The collected works of Dr. P. M. Latham : with memoir by Sir Thomas Watson. Edited for the society by R. Martin.

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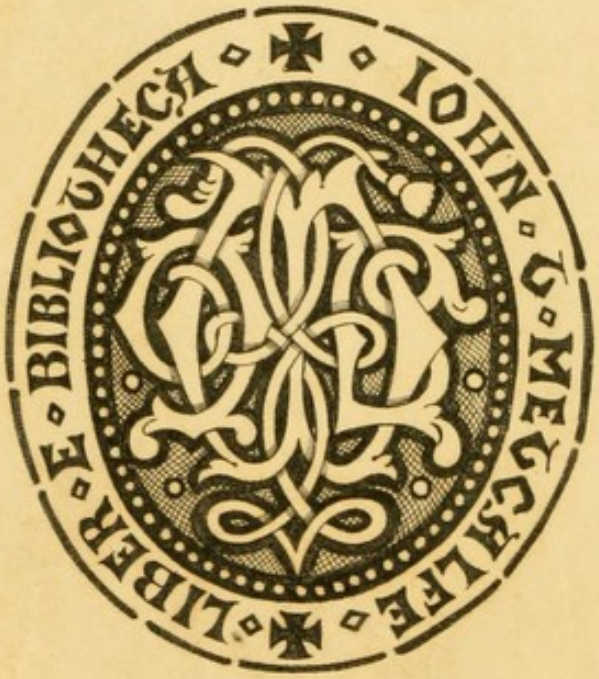
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THE NEW SYDENHAM SOCIETY.

INSTITUTED MDCCCLVIII.

VOLUME LXXX.

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THE

COLLECTED WORKS

OF

DR. P. M. LATHAM.

EDITED FOR THE SOCIETY

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VOL. II.

(WITH INDEX.)

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EDITOR'S PREFACE.

In arranging the order in which reprints of Dr. Latham's published works should be given by the New Sydenham Society, it was considered advisable to depart from the strict chronological sequence in which they first appeared. The two octavo volumes in which were contained his "Diseases of the Heart," written, as they were, when his always feeble physical powers perhaps best served him, and when he was in the fullest enjoyment of his always great mental vigour, have been held by general consent to constitute his "opus magnum," the work by which he has been most widely known, and from which, indeed, he soon came to be especially known as "Heart Latham." These lectures, therefore, seemed naturally to demand priority of publication; and the convenience they offered for making in themselves a first volume, of manageable size, being not lost sight of, they were allowed to take precedence of writings which had been published some twenty years earlier.

Next in importance to the Lectures on "Diseases of the Heart," and comparing favourably with them in all the vigour of expression and grace of diction of which their author was so great a master, comes the series in which he treats, more generally, a variety of subjects connected with Clinical Medicine; and with it the volume now offered to the members of the New Sydenham Society begins.

Written more than forty years ago, when the science of auscultation was only in its infancy, and as yet had done little, if anything, for the elucidation of heart disease, they were intended especially to simplify auscultation for practical purposes in the diagnosis of diseases of the lungs. But this formed a part only of the material of the Lectures; and, admirably as the subject which Dr. Latham had especially in view was treated (the leading points of his doctrine still stand fast and true), the student of the present day, be he "tender juvenal" or "tough senior," will derive equal pleasure and profit from a perusal of that earlier portion, in which he handles the subjects

of medical education, of case-taking, of pathology, and of the doctrine of symptoms.

Next in the order of our reprints, but published originally in 1825, comes an "Account of the Disease lately prevalent at the General Penitentiary." Sir Thomas Watson, in his biographical memoir of Dr. Latham, has written of this work, that it is "pregnant with evidence of acute and patient research, and of clear and cogent reasoning;" and to this may surely be added the assertion that so graphic, and at the same time so picturesque, is the narrative, as to give to the reading all the charm of a really good romance. The story of the sanitary vicissitudes of the General Penitentiary since Dr. Latham's day is so interesting, and so instructive, that it has been thought well to bring it up to the present time; and, accordingly, some extracts from the writings of Dr. Baly, of Dr. Guy, and of their successor, Mr. Gover, the present Medical Officer of the Institution, all of whom have contributed to making the prison at this moment one of the healthiest temporary residences in England, will be found at the end of this Preface.

A paper, which was read before the College of Physicians in 1832, on "the use of Opium in Fevers," short, but perfect as a model of style in embodying the results of patient observation, follows next, and contains the germ, which has since developed, under the fostering care of Graves and others, into a recognised and full-grown mode of treatment.

Last of all are given a series of articles, written, and published in the *British Medical Journal*, during the years 1861, 1862, 1863, having for their common title, "General Remarks on the Practice of Medicine," together with one, consisting of a few pages only, and containing Dr. Latham's last wise words on the subject which, through all his long life, he had most at heart—"Medical Education."

Born on the 1st day of July, 1789, Dr. Latham was 72 years of age in 1861, when the first paper of this last series was printed; and 76 in 1864, when the last of them appeared. To how few men in their eighth decade has been given the power of sustained mental effort, even when their physical force has been but little abated! How many men have there not been, who, tempted, at that age, once more to return to literary work, and failing to recognise the waning of their powers, have

gone far by their later labours to imperil their early fame, and have laid themselves open to the imputation of writing "Homilies beginning to smell of apoplexy." Dr. Latham was one of the favoured few.

Sir Thomas Watson writes of him: "That he lived so long was a marvel to all who were aware of his bodily constitution. I have said that in childhood his health was delicate. In mature life, when he was living in London, besides a slight twist in his spine, which tilted one shoulder a little upwards, his lungs were known to be extensively emphysematous, and his paroxyms of asthma by night were so extremely violent and exhausting, that many times he fully expected to die before the morning. Had he remained in London it is almost certain that some one of such paroxysms would have been fatal."

It was during the last four years of his residence in London, and during the merely comparative ease afforded between the paroxysms of, often well-nigh mortal, distress which are described in the paragraph above quoted, that Dr. Latham produced the really philosophical essays with which this volume concludes.

Where all are so admirable, it were perhaps well to avoid the singling out of any one as though pre-eminently good. Still, it may be permitted here to direct especial attention to article No. VI., page 390, on "Cure," and to beg of the reader, who has gone thus far with us, to turn at once to it for enjoyment, and for a foretaste of much like excellent fare which is in store for him.

A careful perusal of Dr. Latham's "Account of the Disease lately prevalent at the General Penitentiary," by the additional light thrown upon its obscurities from the researches of Dr. Jenner in the different forms of Continued Fever, and of Drs. Baly and Guy in the subject of Scurvy, will leave but little doubt in the minds of any reader that the disease was Dysentery, complicated, in most of the cases related, with Scurvy, and in many, with Typhus (?), Typhoid, or Relapsing Fever.

Dr. Latham, writing for himself and the physicians associated with him, summarizes his belief as to the causation of the disease in the following words, p. 314:—"That it was derived

originally, and essentially, from something noxious in the situation, and was rendered epidemic by an impoverished diet and a severe winter."

Leaving, for awhile, the first factor here mentioned, the "something noxious in the situation," let us see how far other and later observers agree with him as to the potency of the second, "an impoverished diet."

