Illustrations of the inductive method in medicine / by William Murray.

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INDUCTIVE METHOD

IN MEDICINE

BY

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PREFACE.

In this volume I have gathered together a series of papers to illustrate a train of thought which has been running through my mind during the leisure moments of a busy life. Some of these essays excited a good deal of attention (see Sir Geo. Humphreys' address, Brit. Med. Assoc.) at the time they were made public, and they have been frequently asked for by those who read or heard them. Trusting they may help, at any rate not hinder, the progress of Medical Science, I venture with great diffidence to publish them under a title which they may hardly deserve. At any rate I have made an attempt to strengthen the place of Medicine among the Inductive Sciences, and to show that its basis is no longer altogether empirical. All honest work must tend in this direction, and the feeblest effort to secure this end will, I believe, meet with every excuse for its imperfections.

Swinburne Castle,

Northumberland,

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INTRODUCTION.

The philosophy of medicine is the study of the principles which should guide us in cultivating the science of medicine. The study of those principles whereby the large mass of material accumulated by researches in pathology and therapeutics may be arranged and classified and lead to the discovery of some general law or laws.

Impressed by the idea that the time has arrived for many such generalizations I have endeavoured in the following papers to illustrate what might be done by seeking for general principles in the daily occurrences of practice, and although I may have failed to make any great inductions on observed facts, I think I have shown what might be done in this way.

If every practitioner were aroused to a sense of its importance I think an effort of this kind would lead many of us to take a much greater interest in

our work, and it would doubtless result in the discovery of laws and principles of vast importance.

The mind of the profession has been so intent on the discovery of individual facts that the importance of reading their collective meaning has, to a certain extent, been overlooked.

The greatest generalization of pathological facts of the present generation is that of Sir William Jenner in the discovery of typhoid fever, and no better illustration of the value of the inductive method exists. There was not only the observation of facts, but also the idea which strung them together, forming a complete induction.

Were such efforts more common the desultory facts of medicine would doubtless arrange themselves more frequently under the head of general laws.

I urge, then, that each practitioner should be imbued with the idea that he is seeking for such general laws and principles in the daily routine of practice.

Such a pursuit would, I repeat, give a zest and interest to work, and give exercise to the finest of our mental faculties. I venture, therefore, to call attention to the following group of papers as efforts of this kind.

Some of my facts are out of date, and some of

my theories might have been modified, but the effort to generalize a number of observed facts, and raise a theory to account for them or discover the natural forces by which they are produced retains its importance.

I am assured of the importance of this method by finding that at least one of my definitions has been arrived at by one of the greatest thinkers of the age. Herbert Spencer's famous definition of perfect life was in substance anticipated in my paper on the physical and physiological action of medicines, written 25 years ago.

He says: "Perfect correspondence would be perfect life. Were there no changes in the environment but such as the organism had adapted changes to meet, and were it never to fail in the efficiency with which it met them, there would be eternal existence and eternal knowledge."

I say: "The exact adaptation of the vessel in all its parts to those conditions in which it exists for the attainment of certain desired results corresponds to health. To disturb these external conditions or alter the appointments of the vessel leads to disease, and the extent of such alteration or disturbance is a correct index of the amount of disease produced."

Herbert Spencer's definition applies to "Life," mine to "Health," but the *idea* is the same.

By such philosophical definitions of large and general application much may be done for the advance of medical science. Dr. Broadbent, in his address to the British Medical Association, has with great ability and ingenuity availed himself of this same idea, and has shown how much may be done by working out the bearing of such a general truth.

By the definite statement of such truths we are able to see where we are in our detailed researches; and desultory facts and observations, which before seemed to have no connection with each other, are thereby brought into close relationship.

We seem to need a work on the philosophy of medicine to guide us. Such a work as Dr. Whewell's "Philosophy of the Inductive Sciences" would do for medical science what his work did for the rest of the natural sciences. Surely some great mind will give us this ere long, and teach us, not only how to observe, but how to read the meaning of our facts, and generalize them, so as to pave the way for larger inductions, and the discovery of great laws to which the confused mass of facts in pathology and therapeutics would cohere.

My work is only an illustration of the kind of work that is needed; it is merely intended to show what might be much better done and on a larger scale. In the first paper, which was originally an address to medical students, I have endeavoured to lay down the general principles of the inductive method.

In the second paper I have endeavoured to show how the various actions of an important medicine (calomel) are in accord with the laws of osmosis; and in the third paper I show how one of our ordinary ailments (dyspepsia) may be largely due to interference with that same physical process.

My object is to show the importance of referring the action of a medicine on the one hand, and a disease on the other, to conditions which increase or diminish the activity of one of the great natural forces known to prevail largely in carrying on the functions of the body.

The fourth paper, on the self-elimination of poisons, is more an illustration of a general principle than of a general law.

The principle running through the action of poisons and medicines is defined as *self-elimination*, but we are yet far from any knowledge of the steps by which such elimination takes place.

Such efforts to distinguish the existence of a general principle are, I believe, admissible in inductive medicine, and are useful in groping our way to more accurate knowledge.

I need not say that this paper, written many years ago, is not in accord with recent discoveries in bacteriology, although the principal laid down is, to my mind, marvellously applicable to the facts which have been discovered, and, with little trouble, the paper could be modified to meet our newly-acquired knowledge.

The minor papers on "Starving into Health" and on "Regular Habits" are introduced as further illustrations of the method of establishing a general principle or minor law which has a more or less important bearing on a number of observed facts.

They have each been freely commented on by the public press and are, therefore, preserved for reference. I have also added one or two practical papers which are, I believe, original contributions to medical science, and they may perhaps prevent the stigma of "mere theory" from being attached to my more speculative essays. Lastly, I have added, as an appendix, the report of "the first case of aneurism of the abdominal *aorta* cured by the rapid pressure treatment" by way of preserving

a record of that contribution to surgical knowledge.

With this apologetic explanation of my object, I venture to send forth my little work. When we reflect on the development of ideas with which Jenner's discovery of vaccination was pregnant, we are impressed by the import or meaning which every fact established by scientific evidence may contain. Every fact, no matter how simple, is thus more or less the parent of ideas. None are unmeaning, and they demand from us the application of our highest faculties to discover their meaning.

To dwell on a fact until you read its meaning is always a stroke of genius; but it requires hard thinking, and this means hard work.

Looking back on Jenner's discovery of vaccination in the light of our present knowledge, we see it was capable of an interpretation which might have led us on more rapidly to new discoveries. It was there all these years, but the faculty to discover its meaning and discern its lessons was deficient.

THE INDUCTIVE METHOD IN THE SCIENCE OF MEDICINE.

(An address delivered at the opening of a winter session in the Newcastle College of Medicine.)

My subject is the application of the inductive or Baconian method in the science of medicine, including not only its use in researches which advance our science, but also its application to the diagnosis of disease. My plan will be, first, to give you some idea of the nature of this mode of reasoning, then to show how the study of anatomy, physiology, and pathology tends to develop the inductive faculty in the student's mind, and, lastly, to point out its application in the diagnosis of disease. It is a subject expansile enough for a volume, or the thoughts of a lifetime compressed within its pages, but its very elasticity is my excuse for trying to press it into the few brief moments allotted to this address.

Were we to review the history of the inductive method, and glance at the part it has taken in bringing us to our present standard of knowledge,

we should find it has shared its honours with deductive reasoning to a very large extent, so that we may regard these two methods of ratiocination as the levers by which the mind of man has raised up the present body of our knowledge of natural science. You must not, therefore, imagine that these two methods of reasoning are at all opposed to each other so as to exclude one another. They rather minister to each others wants, and by acting inversely they confirm and verify the results at which they mutually arrive. You will find, indeed, that you need to have a firm hold of both these handles of science in order to be successful students of nature. By induction, we ascertain general laws from the observation of particular facts; by deduction, we arrive at particular facts by reasoning down to them from general laws; by induction, we ascend, as it were, from the experience of the senses to the ideas of the mind, from concrete facts to abstract principles; while by deduction, we descend from the ideas to the senses—from causes to their effects—from general principles to individual facts. In short, by induction we investigate in order to arrive at truth, by deduction we reason from a general truth to all the details involved in it.

Illustrations of this difference will abundantly

occur to you as we proceed; for the present let me observe that the mind of man has unconsciously leaned to the use of the one or the other of these methods at different stages of the world's history, different nations have shown a kind of natural proclivity to the one or the other, and very few men have existed whose minds were sufficiently well-balanced to give due weight to each mode of research. It is evident that Plato and his followers sought their knowledge by deducing facts from ideas, while Aristotle and the Peripatetics obtained their inductive inferences by observing nature, and thus were laid the foundations of both methods of research. Down through the dark ages a series of struggles went on in the minds of men as to the superiority of the one or the other, until the rise of the inductive school, as moulded by the hands of Bacon, gave men true ideas of its importance, and showed more correctly the relation it bears to deduction. Since Bacon's day, it is curious that in Scotland and Germany most of the great thinkers have pursued the deductive method, while in France and England the Baconian philosophy has largely prevailed. Amongst the few who have used both methods of research with the greatest success we delight to class John Hunter with Sir Isaac Newton, both of

whom knew how to ascend by successive generalizations to nature's greatest laws, as well as how to follow an idea, or a general proposition, to its utmost consequences, by close deductive reasoning.

There are three grand stages in the process of induction which it will be important for us to note. (1) The first consists in the observation of facts, (2) the second in classifying or arranging these facts as to form generalizations whereby they can be spoken of under one head, (3) and the third consists in the induction of the laws of nature by the formation of well-established theories, or by legitimate hypothesis.

First, then, you must remember that all induction depends upon facts as its basis, and deals only with those facts which have been fully ascertained and well defined. The observation and definition of facts thus becomes the groundwork of the inductive faculty. The facts of science are not, however, allowed to accumulate promiscuously in the field of knowledge, for the mind of man immediately attempts to arrange and classify them after some natural or artificial system of classification. And I cannot too strongly point out to you that these attempts to arrange and classify the facts of science brings into operation another

faculty of the mind very different from that of observation. The idea or plan on which the arrangement of facts is conducted is something supplied by the mind itself, something which does not necessarily arise out of the contemplation of facts, but rather results from that peculiar sagacity or inventive talent which belongs to the genius of the mind. It is the constant application of this faculty to the facts of science which enables men of genius to grasp the idea of their natural relations, and so to form a system for their classification, a head under which they can be generalized as one term.

The systematic arrangement or generalization of facts, whereby they can be spoken of as one term, is beautifully illustrated by the classes and orders of the vegetable world, or by the genera and species of animal life. This constitutes the second step in the inductive process.

In most instances we find that the contemplation of these facts in nature which have been observed, defined, arranged, and classified, is followed by some grand generalization involving the revelation of a universal law. Coming, as the result of much toil, from the mind of some great man who has laboured and thought with great zeal and rigorous rectitude, such a law sheds a

flood of light upon the whole field of science to which it is related. I need only point to the science of astronomy as a proof of what has been done in this way. From the earliest ages the observation of facts was carried on; they were classified, first on one theory, then on another, each more or less true, but failing to grasp the whole truth; Kepler, Copernicus, Galileo, and many others applied conceptions of their mind to explain the movements of the heavenly bodies, and each made some advance, discovered some minor law, or devised some theory which was partially true, but it remained for our own Newton to bring out the grand generalization and reveal the universal law of gravitation. This last step in the inductive process is, as we have said, the region for hypothesis and theory, for the induction of the laws and causes of facts and phenomena —a region only to be reached by those who have been well trained in the humbler schools of observation and cautious generalization—"where the powers of the mind gain their freedom, not as the slave who escapes his master's shackles, but as the freedman, who, by hard work in a subordinate capacity, has obtained his liberty."

Let us now endeavour to see in what manner the study of anatomy will tend to develop in you

an aptitude for each of these steps in the inductive process.

There is no science which has called forth fewer attempts to theorize than anatomy. It is cultivated chiefly by the first part of the inductive method, and you will be unable to understand its classifications and theories until you have mastered the details of practical anatomy by dissection. It is here, therefore, that you will find ample opportunity for the cultivation of your powers of observation, here that your eye will be trained correctly to observe facts, and your memory minutely to retain what you have seen. You will be trained to the most rigorous methods of finding what is to be found, and you will find that every tendency to discover what is not to be found will at once be checked. In the science of anatomy, every step is taken by the most careful observation, not surpassed even by the astronomer who determines with such marvellous accuracy the movements of the heavenly bodies. Each nerve, muscle, and blood-vessel must be carefully scrutinized before you have seen it with the eye of an anatomist; its size, its form, its attachments, its origin, its destination, and its relations to other parts are all to be noted and retained in the mind's eye with the utmost exactitude—an

exercise we maintain well calculated to prepare you for the observation of facts in other departments of your professional studies.

The science of anatomy is not, however, limited to the observation of facts, for in it the various methods of classification have been carried to their highest point, but a full comprehension of these is only to be attained by extending your studies to *comparative anatomy*. You can have no idea of anatomy as a science unless you do this, for its systems of classification as applied to the various organs and their homologues in other animals cannot be understood from a knowledge of the human body only. Human anatomy, as learned in the dissecting room, is only the unravelling of a wonderful mechanism wherein dry details fill the mind to overflowing, until those details are arranged and understood by the light of comparative anatomy. In obtaining this extended knowledge of anatomy, you will find the difficulty of remembering details lessened rather than increased, for you will no longer regard them as isolated facts, but as parts of a grand scheme of organization. As an illustration of the value of classifying facts, I need only remind you of those wonderful inductive generalizations of Baron Cuvier, by which he classified the whole

animal kingdom on distinct anatomical ideas, whereby the facts of anatomy have been bound together and are now capable of being regarded as a compact and connected body of knowledge.

Nor is the science of anatomy devoid of useful and beautifully developed theories betokening the highest order of mind on the part of those who have produced them. To prove this, I need only mention the vertebral theory in skeletal anatomy, by which the bones of the skull and spinal column have been arranged on a distinct idea—an idea started by the genius of the poet Goethe, and worked out to its utmost limits, inductively and deductively, by Owen and Huxley in our own day. By means of this, the vertebral theory, you need no longer look upon the complicated architecture of the human skull as an unmeaning puzzle, nor regard the distinctions of the spinal vertebræ as useless details of knowledge; on the contrary, you can simplify the subtle refinements of the one, and throw meaning into the simplicity of the other, by referring them both to the place they occupy in the grand archetypal vertebra, from whose modifications the whole skeleton is formed. As another proof of the value of an anatomical theory in giving uniformity to facts, I would ask—"What would the revelations of

the microscope have done for biology, unless—in addition to the structure of the individual tissues —it had shown that in each there was an element common to them all?" Without the aid of the cell theory, as developed by such men as Schlieden, Schwann, and Virchow, the anatomy of the tissues would have been as difficult to understand and remember as the words of a language without a meaning attached to them. Such a theory was an induction on the facts of minute anatomy, and, by its aid, every tyro with his microscope can intelligently use his instrument, not as a mere toy for the display of nature's handiwork, but for investigating parts of a grand scheme of organization, obedient to an all-pervading law, where every cell he sees, and every modification of which cells are capable, tends to enlarge his mind by showing him how the Creator of the organized world has proceeded upon a distinct and uniform plan, in obtaining the vast and diversified results of animal life. I need scarcely repeat that in anatomy, as a science of observed facts, extensive generalizations and theories which account for, and throw order into, the facts, you have one of the best and easiest exercises for the training of your minds.

When we pass on to the science of physiology,

which has for its object the study of the functions of organs and tissues, and bears to anatomy the relation which function bears to structure, we find the path of inquiry well trodden by the steps of inductive philosophers, and experimental inquiry has here been followed by wonderful success. As instances of this latter witness the efforts made by Sir Chas. Bell, Marshall Hall, and Brown Sequard to discover the functions of the nervous system, and see how in the hands of each of these experimental inquirers nature has yielded up her secret plans of action; responding to the experiments of Bell by a revelation of the functions of the spinal nerves, to those of Marshall Hall by attesting the truth of his theory of reflex action, and disclosing to Brown Sequard with marvellous accuracy the pathway of the currents to and from the brain. But if you wish, gentlemen, to receive that mental training which such inquiries leave upon the mind, you must not be content to accept on hearsay the mere results arrived at. You ought to read with care the details of the experiments by which those results were obtained. The culture you would receive in such detailed study would do more to enrich the powers of your minds than the most complete mastery over the most advanced text-books of the Schools. In our

day we think of nothing but results, scarcely have time for details, and so miss that deep acquaintance with the science of our profession which would best qualify us to become original observers and thinkers, and best fit us to practice our profession with the greatest zeal and success. I might go on to illustrate afresh the effect of these studies in developing the powers of observation, generalization, and theory, but time would fail me, and I must use the subject for the illustration of that most important part of the art of induction, termed "The Analysis of Phenomena" or the decomposition of the complex phenomena of life into their component parts. Most of our advances in physiological science consist in this, and by it we are able to winnow out from each phenomenon all those parts of it which are beyond our ken and put them down as the "residual phenomena" or unknown quantities in our physiological equation; we thus learn how much of each function is due to the operation of forces with which we are already acquainted, and how much belongs to that mysterious power called vital force.

Let us, by way of example, take the process of respiration, and analyze it. Is it one thing, or many? We shall find the number of elemen-

tary parts into which it can be decomposed is truly marvellous. We can unravel it into the mechanical enlargement of the chest, the filling up of the enlarging cavity by atmospheric pressure, the diffusion of gases of different density in the air passages, and the diffusion of these same gases through an animal membrane by which one gas enters the blood and another is extricated from it; we have further the accumulation of carbonic acid in the blood while circulating through the tissues, with all those chemical changes upon which the production of carbonic acid depends; we have also the disposal of the inhaled oxygen with its chemical results, and lastly we have that nice adjustment of nerves and nerve centres for detecting the presence of carbonic acid in the lungs which leads to the reflex actions of the respiratory muscles in enlarging and diminishing the cavity of the chest. Now all this is the result of the analysis of a phenomenon which was in time past regarded as a mysterious vital entity. Whereas we now find that, as an entity, no such thing as respiration exists, it can be completely broken up into functions in no way peculiar to itself, and we are able to see that, except the contraction of the respiratory muscles and the action of the respiratory nerves, the

whole of it can be referred to the ordinary physical forces of the outside world. Were we to pursue the same method in the case of muscular action and the action of the nervous system we should find that even in their case the unknown vital quantity is mysteriously small, and with the advance of knowledge it will grow smaller still. Such physiological studies as these are well calculated to train you to look beneath the surface for the real nature of things, and they will stimulate you, in examining phenomena, to seize upon all that is capable of being known, and to leave the unknown residuum for further examination.

