles, scarcely prominent, closely crowded together, and requiring a microscope sometimes to render them distinctly visible. They terminate either by the reabsorption of the fluid they contained, or by the formation of superficial moist excoriations. Eczema is not contagious.

There are several species or varieties of this form of cutaneous disorder also It is sometimes produced by great heat, and particularly by the heat of the sun; and this is named eczema *solare*, *heat-spot:* sometimes by the contact of irritating substances with the skin, as in what is vulgarly called the grocer's itch, affecting the hands of those who are much conversant with sugar. Eczema often occurs upon the scalp, and constitutes, I believe, the most frequent form of what is vulgarly called scald-head, porrigo, or tinea capitis. But the most severe of all its species is that which has received the names of *Hydrargyrium*, *Erythema mercuriale*, and *Eczema rubrum mercuriale*. This, as these names imply, is an occasional consequence of mercury; an unusual consequence, no doubt, and one that happens only in a few peculiar constitutions: but you ought to know it, in case it should follow the use of mercury prescribed by yourselves.

The eruption begins usually in the groins and upon the thighs. It is at first red, and is accompanied by much heat and itching. It soon extends, in the severer cases, over the whole body; and an innumerable multitude of very minute glittering vesicles may be seen, with the aid of a magnifying glass, from the beginning. Like that of erysipelas, the eruption is attended with a good deal of swelling. The intumescence of the face is such as to close up the eyes: and the disorder *becomes* febrile, in its course; for there is seldom much fever at the onset. The vesicles increase in size, turn milky, burst, and pour forth an acrid exudation, that irritates and inflames the skin with which it comes in contact, and thus increases the local complaint. The distress and worry occasioned to the patient by the fœtid smell of the discharge, by the stiffening which it causes of his body-linen, and by the heat and itching, are, I conceive, the main causes of the febrile disturbance. The discharged matter is apt to become thick and hard, and to present the appearance of large scabs: and in this state the nature of the disease may very easily be misunderstood, it being impossible to say, when it is seen for the first time under these circumstances, whether it was originally vesicular or not.

The duration of this harassing distemper is variable. It may be over in a fortnight, or it may last several weeks. It terminates by the cessation of the discharge, and then the cuticle detaches itself in large flakes. Sometimes in this disease also the epidermis falls entire from the hand, like a glove.

Without being dangerous to life, this disorder is apt to be obstinate. It is not much within the control of remedies. What little can be done is chiefly palliative. The mildest local applications must be used: tepid water, barley water, strained gruel. The warm bath, when circumstances permit. Poultices are sometimes of much service, in preventing the hardening of the matter that exudes, and so obviating one source of irritation. Flour, or powdered charcoal, may be sprinkled over the eruption in the slighter and early cases, for the purpose of absorbing the discharge. Equal parts of olive oil and lime-water make a soothing liniment, which may be applied by means of a feather. The local remedies may fairly be varied, for sometimes one, and sometimes another, is found to give relief. The patient's linen must be frequently changed, especially whenever it becomes stiff and hard with the exudation.

With respect to the general treatment, opiates to procure rest, and to allay irritation, are probably indispensable. The bowels must be kept moderately open, but no severe purging should be employed, for the patient must at all events undergo a long and weakening process, and therefore it must be our care that the *treatment* be as little weakening as possible. In protracted cases, where there is much exhaustion, wine may with propriety be given; and, almost always, good strong broths. As the disorder declines, some of the reputed tonics may be prescribed; the mineral acids, quina, sarsaparilla.

I need not say that, in such cases, you must be scrupulous in seeing that no more mercury is administered or applied.

Pompholix.—A separate class of cutaneous diseases, very analogous, however, to that

which we have been describing, is the class of *bullæ*, or blebs. Anatomically speaking, there is but little difference between the two: *bullæ* are *large vesicles*. When the eruption is at its height, it is composed of hemispherical prominences of various sizes, from that of a pea to that of a hen's egg, and having the shape and appearance of the bubbles raised in a pool of water by a hard shower of rain. They are formed by the effusion of a serous, or a sero-puriform fluid, between the true skin and the cuticle. You can only be sure of the diagnosis when you see the eruption in this stage of its progress.

The best example of this class is that which is called by some writers *Pemphigus*, by others *Pompholix*.

It is characterized by the presence of bullæ, varying in their magnitude, commonly distinct, but numerous, springing up in successive crops, on one or more parts of the surface. At first these bullæ are nearly transparent, and contain a thin limpid serum; but they become gradually opaque, pearl-coloured, and ultimately many of them acquire a reddish tinge.

Pemphigus has been described as being sometimes acute, sometimes chronic. The acute form is attended with smart fever, the bullæ rise spontaneously or in quick succession, run their course, and disappear; and then the disease is over. This is a very rare form. In general the bullæ continue to come out; the complaint is spread over weeks, or months, or years; and it is accompanied by little or no febrile reaction. This, on the other hand, is a common form of disease. It is the *Pompholix diutinus* of Willan and Bateman. The eruption often occupies all parts of the body at the same time, or in succession; in other cases it is confined to a limited space. I have most frequently seen it on the forearms and legs. When the bullæ are very numerous, they may give rise to some febrile symptoms, but not else. The complaint may be indefinitely prolonged by successive crops.

The eruption begins in small red points, the formation of which is attended with a slight pricking sensation. Some patients have likened this sensation to that which accompanies the passage of the electric spark. In the centre of each of these spots, the cuticle becomes lifted, while the circumference of the spot enlarges, so that bullæ are rapidly formed, often in the space of a few hours only, as big as a hazel-nut, or a walnut: or the blebs may even be much greater than that. Either in consequence of their distension, or of the pressure made upon them by the movements of the patient, some of these bullæ burst, and a straw-coloured serum exudes. Then the epidermis collapses into folds and wrinkles; or if it be detached at a part of the margin of the bulla, it is rolled back, so as to expose a portion of the red, painful, and smarting surface beneath it. Towards the third or fourth day, when the bullæ lose their transparency, and the liquid they contain becomes reddish, those bullæ which have not been broken sink down and wither; the cuticle is no longer stretched; but, sodden by the serous fluid, it assumes a whitish hue, becomes opaque, and forms at length small brownish flat crusts, of no great thickness.

In the meantime fresh bullæ appear by the side of the former ones, and pursue the same course; so that generally you may see, in the same person, tense bullæ containing a transparent and yellowish serum; thin crusts; and irregular patches of various size, slightly excoriated.

This is the ordinary course of chronic pemphigus: and I say it may thus go on for months or years.