Dr. Guy, who succeeded Dr. Baly as Physician to the Penitentiary, read an elaborate paper before the Statistical Society (June 15, 1863), upon "Sufficient and insufficient dietaries, with especial reference to the dietaries of prisoners." In this paper he discussed the question of the causes which led to the Millbank outbreak; and inasmuch as he has given, in addition to his own observations upon this head, a good digest of Dr. Baly's earlier researches in the same direction, it will be well to quote his remarks at length:—"I must now revert to the outbreak of scurvy and dysentery which occurred at Millbank in 1823, and endeavour to answer the important question whether that fatal epidemic ought to be attributed to the reduction which took place in the quantity of food, or to the omission from that dietary of some important element. Now, the reduction effected in the original dietary of the prison went to the extent not merely of cutting down the quantity of bread, meat, and potatoes from a total of 304 ounces to one of 168 ounces per week, but the meat and potatoes were struck off altogether, except such small quantity of the juice and fibre of meat as was to be found in a broth containing one ox head, and even less, to one hundred rations. On a liberal estimate, the quantity of meat in the weekly rations of soup did not exceed ten ounces for each prisoner. In order to ascertain whether the mere reduction in the quantity of the food could have been productive of such disastrous effects, I must again refer to the diet scales given in the earlier part of this paper. I find that the least liberal of the six pauper dietaries of 1836 allowed only 145 ounces of solid food, and 18 pints of liquid food; that the average of these dietaries only exceeded the reduced Millbank allowance in the solid elements by 5 ounces per week, while the liquid elements amounted only to 10 ounces in lieu of 21; that the minimum of the twelve pauper dietaries sanctioned by the Poor Law Board (doubtless after experience of their

sufficiency) gives only two additional ounces of solid food per week, the liquid food amounting to only 11 ounces as against 21 of the Millbank dietary; that all the dietaries prescribed for the Scotch paupers in 1847, by Drs. Alison and Christison, fall greatly short of the Millbank reduced standard in their solid constituents; that the diet for soldiers under solitary confinement for less than 56 days also falls short of it both in the solid and in the liquid constituents; that the seventh of the Glasgow dietaries has 21 ounces less of solid food and 10½ pints of buttermilk for 21 pints of gruel and broth; and lastly, that the consumption of agricultural and other labourers, according to Mr. C. Mott's enquiries, falls short of the Millbank reduced dietary by no less than 28 ounces. This last comparison, if the consumption of the labourer is correctly estimated, is the most important of all, inasmuch as of the 140 ounces said to be consumed by him, 136 consist of bread, of which the quantity given at Millbank amounted to 168 ounces.

"After making due allowance for the long terms of confinement to which the prisoners at Millbank were subject in 1822, I am still of opinion that the mere reduction in the quantity of the food would not account for the outbreak of scurvy, even in a site so notoriously unhealthy as Millbank then was. Nor, with the experience of Devizes, Stafford, and Glasgow before us, would it be safe to attribute the outbreak to the omission of meat from the dietary. There still remains a possible explanation of the event to which the researches of the late Dr. Baly lend an air of probability.* In the 'London Medical Gazette,' February 10th, 1843, Dr. Baly published a short paper 'On the Prevention of Scurvy in Prisons, Pauper Lunatic Asylums, &c.,' in which, after citing Sir Gilbert Blane and Mons. Julia Fontenelle in favour of the anti-scorbutic virtue of the potato, whether raw or cooked, he proceeds to make the following important statement:—'In the Spring of 1840, I found that scurvy was a disease of rather frequent occurrence amongst one class of prisoners in the Millbank Penitentiary—the military offenders sentenced by court-martial; whilst amongst the other more numerous class of inmates, the convicts, it was never

* For "lend an air of probability," may not here be read, "*give well nigh absolute certainty?*"—EDITOR.

seen.' This led Dr. Baly to a comparison of dietaries, of which I append the particulars in a tabular form, the arrangement only being altered from his own Tables. I omit the column showing the dietary for women, as this paper must be understood to deal throughout with the dietary of adult males only.

	Military Offenders.			Convicts
	First 3 Months.	Second 3 Months.	Over 6 Months.	
	ozs.	ozs.	ozs.	ozs.
Bread	168	168	168	176
Meat	12	18	24	20
Potatoes	Nil.	Nil.	8	80
Cheese	Nil.	Nil.	Nil.	4
Onion	Nil.	Nil.	Nil.	1
Total solid food ...	180	186	200	284
	pints.	pints.	pints.	pints.
Rice soup, without vegetables	2	2	2	—
Peasoup, with vegetables ...	—	1	1	1½
Gruel	17	15	14	11
Broth	—	—	—	3
Total liquid food ...	19	18	17	15½

The following is an abbreviation of Dr. Baly's valuable commentary on these facts:—

"Nearly all the cases of scurvy, he says, occurred in soldiers who were passing through the second three months of their confinement in the Penitentiary, during which period not only had they very nearly as ample a supply of animal food as the convicts, male and female, but they had as much soup seasoned with vegetables as the female convicts, who, although undergoing far longer terms of imprisonment, yet were free from scurvy. This exemption of the convicts from the disease could, therefore, only be attributed to their weekly diet, containing 5 lbs. of potatoes and an onion. In order to afford to the soldier a larger supply of vegetable food, Dr. Baly suggested the substitution for the rice soup, which contained no fresh vegetables, of pea-soup with vegetables. The quantity of soup containing succulent vegetables was thus made to exceed the quantity given to the convict, and yet scurvy continued to appear among the soldiers. It prevailed to nearly, if not quite, the same extent after the change of diet as before; and it was evident that the quantity of vegetables usually contained in two or three pints

of peasoup given weekly was inadequate to prevent the occurrence of scurvy. Dr. Baly accordingly recommended that the soldiers, as well as the convicts, should have 1 lb. of potatoes with each dinner of meat. The soldiers thus came to have 2 lbs., or 32 ounces of potatoes every week during the first three months of their imprisonment, 3 lbs. during the second three months, and 4 lbs. after the expiration of six months. This addition to the dietary of the military prisoners was made in January, 1842, and not a single case of scurvy occurred up to February 10th, 1843, the date of Dr. Baly's paper." Dr. Baly then proceeds to give a short sketch of the outbreak of scurvy and dysentery at Millbank in 1823; and, after stating that he deems it "unnecessary to argue that the want of animal food could not have produced the scurvy," and showing that the reduced dietary "was not deficient in vegetable constituents, except as regarded the potatoes," gives it as his opinion "that the withdrawal of the supply of potatoes was, in all probability, the cause of that part of the epidemic which was constituted by the scurvy;" and this inference he strengthens by the remarkable statement, that since the date of the outbreak of scurvy and dysentery, the diet of the convicts "has contained an abundant supply of potatoes, and scurvy has never again attacked them, although other forms of disease, which were described as parts of the epidemic of 1823, namely, the fever, dysentery, and nervous affections, have frequently reappeared.

"Dr. Baly then fortifies his opinion thus expressed, by citing the case of the Oxford County Gaol, in which a diet, consisting of bread 168 ounces, meat 4 to 12 ounces, and 14 pints of gruel per week, but with no regular allowance of vegetables, 'potatoes or green vegetables being given only occasionally on Sundays, when the prison garden would furnish them,' issued in the production of scurvy; and the case of the Northampton County Gaol, in which scurvy, having arisen under a dietary of bread, soup, and gruel, disappeared after the addition to it of 4 lbs. of potatoes weekly. Other analogous facts are cited, and especially the striking case of the Stafford County Gaol, which I have already noticed, where, under a liberal allowance of bread, potatoes, and gruel, but no meat, no soup, and no milk, scurvy did not show itself.