Physiology has, however, failed to obtain by induction any great law which will account for all the facts of life. No one has yet arisen to propound a satisfactory theory of vital action. We are waiting for this, and we are busily engaged in accumulating materials to be used by him for whom this honour is reserved. That such a revelation is fast coming we doubt not, and we hail its approach as one of the grandest events in the history of science. Some men seem to shrink from the contemplation of such a step, fearing that such knowledge is too wonderful for man—too sacred to be touched by him—but its approach

is certain, and the question at issue is fast becoming very narrow, narrow enough for the comprehension of all of us; what we call vital action, or, in other words, that part of life which we do not understand, must be explained in one of two ways; either its phenomena are due to the operation of some force mysteriously correlated with the other forces well known to operate in the economy of life, or they are due simply to the combined action of the forces with which we are already acquainted in their operations outside the body, forces common to the whole realm of animate and inanimate nature. In either case it is manifest that, however essential this vital spark may be, the part it plays in carrying on most of the great functions is very small. In the case of respiration, as we have seen, it is scarcely perceptible, and so with digestion, absorption, secretion, circulation, tissue change and animal heat, it is almost *nil*. For my own part I doubt whether anything but the functions of the nervous system can nowadays be considered due to something more than the ordinary chemical and physical forces of nature. So that you will see the question at issue is very narrow, and it only remains for some master mind to fall upon the happy idea which will account for all the

phenomena of life, and reduce them to the operation of known laws.

Our confidence is that the development of some satisfactory theory, which in its consequences will account for all the phenomena of life, will do for medicine what the atomic theory has done for chemistry, what the undulatory theory has done for optics, or that of gravitation for astronomy.

The higher region of inductive inquiry is well adapted to the exigencies of physiological science in seeking for the laws and causes of the facts and phenomena of life. Grounded on facts though all physiological laws must be, the horizon of the science is clouded by uncertain speculations, which are a grand stimulant to the imaginative powers; and the building up of physiological theories by a strict comparison of these with facts is a capital exercise for the *verifying* faculty, whereby the consequences of your hypothesis are brought face to face with the results of experience. When, by induction, you have ascended step by step to a hypothesis respecting the functions of an organ, your next step is to submit all the consequences of such a theory to the tests of experience and experiment, and by these data to determine the validity of your hypothesis. Such a procedure is termed "the verification of your induction," and even

when you have not ventured to theorise for yourself you will be called upon to apply this faculty to test the theories of other inquirers, for the progressing science of life is at present supported upon a skilfully arranged scaffolding of theories and tentative hypotheses, demanding most careful and rigid examination by those interested in the stability of its growth.

Pathology is another department of your studies which will shortly engage your attention. It has for its object the study of the causation, conditions, and consequences of disease, and forms, as it were, the stepping-stone between physiology on the one hand, and the bedside study of disease on the other. By studying pathology you will be led to see the battle ground on which the struggle between disease and medicine takes place; where you must meet disease, attack it, arrest its progress, and so dispose of it as to leave the body in a state of health, or where, on the other hand, you must yield to its power. When its hold is too strong or the conditions too favourable to its progress, the treatment of disease will fail in the hands of the best of us, and we must not forget that in the case of every mortal being the medical man must eventually bow before the irresistible majesty of death.

Let us first of all trace the evolution of some of those great pathological principles by which men have been guided in searching after the laws of disease, and then let us glance at the application of the inductive process to the elucidation of the cause of death by post-mortem examination.

When we attempt to analyse the means by which pathology has been built up we find that unusual complications have arisen in the progress of that science, nor is it difficult to account for these, because of the peculiar position of the subject as being the science of that which is abnormal to the body in health—a science of departures from our ideal of perfect life in which the laws of such abberrations are to be studied. When the minds of physicians in the infancy of science searched in vain to account for the phenomena of healthy vital action, we need not be surprised to find that they were utterly bewildered in their attempts to define and generalize disease, nor to learn that they took refuge in ascribing disease to the operation of some lawless entity, instead of seeing that the grounds of inquiry are similar to those of health. At one time a spiritual agency, such as the Archœus of Van Helmont, at another a chemical agent, such as the Tartar of Paracelsus, was said to be the cause

of "all the ills that flesh is heir to." They thus failed to grasp the true idea that in disease the body is not under the influence of forces different from those of health. The dynamic and material conditions are changed (in degree or kind), but the forces are the same. Just as the conditions requisite for the success of an experiment alone determine the result, so the conditions of the grand experiment of life alone determine whether that life shall be health or disease. The laws, therefore, which the conditions of vital action follow when they so deviate as to destroy the success of the experiment, form the true field of pathological inquiry.

In constructing the science, men were obliged to adopt the deductive method to a very large extent, and thus we hear of pathological laws and principles long before the facts of the science warranted any such generalizations. Pathologists speculated freely in every direction by assuming the major premises of theorems which would, in their unfolding, account for the varied phenomena of disease. In this way two great pathological theories were projected as general propositions, and from them the explanation of every disease was carefully deduced. I allude, of course, to what have been called the Humoral and Solidist

theories. The one assumes as its major premiss that all diseases have their origin in the blood and fluids; the other ascribes the sole executive in this matter to the nervous system and the solids. Both Humoralists and Solidists erred in this—they mutilated their problem by suppressing the premises held by their opponents, a mistake fatal to the safety of deductive reasoning in such a science as medicine. The result of all this has, nevertheless, been favourable, inasmuch as we have handed down to us all that could be suggested on the one side and on the other by the master minds engaged in support of their favourite theories, and the theories have served their purpose in stimulating these great minds to search for analogies and facts which otherwise might never have been heard of. (Cullen's Lectures are full of such in his efforts to establish his solidist theories.) Further, we must remember that the lines of argument have been parallel with each other, and that the modicum of truth which is wanting in the one view is often supplied by the other. To this day we are unabled to decide exactly upon the claims of these theories, for, while we are sure that truth lies with both, we cannot as yet say whether the one includes the other, or, as is more probable, they are both included in

some higher law. Let me show you how the difficulty still exists in the case of the cellular and humoral theories of morbid growths. When we endeavour to explain the origin of a morbid growth, such as cancer, by the light of these theories, we find that in spite of all the observations that have been made we are still unable to say whether cancer originates in the blood or in the tissues. The cellular theory holds that it depends upon the morbid action of the tissues in which it arises, while the humoralist affirms that the blood pours out a cancerous exudation, which develops irrespective of local action. In practice we are continually confronted by this difficult question. For every time such a disease presents itself we immediately ask ourselves, "Has this come by the local action of the parts around, or has the blood produced it?" and the treatment to be adopted depends upon the answer we incline to give. If the humoral theory alone be true, what avails the removal of such a growth by the surgeon's knife? but if the cellular theory be true, and the disease has indeed a local origin, we cannot too soon or too effectually remove it. The same remarks apply to most, if not all, such diseases, consumption not excepted, for if tubercle have a local origin, and the system is subsequently affected, its treatment

cannot be the same as if it were but the local result of a crasis of the blood.

Of late the science of pathology has been cultivated on a larger scale, and a truer method than of old. The inductive method has come into vogue, and has caused these theories to become almost obsolete in the minds of men. Facts have been observed, and they have been generalized with great success. First of all, the simple plan of observing all the morbid changes to which each organ and tissue is liable has been carefully followed, and then by a strict comparison of the morbid conditions of the various organs, pathologists have been able to trace the same pathological process under different aspects. The result of identifying the same change as it appears under different aspects in different parts has been a generalization of the most important nature. Tubercle has thus been traced, and the history of its origin, growth, and decay has been thoroughly understood. Albuminoid changes are also being subjected to this search with great success, and almost every morbid product is thus having its entire history traced and reviewed. Even the natural decay of the body in old age or degeneration of the tissues is fast becoming a science in itself.

Let us now see how the investigation of the cause of death by post-mortem examination forms part of the inductive method. Whenever you can divine the cause of death by examining the fragments of the wreck which lies stranded upon the further shore of the sea of life, you do so by an induction upon the facts submitted to your observation. But when you have seen the patient during life, and have formed a diagnosis of the case, you make your post-mortem examination to verify your diagnosis. In the former case you examine the body without any preconceived opinion, and judge according to the facts you observe; in the latter case you deduce all the consequences of your diagnosis, and these you seek for in the body after death to confirm and verify, it may be to reject or modify, your opinion. I allude especially to this distinction because it brings out the difference between the ascending by induction and the descent by deduction to verify your induction.

We have thus hastily glanced at a few points in each of these elementary medical sciences in order to show the principles upon which they are built up. The review, I am painfully aware, is both meagre and superficial, but my excuse is that it aims at nothing more than an illustration of

the extent to which Bacon's method has been followed in them. These sciences do not by any means give that glory to Bacon which the almost complete sciences of astronomy and chemistry have bestowed on him, but we are content to know that his method is being followed, and we confidently anticipate the same triumphant results here as elsewhere.

Although I have not thought it necessary to dwell at length upon the distinctions between induction and deduction, I trust I have said enough to show that while the last is a part of logical argument, the first depends upon the work and sagacity of a philosophical mind. In the former case the material for thought is ready at hand; in the latter it has to be sought for, and then moulded into proper form before it can be used.

The practical object of all your studies is obviously to fit you to detect and treat disease. It becomes, therefore, a most precarious and delicate task to show how these rigidly performed exercises in established sciences are the schools in which you learn the free use of a powerful and correct method of research.

Induction, with deduction as its assistant, is the art of observing and thinking, and, like any other

art, to be skilfully practised must be learned by rule before your own peculiar talents can be thrown into it. To some men the skill for doing this comes almost naturally, and they scarcely recognize the existence of the rules they so intuitively follow; with others there is no other path than that of careful training in the rules of thought. In the case of medical men this distinction comes out in bold outline; one by innate sagacity and rapid intuition forms a correct and decided opinion at a glance, and can scarcely tell you how or why he has done it; the other by careful observation, slow generalizations, and cautious induction, arrives at the same result; "the one has gained the top of the stair at a bound, the other has trodden every step;" but in each case it has been by induction—by the use of the faculty acquired or cultivated in the manner I have indicated in this address. Let us endeavour to trace the steps of the process.

Observation by the senses, the eye, the ear, the nose, the hand, and the application of conceptions of the mind to the experience derived from these observations, form the basis of our knowledge in natural science. What you observe, supplemented by what you infer from your own observations, is the constantly repeated process of all minds in all

the affairs of life. In this respect, at least, we doctors differ in no way from our fellow-men. By way of illustration, let us suppose a case:—"A railway guard (in those miserable days when the companies provided no smoking carriages) looks into a compartment, detects a suspicious odour, sees a thin cloud vanishing from the window, a cigar ash on the floor, and a self-conscious looking traveller in the opposite corner—he at once infers, as a moral certainty, what he has never seen, and the result is a fine not exceeding forty shillings, with the fact well published at the various stations on the line." The guard's theory to account for the facts is enough to convict the unfortunate offender. Now, this is exactly what is done by the medical man when he observes a variety of external signs from which he infers the existence of disease—from the coated tongue, the loss of appetite, and other signs he infers the disordered stomach, which, of course, he never sees, nor needs to see; and thus, in a large number of ordinary cases, the process stops short at this simple stage.

In many cases, however, the task before us is of a very different kind. Cases occur where it is difficult to observe the signs or detect the symptoms, cases where we can conjure up no theory nor known cause to account for what we

see, and where all the pains, labour, and thought of an original discovery are necessary. In all such cases, however, we must proceed on the following method of investigation :—You first learn the history and present circumstances of your patient, you next submit him to a close scrutiny for the signs and symptoms of disease, and then attempt to generalize the symptoms under the head of some known disease. Now, if you cannot classify the symptoms under the head of some known disease, you must search for the truth by forming a variety of hypotheses to account for the case, and when your hypothesis is the right one, in other words when you have guessed the truth, you must prove that it is so by finding that the symptoms of the case agree with those of the disease you have hypothetically assumed to exist; in short, your induction must be verified. To take an example :—A patient presents himself with a bare tongue, loss of flesh, a slight increase of appetite and thirst, and a failure of his strength. To account for these symptoms you call to mind first one disease and then another, until the idea that diabetes may exist occurs to you; you immediately deduce the symptoms of diabetes, and examine the urine to see if your hopothesis is right. The finding of sugar in the urine is the

verification of your induction. In the same way you seek for albumen where you suspect Bright's disease, or sound the lungs where the symptoms suggest consumption. These verifying facts are all-important, and always to be sought for. To repeat, the process of inquiry is this—to guess at a diagnosis, deduce all its symptoms, and search for them to verify or reject it. While standing at the bedside, the mind of the physician is thus continually engaged in ascending the truth, and deducing its consequences so as to test and try it; when one hypothesis fails to stand the verifying test he tries another, and thus he rapidly calls to mind first one disease and then another until he hits upon one which is at variance with none and accounts for all the facts of the case. How consummate is the skill required for this, and how constantly the mind needs to be exercised in its use, only those who have tried it can understand! To be master of this field of inquiry it is absolutely necessary that you should be fully acquainted with all the symptoms of all diseases. Unless you have all these in your mind's-eye and be able to recognize them under every form, you cannot expect to have the nature of a case suggested to your mind; but when fully alive to the import of every symptom, and fully on the alert to detect

these, you will scarcely fail to arrive sooner or later at the truth. At first, such efforts are performed slowly, cautiously, and with difficulty; by practice they become rapid, bold, and easy, so much so that some men can scarcely recognize their existence. There is an anecdote of (I think) Abercrombie to this effect: He was called by a young practitioner to see a patient said to be suffering from a head affection, and without asking a question Abercrombie said to his friend, "Your patient is dying of a kidney disease, and cannot live many hours;" on his friend asking him how he came to that conclusion, Abercrombie declared he could not tell him, nor could he impart his method. The induction was so rapid and unconscious that he was not aware of it.

I cannot conclude this address without adverting to the ultimate result of all inductive inquiries. We begin by the observation of simple facts, and notice the rules of their occurrence—the law of nature which they obey. "But we rest not here; our minds seek for the causes, as well as the laws, of natural phenomena; and even when we have ascended to a cause, our minds are not satisfied. When we can refer our facts to the operation of one or other of the great forces of nature, we still feel that we have only reached a proximate cause,

behind which there are ulterior causes—perhaps even a succession of such. And after tracing the links of cause and effect to their highest point, we are still obliged to ask, 'What is the Power which holds the end of the chain?' Thus we are referred back step by step in the order of causation, but we reach nothing final. The last question answered, the answer only suggests another question still, until we are brought to the idea of a First Cause." We are thus led by induction from the created to the Creator—from the contrivance to its Author—from the law to the Lawgiver. Do not fear, therefore, to follow your inquiries higher and higher in the inductive process; they can only lead you to Him who is emphatically the Truth and the Life of all things.

THE PHYSICAL AND PHYSIOLOGICAL ACTION OF MEDICINES.

It is exceedingly dangerous to draw too close a comparison between the action of a living body and an inanimate mechanism, no matter how skilful, delicate, and perfect its device may be. Even as an illustration such a comparison may mislead the unlearned, by giving them to suppose that the likeness goes beneath the surface; for not in appearance only but especially in internal arrangement is the handiwork of man rude and mechanical beside the simplest organism. If, therefore, the thing itself is beyond comparison, we might naturally suppose that the forces operating in and on an organized being could find no counterpart in, or relationship with, the forces by which other machinery is propelled; but this is not so, for the advance of knowledge tends to show that the forces in action are to a great extent the same in the case of the steam-engine and the human body, or in the floating ship and the buoyant bird. The difference lies in the things acted on, *not* in the agents at work; and it lies

chiefly in this, that the inanimate mechanism of the one offers a field for the operation of one or two forces which lead to a limited number of results, while the living organization of the other offers a field for the operation of almost all the forces leading to an infinite variety of results.

With these precautions we will venture to illustrate what we mean by "The physical and physiological action of medicines," by comparing their effects in the human organization to what takes place in the working of any piece of skilful mechanism which is affected by the ordinary forces of nature. Let us take an ordinary steamboat for our comparison. In it we have internal arrangement, external device, general configuration, and adaptation to meet the various agencies which may be at hand, and to use them by affording a field for their operation. By altering any of these parts of the vessel she may be improved or injured, brought up to or allowed to depart from the standard of perfection; and by altering the character of the propelling forces, or external agents, we may obtain similar fortunate or unfortunate results.

The exact adaptation of the vessel in all its parts to those conditions in which it exists for the attainment of certain desired results corresponds to health;

to disturb those external conditions, or alter the appointments of the vessel, leads to disorder or disease, and the extent of such alteration or disturbance is a correct index of the amount of disease produced.

A truly expressed science of medicine will never be produced until we start from these data, and characterize diseases by terms expressive of the exact kind and degree of their departure from the standards of health; nor will the true actions of medicines, or other aids to health, be appreciated till we know how and to what extent they restore these conditions of health. What is wanted is to trace the steps by which medicines proceed to the cure of disease, and see whether that can be discovered which will enable us to say *à priori* what result will follow the administration of a medicine; not because we have learned this from experience, but because we know what kind of action it will produce, and because we know *à priori* that this action will exactly remedy the lesion which exists. From the days of Hippocrates till now we have gone on accumulating mere empirical facts, and in this way we have sometimes managed to cure disease; but to this very day we have failed to ascertain the steps by which our cures were

achieved; and medicine will hardly maintain a place among the advancing sciences unless physiology dawns upon her darkness and enables us to see more than the mere beginning and end of what we are doing.

In all sciences there are certain facts which are said to be ultimate; facts incapable of further explanation and not the result of any combination of other forces than that which they themselves express: such a fact is the attraction of bodies to the centre of the earth resulting from or the expression of the force of gravitation. Around all the great forces there cohere a large number of these ultimate facts, and when we have traced anything which occurs in the animal economy to its source in those ultimate actions which rest upon a force whose laws we understand and can test by exact experiment, we have traced it to its origin. In the body, however, we constantly meet with processes which are not in accordance with any known law of physics, chemistry, or physiology; so that we cannot expect to go far in the endeavour to explain the effect of medicine on those processes which are as yet beyond the pale of science. Nevertheless it is our duty to try for an explanation in those physical, chemical, and physiological laws which are known to prevail,

and failing here, to wait until these sciences have thrown more light upon the mysterious region of the so-called vital forces.