The disease is most frequently observed in persons of debilitated habits. It is sometimes apparently the result of intemperance; or of the use of bad or insufficient food. In Biett's experience it has often been coincident with the fatty liver. When the disorder is chronic and uncomplicated, the treatment found useful is such as we might expect benefit from, knowing the constitutions in which the disease is most apt to occur, and the causes which seem at least to favour its occurrence, if they do not produce it: regulation of the bowels; good nourishing food; and tonic medicines, bitters, and especially quina and the mineral acid. Biett describes this kind of treatment to have been very successful in St. Louis; and that not only in old and worn-out subjects, but also in the young, especially when the complaint has been chronic. Bateman recommends the same general plan.

Local applications have seldom been much employed, except some mild ointment to

the excoriated parts. Biett advises emollient lotions, or even opiate washes when much irritation exists; but a case recorded in the *Medical Gazette*, by Dr. Graves, of Dublin, affords a remarkable instance of a cure by local applications alone: and it is a case worth recollecting, although, as he justly remarks, we ought not to generalize from a single instance.

His patient was a boy, fourteen years old, of slender frame and delicate constitution, yet enjoying uninterrupted health, except the cutaneous disease, which had lasted five years. During that time the succession of bullæ had seldom ceased. The bullæ were very numerous, occupying not merely the face and extremities, but the trunk also; and they were in various stages of their progress, some healing after having burst, some of a larger size and unbroken, others small, and recent.

Dr. Graves observes, that from the descriptions of Bateman, and of Biett, although both authors describe it correctly, we should scarcely form a notion of the occasional severity of this disorder. He had seen two examples of it in young men, where the irritation and suffering produced by the constant exposure of large portions of skin denuded of epidermis, had operated most unfavourably on the general health, almost banishing sleep, and reducing the patients to a state of great debility. These cases did not yield to the method of treatment recommended by authors; and, therefore, Dr. Graves determined, whenever another opportunity should occur, to have recourse to a new plan.

In the boy in question, therefore, he had all the bullæ opened with a lancet, and the denuded surface of the corium was then touched with a stick of lunar caustic. The nitrate of silver was also applied to the skin around each bullæ, for the breadth of a line: and the recent pimples, which indicated the formation of future bullæ, were all treated in the same way. The boy was then washed, and supplied with clean linen.

This single application of the nitrate of silver had not merely the effect of entirely destroying the morbid action in the portions of the skin which were at the time affected, but (what was very remarkable) no fresh bullæ made their appearance afterwards: none at least had appeared for four months, when he wrote his account. The only part where a repetition of the process was required, was the palm of the hand, where the thickness of the cuticle rendered it more difficult to expose the diseased surface of the cutis to the full action of the caustic.

It might strike you from this cure of a long standing disorder, so readily, by mere local means, that the disease propagated itself from one part of the surface to another, by a sort of re-inoculation. But it has been fully proved that the disease is not contagious. A Mr. Gaitskill engrafted himself, with impunity, with the fluid; and analyzed it, and found it apparently like the thin serum of hydrocephalus. Dr. Graves, therefore, supposes that the cure was owing to the simultaneous destruction of all the parts of the skin that were in a state of morbid action: a morbid action which would have been otherwise propagated to other portions of the surface, by what is called the sympathy of *continuity*.

Lepra.—The class of scaly eruptions—the *squamæ*—is distinguished by the occurrence of red spots or blotches, upon which laminæ of altered cuticle form, and are thrown off, and constantly renewed. You will perceive that, anatomically, this class of cutaneous disorders has a close analogy with the rashes; and yet it is separated from them by very obvious particulars. In the exanthemata of Willan and Bateman, the redness is *followed* by desquamation; in the squamæ these two appearances coexist; in the exanthemata the sequence of redness and desquamation takes place, in general, once only; in the squamæ the morbid cuticle continues for an indefinite time to scale off again and again, in successive fragments, from the abiding red patch of skin.

Lepra, psoriasis, and pityriasis, and some syphilitic eruptions, constitute the principal of the squamous affections.

Lepra is a very common disorder of this class; hence its name, lepra *vulgaris*. It consists in red scaly patches, of various dimensions, but always affecting a circular or elliptical shape, and scattered over different parts of the body. It commonly begins on the limbs, most usually near the joints; just below the knees, or the elbows; and Dr. William Budd has pointed out the curious fact that these patches, especially when they are few, and the disease is recent, are distributed symmetrically, each spot on the one limb answering in

situation to a similar spot on the fellow limb. This shows that the disease is a blood disease; that it depends upon some poison, introduced from without, or, more probably, bred within the body. By degrees the patches both enlarge in size, and multiply in number, and extend along the extremities to the trunk. The eruption is seldom seen upon the hairy scalp, or upon the hands. As the patches enlarge they sometimes become confluent; but even then, the outline of the confluent scaly space is defined by arcs of circles, and the disorder is sufficiently distinguishable from *psoriasis*. It is not easy to set these things before you in mere verbal description. To have *seen* lepra once, is to know it for ever.

When the patches begin to get well, the restoration of the altered surface to its natural condition and appearance commences in the centre—*i. e.*, in the spot first affected—and proceeds outwardly towards the circumference: so that the scaly redness assumes a ring-like arrangement. This ring becomes gradually narrower and narrower; at length its continuity is here and there broken; and at last it vanishes entirely.

The eruption does not, however, run any definite course. Sometimes it goes rapidly through its phases; in other cases it persists for a very long period. It is not at all contagious.

Neither is lepra attended, in general, with much local inconvenience, nor with much constitutional disturbance. When the eruption is very copious and extensive, and especially when it is plentiful or almost continuous around the larger joints, it renders the movements of the limbs stiff and difficult; and even sometimes painful, from the cracking of the inflamed surface as it is stretched in the bending of the joint.

But I have seldom found lepra to exist *unconnected* with some disorder of the digestive organs. Usually the connection is that of alternation, and not of coexistence. The patient is dyspeptic till the eruption comes out, and then the dyspepsia is relieved: and it often returns as the leprous patches disappear. The eruption is the more unsightly; the dyspepsia is the more troublesome. This alternation would seem to mark the shifting location of the *materies morbi*.

When the patches are small, and chronic, and white, that variety is no longer called lepra vulgaris, but lepra *alphoïdes;* and there certainly is another distinct variety, of a more blue, or livid, or copper colour than the ordinary; and a result of the poison of syphilis. It is named accordingly syphilitic lepra. This species will get well under the influence of mercury; which, so far as my observations go, does not cure the others.