"It is worthy of remark that the diet of military offenders for the first three months approximates very closely to the reduced dietary at Millbank. The quantity of bread is exactly the same, the 12 ounces of meat is little more than the equivalent, in the solid form, of the 10 ounces of meat in the soup at Millbank; while the two pints of rice-soup, without vegetables, and the 17 pints of gruel, would probably contain less than the 14 pints of gruel and the 7 pints of soup after the supposed removal from it of the 10 ounces of meat.

"The scurvy, which was a new disease at Millbank, in 1822, occurred, therefore, under the long-continued use of a diet differing little from the military diet of the first three months, and not falling greatly short of that of the second three months. The condition of the prisoners in Millbank in 1822 resembled that of the soldiers in 1840 in the total omission of the potato, the site of the prison, the season of the year, and the scurvy.

"It is probable, therefore, that the epidemic scurvy, which associated itself with the epidemic dysentery of Millbank as a mixed epidemic, in 1823, was not due to the mere reduction in the quantity of food, nor to the omission of solid meat from the dietary, but to the total exclusion of the potato element.

"In this outbreak of disease, therefore, we have no clue to the solution of the question of sufficient or insufficient dietaries. It is obviously quite possible that the reduction of the quantity of bread from 168 ounces a week, or a pound and a half per diem, to 112 ounces a week, or a pound per diem, and the substitution for the half-pound of bread of the same quantity of potatoes, would have saved the prisoners from the scurvy, and the Government from the anxiety, trouble, and cost which the sad and perplexing epidemic of 1823 entailed upon it."

Now, to return to the other factor, "something noxious in the situation," as a cause of the disease. Dr. Baly, after many years of unremitting labour as physician to the Penitentiary, during which time he had under observation several hundred cases of dysentery, and after experiencing at least one very severe and fatal epidemic of the disease in 1842, discussed the question of a "local noxious influence" very fully on two occasions: first, in a paper read before the Medico-Chirurgical Society, in 1845, on "The Mortality in Prisons," and later, in

his Gulstonian lectures on dysentery, delivered at the Royal College of Physicians, in 1847.

The latter of these two works embodies the conclusions arrived at in the former upon the question now before us, and inasmuch as the lectures are not readily accessible to the majority of readers, and because they contain a masterly summary of the reasons for and against their author's expansion of Dr. Latham's expression, "something noxious in the situation," some pages of his text are here given as a valuable contribution to state medicine:—

"The physicians who had the medical charge of the Penitentiary during the epidemic of the year 1823, reported as their final opinion that the disease had been produced by a local noxious influence. They adduced good reasons for holding this opinion, and subsequent occurrences have shown its correctness. Diarrhœa of a very mild character has seldom been altogether absent from the Penitentiary: dysentery has been a frequent disease there; and in one year this disease has prevailed as a severe and fatal epidemic.

"It cannot be doubted, therefore, that the cause of the disease is a noxious influence fixed on the spot, but capable of undergoing variations in its power of action. The term noxious influence, however, is a very general one. Cannot the nature of this influence be more closely defined? I think it can. Here, as in other instances where dysentery is endemic in prisons, workhouses, or lunatic asylums, the cause really producing it is, I believe, a malaria rising from the surface of the ground around the building. There are other influences from which dysentery might be supposed to arise, namely, diet, the water used as drink, defective ventilation, and defective sewerage. None of these, however, can have been the efficient cause of the disease in the Penitentiary. I think it unnecessary to detail on the present occasion the facts by which this has been rendered certain. I shall therefore at once proceed to adduce those reasons which, to my mind, prove the dependence of the disease on a gaseous poison or malaria derived from the soil of the surrounding grounds.

"One of these reasons, and an important one, is the relative frequency of the cases of dysentery and bowel complaints in general at different seasons and in different states of the weather.

They prevail most in the autumn and in the spring, especially in a wet autumn following a hot summer, or in a mild spring when the preceding autumn was wet and the winter severe; in other words, at those times when, from the state of the soil and atmosphere, the decomposition of the organic matters in the soil is necessarily most active. It may be recollected that the epidemic of the year 1823 commenced at the close of a very cold winter, and that the bowel complaints especially became prevalent and severe when mild spring weather ensued. The rise and progress of the epidemic of the year 1842 have a similar history. The latter half of the preceding year was remarkable for long-continued rains; and in the month of October the low garden grounds to the north of the prison were inundated, owing to the river overflowing its banks. A cold winter followed. In the latter part of February, 1842, the weather was mild, while the atmosphere was humid; and then it was that cases of dysentery suddenly became very numerous amongst the prisoners, and continued so throughout the month of March. From the beginning of April to the middle of July the weather was dry and cool, and the dysentery gradually subsided; but at the close of July very hot weather set in, and then the disease became prevalent more or less throughout the country, and again attacked a large number of the prisoners in the Penitentiary. At the end of September the hot weather was succeeded by a cold and dry state of the atmosphere, and the prison once more became very healthy. Lastly, about the middle of October, a moist and foggy state of the air ensued, and then dysentery returned, together with fever. This coincidence between the prevalence of dysentery and bowel complaints on the one hand, and particular states of the weather on the other hand, has been observed in years when those diseases have been less prevalent than in the year 1842, and it seems to me reconcilable with no other theory of the cause of the diseases in question than that which ascribes them to the influence of a malaria rising from the soil.

"Another class of facts, strongly supporting the same theory, are those which show the close alliance between dysentery and other diseases which are more indisputably of miasmatous origin, namely, common cholera and fever. Not merely have epidemics of dysentery in the Penitentiary been preceded or followed by

the prevalence of one or other of those diseases, but an attack of dysentery has often been ushered in by cholera, or has been combined with fever in the same patient. The direct transition from the choleroid state, attended with rice-water evacuations, to a condition characterised by all the symptoms of inflammatory dysentery, has been often observed, especially in hot summers and autumns.

"I have already mentioned the frequent co-existence of typhoid fever and dysentery in the same patient, in speaking of the morbid anatomy and of the symptoms of the latter disease. Dr. Latham, too, noticed the association of a fever with the other disorders which constituted the epidemic in the Penitentiary in the year 1823, and he has given an account of its relation to those other disorders according so closely with what I have myself observed, that I shall quote the passage:—

"'While the flux of the bowels and the disorders of the brain and nervous system,' Dr. Latham says, 'prevailed to their greatest extent, the cases of fever were rare. It was not until these complaints began to subside, that the fever showed itself in a sufficient number of cases at once to make us accurately acquainted with its type. At no time did it pervade the prison to an equal extent with the other two forms of disease, but it had a just claim to be considered as a part of the disease of the Penitentiary; and the manner in which it was mixed up with the disorders of the bowels and the brain and nervous system led to the belief that they had all a natural relation to each other, and that they all sprang from one and the same morbid condition of the constitution at large.'

"The fever thus associated with dysentery at the Penitentiary has, during the period in which I have observed it, been generally characterised by the predominance of intestinal symptoms; sometimes, in fact, the affection of the larger bowels formed so important a part of the disease that it was difficult to say whether the patient was labouring under fever complicated with dysentery, or under dysentery with unusually active symptomatic feverishness. In other cases the principal disease was evidently idiopathic fever, the symptoms of the bowel affection being by comparison only faintly developed.