Seeing that the mysteries of life are every day becoming more evidently resolved into the operation of very simple forces, the prospect of rescuing the action of remedies from obscurity is most encouraging; and it bids us look for very simple explanations instead of shrinking from the effort with awful convictions that no explanation can be given.

Let us try our method on one of our ordinary remedies, and let us take one whose actions have been well observed but never explained. *Calomel*, which I use as a term for any mercurial preparation, effects various changes in the body, and has been called a purgative, a cholagogue, an alterative, general resolvent, deobstruent, etc., etc.

One and all of its actions can, however, be referred to a series of processes which lead to a rapid passage of fluids through the various membranes of the body. Nearly all the absorbing and secreting processes are accelerated by its action, and it promotes the passage of fluids through membranes in all parts of the body. Bringing the action of this remedy face to face

with forces known to prevail in the body, we are struck with the likeness which it bears to the action of the *osmotic forces;* these, by encouraging a mutual interchange of fluids through animal membranes, being pre-eminent in bringing about absorption, secretion, and nutrition. May not calomel act therefore by affording such conditions to the osmosing membranes and fluids as will greatly facilitate their activity? and may not its action be related to the still more complicated series of physical laws which regulate the dialysing properties of these same animal membranes and fluids? conferring upon them not only the power to pass fluids to and fro, but also the power to select what shall and what shall not be disposed of in this way.

From these considerations calomel ought to cause the absorption of material which is deposited in the tissues—and so it does! It ought also to eliminate certain matters from the blood, which it does; and it ought to be a promoter of nutrition, because the passage of matter from the blood to the tissues is another important process dependent on the osmotic and dialytic forces. Is calomel a promoter of nutrition? We affirm it is, whenever the extent to which it induces the passage of

nutrient matter into the blood and from the blood into the tissues is not exceeded by the drain of secretions from the blood.

That there are cases in which it thus acts is certain; it often acts as a nutrient in the child by promoting osmose in the right direction, and in the child it seldom acts on the secreting organs to such an extent as to impoverish the blood.

These double actions in contrary directions are at the basis of all osmotic changes, and in their action in the body we see how beautifully osmosis is adapted to maintain the balance of the absorbing and secreting processes. When the osmotic balance is lost we have disease, and when it is restored by medicine (calomel) we have health.

In ptyalism the action of calomel on the bowel is generally checked, and its osmotic effects thrown upon other secreting organs, such as the salivary glands; the kind of action, *i.e.*, the *direction* of the osmotic current, is, however, the same, for in ptyalism we continually observe the absorption of material from the tissues into the blood. The effect of mercury upon the quantity of fibrin in the blood is not at all adverse to the correctness of our theory; but want of space forbids us to enter on this question here.

We have shown, therefore, that we may have

these trans-membranous currents from the mucous surfaces into the blood vessels, and from the blood vessels into the tissues accelerated by the action of calomel under some conditions; while we may have contrary currents from the tissues into the blood, and from the blood on to the mucous surfaces equally stimulated under other conditions of its action. The absorption of fluid from the peritoneal cavity by the catharsis of calomel or jalap is one of the best illustrations of the action of the endosmotic and exosmotic currents set up by medicines.

To return to our illustration, we have been doing what the mechanic does when he discovers something which alters the speed of his vessel. He investigates the steps by which the alteration is brought about, and finds it, it may be, in something which causes more carbon to be burned, more heat to be developed, more steam produced, or more pressure to be exerted on the piston. Wherever the cause is found the secret of its operation must be discovered, or he has gained in the result nothing more than an empirical fact applicable to a single case; but when he has discovered the real nature of the result and the steps by which it is produced, how immense have his resources become! This discovery once made enables him to apply his

knowledge in a thousand ways hitherto beyond his conception. With the mechanic the steps of his investigation are comparatively easy, but the success attending it calls upon us to follow his example with unwearied exertion. We may accumulate the results of medicinal action for ever without making one-half the advance in our power to heal disease, which would be attained in the thorough understanding of the exact process by which a few of our well-known remedies bring about a cure—and we go further still when we discover the forces which are brought into operation by our medicines, especially if we are fully acquainted with the laws according to which these forces act. This reduction of the complicated action of a medicine to the operation of a single well-known process in Physiology or Physics is the aim of our study of the Physical and Physiological action of Medicine.

The above remarks, I am aware, are suggestive rather that demonstrative, and intended to point out the direction which our inquiries may profitably take.

THE RELATION OF DIGESTION AND DYSPEPSIA TO OSMOSIS.

While pathological anatomy inclines us to seek for an explanation of the symptoms of disease in the structural alteration of the organs where the symptoms arise, physiology directs our attention to remote parts of the system with which the disordered organ deeply sympathises or to which its functions are intimately related. Physiology, for instance, instead of directing our attention to the structure of the kidney (or the eliminating organs) for an explanation of Diabetes, suggests to us the better path of inquiry into the state of the processes carried on in the assimilating organs of the body. Pursuing this latter course of inquiry we may possibly be led to see that the assimilating organs in their turn are *less* frequently than is generally supposed the true source of the symptoms expressed by their disordered action. In short, we may find that in most cases Dyspepsia is not related to the stomach at all as effect is related to cause; no more so than gout is dependent on the state of the big toe previous to an attack of that

disease, or pericarditis on the state of the heart previous to an attack of acute rheumatism. If this be true, even to a limited extent, its immense practical importance in the treatment of gastric disturbance is at once my plea for a patient hearing and a considerate judgment of the crude theory I am about to submit.

Perhaps no organ in the body is so subject to faulty and disordered action as the stomach, even when dieted with food most convenient for it. Many people complain throughout life of the uncertainty of its action; and yet, in numbers of these chronic dyspeptics, a most careful examination of the coats of the stomach after death fails to detect any serious alteration of its structure. In these cases, at least, we are justified in looking elsewhere for an explanation of the symptoms manifested during life. As an explanation of the view I am about to propose let me state, thus early in my paper, that in such cases indigestion does not result *directly* from overloading or overfilling the stomach, but from the stomach having overfilled the blood and from the blood having overfilled the tissues, or rather from the supply of nutriment to the tissues and blood not having been sufficiently exhausted before the stomach is refilled. The stomach, as I have observed again and

again, may be absolutely empty, moreover, it may at the same time be in a perfectly healthy state, and yet, from a want of action in the excreting organs, or from a want of exercise and waste of the nervous and muscular tissues, such a stomach may yield no sense of appetite and fail to digest the lightest meal. On the other hand, a very weak stomach, and a very disorderly stomach, will under certain conditions of the blood and tissues assume most marvellous powers of digestion, and cannot be overfilled. For example, a man usually dyspeptic takes fever, and vomits almost everything administered to him for fourteen days; he then passes the crisis of the fever, begins to crave for food, which may often be given with safety, and in a day or two he eats two or three times as much food as ever he did in his life. Again, a patient has long complained of indigeston, says it is his greatest enemy, and cannot or dare not eat much; but slowly he observes a change in his digestive powers, rejoices in his increase of appetite, and "good digestion waits on appetite," when to his surprise his physician tells him that diabetes is the cause of both his appetite and increased digestive powers. This then is a statement of our cases.

"There are instances in which the state of the

blood and tissues, as dependent on the action of the eliminating organs and of the muscular and nervous systems, is the only real cause of dyspepsia, and digestion is increased or diminished just as the state of the blood and tissues may determine it."

We shall now proceed to show how these conditions of the blood and tissues operate upon the functions of the digestive organs.

The force whose operation largely determines those digestive powers of the stomach above referred to is that which carries on the various forms of osmose in the body. We know well that the osmotic processes maintain a balance of all the vital fluids, preserving to each of them the specific gravity necessary for the office it has to perform. Among the fluids a strict relationship in this respect is thus provided for. The maintenance of this relationship is of course most important in the case of the blood, because it is to and from this central fluid that all other fluids come and go in absorption and secretion. For instance, a very heavy condition of the blood being induced by the removal of a large amount of its watery constituents would lead to a rapid absorption of liquids, and a slow absorption of solid food; while a great waste of solid matter would lighten

the blood and lead to the rapid absorption of fluids of a very high specific gravity, *i.e.*, fluids laden with dissolved food. Further, we may remark that without doubt there is continually going on in the body a kind of ebb and flow of osmotic activity, at one time the processes are most active at the absorbing surfaces, at another at the excreting or secreting surfaces. The tide turns in the one direction or the other according to the state of the blood. When the blood is replete from absorption its contents are at once absorbed into the wasted and hungry tissues, or thrown out of it by the secreting and excreting glands; and, on the other hand, when it has thus become depleted, it presents to the absorbing surfaces of the alimentary canal a favourable condition for their activity. Before proceeding to apply the above consideration to the study of digestion or indigestion, I would advert to the still more intimate relationship the osmotic currents bear to the structural elements of the blood and tissues. No doubt the absorptive power each cell possesses is dependent upon the conditions of the fluids by which it is surrounded; and among the correlated forces at work in cell life *this we speak of* plays its part. A cell is a small osmometer, in which may be measured the rapidity with which nutriment

fluids pass in and waste matter passes out. This osmosing power of cells may be witnessed in the blood itself, when its cells are examined in fluids of various specific gravity; at one time its cells are seen to be distended and round when the fluid is light, while they become flat and shrivelled when they inhabit a heavier medium.

Let us now, for a short time, consider the various conditions of the body recognized as conducive to good digestion, and see whether they are related to that state of blood which we have seen to be favourable to the absorption of food from the digestive organs; and then I will call attention to those opposite states which are unfavourable to good digestion, and point out the condition of the osmosing streams (blood, etc.) under these circumstances.

I have already shown that diabetes is often accompanied by large powers of absorbing alimentary matters, and I have shown that the starved condition of the blood and tissues in convalescence from acute wasting diseases powerfully increases the digestive and absorbing power of the stomach. In each of these cases the state of the tissues and the blood are the real source of the power of the stomach to deal with large quantities of food. In the same category we may place

those cases where vigorous exercise restores the tone and power of weak and sluggish digestive organs. The increased exercise tells first upon the tissues, exhausts and unloads them, creating a demand upon the blood for increased reparative material, and so prepares that fluid to take up larger quantities of food from the digestive canal. This demand for food is so commonly attributed to its right source that I need only advert to it, to point out the true mode in which it acts. Increased oxidation has a similar action on the tissues, and therefore a similar effect upon digestion. Fresh air we all believe brings appetite and good digestion with it. It does so, not by direct action on the stomach, but indirectly through the rapid tissue change it induces. In the same category we may put the man who constantly craves for food and digests it without any increase of nutrition. The craving of the stomach and the rapid digestion of these lean men is constantly accompanied by great activity either of body or mind. We say they are "no better for what they eat," because the supplies received by the tissues are so rapidly disposed of and a fresh demand on the digestive organs is thus so rapidly created; by way of contrast we might instance the case of some stout and lazy people who say

they live on almost nothing, and have a very small appetite. In these the tissues change slowly, and the osmosing current is never sufficiently exhausted to draw largely on the absorbing surfaces. We admit, of course, that in many of these cases there are unaccountable peculiarities forming exceptions to the above rule; but even in these, the large and small appetites often depend chiefly on a natural tendency to rapid tissue change in the one and slow tissue change in the other. The diet used by each individual will of course have another material influence in many cases, but I believe that most people are instinctively prompted by the very law we are speaking of to eat the food most demanded by the tissues of the body. Space forbids me to give further examples of the fact that most of those conditions of health which increase the appetite do so by wasting the tissues and drawing on the blood. Side by side with this we must put the condition of the secreting and excreting or eliminating organs of the body in relation to digestion and appetite. It is needless to go into this part of the subject, for we all admit that a sluggish state of the intestinal secretions, or of the liver, or of the kidney, or of the skin, or of the pulmonary *gaseous* excretions, tends to stop

digestion, while a vigorous flow of these secretions tends to give a powerful and healthy action to the digestive apparatus, and especially to its absorbing functions. Confirmatory of this is the striking effect of eliminative and alterative medicines which do not act on the stomach at all. A good purge, a diuretic, a Turkish bath, or a good supply of ozone in the atmosphere, each promotes the action of the stomach on its contents in the way we have indicated.

We have now to consider the second part of our proposition : " those conditions of the body which are accompanied by small digestive powers are usually related to a diminished capacity for osmosis on the part of the fluids." We may advert first of all to the converse of all those conditions before enumerated as favourable to digestion. Fever, before its crisis, finds its subject laden with non-eliminated matters circulating in the blood, and the supply of these matters to the blood is kept up by the oxidation of tissues which, for the time, admit of no repair. So also with the converse of healthy exercise in repose of mind and body. The digestive powers fail to deal with large quantities of food, and the usual quantities which may have been suitable during periods of exertion most certainly lead to dyspepsia, unless, as is not un-

common, the individual have large capacities for storing up food in the system; then the absorption of carbonaceous matter by the adipose tissues may enable the blood to go on receiving large supplies from the stomach without indigestion. Should this not take place, indigestion will certainly follow. What is called biliousness is frequently this very state of things; the blood is replete, the tissues are not wasting, and will not receive nor store its supplies; a heavy meal is then rejected by the absorbing surfaces with the usual symptoms of indigestion described as bilious. In this state of things a free action of the bowels, or the liver, or a good dose of calomel, which leads (as I have shown in my former paper) to a free metamorphosis of tissues and the elimination of effete matter, at once puts the patient right for a time. Such a person, thus relieved, goes on eating and reposing again till the same state of things ensues, and he is for ever being put right by means of blue pill and black draught, etc. It is our duty as physiologists to point out that this is a very unnatural mode of restoring the absorbing powers of the stomach, and to insist upon a more natural method, either by giving less food or more healthy work. This is the cause of the dyspepsia of nine-tenths of our patients who spend their lives in

offices, in routine work, without mental exertion, and of almost all the dyspepsia of well-to-do people who won't work and will eat. The same rule holds good with regard to nitrogenous food, and with greater force, because it cannot be stored up as fat in the tissues. The influence of diuretics, diet, exercise, and fresh air is its only antidote.

And here let me enter my protest against the aimless cramming which goes on in the treatment of such diseases as phthisis and continued fevers. In the former disease the stomach is often burdened with food which it cannot digest, and an utter failure of assimilation is the result; even cod-liver oil becomes a source of evil rather than good in most cases of phthisis unless its assimilation is fully assured. In the case of fever there is no worse treatment than to overfill the blood when it is already charged with effete matter, which it ought to be relieved of as rapidly as possible.

The several forms of dropsy which represent a backward flow in the exosmotic currents, ending in that which should have been *excreted*, being effused into the tissues or serous surfaces, are always accompanied by an inability to receive and digest food. The vomiting of Bright's disease is doubtless in part due to this cause, and the influ-

ence of any medicine which removes the dropsy is at once seen in improved digestion.

It remains for me to say that there are many cases of indigestion in which the osmotic power of the fluids plays a very subordinate part. It is quite possible, for instance, to have the most exhausted condition of the tissues and a powerful capacity for osmose in the blood without good digestion. Such a condition prevails in phthisis. Again, the various kinds of indigestion are often associated with structural alterations of the stomach, which interfere with the application of the above remarks to those varieties of that disease. But while we admit that in all these cases a subordinate part is played by the osmosing condition of the membranes and fluid, it is a part sufficiently important to be recognized in the treatment of all cases; and we are certain that in many other cases, such as we have enumerated, it plays the *most* important part, and treatment, to be successful, must be specially directed to it. Calomel, and all mercurials, the alkalies, potash and soda, magnesium, salts, purgatives, sudatorial remedies, diuretics, and the various cholagogues, doubtless derive most of their usefulness from their power to increase the absorbing and secreting

powers of the mucous membranes, by promoting osmosis to and from the blood current. If their value does not end here it is because they have a further power of causing the metamorphosis of sluggish tissues and of eliminating waste matter from the blood. (See action of calomel in preceding paper.)

ON THE SELF-ELIMINATION OF POISONS.

THERE exists in the human body a sphere for the operation of almost all the physical forces which are at work in the organic world; and, in all probability, the correlation of these forces with the vital force brings about the life and functions of which the body is possessed. If this be true, we shall find that the general laws which regulate the forces of the outer world apply also to the regulation of the vital economy. To select one of these laws and apply it may test the truth of our remark. The great forces around us do not manifest their power, nor even their existence, unless they meet with appropriate material to act upon; *e.g.*, the chemical forces are without manifest existence until bodies with an affinity for each other give them an opportunity to act; and the electrical force is without effect in the presence of non-conductors. So that to present appropriate conditions and material to act upon is necessary in order to manifest the forces and call forth their effects. In like manner, there are powers in the body, latent, until appropriate material is introduced,

which calls them into action; and we desire to show that when certain poisons are introduced, they afford a material on which the forces of the body act in such a manner as to eliminate the poison; in other but less definite terms, poisons bring into play a *vis medicatrix naturæ* which destroys them. I wish, therefore, to demonstrate that we are often dependent on the poison for producing the very action which best of all eliminates it; and further that, were no such extraordinary action set up by the poison, it would remain in the body until removed by the ordinary processes of elimination, and, while so remaining, would do harm.

The great and almost universal principle of action and reaction finds a beautiful illustration in the effects of poisons on the body and the consequent reaction of the body on poisons.

At the outset, I ought to state that I do not use the word *poison* in a narrow or limited sense. I give it a larger signification than is allotted to it by medical jurist or toxicologist; and I mean by it any substance which is foreign to the body as food or drink, and which has the power of altering or deranging the natural structure or functions of the tissues. I therefore include in the term many substances which we call medicines whose therapeutic effects depend upon a power to poison or

alter the action of a part so as to enable it to overcome conditions which are the consequence of disease. For instance, a diuretic medicine, such as digitalis, is a poison which excites the kidney primarily for its own elimination, but, in so doing, a dropsical accumulation is removed or reduced. The effects of poisons cannot, however, be called therapeutic or medicinal in all cases; they are sometimes purely morbid, but nevertheless they lead to the elimination of the poison. Such, for instance, is the suppuration which follows the introduction of virus, where the poison is eliminated in the pus of the pustules or abscesses which follow. This principle holds equally true in the case of poisons which produce cutaneous eruptions; it is true of the exanthematous fevers, where the eruption carries off the poison; and it is true of blood-poisons of every kind, which lead to inflammation, desquamation from epithelial surfaces, or suppuration. When the poison is eliminated it either comes away as a secretion from a free surface, or it is contained in cells which are themselves discharged. The former holds good with the milder poisons; the latter especially applies to the severe poisons.