Psoriasis is closely allied to lepra. When it occurs in distinct patches it is often difficult to say to which genus the eruption belongs. In general the patches of psoriasis are not so broad as those of lepra; their edges are less raised, and their centres less depressed; the scales adhere more firmly; and the patches are less uniform and less circular.

But, psoriasis frequently spreads itself over large portions of the skin, and it may come to occupy nearly the whole surface of the body. It is then called psoriasis *diffusa*. It often renders the patient hideous to look at. The scaly encrustation is interspersed with chaps, furrowing the skin in all directions, and following particularly its natural folds and angles. These cracks, when the skin is put upon the stretch by the movements of the patient, are apt to bleed. In these severer cases (which are said to be examples of psoriasis *inveterata*) the laminæ of altered cuticle are thick, and very abundant. They fall off perpetually, or are rubbed off, and may be shaken from the patient's clothes, or collected in handfuls from his bed.

Both these scaly disorders, lepra and psoriasis, require the same kind of treatment.

I believe that external applications are of but little use. I have tried a good many, and have lost all confidence in them, with the exception of the warm bath. Whatever tends to improve the general health, will hasten the departure of these eruptions. I believe that they sometimes depend upon the presence, or the generation, of an excess of acid in the system; and that they are often to be cured by alkaline remedies I am sure. I have seen many cases of psoriasis rapidly improve, and get ultimately well, under full doses of the *liquor potassæ;* from half a drachm to a drachm, three or four times daily, in a glass of milk, or of water, or of beer. Another internal remedy from which I have seen manifest improvement result, is arsenic; given with the cautions, and in the doses, which I have

more than once spoken of. These are the two remedies of which I have the most experience; but neither of them is infallible; and you will have to try many things in succession, for patients are very desirous of getting rid of the disfiguring eruption, even when it does not interfere with their health or comfort. Now the Harrowgate waters, a strong decoction of dulcamara, pitch-pills (and if pitch-pills, I should suppose *à fortiori* creasote), tincture of cantharides, and the iodide of potassium, are remedies of some renown for these scaly diseases. Of the syphilitic lepra I repeat that mercury will prove a cure. In all cases the diet must be regulated, and all kinds of stimulating food abstained from. Dr. Bateman knew a man who was always attacked with lepra if he took spices with his food, or drank ardent spirits: and a patient of my own got rid of long-standing and very troublesome psoriasis of the scrotum, upon adopting, for other reasons, a very abstemious and simple mode of living.

Impetigo.—Among the *pustular* diseases of the skin there is one which assumes many forms, and is termed *impetigo*. Whatever may be the minuter peculiarities of this eruption, its general characters are the following. It consists of crops of pustules, sometimes scattered irregularly, sometimes collected into groups. The pustules burst, or are broken, dry up, and scab over. The crusts are yellowish, and very friable, and resemble in appearance little masses of candied honey; or sometimes they look like small pieces of dirty plaster. From beneath these crusts a considerable discharge continues to take place; the crusts become thicker and larger, and around the margins the skin is red and raw, as it is also beneath them.

We have, I say, various forms of this complaint; impetigo *figurata*, impetigo *sparsa*, &c. It often borders closely on eczema, so that authors describe an eczema *impetiginodes*, or an impetigo *eczematodes*. These varieties are delineated by Rayer, by Willan and Bateman, and by others; and knowing their characters, you can examine and study their appearance for yourselves. Impetigo is a non-contagious disorder.

Sometimes this complaint occurs in an acute form, and is attended with fever. In such cases its removal will be accelerated by bleeding the patient; and the blood drawn will be found to present the buffy coat. Whatever local applications are made should not be unctuous. It is seldom that impetigo will bear, or be the better for, ointments. Purgatives and alkalies internally, and very weak spirit or alkaline lotions externally, with a scrupulous diet, constitute, I believe, its best treatment. When the complaint is chronic, and the discharge copious, the oxide of zinc has often a very beneficial effect. It may be dusted over the affected surface, from a thin muslin bag; or it may be applied in the shape of a lotion—fifteen grains to an ounce of rose water. You will find this a most useful lotion for that disfiguring impetiginous eruption which sometimes covers the face of children like a mask, and is called *crusta lactea*. The phrase crusta lactea is, however, very loosely employed by medical men.

Boils.—There is a very common, and a very teasing pustular disease of the skin, usually called a *boil*, in some parts of England a *push*, and by the learned *furunculus*.

First, there is a slight degree and extent of hardness to be felt, a tender knot, just beneath the surface, which soon begins to look red, and a small swelling arises, which gradually increases up to a certain size, that of a large pea, or of a hazel-nut, or of a walnut. The tumour is painful, and undergoes a process of slow suppuration. Some time from the fourth to the eighth day it acquires a conical or pointed form, and its apex becomes of a white or yellow colour. At last the cuticle gives way, and the patient begins to congratulate himself that the little abscess is ripe, and that his troubles are nearly over. But he is disappointed; an insignificant quantity of pus mixed with blood escapes, and leaves visible a mass of dead cellular tissue—a *core* as it is called—of greater diameter than the opening, which is commonly small. At last, two or three days perhaps after this, the slough is expelled, in company with more pus, and a deep cup-like cavity remains, which soon however fills up, and the boil is really over.

These little phlegmons frequent the buttocks, the thighs, the arm-pits, the nape of the neck, the abdomen. They may occur almost anywhere. They are apt to come in crops, or in a series: and any kind of irritation suffices to cause them when a constitutional

tendency to their formation exists. I have known a piece of soap plaster applied to the skin give occasion to a long succession of boils. Poultices, applied to promote the suppuration of any existing furunculus, are believed, by their warmth, to encourage the growth of others around it. In truth, these phlegmons belong primarily and essentially to the subcutaneous cellular tissue rather than to the skin. Dr. Prout corroborates the statement of Cheselden that they are often accompanied by a saccharine condition of the urine. You know probably that, in Dr. Prout's theory of assimilation, the cellular tissue represents the saccharine aliment.

The individual boils are intractable; the state of system which engenders them may perhaps be corrected. Some dab them, when nascent, with a solution of corrosive sublimate in spirit; some support them with sticking-plaster; others apply poultices; and others again cut the hard tumour through while it is yet crude. Do what you will, you will seldom prevent or accelerate their deliberate course; but I believe that by applying leeches, or cold, you may prolong, though you cannot arrest that course.

Sarsaparilla, and the liquor potassæ; and where the system is below par, the sulphate of quina and a generous diet, are found useful. Sugar, and saccharine food of all kinds, should be scrupulously avoided.