"I must here, however, remark that fever at the Penitentiary has not always had these characters; sometimes other

organs than the intestines, namely, the brain or the lungs, have chiefly suffered. In a few remarkable instances, too, the sole characteristic feature has been profuse and constant sweating; the other symptoms being headache, pain in the loins, a thickly-coated tongue, a quick and feeble pulse, and great general debility. In these cases life was sustained, and health at length restored, only by the aid of wine and bark very freely administered. Cases of a similar kind seem to have been observed by Dr. Latham, though, in the instances he notices, the sweating and debility did not come on till the second week of the disease, while in those I have seen they existed from the first. The fever, however, of most frequent occurrence in the Penitentiary has been that in which abdominal symptoms predominated, and in which, after death, not only the ileum, but the colon also, was found affected in various degrees.

"The close alliance subsisting between dysentery and continued fever in London strongly impressed the mind of Sydenham. This is shown not merely by his denominating a certain fever that was associated with dysentery the 'dysenteric fever,' but also by his oft-quoted remark that 'dysentery was the very fever itself, with this sole difference—that it was turned inwards, and discharged from the system through the bowels.'

"A close connexion, then, subsists between dysentery and fever; and the prevalence of dysentery, and of the bowel complaints allied to it, is greatest at those seasons, and in those states of the atmosphere, which most favour decomposition of organic matter in the soil. Now these two facts, together with the constant, or almost constant, presence of the disease in a mild form, and the absence of other causes capable of accounting for it, satisfy my mind that the dysentery observed in the Penitentiary at Millbank is really produced by malaria.

"But several objections may be opposed to this conclusion. The first I shall notice is, that dysentery, or at all events, diarrhœa, is a common disease in prisons and workhouses, and in them appears to be produced by deficient nourishment; an increase in the diet having, it is said, in many instances, caused the disappearance of the disease.

"To this objection I would answer, that, the prevalence of bowel complaints in the establishment here referred to has not

been caused by the poorness of the diet. During the summer of the year 1842, I visited many prisons, workhouses, barracks, and lunatic asylums, in various parts of England, with the view of gaining information which might throw light on the origin of the disease prevailing at Millbank. And I found that the degree of prevalence of bowel complaints in the different establishments bore no constant relation to the poorness of the diet, any more than it did to defective ventilation, or impurity of the water which the inmates drank. Dysentery and diarrhœa prevailed in barracks and lunatic asylums where the dietary was abundant, and were absent from prisons in which the allowance of food was scanty. In many instances, too, where a salutary effect had been produced by an increase of the dietary, the bowel complaints, before rife, had only been rendered less prevalent; they had not disappeared. It seemed, therefore, impossible to believe that poorness of the diet was the primary and essential cause of the disease. On the other hand, I found that the frequency of bowel complaints in the different establishments I visited *did* bear a relation to the character of their sites. Where those complaints were of frequent occurrence, there the site of the building was low, the ground around it damp or imperfectly drained, and the subsoil often formed of peat or clay. Where bowel complaints were infrequent, the site was elevated, the soil dry, and the subsoil generally composed of gravel. The conclusion, therefore, I arrived at respecting the origin of bowel complaints in these different institutions was, that they were really caused by malaria, and that their frequency and severity had been affected by the diet only in so far as poorness of the diet had in some cases produced a great susceptibility of the influence of the malaria, and the subsequent increase of the diet diminished this susceptibility.

"The second objection to the theory which ascribes the bowel complaints prevalent in the Penitentiary to the influence of malaria, is that the inhabitants of the immediate neighbourhood do not suffer in a similar way. This is true. Even the private families residing in the Penitentiary are seldom at all affected with the prevalent bowel complaints, and very seldom indeed affected in a severe degree. But surely this is no valid objection. For it must be remembered that these free persons are living under conditions very different from those which

surround the prisoners. They are not constantly confined to the atmosphere of the locality; they commonly drink in the course of the day some fermented liquor, as well as other stimulants of the nervous system, such as coffee and tea; their bodies are kept in a healthy state by active and voluntary exercise, and their minds by cheerful and varied trains of thought. The prisoners have none of these advantages. It might be expected, therefore, that the former class of persons would be insusceptible of disease from causes which might affect the prisoners very generally. There is, indeed, nothing wonderful or remarkable in one class of persons enjoying a comparative immunity from the effects of morbific agents which prove hurtful, and even destructive, to those of another class. During the prevalence of almost every epidemic malady, we see that the poor are cut off, while the rich generally escape. And this has been especially observed of dysentery, when it has been most destructive to our troops, not only in military campaigns during the war, but also in time of peace, when the barracks have had unhealthy sites. The officers have been far less affected than the private soldiers.*

"The consideration of these objections, then, tends by no means to weaken my conviction that the bowel complaints prevalent in the Penitentiary have been caused by malaria. On the contrary, it seems to me to render this theory more probable, since it shows why the effects of malaria on the prisoners have been so well marked, though the neighbourhood of the Penitentiary has not manifestly the features of a malarious locality.

"For it has been asked, whence comes the malaria? There is, indeed, no marshy ground near the prison, no considerable extent of stagnant water: there are none of the more obvious sources of miasm. Still, if we examine closely the state of the ground around the Penitentiary, we find in it many points of resemblance to those tracts near the borders or mouths of rivers where dysentery is so often an endemic disease. The tracts of country to which I allude are low, and have a damp alluvial soil, containing much organic matter. Now such, in a less degree, is the character of nearly all the open ground around

* Pringle, Diseases of the Army; Tulloch, Reports of the Health of the Troops; Annesley, Diseases of India.

the Penitentiary. The building itself stands upon an artificial hill, but the surface of the surrounding ground is below the level of high water in the river. The soil is loose, and contains much organic matter, and the subsoil is formed in most parts of an imperfect clay, beneath which are alternate strata of peat-earth, marl, and sand. The ground has hitherto been imperfectly drained. But the chief defect of the site seems to consist in the beds of sand just mentioned being full of water, derived, in all probability, from rain which had fallen on distant and higher grounds. In rainy seasons this water rises through the many breaks in the superficial clayey stratum, and keeps the soil itself, at all events its deeper layers, in a very wet state. These characters show that the exhalation of miasms from the ground around the prison is by no means impossible. And there will, I think, be no difficulty in admitting even the probability that sufficient malaria is produced there to affect persons in whom, from mental as well as physical causes, the power of repelling the attacks of morbific agents is weakened. Indeed, that spot must be a remarkably healthy one where no disease attributable to malaria would show itself amongst persons in the condition of prisoners. And in the Millbank Penitentiary, at the periods when dysentery has chiefly prevailed the prisoners' susceptibility of disease must have been unusually great, owing to the long terms of confinement then enforced. I believe, therefore, that the dysentery and other bowel complaints which have been prevalent in the Millbank Penitentiary are attributable to its site. But, at the same time, I must remark that the site is not an eminently unhealthy one; this, indeed, is proved by the fact, that the inhabitants of the neighbourhood, and the families residing in the prison itself, have seldom been affected with any disease attributable to any endemic influence; and I may add, that the site would never have been discovered to be other than perfectly healthy had not a prison been built there.