We say, therefore, that poisons lead to their own elimination by the action they set up in the cells of

the various tissues; and so acting, they sometimes produce a therapeutic, sometimes a morbid effect; but in either case their action is self-eliminative.

In order to explain this principle, which pervades not only the science of pathology, but also the domain of the therapeutic action of medicines, I shall first describe its application to individual cells, then illustrate by examples its application to diseases depending on poison in the system, and conclude by showing that the curative action of medicine depends upon its operation.

1. What occurs in a cell when a poison acts upon it? This depends very much upon the kind of cell so acted upon, and for this reason we must study the process on three different classes of cells, remembering that in all cells there is an absorption of nutritive matter, which, being applied to the growth of the cell, becomes endowed with the cell's properties. In the first class of cells this process of the absorption of nutritive matter, and the endowing it with vital properties, forms the chief part of the cell's life and work, the nutritive material being retained in the cell till used or oxidized in the performance of its work, and then cast out as waste or decomposed matter. Such cells exist in the nervous system,

and in all non-secreting organs; when a poison acts upon them, and they are stimulated by it, they rapidly fill with nutritive matter with which the poison passes into their interior, they multiply, and the result is what we call an inflammation, with effusion, exudation, or suppuration, as the case may be. It is on this class of cells that poison acts most seriously, because it is so long retained in their interior; hence an especial danger in our poisons being determined to the nervous system. Cells of the *second* class absorb matter into their interior, and, having changed its chemical and vital character, discharge it as the elements of a secretion. The cells of the liver, pancreas, and salivary glands are of this class. When a poison acts upon them their action is increased, whereby they secrete largely from the blood, and, having a special affinity for the poison, they withdraw the poison from the blood, and throw it rapidly out of the system. When our poisons act on the secreting organs they are therefore somewhat rapidly eliminated. The *third* class of cells is purely excretive; they simply withdraw materials from the blood into their interior, and pass them on without change as the constituents of an excretion. The individual elements of the excretions pre-exist in the

blood, and are simply filtered off by these excreting cells. The cells of the kidney, the skin, and lower bowel are of this kind; a poison entering them excites them to increased activity, whereby more poison still enters, and keep up in them the eliminating process until the poison is exhausted. For two reasons it is fortunate when our poisons select these cells for their chief seat of action—First, a desquamation of the skin or renal tubes, or a large discharge of epithelium from the bowel, is a slight matter compared with similar cell-changes in the brain; and, secondly, when once a poison is eliminated by the skin, kidney, or lower part of the bowel, reabsorption of it can but slightly occur. The practical conclusion of all I have said amounts to this: we are to endeavour to determine the attack of our poison to the organs with excreting cells—the skin, the bowels, the kidney, and to promote the activity of these organs as far as possible within the bounds of health, and, when one excreting organ fails, or is unequal to the work, to excite by medicine the eliminating powers of the rest.

When the stimulus of a poison is applied moderately, we get increased activity in the functions of a part; when the stimulus increases, we get various degrees of cell-multiplication, ending

in the production of exudation-cells, pus-cells, and other departures from the normal standard of cell-development.

2. Passing to the second division of our subject, let us now consider the application of our law to those diseases which depend upon the presence of a virus in the system. We often gather valuable information by observing things under their simplest aspects first. We shall, therefore, regard the effects of poison or virus introduced through the skin by the ordinary nettle or by a bug-bite. We have first the elevation of the epidermis in white patches, and around these considerable vascularity of the skin; the white patches consist of epidermic cells in a state of great activity and tumefaction; and we find that, after the lapse of a certain length of time, the pain, swelling, and poison, with more or less desquamation of dead epidermic or epithelium cells, have disappeared; in short, the excessive action of the cells has disposed of the poison without its entrance to the system. As a rule, we shall find that where poisons excite great local irritation, with excessive cell-action, the constitutional effects of the poison, *cæteris paribus*, are diminished.

We adduce as our next illustration the exanthematous fevers, which, we admit, depend on the

introduction of a special virus. This virus, having found its way into the system, is the occasion of a series of processes which have for their object the elimination of the poison.* In the exanthemata we have, with the first symptoms, rapid growth of epithelial cells in the skin and kidney, and, according to the able researches of Dr. Fenwick, in the mucous membrane of the digestive canal; from each of these excreting surfaces epithelial cells are discharged, laden with granular matter, the result of their increased activity; each cell having performed its quota of eliminating work, brings away with it a portion of eliminated poison. In these active exertions of the eliminating organs it is not so much a new kind of action as an increased degree of that same action which constitutes their normal function. An extraordinary amount of effete matter is circulating from the increased waste and oxidation going on in the system, and the poison is stimulating the cells to their increased work. In measles, the respiratory mucous membrane joins

* And I affirm that these salutary processes, which we call symptoms, are as dependent on the poison for their production as those other symptoms in connection with more vital organs, which seem to have no beneficial result. In fact, the disease in these cases leads to its own cure, according to a beneficent law, by which poisons set up that very action which will lead most rapidly to elimination.

with the skin in the work of elimination. In small-pox, the virulence of the poison sets up still more decided cell-changes, and elimination does not take place till the cells have reached the pus-cell stage. In each and all of diseases, when they run a favourable course, we doubt not that the right organs for elimination are selected by the poison, and the right kind and degree of cell-activity is set up for the most effectual disposal of the poison. Let us remember that, as for the body in health, so there are laws which regulate the body in disease, and it is for us to watch lest anything disturbs the laws which govern these natural efforts. When they are too slight let us encourage them; when too active for the endurance of other important vital functions let us try to modify them, and let us try to remove any hindrance which may prevent them following the course which makes for health. We cannot regard these eliminative efforts as the result of some instinctive power which resides in the body to meet emergencies; they must follow the poison as effect follows cause, and, though not so clearly seen step by step, I believe they in no wise differ in principle from the production of hydrogen gas by adding sulphuric acid to zinc filings, or

carbonic acid by adding it to chalk. In the former case the poison is doubtless as distinctly the excitant of its own elimination as is our strong acid the cause of carbonic acid or hydrogen being set free.

Passing to another class of diseases depending on the introduction of a poison, we have specific disease, dissecting wounds, and poisonous bites, which affect the whole system. In the *worst* of these cases the poison does not produce local effects with profuse suppuration and discharge, and the organs chiefly affected at first are not organs of excretion; thus ulterior and more serious processes are required for the elimination of the poison. In the first case I regard the inflammation of an inguinal gland as a process set up by the poison, which will lead to its own elimination; and I regard the suppuration as containing a large quantity of eliminated poison.

Before leaving those diseases which depend upon a poison introduced from without, I beg to refer to cholera as a good illustration of our theory. The chief symptoms of cholera are evidences of activity in the eliminating organs. The poison has been introduced, and intestinal discharges are really efforts to throw out the poison. The

patient dies when the elimination ceases, or when the poison is too strong for the eliminating process.

In the study of bacteriology we find many striking instances of the application of this theory. When a living poison in the form of a micro-organism is introduced into a healthy system it sets up a complex set of processes, the tendency of which is to destroy the invading microbe. Microscopical examination of the spreading edge of a patch of erysipelas shows that here a struggle is going on between the micrococci which cause the disease and the phagocytes, both of which are present in great numbers. The micrococci endeavour to invade the surrounding tissues, while the phagocytes strive to prevent their advance by destroying them. If the microbes are the stronger many of the phagocytes are destroyed, and the disease continues to spread by the multiplication of the micrococci. If the phagocytes succeed the micrococci are destroyed, and the progress of the disease is arrested. The inflammation, which is accompanied by the escape of the phagocytes from the blood-vessels, is probably excited by substances excreted by the microbes themselves acting as an irritant. Thus the invading organism, by exciting inflammation and the accumulation of phagocytes

in the tissues, brings about its own destruction and elimination.

Then, again, we know that pathogenic organisms are continually excreting substances which, if allowed to accumulate, arrest the development of, and finally kill, the organism itself. In this way, also, those living poisons are continually striving to eliminate themselves.

If I were asked to prove that poison is thrown out by the morbid process it sets up, I might adduce the vaccine pustule which is produced by vaccine virus, and from which vaccine virus is discharged. In such an instance the thing is clear, a poison comes in, cell action with inflammation is set up by it, and this very process reproduces or brings out the poison. Another instance, equally clear, is to be found in gout, where uric acid pre-exists in the blood, sets up a morbid eliminative process in a joint, by which the acid is eliminated from the blood into the joint, and thus the blood is purified. The beautiful experiments of Garrod, showing that fly-blisters over inflamed joints in gout eliminate the poison, give us a valuable practical hint here. Doubtless, rheumatism has its poison thrown out in a similar manner, and when these poisons are known to have a strong affinity for vital parts, and are

liable to be eliminated by them, rational medicine comes in and diverts the action of the poison to the great eliminating organs, the skin, the kidneys, and the bowels.

3. I must conclude this somewhat lengthened paper by showing that the effects of many of our medicines depend on their powers of self-elimination. They set up an eliminative action, and thus carry with them out of the system large quantities of the secretions of the organ on which they act. It may be well to illustrate this by referring to medicines which act upon each of the great secreting organs. It will be necessary to show, in the case of each mineral medicine, that its mineral base, at least, appears in the secretion of the organ on which it acts. With organic substances this is not necessary, as decomposition of them often occurs in their passage through the eliminating organs; such, for instance, is the decomposition of benzoic into hippuric acid, and such also the decomposition of vegetable acids, which pass out in urine in combination with the alkalies as carbonates.

Looking to the liver first, I will take as an illustration the salts of mercury as the great promoters of biliary secretion. Do we find calomel in the secretions of the liver? Most decidedly; and we find it in the secretions of

every organ whose activity is increased by it. In the case of the liver, we would say that between the mercurial particles and the hepatic cells there is a special affinity, which brings into play a force or forces by which the calomel is withdrawn from the blood. The greater the quantity of calomel, the stronger becomes the process for its elimination; and the osmotic forces, finding appropriate material to act upon, and acting through the animal membranes as through a dialyser, select for their action those substances which make up a secretion, and with them mercury is eliminated. The mercury gives, as it were, a fillip to the hepatic cells.

Again, do we find our diuretics eliminated by the kidneys. Most assuredly. To name a few, we find antimony and nitrate of potash so eliminated; so also with juniper, turpentine, and uva ursi. The acetate and other salts of potash having a vegetable acid in their composition are eliminated as alkaline carbonates. Many organic diuretics are decomposed in passing out of the system; but some still preserve their entirety, and appear in the urine. We have here, then, another instance of the secretion of an organ being increased by a class of substances which it eliminates from the system.

Sudorifics act in the same manner on the skin,

because they are partially eliminated by the cutaneous surface. To take sulphur as an example, we find it opens the skin, and is thrown out by the skin. I might here advert to the great advantage of combining these self-eliminating medicines with specifics. By so doing, we excite the cells of any one eliminating organ on which we may wish the specific to act.

I have found, by crucial experiments, that when arsenic is given it often fails to cure a skin-disease until sulphur is given with it to determine its action to the skin.

The action of iodide of potassium on the salivary gland is peculiar, inasmuch as the salt being swallowed with the saliva is reabsorbed, to be again in part eliminated by the saliva, until entirely removed by the kidneys. That this is practically true is proved by the fact that iodine may be detected in the saliva for several days after taking a single dose.

The same is more or less true of all poisons which are eliminated into the digestive canal by the salivary glands, the stomach, liver, pancreas, or small intestine. They are eliminated and re-absorbed, and again eliminated, till wholly removed by the purely excreting organs. In this we see another reason why we should

determine as far as possible the action of poisons to the skin, kidneys, and lower bowel. Some poisons are eliminated by the stomach, and unless vomited are very liable to reabsorption; *e.g.*, arsenic injection under the skin is often eliminated by the stomach and may be detected in its contents; and when this occurs in those animals which cannot vomit because of the shape of the stomach, the effects of the arsenic are greatly increased because it is reabsorbed.

Finally, aperient medicines for the most part do not act by their local effect on the coats of the bowel, but, entering the blood, they are eliminated by the intestinal glands, and carrying with them the watery parts of the blood and mucus with epithelium, they produce watery evacuations.

The issue of my paper leads me to look upon the symptoms of diseases depending on a poison in the system as of the same nature and in the same light as I regard the action of medicine or effects; and I am compelled to regard them both as salutary. *I look upon the diarrhœa of cholera and typhoid, the arthritis of gout and rheumatism, the cutaneous eruptions of the exanthemata, or a suppurating gland from a dissecting wound, as being the effects of poison no less salutary in their*

tendency than the purging of calomel, the diuresis of antimony, or the sudatorial effects of sulphur. They are each and all our friends, and they invite us to unite with them in driving out our common enemy, the poison ; and they tell us to interfere only when our experience shows us that without us they cannot perform their work, or can perform it only in such a way as to seriously injure our patient. Surely, then, the highest office of the physician is to watch with careful eye lest the eliminating processes are carried too far for the strength of the patient, lest the eliminating organ should be injured in the work, and lest the poison fail to operate upon the organs best calculated for its elimination.

ON STARVING INTO HEALTH.*

The natural history of disease, and the accidental events which occur in illness, not unfrequently reveal lessons for our reading, which are both novel and instructive, and the cultivated powers of observation may often detect a hidden law in the casualties or peculiarities of a case. The fact that a patient may occasionally be starved into health has been illustrated by my experience in several cases, and the starvation which was looked upon at the time as a misfortune has proved to be the salvation of the patient. I feel sure that a like experience is not altogether uncommon, as several eminent medical friends of mine have confirmed the views I am about to express in this paper. The principle for which I contend has been more or less put into practice in the treatment of a number of local and diathetic conditions. For example, the starving of certain tumours, notably fibroids, by lowering the diet, and withdrawing fleshy food, has been regarded as excellent treatment. The same idea has pre-

* Read before the Clinical Society of Newcastle-on-Tyne.

vailed in the treatment of aneurism, and the withdrawal of certain articles of diet in gout, diabetes, and obesity, has been the universally accepted treatment for these conditions; but the practice of absolute starvation, or the entire withdrawal of food—both solid and liquid—has, as far as I know, neither been adopted nor advocated, yet, as I shall show, it is possible to save a patient's life by this method.

Let me state here that the process of blood-letting in many desperate cases rested to a great extent on the principle of starving the disease or the patient. One of my cases will strikingly illustrate the advantage of starving and blood-letting in combination; and in contemplating this case we shall, I feel sure, pay a tribute of respect to the memory of those great masters in medicine who treated their patients by bleeding and low diet. The first case to which I draw attention was a severe attack of apoplexy, occurring in a stout, plethoric man, who had indulged to excess in eating and drinking for many years. The patient was leeched, purged, and blistered very severely, but in spite of this heroic treatment he gradually passed into a coma so profound that the administration of a drop of water was a matter of the greatest difficulty. This state of things con-

tinued for fourteen days; all hope of recovery was abandoned, and further treatment was considered useless or impossible. During this period the patient's huge body began to shrink, and a considerable reduction of his bulk took place. At the end of fourteen days of absolute abstinence, the patient's consciousness returned, his respiration gradually ceased to be stertorous, swallowing became easy, and the paralyzed limbs began to move; in short, the patient made a rapid recovery, and he lived for several years with but slight traces of his terrible illness. His recovery astonished all concerned—doctors, nurses, and friends, by whom he was given up as a moribund, or, at least, a hopeless case. I think it is fair and legitimate to infer that this man's life was saved by his fourteen days' abstinence, and that the cause of his recovery was the rapid absorption of redundant matters which took place during this period. The enforced starvation in this way doubtless removed the brain pressure which threatened to kill him.

My next case goes still further to illustrate the effects produced by the removal of redundant material from the body. Abstinence from food is at least a negative method of starving the body; blood-letting is a more positive and rapid method,

and the two combined are of course by far the most powerful factors we at present possess for reducing the volume of a patient.

A celebrated sculler on the Tyne retired from the active pursuit of his profession and became an innkeeper. His new mode of life, accompanied by good living and want of exercise, soon changed a lithe active fellow into an enormously fat man. Nemesis did not leave him here. His excessive obesity was in due time followed by general dropsy. His heart and kidneys gave no sign of disease; in fact, there were no pathological conditions, but excess of fat, to account for his dropsy. The case resisted all the ordinary remedies for dropsy and the strictest dieting for obesity. He consulted several eminent local practitioners, and had exhausted every effort to obtain relief when it was deemed advisable by his attendants, Mr. Henry Newton and myself, to puncture his enormously distended scrotum. This was done by my advice with a lancet point, and the scrotal veins were accidentally and unavoidably divided. Owing to the immense depth of the dartos prolonged bleeding took place before it was finally stopped by tying the veins. In this way the patient lost a very large amount of blood, and when he was rolled into his bed he was in a state

of profound syncope. For several days after this he lay unable to move or speak, or receive more than teaspoonfuls of liquid nourishment. In short, he passed through a period of almost complete starvation after his severe bleeding. During this time his huge and almost bloodless body began to shrink rapidly, and the ultimate result was this—that in a few weeks the man recovered his health; the dropsy was gone; his fat had disappeared; and he was almost the same lean, lithe man he had been before he took the public-house. "He became the victim of obesity, and he was bled and starved into health." The exact progress by which the above results were brought about seems to be, first of all, a reduction in the volume of blood in the body, thus relieving the heart and circulating system; then comes an absorption of extra-vascular matter, according to the laws of osmosis, thus relieving the tissues and organs of their too abundant supplies; and, lastly, the adipose tissue is consumed as carbonaceous food. The clogging of organs by pressure is thus accompanied by a reduction of the weight of fat they have to carry, and increased freedom in their action is secured; thus the heart relieved of its load no longer maintains an unequal struggle in carrying on its work; the descent of the diaphragm

into an empty abdomen is free and easy; the lungs expand more easily into a vacant mediastinal pace, and the return of blood through the portal system is no longer interfered with by mesenteric fat and loaded intestines.