Carbuncle.—Carbuncle, *alias* anthrax, is a gigantic boil; but it is far more serious than the common furuncle, not only in respect to its magnitude, and to the amount of suffering which it occasions, but also on account of the constitutional vice that it betokens. A carbuncle is a large, flat, circumscribed, very hard, and very painful tumour, of a purplish red colour, and attended with a sensation of burning heat. Its ultimate diameter may be three or four inches, or more. It ends in the formation of a deep slough, of more than corresponding dimensions, and the destruction of the skin above it. A number of pinhole openings at length present themselves, on the dark red surface, and disclose the immense core beneath.

This serious complaint occurs chiefly in advanced life, in corpulent males, and in persons who have lived fully. It is frequently attended with diabetes. The tumour is more often situated upon the nape of the neck, or between the shoulders, than elsewhere. It produces high constitutional disturbance and irritation. Surgeons are in the habit of dividing the firm mass into quarters, by deep crucial incisions. This is a sharp remedy, but it purchases speedy ease, by removing that tension of the inflamed parts whereupon the pain chiefly depends. I am persuaded, however, that this severe operation is done too indiscriminately. If the tension be not manifest, and there is not much complaint of pain, you had better, in my opinion, leave these tumours to the care of nature, and address your remedies to the system at large. Support is often needed; and opiates are sometimes indispensable; and the bowels must be kept clear by purgatives.

Purpura.—I shall not attempt to discuss, even in this cursory and disjointed manner, any more of the inflammatory affections of the skin, whether acute or chronic: but I wish, before I conclude, to direct your attention to a peculiar morbid condition, of much greater interest and consequence than most of those which I have just been describing. I mean the malady which is best known by the appellation of *purpura*, or the purples, and which usually, though it must be confessed very incorrectly, is ranked among cutaneous disorders. It is strictly a hæmorrhage. Its external phenomena are so obvious, and so well known, that I need not dwell upon them. Small round spots appear on various parts of the surface, generally upon the legs first and most plentifully, of a dull red, or of a deep purple colour. They are accompanied by no local pain, by no sensation of any kind. Pressure upon them does not efface the colour, nor render it fainter, as it does that of inflammatory spots of the skin. There is scarcely ever any prominence of the purple stigmata; but they are sometimes intermixed with livid blotches, with appearances exactly resembling bruises: and both the circular spots and the ill-defined vibices undergo, before they disappear, the same changes of colour, from red to a greenish yellow, which a bruise undergoes. In fact, the anatomical condition of a bruise is exactly the same with the condition of the diffused livid blotches of purpura. In each case the colour is the

result of ecchymosis. With all this, passive hæmorrhages from various parts, and particularly from the mucous membranes, are common.

It is clear, therefore, that this complaint cannot be regarded as a cutaneous complaint, even in the loose sense in which that epithet is sometimes applied to affections that are really *beneath* the skin, but visible *through* it. The hæmorrhage takes the form of red or purple spots when the quantity of blood extravasated in the same place is only a drop. And the spots are not peculiar to the skin, nor to the subcutaneous tissues, but are found, occasionally, upon all the internal surfaces also, and within the substance of the several viscera. I have seen these purple spots on the mucous surface of the mouth, the throat, the stomach, and the intestines, on the pleuræ and pericardium in the chest, on the peritoneal investment of the abdominal organs, in the substance of the muscles, and even upon the membranes of the brain, and in the sheaths of the larger nerves: and I have known them to be accompanied with large extravasations of blood in most of the vital organs of the body.

Scurvy.—The superficial markings of purpura, the red and purple spots and livid blotches, exactly resemble the spots and bruise-like stains which characterize sea scurvy: and I confess that I formerly regarded the two affections as being identical, or as mere varieties of the same disorder. But it is not so. For a very full and interesting account of scurvy, I must refer you to an essay by Dr. Budd, in the *Library of Practical Medicine*. He has there collected from various sources, and exhibited in a clear light, convincing evidence that scurvy is caused—neither by contagion, nor by cold weather, nor by impurity of the air, nor by the continued use of salt provisions, all of which have been alleged as sources of the disease, but—by the privation, for a considerable length of time, of fresh succulent vegetables. Now purpura often makes its appearance when there has been no deficiency of such food, and no remarkable abstinence from it. Scurvy is most common in winter, or the beginning of spring; purpura in the fruit seasons, in summer and autumn. In scurvy the gums are uniformly soft, and swelled, and spongy, and bleed readily; this is no necessary feature in purpura. Scurvy is marked by extreme debility and dejection of spirits; it is always rendered worse by blood-letting and by mercury; and it is infallibly and rapidly cured by the administration of lemon-juice, or of other fresh fruits and vegetables. Purpura, on the other hand, often requires venesection for its cure; is not constantly nor surely, if ever, benefited by the anti-scorbutic juices; is not always attended by sponginess of the gums, nor by feebleness of the mind and body; and I have seen it clear speedily away upon the supervention of mercurial salivation, and hypercatharsis.

Lemon-juice is really a specific against scurvy, whether it be employed as a preventive or as a remedy. It supplies something to the blood which is essential to its healthy properties. Its virtues were known in this country full two hundred years ago, as appears by the work entitled *The Surgeon's Mate, or Military and Domestic Medicine*, by John Woodall, Master in Surgery: London, 1636. But the merit of making the fact generally known, and of procuring the systematic introduction of lemon-juice into nautical diet, by an order from the Admiralty, is due to Dr. Blair and Sir Gilbert Blane, in their capacity of Commissioners of the Board for Sick and Wounded Seamen, in 1795. "The effect (says Sir John Herschel) of this wise measure may be estimated from the following facts. In 1780 the number of cases of scurvy received into Haslar Hospital was 1457: in 1806 *one* only, and in 1807, *one*." He adds, "There are now many surgeons in the navy who have never seen the disease."

Dr. Budd, however, has assured me that the Dreadnought Hospital-ship, at Greenwich, is often full of cases of scurvy; most of the patients so affected having just arrived in *merchant ships*, from a long voyage. This surely ought not to be. It *could* not be if the owners of those vessels knew how easily, surely, and cheaply, this truly dreadful scourge may be averted.