"My remarks on the causes of dysentery in the Penitentiary and similar institutions have been extended; for my personal knowledge of the facts seemed to justify my entering into details on the subject. In speaking of the disease as it is seen under other circumstances, I shall be more brief, as my conclusions must here be drawn, for the most part, from the observa-

tions of others, and must consequently be less positive, and less entitled to attention. I cannot, however, altogether refrain from noticing the extremely unsatisfactory character of the doctrines generally propounded respecting the cause of dysentery.* It seems to be admitted by most writers on the disease that almost any agent capable of making an injurious impression on the body may produce dysentery. Some of these reputed causes of the disease we may, I think, safely reject. Such are, 'acrid ingesta and irritating secretions from the liver, pancreas, and upper part of the alimentary canal,' all of which have been supposed capable of producing the disease.† Such causes as these might occasion irritation of the parts of the canal through which they passed, and consequent purging, but it seems highly improbable that they should produce a severe and rapidly destructive inflammation, confined to the part of the canal they would last reach, and commencing around particular elementary parts of the mucous membrane, namely, the solitary glands. The idea that the autumnal dysentery, diarrhœa, and cholera of this climate, are due to unripe fruit, or other acrid ingesta, if entertained, would be negatived by the fact, that the same complaints prevail during the autumn in prisons, where those causes do not exist, and prevail there to even a greater extent than among the free population. But such an idea is, I believe, no longer entertained by any educated member of our profession in this country. The medical officers of the army in India, however, still reckon the influences I have mentioned among the causes of dysentery; and this seems the more extraordinary when we consider the frequently extreme severity of the inflammation of the large intestine in the dysentery of India, and the acknowledged existence there of a general and more adequate cause for the disease, namely, a noxious state of the atmosphere.

"Another class of the causes of dysentery admitted by most

* When this lecture was delivered, the author was not acquainted with the excellent article on dysentery, in the "Elements of Medicine," of the late Dr. Robert Williams.

† See Annesley, Researches on Diseases of India, vol. ii. p. 234, et seq.; Parkes, Remarks on Dysentery, &c., p. 131, et seq.; Copland, Dictionary of Practical Medicine, art. Dysentery, pars. 70, 71, and 72; O'Brien on the Dysentery of Ireland, p. 24.

writers, comprehends mere high temperature of the atmosphere, or cold and moisture combined, or sudden alternations of heat and cold, and especially the suppression of perspiration by cold dews during the nights succeeding to hot days.* But it is difficult to conceive why these causes should produce dysentery any more than any other internal inflammation, while it is obvious that a high temperature favours the rise of malaria from the soil, and that hot days and cold night-dews are characteristic of those countries and seasons in which malaria is most abundantly generated.

"In India, and in all climates where dysentery is a frequent disease, the influence of the season and weather on its degree of prevalence is the same as I have shown it to be at Millbank. Its close alliance, too, with typhus fever, and with cholera, has been generally observed. Even the fatal Asiatic cholera which visited Europe fifteen or sixteen years ago, manifested a close relationship to dysentery. The transition of cholera into dysentery was often seen in particular cases, and the epidemic of cholera in several places passed gradually into epidemic dysentery. Moreover, although cholera was not essentially characterised by disease of the large intestine, yet in many instances it was attended by a lesion of that part of the canal very similar to the lesion characteristic of dysentery. This fact is illustrated by one of Cruveilhier's plates (Livraison 14, Pl. 5.)

"From these facts, then, as well as from the consideration of the whole character of the disease, I infer that dysentery is always produced by a poison introduced into the system from without, and that in most instances this poison is generated by the decomposition of matters contained in the soil. Many other of the reputed exciting causes of dysentery, such as intemperance, or exposure to cold, may have a share in the production of the disease; not, however, by acting as the efficient cause, but merely by disturbing the general health, and thus rendering the body obnoxious to the influences of the atmospheric poison which it previously resisted.

"The most general source of the poison producing dysentery is certainly the surface of the ground, and the soils generating

*Twining on Diseases of India, pp. 2, 3, and 4; Annesley, op. cit. p. 244; O'Brien, op. cit. pp. 24-28.

it in the greatest abundance are those which are rich in organic matter, and are imperfectly drained. But still there are facts which show that dysentery may be produced independently of the source of malaria just mentioned. It has not unfrequently broken out in ships at sea, especially in tropical latitudes. In these instances the poison of dysentery may have been produced as an exhalation from bilge-water, or from decaying vegetable or animal matters in the ship; or the water used for drinking may have become putrid, and the poison having been developed in it, may have been carried into the system through the stomach. For there is no difficulty in admitting that the same noxious matters commonly disengaged from the surface of the ground under the influence of heat and moisture may be produced by the decomposition of organic matter in water, and that the water containing them being taken into the stomach, and thence absorbed into the blood-vessels, may produce the same injurious effects on the body, as result from the admission of those matters with the air into the lungs.

"I must here also remark that there appear to be other conditions of the atmosphere besides heat and moisture which favour the development of malaria from the soil. The degree in which dysentery prevails in a particular locality is not always obviously proportionate to the circumstances which are known to promote the decomposition of organic matter and the disengagement of the gaseous products of decomposition. Dysentery is in some years unusually prevalent over considerable tracts of country without any obvious cause. In these cases, however, the disease is especially severe and general in the spots where it is at other times endemic in a milder form, and where, from the character of the soil and disposition of the surface of the ground, the presence of malaria might be expected. In the same way, Asiatic cholera, when it visited Europe, showed a preference for the banks of rivers and low moist spots, although some general state of the atmosphere was undoubtedly necessary to its production.

"Some unknown state of the atmosphere, too, appears to have a share in determining whether the malaria shall produce a mild diarrhœa, a severe dysentery, cholera, or fevers of different kinds. For many of these diseases appear in the same locality at different times, without our being able to determine exactly why

at a particular time one disease and not the others should prevail. Sometimes, indeed, two or more of these diseases co-exist on the same spot, and it might hence be supposed that these variations in the effects of malaria are due to the greater or less intensity of action of the poison, and to varying states of the system of the persons on whom it acts. But this supposition would not accord with the fact that all forms of endemic disease are not met with in every malarious locality; that ague, for example, is never seen in certain spots where other endemic diseases are rife, and that in some localities where ague is prevalent other endemic maladies are absent. This fact, indeed, seems to show that there are as many distinct varieties of atmospheric poison as there are forms of disease belonging to the class of endemic and epidemic disorders.

"I must notice still another question connected with this part of my subject; namely, how the poison, when it has entered the body, produces the diseased changes. Does it itself circulate with the blood, and on reaching the part of the body with which it has some chemical affinity disturb its normal composition, and thus excite diseased action? Or, does the noxious agent act at first on the blood and produce with some of the elements of that fluid a new compound which becomes the immediate cause of the local disease? This question in the present state of our knowledge it is impossible to answer; but it seems to me equally impossible to doubt that in dysentery as well as in fever the intestinal lesion is the effect of a chemical action taking place between the glands, or the tissue immediately surrounding them, and a morbific matter circulating with the blood. The time, perhaps, will come when chemistry shall be able to detect the subtle agents which produce these and other disorders in our bodies, and shall explain the play of affinities which, disturbing the normal constitution of particular parts, causes destructive inflammation to arise in them.