Time fails me to give details of other cases which have more or less illustrated these somewhat startling effects of total abstinence, as I wish to say a few words on another subject in close relationship with it—viz., the starving, blood-letting, and mercurial treatment of heart disease. I refer especially to this treatment in cases of dilated heart, with or without hypertrophy of the ventricles, and to cases of mitral regurgitation with similar dilatation and hypertrophy. In such cases we often see the consequent dropsy resist treatment by purgatives, diuretics, cardiac stimulants, such as digitalis and strophanthus, and the various preparations of iron. What are we to do for a patient in these straits? There still remain blood-letting, abstinence, and mercury.

Blood-letting I will set aside with the following statement:—I have seen cases where the patient was reduced to the last extremity of suffering from dyspnœa and dropsy get complete relief by withdrawing a pint of venous blood. In treating these cases we often begin at the wrong end—we

try to strengthen and stimulate the heart when we should cure its burden; instead of whipping the flagging horse we should take off his load, and thus make hard work light and easy. I will here quote a case showing the marked value of this mode of treating a case of cardiac dilatation with dropsy by means of mercury and abstinence, the latter being in this case an unavoidable consequence of the patient's condition.

A schoolmaster, who was afterwards celebrated for introducing the halfpenny porridge breakfasts at Wallsend, returned from treatment in Edinburgh in the following condition:—Heart sounds scarcely audible; pulse at wrists scarcely perceptible, anasarca of the extremities, ascites, and effusion into the pleural cavities; breathing intensely difficult and distressing—in short, he presented all the worst symptoms of the last stage of heart disease and dropsy. I saw him several years ago with my friend, Dr. Wilson, at Wallsend, and suggested that we should abandon the previous treatment by cardiac stimulants, etc., and treat him with full doses of mercury by way of draining his congested organs. He was put on five grains of blue pill three times a day, and a jalap purge every other morning. The effect of this treatment was so marked that in six weeks I

met him in Newcastle in fairly good health. Further, he has remained in good health for several years, not having taken any cardiac tonic or stimulant, but frequently taking his blue pill when his heart troubles him. I am aware that since this case occurred the treatment of cardiac dropsy by mercury has been advocated by Jendrassick and other Russian physicians, but the treatment has made very slight impression on the professional mind of this country, and I am anxious to urge its importance. With reference to its mode of action, I think we must admit it acts chiefly as a diuretic, but not as a diuretic only, for its action on the liver, on the intestinal secretions, and on all the osmosing currents, doubtless contributes largely to the effects of calomel and blue pill in cases of cardiac dropsy.

Such fragmentary evidence as I have produced falls very far short of absolute proof of the truth of the views I wish to advance. New light on the treatment of disease is often faint and flickering at first. I think, however, my cases indicate the possibility of pushing the starving treatment with success in several conditions where the patient's life is in extreme danger, and where other treatment has failed. The risk attending this treatment is not great; in suitable cases it is

nil. The chief difficulty is in selecting suitable cases for it, as its indiscriminate use is of course hazardous. Starvation in a healthy body under suitable conditions has been proved by Dr. Tanner, Succi, and others to be comparatively free from danger, even when pushed to extremes, far beyond those I have detailed to you. My own experience has shown me that when the body is at rest in a suitable temperature, it can bear starving without much harm, and often with much benefit. This mode of treating some forms of dyspepsia and congested liver has been indicated by me in the paper "on osmosis in dyspepsia," and further experience has fully confirmed my views. Instead of purging a bilious patient, starve him; instead of using colchicum and saline aperients for gouty patients, starve them; instead of ordering violent or prolonged exercise for the man who is putting on too much fat, let him rest and starve him. It is of no use to tell a man to take more exercise for reducing his fat unless you can make him eat less too, and he can't take his exercise unless he takes adequate supplies of food. Starve him first, and get him accustomed to less food while he is resting. By this means his weight and size are reduced; and the need for large supplies of food

is also reduced. My experience goes to show that the process of starving is not nearly so trying as is generally supposed. In truth, the patient often experiences great relief after he has endured the first pangs of hunger from the withdrawal of his usual supplies.

I can well imagine that some of you are inwardly remarking that my views are extravagant and extreme, but the full swing of the pendulum of medical opinion is needed before we can arrive at truth. Thirty years ago we were at the opposite pole—patients were stimulated and crammed with food, and yet, as I can testify, they died of exhaustion faster than if they had been starved, and this cramming system was itself a reaction against the bleeding, purging, and low diet of still earlier days. The truth is the vista of medical experience is a very long one, and in order to complete the perspective we need to have every shade of opinion brought into full relief. As I understand it, those who belong to this society come to its meetings not so much to teach as to learn; I shall, therefore, be glad to know how far your experience confirms or refutes the ideas I have endeavoured to lay before you.

It now remains to us to consider first, the class of cases to which the fasting system is applicable;

and secondly, the mode or method of its application. I have already indicated its use in cases of apoplexy, obesity, and dropsy, whether from heart disease or obesity. Other cerebral affections somewhat allied to apoplexy, such as cerebral congestion and all kinds of brain pressure, and serous effusion into the ventricles, seem likely to be relieved by a prolonged abstinence, especially total abstinence from fluids, so as to increase the density of the blood, and thus cause it, as it were, to suck up or absorb such supplies of extra-vascular fluid as would be found in the ventricles, in the meshes of the *pia mater*, or in the arachnoid space. Congestion of the liver and portal congestion with turgescence of the vessels of the stomach and intestines are materially relieved by fasting more or less severe. In such cases the treatment by alteratives and purgation is often temporary and transient, as the patient after the action of these medicines feels exhausted and hungry, and returns freely to a diet which re-engenders the disease. The effect of a total alteration in the food supplies, with, or even without, medicine, is of a much more permanent nature, and is, I think, more rational treatment.

I now proceed to consider the second point—viz., its mode or method of application. The

cases I have quoted were cases of necessity; nature and accident performed the experiments in spite of us. I should long have hesitated to adopt such heroic steps, however desperate the patient's condition, but with these facts before us I think we are justified in pushing this system to extremes in all such desperate and apparently hopeless conditions. In cases where there is hope of recovery, we ought, I think, to adopt a tentative method at the outset, and watch our patients carefully to see how they bear the entire withdrawal of food. Serious failure of the heart's action, a tendency to syncope, or a feeling of great exhaustion, are of course to be carefully noted, and measures used for their relief—such as a few doses of alcohol or ether. The great point is to get the patient accustomed to do without food, and to cause the system to fall back for its food supplies on the superabundant quantity of fat, fluid, or blood stored up for use. Some such change as occurs in a hybernating animal is to be established. In the case of hybernating animals we have this very process or experiment constantly occurring in nature. An immense store of carbonaceous matter, and doubtless of all other needful nutritious matter in superabundance, is laid up. When this is done the animal ceases to

take food, and begins forthwith to exist on itself, and during a long winter it is starved back into a lean, lithe, active creature. This lesson in natural history is our clue. We are to select cases of disease depending on an excess of material in the body, and we have nature's authority for treating them by fasting. One word of caution—avoid this treatment in all forms of anæmia, phthisis, and degeneration of organs or tissues, and weigh well the general conditions of the patient before you adopt a mode of treatment, which I admit is as potent for evil as for good, unless it is suitably applied.

THE DANGERS OF REGULAR HABITS.

This paper consists of an address given at a Conference of the Church of England Temperance Society. Its introduction here is due to the extraordinary amount of interest and bitter opposition it excited among the advocates of total abstinence in the North of England. For my own part, I can, at this date, see nothing in it to excite alarm, as the whole bent of the argument tells far more against than for the general use of alcohol.

As the paper is based on a general principle or law which affects social life to a very serious extent, I have not hesitated to class it among my efforts; at the same time the subject is hardly treated as a purely medical question, for the occasion of its delivery demanded that the subject should be treated in a somewhat popular form. Medical men will, however, recognize the existence of the principle I indicate, and will no doubt find many more important applications of it in studying the causation of disease.

I ought to explain that by the term regular

habits I mean habits which are repeated day by day in a monotonous routine of life without a break in their uniformity. The word regular has many meanings, and it is necessary to define what is meant by it in this particular case.

I propose to refer especially to the bad effects of "regular drinking habits." A few general remarks on the effects of regular habits in respect of eating, sleeping, and exercise, will enable us to understand more clearly the principle I wish to lay down, and to see its bearing on this all-important question of the regular use, and the regular (or complete) non-use of alcohol.

I have often had occasion to doubt whether the secret of good health depends, as is generally supposed, on the regularity of a man's life.

Regularity as to food, drink, sleep, exercise, and daily work is so often spoken of, even by medical men, as the secret of health, that we scarcely dare to doubt the truth of the axiom or to glance at the possibility of evil consequences arising therefrom.

The tendency of long-continued action in the same direction is to bring about results proportionate to its duration; and the question to be discussed may be stated thus:—

Are not the consequences of the unaltered action of too regular habits sometimes evil?

Is there any effect of unaltered or regular action which can be pushed to an extreme without some evil consequences arising therefrom?

Is the human economy, after all, like other machinery, worn out by constant friction, or has it in itself such reparative power and adaptability as will enable it to go on the same undeviating track without harm?

Or the question might be put thus: "Would it not be the better for a change of that which, for the time, is regarded as best for it?"

Let us glance first of all at the effect of regularity in the use of food.

I need hardly say that to live on the same diet day by day, in childhood, manhood, and old age, winter and summer, would be both absurd and disastrous.

But what of a man who, of his own free will, determines to lead a very regular life in respect to diet?

He determines to breakfast at eight, eat a biscuit at one, dine at seven, and nothing will induce him to change his habits. I have known such a one carry out his plan until his weak heart no longer tolerated the long fast, and a fatal attack of syncope was the result; and I have known another go on thus encumbering his diges-

tive organs with too great a load night after night, until equally serious results in another direction were developed.

Would not these men have lasted longer and enjoyed better health if they had from time to time exchanged the midday biscuit for a more substantial lunch, and let it be unnecessary to load the system with food so late at night?

So, too, with regard to sleep, the habit of being regular may not be exactly suited to the case; one man may be sleeping night by night just short of his needs, and in the end he breaks down by finding he cannot sleep at all.

He would have been saved by a very free indulgence in that very useful commodity from time to time; in fact, by breaking his too regular habit. Or the system may be rendered sluggish and lazy by too much sleep. Every function flags; the man thinks he cannot work, he cannot eat with appetite; when one fine morning he is called from his sleep, and compelled to put forth an early effort, he is surprised to find that he eats with a most unusual zest, and feels a new energy imparted to all his faculties. A sluggish dyspeptic is often best treated by being dragged from his bed occasionally.

Even daily exercise may have in it too much

routine. To a man who takes moderate exercise daily, it is a great boon to extend the daily constitutional into a long walk, where with fresh air his tissues may be thoroughly changed and renewed.

One of the healthiest signs of the times is the recognition of the importance of this principle; hence the vigour of our young people is renewed by manly exercises, and they are better able to resist the depressing influences of town life. On the other hand, the athlete may carry his regular exercise too far, and by training too much may expose some weak spot in his system to a dangerous strain. He needs to break his habits by timely periods of repose.

Need I go further to illustrate my principle?

Such breaks in the routine of life are sanctioned both by scripture and the usages of the Church.

One of the earliest commands from heaven is to break the week of labour by a Sabbath's rest; and the Christian Church has at all times advocated fasting as a break useful both to mind and body.

Now we come to the question—I should say the vexed question—of drink.

Let me say, at the outset, that I treat it to-day as a scientific problem which knowledge and ex-

perience alone can solve. As yet we have but touched the hem of its garment.

The highest opinions are divided.

There are doubtless many cases of disease brought about by the temperate, but too regular, use of alcohol, which might have been prevented by a timely and temporary abstinence.

For instance, a gentleman or lady has inherited gout from a free living ancestor. He or she indulges daily in a glass of sherry, port, or beer, and is surprised one day to find some very ugly twinges in a certain digital member.

There is no question here of over-indulgence, but the tendency to disease has been encouraged by too great regularity of habit. A break in the habit would have prevented the accumulation of the poison, or a change of alcoholic beverage, for instance, to a lighter wine, would have broken the spell, and a terrible, and often fatal, malady might thus have been checked.

A break in the regular use of stimulants is often followed by a marvellous improvement in health.

A case of this kind often occurs.

A patient complains of dyspepsia with all its miseries. Appetite has failed, and food when taken is not a boon, but a burden. Sleep is dis-

turbed, and nocturnal wakefulness, accompanied by melancholy thoughts, distress the sufferer, leaving the nervous system utterly unfit for the duties of the day.

Let such a one knock off his wine and spirits, and he will find in a few days that he has passed into an elysium.

Appetite, digestion, good spirits, and sleep have all come back, because the alcoholic poison in his system has ceased to irritate and disturb his functions.

There is, therefore, a very strong reason here, on the ground of health alone, for sometimes abstaining; but there is a far stronger reason for doing so from a moral point of view.

The most moderate drinkers ought to prove to themselves from time to time that they can do without it if they like. Unless they do so, the day may come when to go back to water only is far from easy, and the *facilis descensus* is on the other side!

Another point never to be forgotten by the temperate drinker is, that in nature as well as morals, the older the habit the more difficult to break it.

Surely the mere feeling of moral superiority in being sure that we are not the slaves, but the

masters, of our habits is worth the sacrifice, however hard. We ought to examine ourselves, or take stock of our position, and see that our balance of power to resist is a good one.

Time fails me to say more on this head, but I have said enough to show that my principle holds good with regard to the regular daily use of alcohol. I don't say "never use it," but "use it only when necessary." Above all, don't use it when you know it is unnecessary. If the regular use of stimulants by the temperate man is not unattended by risk, what shall we say of him who habitually consumes too much, of him who daily saturates his system, befogs his mind, or seeks comfort in stimulants?

I could draw a picture, startling indeed, but common enough, of men who start the day on it, can't get through their forenoon business without it, can't eat lunch without it, repeat it before dinner, in dining flood the system afresh, and sleep on perhaps more than one glass of ardent spirits.

Such men are not drunkards in the ordinary sense of the word, at least they are never drunk, nevertheless they drink, and their drinking is of a most deadly sort.

It is well known by medical men that such

men become diseased and die far sooner than the man who has an occasional outburst and drinks furiously for a short interval. The latter breaks his habit, and is sober enough three weeks out of four, but the other is a habitual tippler and never breaks his bad habit except by increasing it.

To break the habit of such a man is a task of the greatest difficulty. He has probably drifted into this deplorable state almost unconsciously.

He has never admitted, even to himself, that he drinks, and he may be altogether such a nice, respectable fellow one hardly dares to attack him until his fate is sealed and it is too late to speak.

To medical men, and sometimes to clergymen, an opportunity may occur. If it does, speak. Speak strongly; above all speak wisely and kindly. It may be the habit has been developed by trying circumstances. Physical weakness or a nervous temperament may have started it. It may have been a resource during a time of trial and misery to assuage grief; it may be a refuge for wrong-doing to allay the pangs of an uneasy conscience; or it may be an inherited disease.

From whatever cause, the case commends itself to our deepest sympathy and warmest interest, and I need scarcely say that the only cure for

such a case is total abstinence. There is no safety in half-measures. The most solemn pledge, the most frequent urgings, and the most wholesome company, where drink is never offered, are all required.

We now come to the other side of the question —the bad effects of the regular non-use of alcohol; in other words, the dangers of total abstinence.

I trust this meeting, where there are many advocates of total abstinence, will bear with me while touching on this part of the subject; while I, on my part, will say no more than honesty compels me to say.

Hardly anyone will, I trust, go so far as to say there are no circumstances or conditions demanding the use of alcohol. Its limited use is, I know, sanctioned by many rigid teetotalers. For my part I have never met with an instance where its use had been resisted in extreme cases. I would go further and say that most men pass through phases of health when not to break the pledge would be serious and to take wine is an absolute duty.

Strong men, full of health and vigour, should be very careful in urging the man of feeble constitution and uncertain health.

Not unfrequently the sudden cessation has been

followed by a most serious and even total break-up of health.

Again, the conditions under which many of these feeble ones live are such as to justify the occasional, and even somewhat regular, use of stimulants. Let me give you a case in point.

Some years ago I had the medical supervision of a very large number of young men and women, and about that time there was a wonderful spurt in the temperance movement, leading large numbers of these young people to take the pledge. In 18 months I saw more than twenty cases of consumption, which had commenced within a few months of this change of diet.

More, I noted the most marked improvement when a proper amount of the right kind of stimulant was administered. I have also seen some sad results in the cases of clergymen, especially young clergymen, who from the highest and most unselfish motives have sacrificed their health by refusing to take alcohol.

Spending their abstemious lives in large towns, breathing the air of unhealthy houses, crowded rooms, and well-filled churches; working, some too early and some too late, they need the occasional help (aye, the somewhat regular help) of good wholesome wine or beer. I have known

many break down without it, and pick up again when the abstinent habit was set aside.

Let me give you a personal experience, as showing the importance of not going too far in total abstinence. Some years ago I was demonstrator of anatomy in our medical college, and spent six or seven hours a day in the dissecting room. For the sake of experiment I gave up the pint of beer I usually consumed daily. At the end of a month I had gained two pounds in weight and had improved in health; at the end of another month I had gained one pound more, but did not feel so well; at the end of a third month I had lost weight, and was much worse in health. I then resumed the beer and recovered.

"But," you reply, "your illustration cuts both ways; you were better without it."

So I was, for a time, but this only illustrates my principle, that as the constant and regular use of it is neither necessary nor advisable, so also the regular disuse of it is not unattended by risk.

The truth is, we have as yet no regular rules for our guidance in the use of alcohol, and what we want is a consensus of opinion on the subject and some rules for its use. A conference like this will, I am sure, recognize the importance of looking at both sides of the question, and this

society has set a splendid example by giving the right hand of fellowship to those who are neither total nor regular abstainers.

I would add one word by way of meeting criticism; as in nature we see every creature changing its environment, and seeking the conditions best suited to its well-being, so man best attains his highest well-being by altering his habits under the ever-varying conditions of his existence.

ON THE REMOVAL OF RENAL CALCULI BY TOXIC DOSES OF BELLADONNA.