Scarcely less—if indeed less at all—of antiscorbutic virtue, belongs, fortunately, to that common esculent root, the potato. *Raw* potatoes have long been in good repute, both for the cure and for the prevention of scurvy: but raw potatoes are neither palatable nor easy of digestion; and it is a great discovery, which we owe to the sagacity of Dr. William

Baly, that this vegetable is equally effective for these purposes when cooked. During some months of continued observation of the prisoners confined in the Penitentiary at Milbank, I had remarked, without being able to account for it, that among the small number of *soldiers*, committed for comparatively short periods, for offences against military discipline, scurvy was not uncommon; whereas I noticed it in one instance only among the much more numerous class of *convicts*, whose term of imprisonment was considerably longer. Dr. Baly was afterwards appointed Physician to the Penitentiary, and the same curious fact soon caught his attention: and he has traced the cause. By the examination and comparison of various dietaries—those, namely, which have been adopted at different periods in the Penitentiary itself, those which, at the same period, were prescribed respectively for the military offenders, and for the ordinary convicts, and those in use in sundry other gaols in which scurvy has occurred with different degrees of frequency—he has shown, most satisfactorily, that the liability to that malady has a strict relation to the amount of succulent vegetables consumed by the prisoners, and especially of potatoes. "Wherever this disease has prevailed, there the diet of the prisoners, though often abundant in other respects, has contained no potatoes, or only a very small quantity. In several prisons, the occurrence of scurvy has wholly ceased on the addition of a few pounds of potatoes being made to the weekly dietary. There are many prisons in which the diet, from its unvaried character, and the absence of animal food, as well as green vegetables, is apparently most inadequate to the maintenance of health; and where, nevertheless, from its containing abundance of potatoes, scurvy is not produced."

Now potatoes are *food* as well as medicine, and they are a cheap kind of food, and it may be hoped that a more general knowledge of their antiscorbutic properties, even when cooked, will abolish this wretched complaint, wherever a good supply of them is obtainable. Dr. Baly believes that from three to six pounds, weekly, for each person, would suffice. He thus accounts for their salutary influence.

"A glance at the chemical analysis of the potatoe at once explains its antiscorbutic virtue. The various fruits, succulent roots and herbs, which have the property of preventing and curing scurvy, all contain, dissolved in their juices, one or more organic acids —such as the citric, tartaric, and malic acids. Sometimes these acids exist in the free state, but more generally they are combined with potash, or lime, or with both these bases. Now potatoes have been submitted to most elaborate chemical examination by Einhoff and Vauquelin; and by both these chemists they have been found to contain a vegetable acid in considerable quantity. According to Einhoff, this acid is the tartaric combined with potash and lime. According to Vauquelin it is the citric, partly in combination with those bases, and partly in the free state. The farinaceous seeds, as wheat, barley, oats, and rye, which are destitute of antiscorbutic property, contain no organic or vegetable acids."

The same cause which gives rise to *sea* scurvy will produce precisely the same effect on land. Of this I must give you one illustration from my own case-book. In August, 1830, I admitted into the Middlesex Hospital a blacksmith, thirty-five years old, covered with round purple spots of various sizes, and with irregular blotches of ecchymosis. He had *vomited* blood on the preceding day. He was continually *coughing up* blood at the time of his admission, and his wife estimated the whole quantity that he had then lost to be more than half a pailful. The interior of his mouth and palate was pouring forth blood from a number of livid fungous tumours, formed by the extravasation of blood into the cellular tissue, beneath the membrane, and the subsequent rupture of that membrane. He was passing blood by the *bowels* also; and his *urine* was loaded with blood.

Here were the *symptoms* of scurvy strongly marked. In the man's history we could trace its peculiar *cause*. He had long been subsisting on very poor and insufficient nutriment, seldom eating any meat, but living almost entirely on tea, coffee, and bread and butter. He had been too ill and weak to work regularly, yet he had been obliged occasionally to over-exert himself to obtain a scanty supply of food for himself, his wife, and a large family of children. He had been a settled dram-drinker, but for some time had taken much less of that stimulus; merely because he had not the means of procuring it. His pulse was frequent and feeble.

I had not much hope that this patient could be saved by any treatment. He was im-

mediately put upon a diet of roast meat, and began to take daily half a pint of fresh lemon-juice diluted with a pint and a half of water. This plan, with some tonic medicine, was commenced on the 3d of August. He improved at once. On the 8th all hæmorrhage had ceased; the fungous tumours in the mouth had disappeared, leaving small scars in the places they had occupied; and the discoloration of the skin was almost gone. The amendment was so striking and rapid, and so immediately consequent upon the institution of the treatment, that no room was left for mistaking recovery for cure.

It is chiefly by investigating the previous history of the patient, and by noting the degree of strength that he possesses, and the condition of his pulse, that we are guided in our diagnosis of ambiguous cases. The late Dr. Parry, of Bath, was one of the first to point out the efficacy of abstinence, venesection, and purgatives, in some instances, at least, of purpura. I may refer you to an example of this kind detailed in the *Medical Gazette* for the 5th of April, 1828. It occurred in one of Dr. Latham's hospital patients; and several of the symptoms were very like those I have just been relating. In particular the whole tongue was livid, one half of it presenting the appearance of a large, black, bleeding fungus; and on the inner surface of each cheek were several black fungoid patches. The patient was voiding also unmixed blood from the bowels. In this case there was no evidence of the operation of any debilitating cause, and the pulse, though frequent, was *hard*. Bleeding from the arm always gave relief to his uneasy sensations: he was purged also, and put upon low diet. Under this plan he steadily improved, and in four or five days no vestige of the complaint remained except the fading spots. For some time afterwards, however, "the frequent use of active purgatives, and a rigid restriction to low diet, were necessary to obviate costiveness, and to keep down the circulation, which had a tendency to become over-active."

You are not to suppose that all cases of purpura bear this sthenic character, or require these heroic remedies. Your treatment must be guided by the previous circumstances and habits of the patient, by the state of the pulse, and by the other symptoms which accompany the purple spots. And when you are in doubt what plan to pursue, make a cautious tentative bleeding. Take away a couple of ounces at a time, into a wine-glass, note carefully the appearance of the blood itself, and the effect of the blood-letting upon the patient; and then go on more boldly, or abstain thenceforth altogether from the lancet, according to circumstances. In many cases your chief reliance will be placed in the watchful employment of purgatives. These have been highly recommended by Dr. Harty, of Dublin, as having proved eminently successful in his practice. The late Dr. Whitlock Nicholl, and others, have spoken in terms of strong praise of the oil of turpentine, administered in moderate and repeated doses, as a remedy in purpura.