"The poison of dysentery and that of typhoid differ much in their action on the human body. The poison of fever has an especial affinity for the glands of the small intestines, and also produces almost immediately an evident change in the constitution of the blood. The poison of dysentery attacks the glands of the large intestines, and in the more common sthenic form of the disease produces no obvious change in the condition

of the circulating fluid. There is, however, as we have before seen, an asthenic variety of dysentery in which the blood does apparently undergo a change in its composition and vital properties; and here we must suppose that the poison of dysentery is modified in its properties, or that it is combined with some other noxious matter capable of disturbing the normal constitution of the blood.

"It remains to inquire what cause or causes produced those *nervous disorders* which at different periods have appeared amongst the prisoners in the Millbank Penitentiary. How has it happened that disorders of this kind have shown themselves prominently in connexion with dysentery only in that establishment?

"The first important fact to be noticed in relation to this inquiry is, that these nervous disorders have not been constantly prevalent in the institution. They have appeared only at those times when dysentery was epidemic in the prison, or was about to become so. This fact suggests the inference that some alliance exists between the cause of the dysentery and the influence giving rise to these nervous disorders: an inference which is strengthened by another important fact, viz. that tetanus and neuralgia have been observed as endemic diseases in malarious countries.

"With respect to tetanus, no doubt exists but that it is far more common among adults in hot climates than in temperate ones, and in hot seasons than in those that are cool; while the tetanus or trismus of infants is seen chiefly in pestilential countries, such as the West Indies, and in temperate climates only under circumstances which favour the belief that it is produced by a noxious state of the atmosphere. Now it has been observed that the adults attacked by idiopathic tetanus in hot climates are for the most part persons who have suffered hardships, or have been more than usually exposed to the deleterious influence of the climate. The infants we may suppose to have been predisposed to suffer from the disease by the delicacy of their whole constitution, and especially by the excitability of their nervous system.

"Will not these facts help us to explain the occurrence of peculiar nervous disorders in the prisoners at Millbank? Besides confirming the opinion that the efficient cause of these disorders

is some kind of malaria, do not these facts also render it probable that a peculiar state of the system of the prisoners has predisposed them to be thus peculiarly affected by a cause which would have produced no such symptoms in other persons?

"We have already seen that the effects of imprisonment on the nutritive system are such that severe inflammation and change of structure is produced in the prisoners by a morbific influence which does not affect free persons who are equally or almost equally exposed to it. May not imprisonment so affect the nervous system, likewise, as to give it an extreme excitability comparable to that which predisposes infants to suffer from trismus under the influence of malaria?

"If we consider for a moment the effect which long-continued exclusion of light has on the eye, the great sensibility of that organ which results, so that it cannot bear ordinary daylight, suddenly restored, without pain, or sunlight without danger, we shall, I think, find it reasonable to expect that the whole nervous system of prisoners who have been very long confined in complete or almost complete seclusion from society and from all the ordinary sources of mental excitement will manifest an exaggerated sensibility to the influence of unusual stimulants. At all events, facts have occurred during the last few years which prove the existence of this sensitive state of the nervous system in prisoners under such circumstances.

"Prisoners sentenced to transportation after being confined for a longer or shorter time in Government prisons in a state of the greatest order and silence, deprived of the society of their fellows and of all the causes of excitement to which they had been accustomed, have been suddenly transferred to convict ships in the river, where they have been thrown together without discipline or restraint of any kind, and exposed to the additional excitement of the parting with friends, and to the tumult which must exist in ships preparing for sea. The effect of this sudden change has been that many of the prisoners have been thrown into fits of epileptic convulsions: not merely men and women previously subject to epilepsy, but those who had never before suffered from the disease, have been so affected. This has occurred, not once only, but several times, many prisoners being attacked with epilepsy in each ship.

"It is only during the first few days after embarkation that

these symptoms of disordered nervous system have been observed; and no serious consequences have been left. But although so temporary, these phenomena are important from their proving that imprisonment in a state of seclusion induces, for a time at least, an enfeebled, and in consequence highly excitable state of the nervous system.

"Now both in the year 1823 and in the year 1842, when the nervous disorders associated with dysentery occurred in the Penitentiary, the terms of confinement to which the prisoners were subjected were much longer than at present. We may therefore, I think, fairly infer that the excitability of the nervous system of the prisoners was at those times proportionably great.

"Can we not now explain the occurrence of the nervous disorders in the Penitentiary? We have seen that they were associated there with other disorders due to the influence of malaria; and that in hot and unhealthy climates analogous nervous affections are met with, in adults of enfeebled frame, and in infants whose nervous system is naturally sensitive. We have seen too that the nervous system in prisoners is brought by confinement to a similar sensitive state; and is it not the natural conclusion from these facts that the peculiar nervous disorders seen in the Penitentiary were due to the action of an atmospheric poison, on persons in whom the nervous system was rendered thus excitable?"

Strong confirmation of the correctness of the theory here propounded is to be found in the history of the prison and its neighbourhood during the thirty years which have elapsed since Dr. Baly wrote.

The whole of the extensive, low-lying area, on the borders of which the prison was built, has been year by year reclaimed and raised and drained (in many places piled), and Cubittopolis has quickly grown upon it. *Pari passu* with the occupation of the surrounding area, and in proportion to the improvement necessarily consequent in the character of the surface, and of the deeper soil, the health of the inmates of the prison has also steadily improved; so steadily that the one amelioration can hardly but be looked upon as in part, at any rate, effect of the other.

It remains to notice one other more than probable source of some, at least, of the Penitentiary diseases—the water supply. Some very interesting facts and figures have lately been published by the present Medical Officer, Mr. Gover, in connection with this subject, and the author of a recent work, "Memorials of Millbank" (Captain Arthur Griffiths), thinks it "more than possible that the continual use of Thames water for drinking purposes was really more to blame than the actual unhealthiness of the site." "This is a point," Captain Griffiths goes on to say, "which Dr. Baly missed, but it came out afterwards."

Dr. Baly cannot be fairly said to have missed the point. On the contrary, entertaining a suggestion made to one of the inspectors of the prison about the year 1844, that the Thames water, cleansed by subsidence only, and not by filtration, was likely to exert a deleterious influence on the health of the prisoners, "he caused a supply of the purest water, from a neighbouring artesian well, to be obtained every second day for two months, and one-third of the prisoners (about 350) were supplied with this water only, whilst the other two-thirds (about 700) were allowed to drink the Thames water as before. The result was that no difference in the health of the two bodies of prisoners was experienced. Proportionally as many cases of diarrhœa, and also of other diseases, occurred amongst the one as amongst the other."*

Whether there was any source of fallacy in this large experiment, it is now impossible to determine; but, supposing the water to have been originally pure (as was asserted of it), and supposing its method of storage and its receptacles to have been without reproach (as Dr. Baly was likely to have insisted upon), the failure to secure immunity from disease, in those who drank of it, left the cause of that disease still undetermined, and strengthened materially Dr. Baly's case for some "malarious influence."