The treatment of renal affections by belladonna has been accepted by the profession for many years, and the efficacy of the drug in relieving renal pain is well established. The pain produced by renal calculi and renal colic is more effectively relieved by belladonna than by any other drug, opium not excepted—at least that is my experience. I was led to infer that something more than mere relief of pain might be expected, by considering the analogous condition of bowel obstruction, in which belladonna has proved so marvellously successful, and the essential point of this treatment consists in following out the analogy between the two cases. In cases of bowel obstruction we push the treatment far beyond the mere anodyne effects of the drug, until the obstruction gives way. In cases of renal colic, we have been content when the agony has subsided. I contend that we have erred in thus stopping short of more decided results; and if the drug be administered sufficiently long, and in large enough doses, the

entire removal of the calculus—first from the pelvis of the kidneys to the bladder, and then from the bladder *per* urethram—often follows.

I will not attempt to theorize on the exact physiological process by which the above results are brought about, either in the case of the bowel or in the case of the renal passages; it seems, however, as if the drug combined in itself the power to relieve spasmodic contraction on the one hand, and produce peristaltic action on the other. Perhaps someone more fully acquainted with physiological therapeutics will enlighten us on this point.

I must, however, proceed to substantiate the above views by quoting some cases in which such striking results were obtained, as to establish the link of causation between the administration of the drug and the removal of the calculus. Let me quote three cases in point:—

Mrs. E— had suffered for several months from repeated attacks of renal colic. During the last of these attacks I was called in, and found her in the agony of a severe attack. Belladonna was administered until decided toxical effects were produced, and the patient was relieved of her sufferings as soon as the drug began to show its physiological action on the eye and throat. It was then pushed until she was fully under its

influence, and in a few hours the stone passed into the utensil with the usual satisfactory click —a lithic acid calculus, as large as a small almond.

The next case was that of a young engineer, who had suffered long and severely from renal pain, with occasional attacks of true renal colic. He had been treated by an experienced practitioner without much relief. I advised the administration of forty drops of tincture of belladonna, to be repeated every hour, or every two hours, until dilatation of the pupil, dryness of the throat, and delirium were produced. This treatment was commenced in the afternoon, and during the night following he passed a calculus as large as a bean. It is to be noted that the treatment gave speedy relief of pain, but, not content with this, the effect of the drug was kept up so as to ensure the passage of the stone.

The third case was that of a youth, who suffered so severely from renal pain that it was determined, at a consultation of the staff of the Royal Infirmary at Newcastle, to remove the calculus by operation. Before consenting to the operation, his parents brought him to me. I suggested the belladonna treatment, promising to send him to the hospital again if it failed. In this case, twenty drops of tincture of belladonna were given at intervals of an hour, and, at the end of four or

five hours, he passed a round and rough calculus, composed of urates; and I was able to send the boy to the hospital to present the stone to Mr. Page, from whom I received liberal congratulations.

These cases, I maintain, are sufficiently striking to arrest our attention, and tend to establish the fact that belladonna relieves the pain of renal colic, and, by its peculiar action on the muscular fibres of the urinary passages, removes the stone.

In the present state of pharmacology, we cannot say what its precise mode of action may be. It may act by simply paralyzing the circular muscular fibres of these canals, thus allowing the stone to be washed out by the urine; or, while paralyzing the circular fibres, it may stimulate the longitudinal fibres. The special point to be remembered is that we are to push the drug to its toxical stage, and keep up its action after the pain has been relieved, until a fair time has been allowed for the expulsion of the stone. We may begin with a forty-drop dose of the tincture, and repeat it every two hours, increasing or diminishing the dose according to its effect on the patient.

P.S.—Since the above was published I am assured by Dr. Wicks and others that this treatment has recently been tried with success in several cases. I ought to add that my first case occurred 12 years ago.

ON THE COMBINED USE OF PEPSIN, PANCREATINE, AND OX-GALL IN MARASMUS AND ANÆMIA.

The use of pepsin as an aid to gastric digestion, and the use of pancreatic preparations for similar purposes is now too well established to need our further confirmation. The use of ox-gall is also based upon experience of long standing. My object is to point out the importance of the combined use of these preparations in certain cases of ill-health which depend upon a faulty supply of the secretions of the stomach, liver, and duodenum.

I know not whether the fault lies in the quantity or in the quality of these secretions, but the indications seem to point to both being at fault.

Let me indicate, in the first place, the kind of case in which the use of each of these preparations separately has given marked results, as a foundation for the use of all of them in other more complicated and more serious conditions, where remarkable success has been obtained.

There are no cases where the usefulness of pepsin is so remarkable as in the exhausted stomach of gastric catarrh, and when this condition complicates the early stages of phthisis, as is often the case, we get the most striking improvement, not only in the symptoms of dyspepsia, but, in addition, we often get a marvellous rally in the general condition of the patient by the use of pepsin.

In many phthisical patients, especially where cod-liver oil and preparations of iron, etc., have been used, we find our patients in a most deplorable state of suffering from digestive troubles being added to the chest mischief. The morning cough, ending in a fit of retching, upsets the stomach early in the day, causing it to resist all so-called strengthening and nutritive medicines which are usually taken in the forenoon.

In such cases a dose of pepsin, combined with oxalate of cerium before breakfast, allays gastric irritation, prepares the stomach for food, and digests, or aids in digesting, the first meal.

In most cases of this kind it is necessary to repeat the pepsin and cerium before lunch or dinner. The stomach is thus prepared to receive a suitable tonic, such as Easton's or Fellows' syrup if it be needed, but, for my part, I reserve

such for patients with good digestion, and give the old-fashioned hypophosphites of lime and soda (Churchill's), with equally satisfactory results.

The use of pancreatine has almost merged into the peptonizing of the various foods, but there are cases where the administration of the pancreatic ferment is eminently useful—where the dyspepsia, for instance, comes two or three hours after eating, and especially where the loss of fat is a marked symptom. As a proof of its value where the symptoms indicate an entire cessation of the secretions, I quote the following remarkable case :—

Mr. H—, a farmer and land agent, was sent to me, ten years ago, by Dr. Walker, of Wooler.

He was suffering from griping pains in his bowels and diarrhœa, which he said was peculiar, the motions being like "same or fat."

On examining this material, I found it was sodden fat. The patient was losing flesh, and he had become extremely weak and exhausted. We put him on a teaspoonful or two of liq. pancreaticus after each meal. In a few days the diarrhœa (which had resisted the usual remedies) ceased, and the motions no longer contained fat. During a period of ten years he continued the use of the

pancreatic fluid, and whenever, during that period, he tried to do without it his diarrhœa and fatty motions returned. This was tried at least twenty times, and always with the same result.

The patient died last year of other maladies, and no post-mortem was obtained.

The case, to my mind, amply proves the need of pancreatic juice for the digestion of fat, and demonstrates clearly the power of our present preparations, such as Benger's liq. pancreaticus, to deal with its digestion when the natural ferment fails.

I now direct attention to the administration of ox-gall in cases of anæmia in men.

Men of advanced years are liable to anæmia, which seems to be connected with a want of action of the liver. The skin and mucous membranes are not only pale, but slightly yellow, the mucous membrane of the mouth and tongue is bare, and the liver dulness is sometimes distinctly diminished in these cases of anæmia. The motions are too pale, and are either like clay or watery.

I know that such symptoms often accompany waxy liver, but these cases are hardly so distinctly

marked as to be classified under that head. I call the condition "hepatic anæmia"—at any rate, the anæmia is the most marked symptom of a failure of the blood-forming and digestive functions, and yet it falls short of pernicious anæmia in several marked features, especially in yielding to treatment. My object is to point out the marked effect which ox-gall produces in these cases.

For example, I was called to see a farmer with the above symptoms, who was more enfeebled than I have ever seen a man who could be said to be "on his feet."

This man took five grains of ox-gall (B.P. prep.), four times a day, with such marked benefit that he was able to bear the journey to Newcastle (ten miles of rough road) in less than a month, and at the end of three months he averred that he was in his usual health. At any rate, his anæmia had disappeared more rapidly than any other form of anæmia yields to iron or arsenic.

This is only one of many cases of this kind which have rapidly improved by means of ox-gall. I suppose its action is due to the increased stimulus it gives to the duodenal digestion; and by affording material for the better digestion of

fat, it probably facilitates its absorption and assimilation by the lacteals.

I now come to the consideration of a class of case which must have occurred to most of us—a case in which all the above conditions of stomach, pancreas, and liver are in existence. Where the stomach fails in the first steps of digestion, where the duodenal uneasiness and almost entire absence of fat in the body indicate the want of pancreatic fluid, and where the clay-like motions and constipation indicate a deficiency of bile. Anæmic, exhausted, attenuated, with misery of body depicted on a dejected countenance, such shattered visions of humanity now and then cross our path, constituting a condition best described as marasmus.

In such cases I have had most satisfactory results from pepsin and cerium, gr. v. ter die. ante cibum —

> Liq. pancreat., 3i. ter die post cibum
> Fell. bovin., gr. x. om. noct.

In two cases all hope of recovery had been abandoned, and yet, under the above combination, perfect recovery took place.

In one of these cases the patient gained several

pounds in weight every fortnight, and was quite well when last I saw him.

As I am not writing a systematic treatise on these cases, the above rough outline must suffice. My object is attained if I have called attention to "the combined use of pepsin, pancreatine, and ox-gall," so that others may be inclined to give the treatment a trial.

NOTE ON "EMPHYSEMATOUS DYSPEPSIA."

During the last thirty years I have met with a considerable number of cases of obstinate dyspepsia, in which the prime factor has been an emphysematous state of the lungs. The *modus operandi* of this condition seems to be chiefly due to the mechanical downward pressure of the enlarged lungs, by which the gastric area is diminished and the liver is depressed. At first sight we might readily imagine the patient to be suffering from enlargement of the liver; but this is not so, as the liver dulness is not increased, but shifted to a lower level. Its lower border may reach half-way to the umbilicus, but its highest point of dulness is limited by the lower margins of the ribs and costal cartilages. All above this line is highly resonant and occupied by the distended lung. Similarly the amphoric note of the stomach is hardly perceptible, except over a small space below the margins of the ribs and cartilages on the left side. These cases occur, for the

most part, in vigorous young men who have led athletic and arduous lives, and whose occupations and habits in no way predispose them to dyspepsia. They occur in gillies and gamekeepers, in paviors and navvies, in athletes from our schools and universities, and in many cases the dyspepsia of men of great physique, who are in high training, is dependent on this condition of emphysematous pressure of the lung on the digestive organs.

The symptoms by which it is characterized are those of slow and laboured digestion, with great languor and lassitude after eating; but the most highly marked symptom which I have observed in most of these cases is a pricking pain in the region of the liver, or more frequently in the inferior costal region on the right side. The patient complains of this pain on exerting himself and after eating, but it is not increased by deep inspiration nor by movements, by which it may be distinguished from pleuritic pain and muscular rheumatism. Its character is not that of intercostal neuralgia, which seldom *pricks* the patient; in short, the pain is peculiar and characteristic of the condition above described, and is often described by the patient as if several pins were pricking the inside of his chest. I have no doubt the cause of this pain is pressure on the adjacent

nerve twigs, either in the pleural surfaces or in the compressed viscera.

Patients will tell us they have tried every variety of medicine and diet for dyspepsia without relief, and that they are, for the most part, not much troubled by short or difficult breathing. In fact, the emphysema has not arrived in these young fellows at that stage where it tells much on the pulmonary circulation or on the heart's action, and any defect in this respect is quickly compensated for by the increased power of the ventricle. I give a case in point :—

J. S.— has distinguished himself as an athlete in many matches. He is a man of immense muscular power, and is as agile as he is powerful; at the age of 25 he becomes "very seedy." His troubles are chiefly dyspeptic, he is miserable after eating, and distressed by a sense of fulness at the epigastrium. His energies fail, and his desire for athletic exercises leaves him. While in this condition he is treated by a skilful practitioner, first for disordered stomach, then for sluggish liver, and so on without relief. At the end of several months, during which he was dieted and dosed pretty regularly, his medical attendant brought him for a consultation. I found that, among his many complaints, he laid stress on the

pricking pains in his chest, and on examination I found extensive hypertrophous emphysema of the lungs. His chest was like a drum, the cardiac and hepatic dulness in the chest was gone, and, of course, the liver dulness in the abdomen indicated considerable increase, and the corresponding pushing down of that organ.

This gentleman gave up his athletic habits, and, under a course of new treatment, recovered from his emphysema to a great extent, and from his languor and dyspepsia entirely.

This brings me to the question of treatment.

Most of these cases have been pretty freely treated by alteratives and purgatives and the usual remedies for dyspepsia, before the above conditions are realized, and it is, of course, important to remove any recognized disorder of the stomach or liver by such medicines; for instance, we must give a fillip to the hepatic circulation, which may be considerably interfered with by the downward pressure on the liver. A suitable cholagogue or alterative, followed by a saline purgative, will secure this. We must also correct any tendency to flatulence in the stomach, as this adds greatly to the patient's difficulties. He will tell you that he obtains much relief from flatulent eructations. This relief can, to a great extent, be secured by

removing fermentible articles from the diet, but we must also get rid of a tendency to fermentation and acidity, the result of laboured or slow digestion, by a dose of this kind, given four or five hours after each heavy meal :—

℞ Sodæ Sesquicarb.
Sodæ Hyposulphitis, āā gr. xv.
Pulv. Rhei, gr. ii.
Pulv. Zingiber, gr. x. M. ft. Pulv.
ex Aqua Sumend.

We shall find, however, that in spite of these efforts, we are unable to give the patient permanent relief. We must, therefore, proceed thus:

First, we must insist on a cessation of all muscular effort which involves a strain on the heart or lungs. The patient must lead a quiet life; he must not run or walk rapidly uphill; he must not pull or haul; he must not blow wind instruments nor sing much, these latter being fruitful causes of emphysema. He must give up any article of his diet which produces flatulence, and take his food in a small compass or concentrated form. He must not neglect to use the means I have indicated to correct the condition of his stomach and liver, and, lastly, we must treat the emphysema by the following medicines: Arsenic,

strychnia, and hydrochloric acid. We may give Fowler's solution in three, four, or five drop doses with food and a mixture of the strychnia and acid separately, but the following combination meets the indication better :—

Liq. Arsenici Hydrochlorici, ♏ v. vel x.
Liq. Strychnie, ♏ v. vel x.
Acid Hydrochloric dil, ♏ v. vel x.

The dose being increased according to the idiosyncrasy of the patient, and it may be given in a large dose once or in smaller doses several times a day. Under this *regimé* and treatment the patient slowly but steadily recovers. His digestion is aided and rendered less laboured by the above combinations, and they soon begin to tell on the pulmonary organs by increasing the tone of the vagus, by increasing the tone of the muscular tissue in the lung, and by improving the nutrition of the pulmonary tissues generally. At any rate, the increased comfort of the patient runs *pari passu* with the decreased area of resonance in the chest, of which I have satisfied myself by more than one carefully observed case.

APPENDIX.

ON THE RAPID PRESSURE TREATMENT OF ABDOMINAL ANEURISM.

As an introduction to the subject of my essay, I shall endeavour to recall as correctly as possible the steps by which I was led to adopt the Rapid Method in the case of Aneurism of the Abdominal Aorta, which is fully detailed in the following pages. I shall also add a few remarks on the origin and nature of the Rapid Method, in the hope of establishing it as an accepted and improved mode of curing aneurism by pressure.

When Wilson presented himself as a patient at the Newcastle Dispensary, I made several careful examinations of the pulsating tumour from which he suffered, and in doing so I discovered that by firm pressure with my own hand I could, to a great extent, command the pulsations of his aneurism. It then occurred to me that pressure which had been successful in other parts might be applied to the abdominal aorta. Then came the idea that to render the necessary amount of pressure tolerable, chloroform might be used, and further, that chloroform would enable me so completely to com-

mand the vessel, that *coagulation of blood* in the aneurism might occur if the current were thus completely arrested.

After repeatedly considering these points, the whole subject assumed a new phase in my mind. Here was a case which had suggested a treatment that seemed to involve an entirely new view of the application of pressure in the treatment of aneurism; a treatment which would not only be more speedy in its action, but also cure the disease by an entirely new process. So strongly was I influenced by these considerations, that I at once proceeded to test their accuracy. The results which followed were more than could be expected, for the rapid manner in which the aneurism was cured bore the most direct testimony to the nature of the curative process, and that which before had been a mere surmise now became a settled conviction.

As my object in detailing the following case is not only to establish the possibility of compressing so large a vessel as the aorta for the cure of aneurism, but more particularly to point out the *nature* of the *curative process*, I call special attention to that part of the report where I described the manner in which the aneurism ceased to beat. The sudden and complete consolidation of an

internal aneurism was, as far as I know, an entirely new fact in medical science; and the repetition of the same phenomenon in other cases which have since been treated by the same means tends strongly to show that all aneurism may be treated on the same principle. I think it is not too sanguine to expect that in future, wherever pressure is used for the cure of aneurism, the administration of chloroform will be considered an essential part of the treatment, and that sudden and rapid cure by the coagulation of blood will be no uncommon result, in place of the slow and gradual change which is brought about by the lamination of fibrin when the older method is pursued. I would further remark that the *principle* on which the rapid method rests is clearly "the complete stagnation of a mass of blood in the aneurism until it coagulates." Whether this is to be done under chloroform or without it, whether by pressure both above and below or only above the sac, and whether the pressure is to be made by the hand, by flexion, by a weight, or by a tourniquet, are questions which in no way touch the principle on which the treatment is based. The importance of the following case is greatly enhanced by the post-mortem evidence of its reality which has been now obtained. Until this was procured I hesitated

to press its claims on the notice of the profession lest there should have been any error of diagnosis in the case. Now that all fear on that account is removed, I would claim for the case great importance, because it involves in itself not one but several facts new to anatomy, physiology, and practical medicine. It establishes —

1st. The possibility of suddenly blocking up the aorta below the renal arteries without injury to the patient.

2nd. It reveals the channels by which blood finds its way to the lower part of the body when the aorta is thus occluded.

3rd. It shows the vast importance of giving chloroform in using pressure for the cure of aneurism; and

4th. It proves that aneurism can be cured in a few hours by coagulation of blood, while the old method, which cured the disease by fibrinous lamination, lasted on an average five-and-twenty days.

HISTORY OF THE CASE.