I have adverted to one peculiar source of danger in purpura, the hazard that blood may be effused in some vital organ where even a slight amount of hæmorrhage suffices to extinguish life. Dr. Bateman states that he had seen three instances in which persons were carried off, while affected with purpura, by hæmorrhage into the lungs. During the course of one week, in the year 1825, I was present at two inspections in the dead house of St. Bartholomew's Hospital, illustrative of the same point in respect to another vital organ, and involving a question in forensic medicine. The subjects of examination were both of them women of middle age, who had been brought into the hospital covered with purple spots and bruise-like discolorations, and suffering hæmorrhage from the mucous membranes. Each of these women declared that the apparent bruises were marks of beatings received from her husband. One of them became suddenly hemiplegic a little while before she died. Of the manner of dissolution in the other case I am not sure. In both instances a considerable quantity of blood was spread over the surface of the brain, between its membranes; and in one of them blood had been shed also into the cerebral substance, which it had extensively lacerated.

It may be worth mentioning that in one of these corpses there were indications, either of unusually rapid putrefaction after death, or (what I think more probable) of some degree of decomposition even before life was extinct. This woman died in the evening, and the body was examined the next day, twelve or fourteen hours afterwards. A quantity of fœtid gas escaped from the cavity of the abdomen as soon as it was opened, and small bubbles of air were seen to ooze from the cellular tissue of various parts of the body.

Even when incisions were made into the *liver*, air frothed up, as it might do, under ordinary circumstances, from a section of the lungs.

I have no time left for discussing the pathology of these complaints. They are eminently *blood-diseases*. In scurvy the blood is starved of some essential ingredient, which the juice of lemons, and other fresh succulent vegetables, readily supplies and renews. When drawn from a vein the blood is often visibly unnatural. A very small quantity was taken, before I saw him, from the arm of the blacksmith whose case I just now mentioned. After standing for some time, it continued to fill the whole area of the vessel in which it had been received, without any apparent contraction, or separation of serum. On its flat upper surface was a thick, gray, semitransparent jelly, and beneath this there was, strictly speaking, no coagulum, but a black semifluid substance of the consistence of syrup. Huxham describes similar appearances. "The blood of such persons (says he), when it hath been drawn off, always appears a mere gore, as it were, not separating into crassamentum and serum as usual, but remaining in a uniform half coagulated mass, generally of a livid or darker colour than usual, though sometimes it continues long very florid; but it always putrefies very soon." In another place, when describing a particular case, he says, "I found that neither of the portions of the blood that had been drawn had separated into serum and crassamentum as usual, though it had stood many hours; but continued, as it were, half coagulated, and of a bluish livid colour on the top. It was most easily divided on the slightest touch, and seemed a purulent sanies rather than blood, with a kind of sooty powder at bottom."

Dr. Budd, however, states that in some cases of scurvy the separation of blood into serum and clot is as perfect, and takes place as readily, as in healthy blood.

When you recognize the disease as genuine scurvy, and trace a previous abstinence, whether forced or voluntary, from fresh vegetables, the treatment is plain; you must supply the kind of nutriment which has been defective, and support your patient's strength in such other ways as the circumstances of the case may dictate.

And now, Gentlemen, I must needs stop. Here ends my course. And if this were all I had to say, I should say it with something like glee, and you, no less than myself, would rejoice that at length a breathing-space and holiday had arrived. But I cannot feel so when I add that this is the last lecture, not of this course merely, but the last of any kind, that I am ever likely to deliver in King's College. I cannot say this without concern and regret.

I am quite aware that my lectures have been in many respects imperfect. They have been very unequal to my own wishes. But they have been as full, and as carefully weighed, as my broken leisure, and irregular opportunities, and slender ability, would permit. I can only hope that at any rate I have not misled you. If I have been intelligible, if I have drawn such a sketch of a great and difficut subject as may help you in studying it for yourselves, I have achieved my task. I told you, in the outset, that I could not here teach you the *practice* of physic, but only its *principles*. It would be idle for me to speculate further upon the success of my endeavours. You are to be the judges of that matter. Whatever rules and precepts I have laid down, you will soon test by your own experience, and adopt or reject them accordingly. The well-known maxims of Bacon apply with especial truth to medical instructors and their pupils. "Etsi non displiceat regula, *oportet discentem credere;* huic tamen conjungendum est, *oportet jam edoctum judicio suo uti;* discipuli enim debent magistris temporariam solum fidem, judiciique suspensionem, donec penitus imbiberint artes: non autem plenam libertatis ejurationem, perpetuamque ingenii servitutem."

Retiring reluctantly from this place, in obedience to the force of circumstances, there are yet many things to comfort and console me. It is a great satisfaction to reflect that I have never had any serious disagreement with yourselves; have never received any but the most respectful and kind treatment either from my present or from any preceding class. I have reason to thank you—and I do thank you—for the courtesy and attention you have at all times shown me. It is a source of gratification also that I carry with me the good will, as I believe, of my excellent colleagues; and that I go without having for-

feited any of that confidence which the Council first reposed in me as their servant, when they offered me, without solicitation, the chair I now resign.

Had I been a few years younger, unembarrassed by previous official engagements, and somewhat more at leisure than I am, I should have been glad and proud to have attached myself to the new hospital, and to have laboured still in the cause of King's College, and of its Medical School. But it is otherwise ordered: and I will mention, as the last source of consolation in taking leave of you, my conviction that to you my loss (if without presumption I may so venture to speak of my resignation) will be more than supplied by my successor. I know that gentleman well. I know, indeed the world knows, his talents. He was highly distinguished in the Senate House at Cambridge. He has since devoted, and will continue to devote, the powers of a very strong intellect, in the investigation of disease. Dr. Budd is one of the most strenuous cultivators of our science that I am acquainted with: and I am confident—without any affectation of modesty—that he will soon give a much better course of lectures than you have heard from me. That you may prosper in his instruction, and afterwards; that by the humane exercise of our noble calling you may do good in your generation, to others, and so to yourselves; is my earnest desire and prayer. I hope it is unnecessary for me to assure you that I shall always continue to take a lively interest in your welfare individually; and that it will give me sincere pleasure if I shall find any future opportunity of rendering you any service. Gentlemen, I do not like this sort of parting, and I will not further protract the pain that belongs to it; but bid you finally, and most cordially—Farewell.

APPENDIX.*

CANCER: ITS SPECIES OR VARIETIES. SCIRRHUS; ENCEPHALOID CANCER; COLLOID CANCER. ITS MODE OF GROWTH AND DISSEMINATION. HABITUDES OF THE SEVERAL VARIETIES.