In the year 1854, however, the use of Thames water was entirely abandoned, and a permanent supply was obtained from the artesian well in Trafalgar Square. The good results of this very desirable change were at once evident in a virtually entire prevention of enteric fever; and this cause, working together (it can hardly be doubted) with the beneficial effects of a more

* Paper on "The Mortality in Prisons." *Med. Chir. Trans.*, 1845.

scientifically assorted dietary, shorter periods of imprisonment, better local drainage, and the improvements in the surrounding district, enabled Mr. Gover to make the following satisfactory report in 1876 :—

"The Prison continues to be almost free from diseases of the miasmatic order, and this is equivalent to stating that diarrhœa and dysentery, small-pox and erysipelas, typhus and typhoid fever, are all but extinct as causes of mortality. With regard to the mortality from typhoid or enteric fever, the contrast between the ten years, 1845-54, and the twenty-one years following is very remarkable. During the first period there were 57 deaths from that disease; during the second only 3. The improvement in the water supply, which was effected in August, 1854, explains this great difference, and constitutes the separating line between the two periods. Prior to that date, the Prison had been supplied with Thames water, pumped in from the river immediately opposite the gates, and afterwards filtered. This supply was cut off, and water from the artesian well in Trafalgar Square took its place, with immediate benefit to the health of the inmates of the Prison, and a large reduction in the rate of sickness and mortality. Fifteen years have now elapsed since a case of typhoid fever originated here, and it may therefore be fairly said that in this Prison that disease is extinct. Millbank, however, by no means stands alone among the Government prisons in this respect. An enviable security is enjoyed, but it is not monopolised. On looking through the annual medical statistics of these prisons for the twenty years, 1855-74, I find that only 8 deaths are ascribed to enteric fever, or 40 per annum, in a population, male and female, averaging 7,913. During the five years, 1870-74, only two deaths were caused by this disease, in an average population of 9,509. If the sanitary securities enjoyed by the inmates of convict prisons could be extended to the general population of the country, many thousands of lives might every year be saved."

QUEEN ANNE STREET,

January, 1878.

LECTURES

ON SUBJECTS CONNECTED WITH

CLINICAL MEDICINE.

ALSO

AN ACCOUNT OF THE DISEASE LATELY PREVALENT AT THE GENERAL PENITENTIARY.

ON THE USE OF OPIUM IN FEVERS.

GENERAL REMARKS ON THE PRACTICE OF MEDICINE.

A WORD OR TWO ON MEDICAL EDUCATION.

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AUTHOR'S PREFACE.

THE Lectures contained in this volume are the first of a series which I am engaged in giving to the Students of St. Bartholomew's Hospital. It is, however, no part of my duty, as one of the physicians to St. Bartholomew's, to deliver lectures. The obligations of my office are limited to the wards of the Hospital; where my business is to attend the sick, and to aid the studies of those who seek the knowledge of disease at the bedside of the patient.

But, since there are many things which must be first understood, in order that our inquiries at the bedside may be pursued more profitably, I have thought that the practical student had a further claim upon me for any information I could afford him which might be necessary or conducive to his purpose. And thus I have been led to give occasional lectures "On Subjects connected with Clinical Medicine."

Considering the time of life at which the majority of those who are intended to practise physic begin their professional education, few can be supposed at once to understand the objects with which it is conversant. I have therefore endeavoured to put in as clear a light as I could, what is the nature of Facts and Opinions, and Theories and Principles, in medicine; and what concern the clinical student has with each; and how important it is for him rightly to distinguish between one and the other.

With respect to the mode of conducting his inquiries at the bedside, I have suggested to him how to observe and what to observe; what demands his present attention and what may wait the season of his more mature experience; what books to read and what to abstain from reading; and the sort of knowledge which is principally auxiliary to clinical medicine.

But the subject to which I have chiefly desired to direct the student's attention is Semeiology, or the Doctrine of Symptoms; not for the sake of pointing out the symptoms of particular

diseases, but of showing what symptoms are in their own nature; what sense, or rather what various senses, they bear; and what is their import and value in enabling us to judge of all diseases which are capable of being known and treated in the living man. For this purpose I have constructed no system, and have adopted only so much of arrangement as was necessary for the convenient discussion of the subject.

The Lectures, now published, embrace one part only of Semeiology. The inquiry, as far as it has gone, has been occupied with the evidence, which each organ is capable of furnishing, of its own diseases by symptoms directly referable to itself, and involved in the actual state of its sensations and functions and structure. This is the simplest and the easiest part of Semeiology.

Among such direct symptoms I have chiefly dwelt upon those by which Auscultation has enabled us to make discovery of diseases of the lungs.

Auscultation is capable, I have thought, of being greatly simplified for practical purposes. At all events, unless it be so, it can never be successfully taught; the knowledge derived from it must be confined to a few physicians of hospitals, and the profession at large can never expect much benefit from its use.

Whether I have succeeded in accomplishing what I think so desirable, and have cleared Auscultation of its mystery in any degree, others must judge. But thus much I can safely say, that the intelligent student, by attending to the few characteristic sounds which I have pointed out, and taking pains to understand their import, and guarding himself against over-refinement, is able in a few weeks to discern the leading truths connected with Auscultation, and in a few months to use it and trust it as his safest guide in the diagnosis of pulmonary diseases.

Such are the objects which these Lectures have in view. They do not pretend to teach the clinical student any single thing peremptorily or dogmatically, but only to furnish him with certain aids and assistances by which he may be better able to teach himself.

LONDON, 1836.

LECTURES

ON

SUBJECTS CONNECTED WITH

CLINICAL MEDICINE.

LECTURE I.

ON THE EDUCATION OF MEDICAL MEN.—DIFFICULTY OF LAYING DOWN ANY FIXED RULE RESPECTING IT.—NUMBER AND EXTENT OF PREPARATORY STUDIES OFTEN MUCH EXAGGERATED.—SOME GENERAL RECOMMENDATIONS UPON THE SUBJECT.

I WISH to address a few observations to those of you especially, who, having passed through various preliminary studies in this great school of medicine, have now reached that department of instruction in which it is my lot to be engaged with you—viz. the clinical observation of disease, and the effects of remedies.

Your education is now at length concentrating itself to its great object: the time is arrived when you are to direct your minds expressly to the knowledge of diseases, and their treatment. All that you have hitherto learned in all the schools of instruction which you have ever frequented, you must bring with you, and make it ministerial to *this knowledge*. I talk not of science and philosophy *only* in those departments which bear immediately upon medical instruction, nor of science and philosophy *only* in any shape; but of everything by which your intellectual and moral nature has been cultivated and improved at any period of your lives. Every good principle received from the counsel or example of parents in your earliest years—every laudable habit derived from fortunate association

with good men—every maxim of prudence and virtue from good books—and, if there be a higher source (as I trust there is), to which some of you have looked for the proper motives, and ends, and hopes and destinies of man, and really know what they are—bring these, bring all that you possess, and engage them in active exercise, and link them to the great business of your lives; for now that business may be properly said to begin. It is that by which you are to live and take your station in the world—to do good, or to do evil—to gain friends or enemies, honour or dishonour; and in which the great accountable talent committed to your use will be well or ill employed.

There are certain stages in the progress of any great design, at which men are apt to pause and look back upon what has been accomplished hitherto, to see if there be any errors to correct, any omissions to supply; that thus everything may be rightly ordered as they go along, and made fit and conducive to the chief end they have in view.

You are now arrived at such a stage in your professional education. Having gone through many preliminary studies, you have reached the most important of all—that for the sake of which all the rest were prescribed to you. And from this point you cannot help looking back; and you must look back with much or little satisfaction, according as, with much or little diligence, you have well or ill secured each preceding stage. The knowledge which lies behind is our natural help to that which lies before. Those, therefore, who have been diligent and learned much, I do indeed congratulate; for their further advancement will be easier and safer, and with a greater interest. But those who have been idle and learned little, I can only exhort to spare no pains in endeavouring to supply their deficiency; for, until they do so, the objects I am concerned to point out to them they must view with little interest, and will very darkly apprehend. Yet these objects are, above all others, important to them; for they are calculated to teach them practical knowledge and practical skill in the exercise of their profession.