Five weeks after the treatment had been employed Wilson was well enough to travel to London. I therefore took him to the meeting of the Royal Medico-Chirurgical Society, held on May 24th,

where he was carefully examined by many eminent members of that Society, and they unanimously confirmed the observations related in the paper which was read by the late Mr. Moore. The following extract from the Medico-Chirurgical Society's *Transactions* contains a full history of the case up to the time when my paper was read:—

An Account of a Case of Aneurism of the Abdominal Aorta, which was cured by Compression of that Artery immediately above the Tumour, by the Rapid Method. Communicated by the late Charles H. Moore, F.R.C.S. Received April 26th.—Read May 24th, 1864. (*Med. Chir. Soc. Trans.*, 1864.)

History.—The patient (Mark Wilson) is a spare man, twenty-six years old. His occupation as a pavior has compelled him to use a large wooden rammer for driving paving stones into the ground. Often, in making strenuous efforts, he has over-reached himself and subjected the trunk of his body to severe straining. Eleven months ago, after a hard day's work, he was seized somewhat suddenly with a very severe pain in the back, of a gnawing character, and preventing movement. Two months later the same pain began to be felt very severely in the abdomen, catching his breath during inspiration. He was treated for these

pains by several medical attendants with leeches, blisters, etc. About two months ago he began to feel a slight pulsation in the belly, and shortly after that time he became my patient at the Newcastle Dispensary, where, after a few examinations, I became convinced that he had an aneurism of the abdominal aorta. This opinion was shared by the resident medical officer of the dispensary.

The following is the condition of the patient as noted previous to treatment:—"His abdomen is somewhat spare, so that a distinct pulsation can be seen to the left of and slightly above the umbilicus; the pulsation is most distinct during expiration. On applying the hand, a hard, slightly moveable pulsating mass, of a distinct globular form, is to be felt extending from about two inches to the left to about one inch to the right of the umbilicus, and upwards to within three inches of the margin of the left lower ribs. The pulsations in it are very strong and impinge upon the hand with a sudden stroke, and the expansion of the tumour very distinctly separates the hands when applied to it. The tumour is about the size of a very large orange. The impulse conveyed to the hand, when laid on the upper part of the tumour, is almost as strong upwards as it is downwards when felt by the hand applied below the tumour. When

pressure is made on the aorta above it, all pulsation ceases, and when the pressure is removed a distinct thrill is felt to accompany the rush of blood into the tumour. (A line drawn across the abdomen over the umbilicus touches at either end the margins of the last ribs, and encloses between the free borders of the ribs a triangular space (the epigastric region); over the left half of this space there is room enough to compress the aorta against the spine.) The aorta below the tumour can be felt, and its pulsations seem in no way to vary from their normal character. By auscultation a feeble bruit can be heard over the tumour. The patient is in good general health; his bowels are subject to occasional attacks of constipation, sometimes being open every day, at others only once in two or three days, which may to a certain extent be accounted for by the large quantity of opiates he has taken to relieve the acute pain which he has suffered. His arterial system is free from evidence of degeneration."

All palliative treatment having failed to relieve him, after much thought and careful consideration, I proposed to apply a tourniquet above the aneurism, and thus attempt to cure it by compression. It happened, as I have before shown, that between the aneurism and the free borders of

the ribs on the left side there was space enough to permit one blade of a tourniquet to press down on the spinal column, and on tightening the tourniquet I found that, by a very nice adaptation, the pulsation in the aneurism could be completely commanded. The instrument used was the ordinary horse-shoe tourniquet, made so as to open rather wider than usual, and thus to grasp the trunk of the body.

I took the patient to the Northumberland and Durham Medical Society to have my diagnosis verified, and to propose my plan of treatment. Several members examined the tumour, and it was admitted by all who did so that the case was unquestionably one of aneurism of the aorta. The President, Dr. Heath, expressed the same opinion in very decided terms. The following is an extract from the Report of the Monthly Meeting of the Northumberland and Durham Medical Society, April 14th, 1864 :—

"Dr. Murray introduced a patient suffering from aneurism of the abdominal aorta. That it was an aneurism was clear from the fact that when a tourniquet was applied above it pulsation ceased; and on removing the pressure a distinct thrilling, rasping sensation was communicated to the hand as the tumour recommenced beating, as

if produced by the rush of blood into its interior. As the patient was young, Dr. Murray proposed to operate on the aneurism by pressure. He thought that treatment fully justified, because the disease was otherwise necessarily fatal. Several gentlemen having asked questions and made remarks, Dr. Heath, the President of the Society, said, 'This was a very interesting case; they sometimes met with instances of pulsation of the abdominal aorta, which closely resembled aneurism, but he thought there was no doubt this was truly a case of aneurism. With respect to the treatment which Dr. Murray proposed to adopt, he thought that since such an eminent surgeon as Sir A. Cooper did once tie the aorta, Dr. Murray was quite justified in employing the milder method of pressure. This was an age of experiment, and he thought the course proposed was a fair and justifiable experiment, and one which possibly might be followed by success.'"

On Saturday, April 16th, the patient was put under chloroform. (Having lately administered chloroform to a patient for fifteen hours without any apparent injury, I had no hesitation in making a prolonged use of it in this case.) The anæsthetic influence was accordingly kept up for two hours, during which time, except during momentary dis-

placements of the instrument, the pulsation in the aneurism and in the vessels of the lower limbs was completely arrested. On removing the pressure no visible effect had been produced; but the patient passed no urine for nearly thirty hours. It was found exceedingly difficult to keep up steady pressure, as the patient, being under chloroform, unconsciously moved about a good deal; the irregular action of the muscles of expiration in the abdominal wall added very much to the difficulty, and it became necessary to sit constantly with one hand on the screw of the tourniquet, and the other on its anterior or applied blade, and thus to secure the constant pressure required. On neither occasion when the pressure was applied did we escape a considerable number of recurrences of the pulsation from displacements of the instrument. This statement, however, does not apply to the *last hour* of the *second and successful attempt*, during which all movement and pulsation were completely arrested.

On Tuesday, April 19th, after much entreaty on my part, the patient again submitted to be put under chloroform. Dr. Heath having carefully re-examined the tumour, and expressed his conviction of the nature of the disease, the tourniquet was applied, and pulsation in the tumour com-

pletely arrested. With the assistance of Drs. Nesham and Spencer, and Messrs. Armstrong and Powell, the pressure and the insensibility were kept up for about five hours. Until the last hour the slightest movement in the tourniquet showed that pulsation in the tumour had not ceased, and that the disease was unaltered. During the last hour the existence of pulsation became less obvious. It was then deemed prudent, lest the patient's strength should be exhausted, to remove the pressure and see what had been accomplished. On finally removing the pressure very slight pulsations were felt, and hopes were entertained that some advantage had been gained. As the femorals did not beat during the application of pressure, the extremities had become cold; and when the patient recovered from the chloroform a fit of shivering occurred. Hot bottles were applied to the feet, and hot brandy and water was given, followed in a short time by a large dose of a mixture of chlorodyne, opium, and belladonna, containing about ʒjss. of the tincture of the last drug, which I find is a perfectly safe dose as an anodyne when combined with an ordinary dose of opium. In the evening he was found restless, and "sore all over," with tenderness at the seat of the aneurism and of the

pressure, and numbness of the extremities. To my astonishment the tumour had now become perfectly pulseless, and every indication of pulsation in the aorta below it had disappeared.

Wednesday, April 20th.—Patient feels restless and slightly feverish, with thirst and hot skin, but the pulse is only 72 and feeble. Bowels open, passes water freely; can stand, although the legs are still numb, and he feels "pins and needles" in his feet. In consultation with Dr. Heath, a most careful examination was made, and the following observations were confirmed by that gentleman. There is no pulsation in the tumour, which is now perfectly stationary, hard, resistant, and lessened in size. Nor are any pulsations to be felt in the aorta below the tumour, in the iliacs, or in the femoral arteries.

Thursday, April 21st.—Patient looks well and feels much better; says he is more free from pain than he has been for several months. There is a very slight movement in the tumour, which is now a hard globular mass, easily felt, and slightly moveable, but evidently smaller than before. At one or two points on the abdominal wall pulsating vessels can be felt, but there is no pulsation in the femorals.

Friday, 22nd.—With Mr. Lightfoot, who care-

fully examined the case, the following points were made out and verified by that gentleman:—A solid hard tumour, of about the size of an apple, lying to the left of the umbilicus, can be felt, and during deep expiration can be seen. It is motionless to the eye, and by the hand the slightest possible forward movement can be distinguished at its upper border, as if communicated from the aorta pulsating above. No expansion, thrill, or bruit, can be made out. Running over the right border of the tumour a vessel can be felt pulsating, which, from its position and size, is probably the superior mesenteric artery. The femorals are pulseless. All numbness is gone from the legs, and the patient declares he feels quite well.

Saturday, 23rd.—Observation of the tumour corresponds with the notes of yesterday in every particular. The pulsation of small arteries in the abdominal wall is now pretty distinct. The patient is sitting up and out of bed, feels better than he has done for months past, and is free from pain. Eats well and sleeps well.

Sunday, 24th.—Going on well.

Monday, 25th.—Still improving, and is moving about freely. The tumour is now much diminished in size, and no pulsation can be distinguished. (Observation confirmed by Dr. Spencer.)

Tuesday, 26th.—The patient has been out this morning, and walked about a quarter of a mile. On ceasing to walk he felt as if a cord was tied round his waist, and was quite numb below that level. He feels his legs numb and weak, but in other respects is quite well and in good spirits, declaring himself to be better than he has been "for eleven months past." After careful examinations, the observations previously made were this morning confirmed by Messrs. Fife and Armstrong. Mr. Rayne, after a very careful examination, also expressed his conviction "that there is now no blood passing through the tumour." Dr. Gibb and many other medical gentlemen in the town, after examining the parts, came to a similar conclusion.

May 1st.—Patient still improving. Legs warm and stronger, but still numb when he walks far. Had an attack of diarrhœa, which ceased on taking a few doses of chalk mixture. No pulsation in the tumour or arteries below it.

5th.—Still improving. Took a long walk two days ago, and, except a feeling of numbness and weakness in the legs, was no worse for it. Tumour carefully examined this morning in the presence of several medical men, when the following points were observed and verified:—"The tumour is

stationary, harder than before, and lessened in size. Its periphery lies seven inches from the sternum (the patient having a long chest) and five inches above the pubes, four inches from the anterior superior spine of ilium on the left side, and five inches from the same point on the right side. It lies a little more to the left than to the right of the umbilicus." No bruit can be heard, and the aorta above can be felt beating in the epigastrium.

11th.—Patient still improving, tumour pulseless, and diminishing in size.

(*The following remarks were appended to the Report.*)

I need offer little comment on this case, especially as the patient will be introduced at the meeting of the Society. I would note, in the first place, that we have in it a most complete triumph for the advocates of "compression" in the treatment of aneurism. The disease, though one which has baffled all attempts to cure it, has in this instance disappeared by means of treatment lasting but a few hours, and involving the use of a known and simple expedient.

Secondly, here is actual proof that a sudden occlusion of the aorta can take place without violent symptoms or great inconvenience ensuing.

Again, the case shows that in compression, as has been noted by an eminent surgeon in the North, "the actual cure takes place very rapidly, probably in less than an hour;" for, till within the last hour of the treatment, the slightest movement in the tourniquet was followed by most violent beating in the aneurism.

As an instance of the dependence of a curative process on the influence of chloroform, this case is most striking; for no man, exhausted with pain and weary of life, could have borne for five hours, without an anæsthetic, such tremendous pressure as was here employed, even though that pressure were to save his life.

Postscript, July 7th, 1864.—The patient has obtained a situation as a "fitter," and feels equal to the work. The only unfavourable symptom now present is a numb pain in the knees after walking, which disappears after resting a few minutes. The lower limbs are plump, but flabby; the rest of the body is well nourished. The tumour is now scarcely to be felt, and the

aorta, iliacs, and femoral arteries are still quite pulseless.

Postscript, September 26th, 1864.—I have seen the patient this morning, and he looks well. He only complains of slight weakness and pain in his back and legs. The numbness no longer exists. He works as an engine-fitter from 6 a.m. till 8, and sometimes 10 p.m., in addition to which he has to walk nearly two miles to and from his work. The abdomen and limbs are now plump and fat. Only slight hardness, which is evident on *deep pressure only,* is perceptible at the site of the aneurismal tumour. There is no pulsation in the aorta below the site of the tumour, but above it a very distinct thud can be felt to strike dead against the applied hand. There is now distinct pulsation in the right femoral artery, but no certain evidence of it in the left. The patient says "he now feels as well as ever he did in his life." Five months have now elapsed since pulsation was felt in the tumour.

Report continued up to 1870.

The patient continued to enjoy good health until the commencement of the year 1870, having

during the intervening six years followed a variety of laborious occupations, and having also on several occasions undergone no small amount of privation through the prevailing scarcity of work. During this period the aneurismal swelling gradually disappeared, the determination of the aorta could be very easily distinguished by its thud against the hand when applied about three inches above the umbilicus, and its course below the thudding point remained absolutely pulseless.

By far the most notable change during this period was the appearance of numerous large pulsating vessels on the front and sides of the abdominal parietes; one, on either side of the rectus muscle on the site of the epigastric arteries, equalled the femoral artery in size, while those on the upper part and lateral aspects of the abdomen varied from the size of the brachial artery to that of the ulnar. The course of these vessels was, for the most part, extremely tortuous and difficult to trace.

About the commencement of the present year (1870) the patient was compelled to resume his old occupation as a pavior, and the strenuous efforts required of him once more brought on violent

pain in the epigastrium, which was speedily followed by other symptoms of an aneurism in that region. This latter disease, when fully developed, was found to lie so close to the diaphragm that pressing the aorta above it was out of the question. The aorta below this new aneurism was very carefully examined, and found to be perfectly free from pulsation; in fact, it was evident that the aorta above the occluded point had given way and become dilated into an aneurism. The usual symptoms of aneurism of the aorta near the cæliac axis were terminated by the sudden death of the patient on June 1st, 1870.

REPORT OF THE POST-MORTEM EXAMINATION HELD ON THE 2ND OF JUNE, 1870.

THIS examination was conducted by Dr. Maclachlan and Mr. Davidson, assisted by Mr. Johnson, in the presence of Mr. Russell, one of the surgeons to the Newcastle Infirmary, and several other gentlemen. To all these gentlemen I am much indebted for the assistance they afforded, but more especially to Dr. Maclachlan, who carefully dis-

sected the aneurism after its removal from the body.

Vessels in Abdominal Walls.

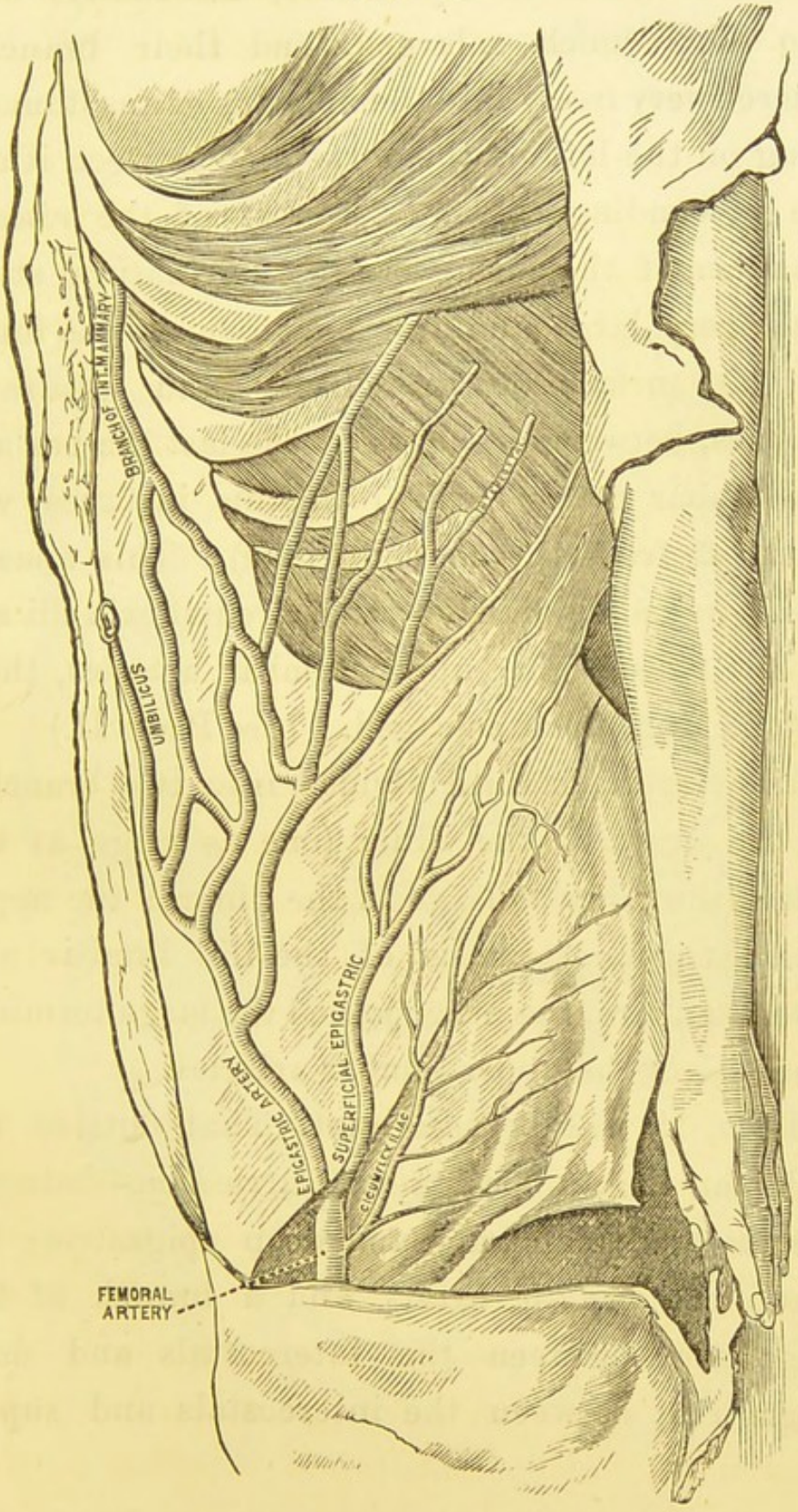
On removing the skin from the front of the abdomen a numerous array of tortuous blood-vessels was found ramifying in every direction; the tortuous branches of neighbouring trunks were seen to anastomose directly with each other, and the terminations of the trunks themselves were observed to be continuous with each other. (1) The deep epigastric artery (as large as the axillary) ran up along the outer border of the rectus muscle, giving off lateral branches; (2) Other branches of the epigastric passed outwards and anastomosed with the lower intercostal arteries, the latter being much enlarged and tortuous. (3) One lateral branch of the epigastric given off from its inner side penetrated the umbilicus, and running along the free border of the suspensory ligament of the liver, entered the longitudinal fissure of that organ and anastomosed with a branch of the hepatic artery. (4) The superficial epigastric artery, enlarged and very tortuous, entered a plexus of vessels formed by it and branches of the lower intercostal arteries. (5) The superficial circumflex iliac followed the

same course, and joined in an anastomosis with the lower intercostals. (*See* Plate I.)