I HAVE more than once coupled cancer and tubercle in the same sentence. Though very different in many respects, they are alike in their intractable character and destructive tendencies. Of the two, cancer, while it is happily much the more rare, is also much the more painful, loathsome, and hideous in its consequences. It is to cancerous diseases that the epithet *malignant* especially belongs. Not resulting from any change in the natural textures of the body, but constituting an addition to them, and therefore assuming, usually, the shape of tumours, they are commonly and correctly spoken of as cancerous *growths*. But there are other growths which, by comparison, are *innocent;* which do not imply any necessary destruction of contiguous parts, nor any inevitable danger to life, nor even any marked deterioration of the general health. Such are certain fatty tumours, and fibrous tumours, and osseous tumours. All these last, as their names denote, resemble in their sensible qualities some one of the healthy and natural textures. They have accordingly been styled *analogous*, or *homologous* growths; while cancer and tubercle, which find no counterparts in the sound body, are said to be *heterologous*. Some varieties of cancer are, however, very similar in outward appearance to the substance of the brain; and microscopic observers say that in their minute and original structure there is no perceptible distinction between the most innocent and the most malignant growths; nay, that both agree in their primary corpuscular elements with the healthy tissues of animals, and even of plants.

This very agreement, if it really be so complete, shows that in *classifying* morbid growths we must reject the aid of the microscope, and attend to their grosser and more palpable features. And, inasmuch as cancerous formations have, by some pathologists, been ascribed (very erroneously, in my opinion) to *inflammation* as their cause, I shall scarcely be going out of my way if I state here some of the broad facts which have been ascertained upon this very interesting subject.

Cancer, or carcinoma, considered as a *genus* of disease, comprehends two or three *species*, which present among themselves very striking differences, and of which the varieties have received a puzzling multiplicity of names: scirrhus, stone cancer, medullary sarcoma, encephaloid or cerebriform disease, soft cancer, fungus hæmatodes, colloid or gum cancer, and several more. The simplest division, founded upon the consistence of the morbid growth, is into hard and soft cancer. But the most modern and scientific system recognizes *three* species, viz. scirrhus; encephaloid, or brain-like cancer; and colloid, or gum-like cancer. The physical characters of these three species offer strong points, not merely of difference, but even of contrast.

Scirrhus, as that word implies, is remarkable, in its early stages, for its hardness. It is as firm as cartilage, and creaks when divided by a sharp knife. The surfaces exposed by its division present a glistening, satiny appearance, and a white, or gray, or bluish-white colour. Athwart this grayish and semitransparent substance run opaque intersecting bands, having a fibrous aspect. By strong pressure a thin juice may be made to ooze from a slice of the scirrhous tumour.

Encephaloid cancer is also well named. It is composed, in great measure, of a soft,

* In the edition of these Lectures recently issued, the author has introduced at the commencement of Lecture xiii., some remarks on cancer, which are here inserted in order to render the work complete.

white, opaque, pulpy substance, very closely resembling, both in colour and in consistence, that of the healthy brain. This cerebriform pulp is traversed and circumscribed by fibrous septa, which are sometimes extremely thin and delicate. In both these species of cancerous growth, therefore, there is a contained and a containing element.

The same feature is still more distinctly marked in the third species, the *colloid cancer*, which exhibits the appearance of small portions of a greenish-yellow transparent gum, or jelly, arranged in regular cells. Hence it is sometimes denominated *alveolar* cancer.

You may ask upon what principle structures so dissimilar in their physical appearance have been assigned to the same genus? Why, for these reasons. They are all strictly destructive or malignant forms of disease. Although in any shape they are of somewhat rare occurrence, yet when they do occur, two, or all three of the species are often found to coexist in different organs of the same individual; nay, in contiguous parts of the same organ. More than this: if a tumour consisting of one species be amputated, and a fresh growth springs (as too often it does) from the same spot, this secondary growth is frequently of *another* species. There can be no doubt that all are connected by some very intimate bond of union; and the facts I have just stated suggest the question, whether instead of being different species of the same genus, they ought not rather to be regarded as mere varieties of the same species.

Of all three it has been ascertained, by much and fatal experience, that occurring in any one part of the body they are prone to multiply in various other parts; that they are commonly attended, during some part at least of their progress, with very severe pain; that they are incontrollable by any known remedy; and tend always, sometimes slowly, sometimes with frightful rapidity, to augment in bulk; eating away contiguous parts by their invasion and pressure; breaking out, when near the surface, into foul and repulsive ulceration; producing often the most ghastly disfigurement; and ultimately destroying life. Sometimes vital parts are slowly disorganized by the corroding extension of these tumours; sometimes large blood-vessels are laid open, and death is suddenly brought about by hæmorrhage; and sometimes the powers of life sink gradually under the wearing influence of the disease, and that degeneracy of the blood which it causes or accompanies.

There is scarcely an organ or texture of the body which is not liable to be attacked by this terrible foe: the brain, the eye, the lip and face, the lungs, the stomach, the intestines, the liver, the kidneys, the breast, the womb, the testicle, the bones. But some parts are more often the seat of cancer than others. Among these may be reckoned the female mamma, the uterus, the stomach, the liver, and the testicle.

The mode in which cancer *originates* is uncertain; the modes in which it *spreads* and *multiplies* are better understood. An individual tumour may enlarge by the progressive insinuation of the cancerous matter into the interstices of the neighbouring tissues, which, thus fastening upon, it consolidates. The disease may be communicated, by imbibition, from one organ to another which is in mere contact with it. But how does it come to occupy at the same time, or in quick succession, several separate and distinct organs? This is a question of the greatest interest and importance, and it admits of a distinct reply.

Cancer often makes its appearance in a single spot on the surface of the body; in the female breast, for instance. We see and feel it there while it is yet small, and while the general health of the patient seems to be otherwise perfect. By degrees the tumour increases, and at length it softens in some places; the glands of the axilla become swollen, hard, painful, and filled sometimes with cancerous matter; the tumour breaks perhaps through the skin, and presents the shocking spectacle of "open cancer;" the general health gives way, and the skin assumes a straw-coloured tint. During this process, unless the patient dies prematurely, or the original disease is removed by a surgical operation, cancerous tumours form in one or in several of the internal organs, and give notice of their presence by appropriate symptoms. There is an original morbid growth, and there are subsequent morbid growths; a primary tumour, and secondary tumours; and the latter are *caused* by the former. This is a most important fact, if indeed it be true.

Now Müller has discovered, by means of the microscope, and the discovery has been confirmed by other observers, that the contained matter, in the several species or varieties of cancer, consists of very minute cells, with nuclei attached to their walls, and of granules still more minute, which are supposed to be the rudiments of new cells. It is (apparently)

by the amplification of these granules into cells, and by the development of the nuclei into other cells, and by the growth and evolution of young cells which, in some instances, are included generation after generation within parent cells, that the original tumours enlarge and extend themselves; and it is by the transference of certain of these cells and granules from the original tumour that a crop of secondary tumours are sown in remote parts of the body. The cells, and probably the granules also, are endowed with the power of self-increase and propagation, whenever they find a fitting nidus. Possessing, like the seeds of plants, an inherent vitality of their own, they merely require, in order to germinate, to be placed in contact with some living tissue, wherewith they may form vascular connections, and wherefrom they may draw the materials of their nourishment. Cohering together, for the most part, with but little force, they are easily detached from the parent mass. It is matter of fact that the secondary tumours form most surely and most rapidly when the primary tumour is of a soft kind; and that when they succeed to scirrhus, it is after the process of softening has commenced in the originally hard structure. These germs—which present, in their forms and mode of generation, striking analogies to those of some of the lower animals, as well as to those of plants—these germs are probably carried sometimes through the lymphatic vessels to absorbent glands in the vicinity of the primary growth; but there can scarcely be a doubt that the *blood* is the main channel by which the seeds of this dreadful malady are conveyed from its first to its subsequent sites, and thence perhaps, if life continues long enough, to tertiary locations. The gross matter of cancer is often to be found in the veins that proceed from the primary tumour—nay, in large venous trunks at a distance: so that some distinguished pathologists have too hastily conjectured that it may originate in the veins. You are probably aware that foreign substances, circulating with the blood, stop or are entangled more often in some organs than in others. Minute globules of mercury, where that metal has been introduced into the veins, are found strewed through the substance of the lungs, and of the liver. Pus, received into the blood in phlebitis, is arrested, and forms scattered points of inflammation and abscess, in the same organs; and it is in the liver and the lungs that separate tumours of secondary cancer are most commonly met with. If this be the true theory of secondary cancerous formations, I need scarcely point out to you the urgent importance of the rule which prescribes to the surgeon the *most complete* extirpation of the primary tumour, *at the earliest possible period* of its existence.

Of these primary formations the origin is involved in much obscurity. It seems, however (and this, after what has just been stated, you might expect), that the germs of the disease are capable of being transferred from one human being to another; and even to an animal of a different species. Langenbeck injected cancerous matter, just taken from a living body, into the veins of a dog. After some weeks the dog began to pine away, and was then killed, and cancerous growths were found in its lungs. Several instances have occurred—I have myself known of two—of cancer of the penis in men whose wives laboured under cancer of the uterus. Here it is presumable that the cancerous germs, received upon a delicate and vascular surface, and suffered perhaps to lodge there through neglect of cleanliness, might fasten upon the part, take root there as it were, and grow. One very curious circumstance connected with this subject is, that the frequent contact of common soot seems to have the power of producing cancer. There is a form of carcinoma, affecting chiefly the scrotum, and familiar to surgeons as the chimney-sweeper's cancer. A case is recorded of cancer of the same variety occurring in the right hand of a gardener, who for years had been in the habit of sprinkling soot over his flower beds with his hands.

There are not wanting, then, plausible grounds for the hypothesis, that the seeds of cancer may be introduced, in some way which eludes observation, from without; that cancerous growths are strictly parasitic, and independent of the body, excepting so far as they derive their pabulum from its juices. The difficulties involved in this supposition are not greater (as we shall see hereafter) than those which hang over the source and origin of certain entozoa, whereby the body is liable to be infested. But whether this hypothesis be true, or whether the cancer cells and germs are merely morbid elements of the native tissues of the body, resulting from some perverted energy of the formative process, remains yet to be determined.

From the tables contained in the Reports of the Registrar-General it would appear that women are more subject to this fearful disorder than men, in the large ratio of five to two. It fixes chiefly upon the female organs of reproduction; the mammæ and the uterus.

The mortality from cancer, estimated from due reference to the whole number of persons existing at different ages, increases steadily as life advances.

There are still some general habitudes of the different varieties of cancer, with which I should wish you to be acquainted.

The secondary formations are most commonly of the encephaloid kind, whatever the primary form may have been.

Encephaloid cancer, as compared with scirrhus, is abundantly furnished with blood-vessels: and upon this difference in their degree of vascularity other remarkable differences between the two varieties seem to depend. First, encephaloid tumours generally augment with much greater rapidity, and attain a much larger size, than scirrhous tumours. Occasionally their magnitude comes to be enormous. Again, cerebriform growths seldom happen singly, but occupy several organs of the body at once. Scirrhus, increasing slowly, occurs also in fewer sites; it is sometimes even solitary. More tissues, too, appear to be obnoxious to the soft than to the hard variety.

Now (as Dr. William Budd has well remarked) a large apparatus of blood-vessels, bringing a proportionally plentiful supply of nourishment to the parasitic tumour, accounts sufficiently for its rank and rapid growth; and the same condition, especially when conjoined with *softness* of the parent mass, affords obvious facilities for the liberal dissemination of its germs through numerous returning channels. In fact, the soft varieties alone have, as yet, been found in the veins.

The same multitude of its blood-vessels, and slender cohesion of its component parts, serve to explain another peculiarity of the cerebriform species. Intermixed with, or diffused through, the brain-like substance, there is often to be seen a quantity of extravasated blood: and when the disease breaks out into ulceration, red, ragged, and *bleeding* growths, of a fungous character, sprout rapidly from the open surface. To these *accidents* of cancer the term *fungus hæmatodes* is to be traced. We do not find scirrhus to be the seat of similar interstitial hæmorrhages.

Encephaloid cancer has less tendency to contract adhesions with contiguous parts than scirrhus has.

Of the alveolar variety, which has been more lately discriminated from the others, and less studied, less is known. It occurs principally in the abdomen, affecting the pyloric orifice of the stomach, and the omentum. It appears also occasionally in the bones, and in the breast and testicle. Although sometimes combined with the two other species in the same person, it is often alone, and limited to a single organ. I believe it has not been met with except in adults.

For more minute information upon this subject, so interesting and important both in its pathological relations and in its practical bearings, I must refer you to a very able and elaborate article on cancer, in the *Cyclopædia of Surgery*, by Professor Walshe; and to a shorter but admirable essay on the same topic, by Dr. William Budd, published in the *Lancet*. From these sources has been derived much of what I have now been stating.

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