Vessels inside Abdomen.

After opening the abdomen, (1) The superior mesenteric artery, as large as the aorta, was apparent; (2) And the colica media branch, enormously enlarged, gave off branches of a very large size, which joined the anastomosis of similarly enlarged vessels given off from the colica sinistra branch of the inferior mesenteric. All these anastomosing vessels were larger than crow-quills even at their points of union with each other. (3). The state of the inferior mesenteric artery was most peculiar, for while giving off these large branches the trunk of the vessel was dwindled to the size of the radial artery, and its coats were thin and flaccid. This wasted state of the vessel was easily accounted for by finding that the vessel entered that part of the aorta which had been occupied by the first aneurism, and which was now a mere fibrous mass. It was evident, therefore, that a very free current had been sent from the superior to the inferior mesenteric artery through this anastomosis of the colica media and sinistra; but it must be further noticed that the sigmoid and superior hæmorr-

PLATE I.


BRANCH OF INT. MAMMARY
UMBILICUS
EPIGASTRIC ARTERY
SUPERFICIAL EPIGASTRIC
FEMORAL ARTERY

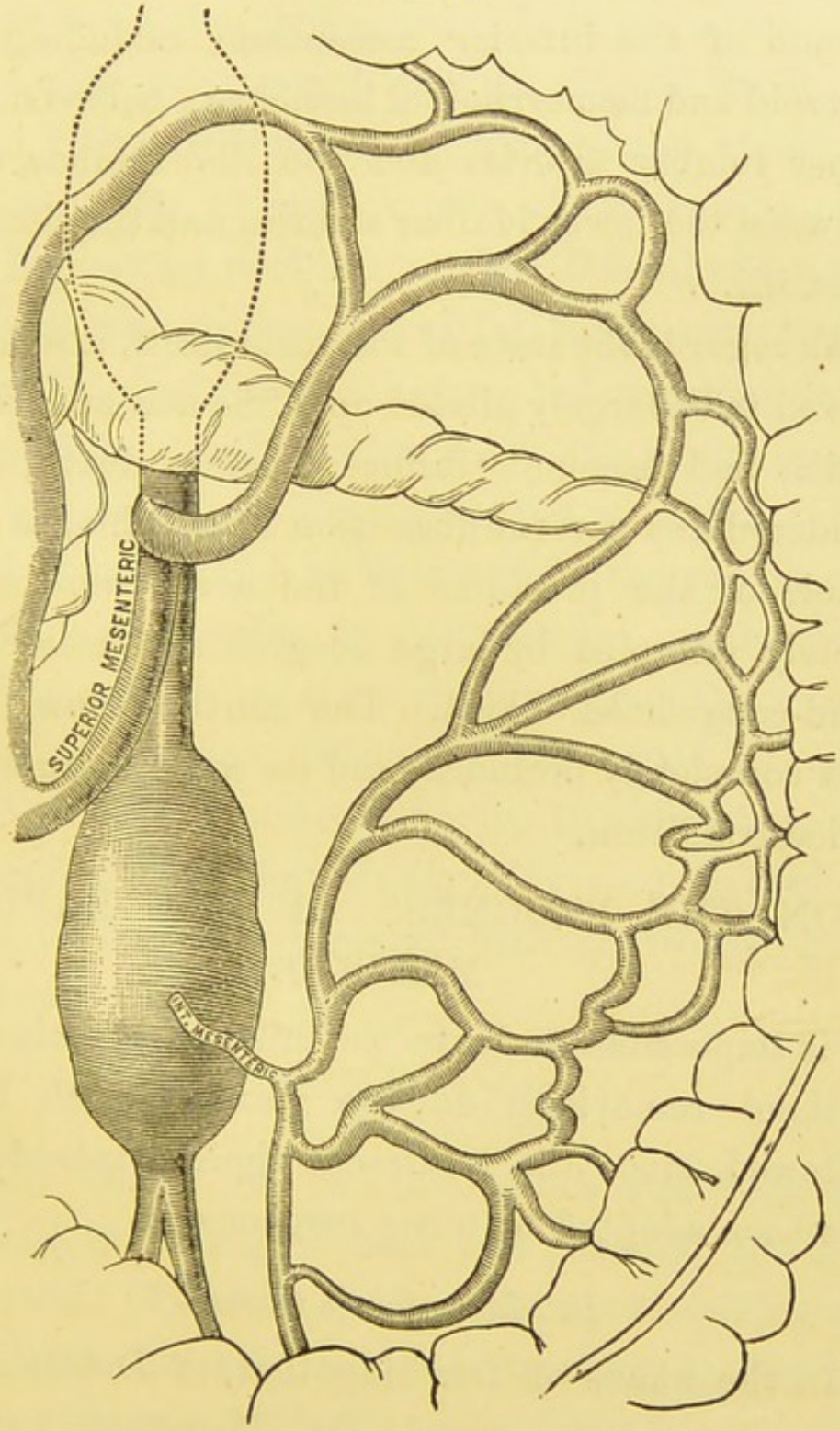
hoidal branches of the inferior mesenteric were also very much enlarged, and their branches entered very freely into the network of anastomosis lying on the left of the aorta and between it and the descending colon. The rest of the *visceral* branches of the aorta were in their natural state, but a very large and free anastomosis was found on the surface of the iliacus and quadratus muscles, between branches of the last lumbar and ilio-lumbar artery (which in this instance was given off from the common iliac). This anastomosis was also joined by the circumflexia ilii and by branches of the upper lumbar arteries, these latter being much enlarged. (*See* Plate II.)

The circumflexa ilii divided into two branches at the crest of the ilium (one as large as the radial, the other as large as the ulnar); the upper joined the anastomosis of the ilio lumbar and lower lumbars, the other joined the large terminal branches of the upper lumbar arteries.

Thus, it will be seen—1st. That outside the abdomen the circulation was carried on—between the internal mammary and deep epigastric; between the *hepatic* artery and a branch of the epigastric; between the intercostals and deep epigastric; between the intercostals and super-

ficial epigastric; and between the lower intercostals with the superficial circumflex iliac.

PLATE II.



2nd. Within the abdomen the circulation was carried on,—between the colica media of the superior mesenteric artery and the colica sinistra branch of the inferior mesenteric, including its sigmoid and hæmorrhoidal branches; between the upper lumbar arteries and the ilio lumbar, and between the lower lumbar arteries and the circumflexa ilii.

As regards the state of the aorta itself, it was observed to be largely dilated up to the new aneurism, which had become "diffuse" before death, and rendered any careful dissection impossible, as the whole of the precincts of the aorta were completely occupied by large coagula and masses of semi-coagulated blood. The aorta below this was completely occluded, and its walls in an atrophied condition.

ON THE PROGRESS OF THE RAPID METHOD.

It is impossible to leave the history of this case without remarking on the results which have followed, since the treatment therein introduced has been adopted by other surgeons.

Dr. Mapother's Cases.

In the hands of Dr. Mapother, of Dublin, two cases have been cured, Dr. Mapother having

strictly followed the plan I pursued, and having tried with it the suggestion made by Dr. O'Ferrall, of applying pressure on the distal as well as on the cardiac side of the aneurism. His first case was pressed without chloroform at first, and afterwards under chloroform, and at the last effort under chloroform the aneurism was cured in four and a half hours. Dr. Mapother's second case was scarcely so successful, the pressure having lasted ninety-seven hours before coagulation occurred. Dr. Mapother holds the same views as I have repeatedly expressed, and concludes "that the blood should be detained in the sac as completely as possible, by compression on the artery above and below it." Holding that the cure is effected by coagulation of the blood *en masse* and not by laminated fibrin, Dr. Mapother also dwells on the desirability of giving chloroform, and on applying pressure both above and below the sac.

Dr. Heath's Cases.

Dr. Heath, of Newcastle, has also been successful in curing at least two cases by the same method. The first of his cases was a striking and brilliant example of the treatment; for the aneurism was distinctly consolidated in the short

space of twenty minutes. I am glad to know that Dr. Heath holds strongly to the idea that complete pressure under chloroform leads to rapid cure by coagulation. The following is an extract from his address on "Surgery," delivered at the annual meeting of the British Medical Association, 1870 :—

"The old or slow pressure method was an advance upon the ligature ; but what I believe was first named by myself the 'rapid pressure treatment,' must be considered to be in some respects even a greater improvement upon the older plan.

"The rapid pressure treatment may be considered but the natural development of the older method, and, like many other triumphs of modern surgery, owes its practicability to chloroform.

"I should rather leave to those who best know what dangers encompass him who is submitted to the knife and the ligature—the shock, the suppuration, the erysipelas, the pyæmia, the gangrene, and the secondary hæmorrhage—the appraisement of a measure which, after a few hours' sleep, leaves the awakened patient free from his disease, with no wound to heal, no further risk to run, and

to say whether any surgical proceeding ever more truly deserved the application of the old maxim—*Cito, tuto, et jucunde.*"

Mr. Holden's Case.

A very striking case is reported in the Bartholomew's Hospital Report, Vol. II., by Mr. Eck. The patient was under Mr. Holden's care, and the pressure was kept up for five hours (during the first hour without chloroform), and it resulted in a remarkable diminution of the pulsations, which finally disappeared in three weeks. In this case some decided change was effected by the pressure, which eventuated in cure. This point, though not favourable to the rapidity of the cure, is highly important as a confirmation of what occurred in most of the other cases, viz., a decided consolidation without absolute cessation of pulsation, the very faintest degree of pulsation being in some cases continued for a short time after the removal of the pressure.

Mr. Lawson Tait's Case.

In November, 1867, Mr. Lawson Tait wrote an interesting paper in the *Medical Times and Gazette*, pointing out the desirability of completely arrest-

ing the current in the aneurism, but he does not mention the use of chloroform. He refers there to the opinions of Mr. Henry Lee and Mr. Bryant, both of whom evidently hold that the rapid method is a great advance on the older and slower mode of treatment. Mr. Lawson Tait was successful in his case without the use of chloroform, and, as far as I can see, the instrument he used may prove highly serviceable when chloroform is inadmissible.

The next case was cured by Mr. Lawson in the Middlesex Hospital, and *it* affords the most striking example of the rapid cure. In this case the pressure was applied for twenty-three minutes only.

Mr. Lawson's Case.

"*Inguinal aneurism cured by compression of the abdominal aorta.*—The patient, a man, æt. 36, was under Mr. Lawson's care in Middlesex Hospital. The treatment was commenced, on August 21st, by the application of Carte's compressor to the femoral artery in the groin, just above the aneurism, which was so applied as to shut off all but a wave of impulse from the sac. The pressure was very badly borne, from the pain it

produced; still the man took great interest in the treatment, and bore it as well as he could. He soon learned to regulate it for himself, screwing it up firmly as long as he could bear it, and only relaxing it every hour or so for a short time, when he could endure it no longer. The tumour, however, altered very little during six weeks of this treatment, the tourniquet being apparently used during the daytime, as described, and left off at night.

"On October 9th and 10th there was a very decided improvement in the condition of the tumour; its density had further increased, and the pulsations considerably lessened in force. Notwithstanding these changes, Mr. Lawson decided to adopt the treatment pursued by Dr. Murray, of Newcastle, and to compress the abdominal aorta so as to completely arrest the flow of any blood through the aneurismal sac.

"On October 10th, at three o'clock p.m. (the bowels having been completely emptied by an enema), the man was placed thoroughly under the influence of chloroform. A Lister's tourniquet was applied over the abdominal aorta, just above the umbilicus, and another tourniquet was placed firmly over the femoral artery, just below the

aneurism. The tourniquets were kept on for twenty-three minutes. Only once at the end of the fourth minute did a wave of blood pass into the sac of the aneurism, when the tourniquet over the aorta was at once tightened; but for the remaining nineteen minutes absolute compression of the vessel was maintained, and no blood entered the tumour. The man now became somewhat collapsed, and began to retch. The tourniquets were then removed, and the administration of the chloroform stopped. The pulsations in the tumour were diminished, but they were perfectly regular. It was, however, impossible to say whether the diminution of the pulsations were not due to the collapsed state of the patient. The man was returned to his bed, and the Carte's compressor readjusted over the artery. When the effects of the chloroform had completely passed off he complained of coldness and numbness of the legs, with soreness in the belly. At seven o'clock in the evening, finding that all pulsation had ceased in the aneurism, the man himself removed the compressor. The house-surgeon was at once sent for, and he found that there was no longer any pulsation in the tumour. Since that time the man has continued to im-

prove. With the exception of some feeling of soreness in the belly, which lasted for about thirty-six hours, the patient suffered no inconvenience from the treatment.

"October 17th.—The aneurismal sac is now felt as a hard oval mass; the artery which leads to it pulsates strongly, but all impulse ceases at the upper edge of the tumour."

Nothing could better illustrate the difference between the slow and rapid method than this case. Contrast the six weeks' application of Carte's compressor (without chloroform) to the femoral artery, with the application of Lister's tourniquet (with chloroform) to the abdominal aorta—the difference between six weeks of imperfect pressure, and twenty minutes of an absolutely stagnated current.

Mr. Russell's Case.

The last case was cured by Mr. Russell, of Newcastle-on-Tyne. I had the pleasure of seeing that in his case the treatment was carried out most carefully, and terminated successfully after a few hours' trial.

There is one point established by these cases which strongly confirms the above theory of the

process by which the cure is effected. I refer to the apparently unchanged condition of the aneurism until the last hour, half-hour, or twenty minutes of the pressure. The value of this fact is still further enhanced when we remember that more than one surgeon has observed that while treating their cases by the old method, they had been surprised to find, after long and anxious waiting, that a sudden and unexpected change had taken place, by which the cure was at once completed. Dr. Banon's case, related below, was in all probability one of this class. The truth is, that in this case, as soon as the current was sufficiently arrested, the new method was unconsciously being tried instead of the old.

Dr. Banon's Case.

Dr. Banon states "that the man had suffered from popliteal aneurism, and compression was used for some time ineffectually. He was worn out by want of rest. He got a large opiate, which produced great somnolence. When in this state the resident pupil screwed down the instrument so as to stop all pulsation, and in six hours the tumour was solid. The man was in as favourable a condition for the adoption of pressure as if

he had got chloroform. This remarkable case occurred in 1849.

I may further state that Sir William Fergusson, Mr. Erichsen, and many other eminent surgeons have expressed to me the opinion that complete pressure under chloroform leads to rapid cure by coagulation; and the late Mr. Charles Moore was a most enthusiastic supporter of the same view.

CONCLUDING REMARKS.

As I am not professing to write a complete history of the rise and progress of the rapid cure of aneurism by pressure, I do not enter into the many interesting steps by which the minds of several eminent men were led to foreshadow the possibility of its introduction. Were I to do this I should have to revert to the late Professor Porter's suggestions and to Banon's case as very early indications of what was to come; and, later than these, I should have to speak of the work done by Porter and O'Ferrall and many other Dublin surgeons, of Lee, Ernest, Hart, Bryant, and Heath in this country, and possibly of authorities in France and elsewhere. I write, however, as having been the first to put the

treatment within the pale of accepted practice by introducing chloroform as an essential part of the treatment. Before this was done, the possibility of completely arresting the current in an aneurism was a matter of great doubt and difficulty, whereas now it is a matter of certainty and *not* difficult. I would add that, as yet, the advocates of the new method do not seem to have grasped the idea that in all aneurisms, of and below the aorta, pressure ought to be made on that vessel, both because of the ease with which it is commanded, and because by pressing it you ensure a complete arrest of current in any of the aneurisms of the lower part of the body. It is for this reason that I fail to see the value attached by Dublin surgeons to Dr. O'Ferrall's suggestion of applying distal pressure. The value of his suggestion is great if the circulation be not completely stopped in all vessels which by anastomosis may throw blood into the aneurism ; but this seems to me to be impossible where the aorta itself is fully commanded. The application of distal pressure is therefore unnecessary where the proximal pressure can be applied above the division of the aorta.

The foregoing case, together with the experience derived from the cases treated since the year

1864, and the inference that can be legitimately drawn therefrom, have, I trust, been sufficient to establish the following points :—

1st. That it is possible to cure aneurisms by pressure more rapidly than has hitherto been supposed.

2nd. That this is to be done by completely arresting the flow of blood through the aneurism for a period of time which is only a fractional part of that required by the old method.

3rd. That chloroform is a necessary adjunct to the treatment in most cases.

I will conclude my remarks by a reference to the following propositions, which contain a statement of the basis on which the new method rests.

First Proposition.—The introduction of chloroform in the treatment by compression in no way interferes with the coagulation of blood or the deposition of fibrin ; by *it* this treatment is applicable to arteries in the most sensitive and delicate situations, and it may be used for several hours continuously without danger to the patient. Its use will add greatly to the ease with which the treatment can be carried out, and therefore to its efficacy. It ought to be used in treating

aneurism in *other* parts as well as those in the abdomen.

Second Proposition.—From the cases now on record it is evident that consolidation of fibrin alone does not cure the aneurism; *this is due mainly to coagulation of blood.* This is proved, 1st, by the clear indications in several cases that the tumour ceases to beat and becomes solid within the space of *one hour* (see report of the cases) when once the conditions of coagulation are established; 2ndly, by the rapid disappearance of the tumour after it becomes solid. In the Newcastle cases the diminution in the size of the tumours was a surprise to us all. The rapid formation and removal of the clot argues powerfully *for* its sanguineous and *against* its fibrinous nature.

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