But what is it, and how much is it, that a student ought to know, before he can betake himself with effect to the observation of real disease, and hope to acquire a practical skill in treating

it? This is a question which, I make no doubt, you have often asked, for your own sakes; since you are especially concerned that it should be satisfactorily resolved. And I, for my own sake, as well as yours, have often thought upon the same question; for I would not willingly mislead, by any rash answer or recommendation, those who, from the situation I hold in this place, might venture to trust to my advice.

There is no subject which at this time more needs the consideration of sober-thinking and reasonable men, than that of professional education.

The subject of education generally is one of great difficulty; yet, strange to say, it is one upon which anybody thinks himself qualified to construct a system, and establish rules, and dictate to a whole community; not aware, perhaps, that the wisest men have exercised themselves upon the same, with very doubtful success.

Milton wrote a treatise on education, and so did Locke. And it was in reference to them that Dr. Johnson said, "Education in England has been in danger of being hurt by two of her greatest men."

Strange things have been said in jest, or in earnest, concerning the studies necessary to form a physician. Sydenham advised Sir Richard Blackmore to read Don Quixote. He probably spoke in jest. But it is impossible to read Sydenham and not perceive that his mind did in truth hardly admit any auxiliary to the exercise of its own observation. What he says of anatomy must startle the painstaking pathologist of modern times, who bestows all his industry in tracing diseases home to the primitive structure in which they are engendered. Anatomy, he has told us, is only fit for a painter. Sydenham might have been *professionally* learned; but there is no evidence in his works that he was so. Perhaps he was offended with the kind of learning then in repute, which tended to make the practice of physic rest chiefly upon authority; or, perhaps, the very structure of his mind was such, that it was really incapable of gaining anything second-hand which it could gather fresh from the reality; or perhaps he had a noble confidence in that wonderful faculty of observation with which he was endowed, and so resolved to use and to trust it to the uttermost, unaided and unencumbered by any foreign helps.

But Sydenham's education, in so far as it was not professional, was evidently of the best kind. His mind had been early acquainted with a strict, indeed, with a scholastic discipline. His book is that of a scholar; I do not mean on account of the language in which it is written, but in its form and structure. Without any apparent artifice, the material facts, and delineations, and histories, arrange themselves all in the right places for being well understood; and they are all so skilfully reasoned upon as to lead you easily and naturally to the conclusions he desired.

Are we then justified, not by the advice of Sydenham, which was probably spoken in jest, but by his example, in concluding that nothing more is required, to fit a man for the safe and successful practice of physic, than a good *general* education, and an industrious use of his own observation in his intercourse with the sick? I think not; and, among many other reasons, especially for the two following:—First, Because you cannot tax the faculty of observation beyond its *common* powers and capacities; you cannot estimate it in the generality by what it was in Sydenham, and look to it for the same results. This would be the same thing as to take measure of all mankind by the proportions of a giant, and make the single wonder of a hundred years the common expectation of every day.

Secondly, Supposing the faculty of observation existed in each of us at its utmost perfection, and enabled each to learn by it all that it is in its own nature capable of teaching; yet there are objects beyond its reach, the knowledge of which has, in the progress of our art, come to be considered as essential to the safe and successful practice of physic as those which lie strictly within its sphere; I mean the knowledge of morbid processes in all their variety, as they affect the structure and functions of our bodies. This knowledge has been derived from other methods of research, from anatomy, from chemistry, from experiment; and you can only gain it either by the use of such methods yourselves, or from the instruction of those who have used them. Sydenham himself could gain it in no other way.

But, in our own day, there is little fear that students will be spoiled by the recommendation of their instructors to be content with a scanty knowledge, and trust to their own sagacity for the rest. They are not likely to suffer harm by

having Sydenham held up as an example for imitation: the fear is of another kind (and it is well grounded), namely, that many men of the best abilities and good education will be deterred from prosecuting physic as a profession, in consequence of the necessity indiscriminately laid upon all for impossible attainments, of which no example either is or can be held forth for their imitation.

The different professions have one way of glorifying themselves, which is common to all. It is by setting forth a vast array of preparatory studies, and pretending they are indispensable in order to fit a man for the simple exercise of the practical duties that belong to them. I have heard lawyers make such a mighty parade of the things which a man must know before he is called to the bar, that, according to the average of human capacities, not one in fifty has the smallest chance of mastering them; and of those who do master them, not one in fifty can employ them to the uses for which they are intended.

I once saw a list of books recommended by a professor of divinity to the study of those going into holy orders. They were more numerous than the majority of even studious men ever read in their whole lives; yet these were a few prolegomena introductory to the office of a parish priest.

We, too, conceive that it befits our dignity to magnify ourselves at certain seasons. The commencement of a session is usually the time chosen; and then, what a crowd of wonderful things are marshalled by authority around the entrance of our profession! And through this crowd, it is implied, every man must press his way before he can gain admission. As if we wished to guard and garrison ourselves against invaders, rather than to gain good and useful confederates! In the affair of literature are reckoned Latin, and Greek, and French, and Italian, and German. In the affair of science, mathematics, and metaphysics, and mechanics, and optics, and hydraulics, and pneumatics, mineralogy, botany, zoology, and geology.

Such are the portentous forms that guard the threshold. But further onward are placed anatomy, human and comparative, and morbid; physiology and pathology; chemistry, general and pharmaceutical, and materia medica; surgery, theoretical, clinical, operative, and ophthalmical; medicine, theoretical, clinical, obstetrical, and forensical.

The general display of objects so grand and multifarious is formidable enough; but not half so formidable as their representation in detail. Of the great cosmogony of medicine there are several departments, and each professor never fails to magnify his own, by counting the cost of time and labour, which you must be prepared to bestow if you wish to make any reasonable progress in it. "Haller (perhaps such an one will say) surely knew what anatomy is, and how much goes to make an anatomist; and Haller has estimated the cost at twenty years of time and labour."

Now I am persuaded that there does not exist at this day in the profession an individual who comes up to the standard which (it is implied) all ought to reach. It has been my happiness to know many men in my time who have had enough of attainment to command my highest respect; some who have reached great eminence during their lives, and some who have been thought worthy of monuments since their deaths; yet I have known one, and one only, who came up to the requirements of an introductory lecture, which I have read; and that was Dr. Thomas Young. But Dr. Young stood alone among mankind. The most learned and scientific men of his time were struck with wonder at the extent and variety of his knowledge; yet Dr. Young was the only person whom any man now alive ever saw learned and scientific enough for a physician, according to the Utopian measure of things.

If all medical students had fifteen or twenty years at their disposal, and could dedicate them all to professional education, we might pardon a little innocent declamation in displaying the rich and varied field of knowledge about to be disclosed to them; but even then, sober truth would compel us to confess that the field so pompously displayed far exceeded in extent what the best minds could hope to compass, even in fifteen or twenty years. When, however, we recollect what space of time the majority of men so addressed really can give to their education, the whole affair becomes inexpressibly ludicrous.

Now I do protest, in the name of common sense, against all such proceeding as this. It is all very fine to insist that the eye cannot be understood without a knowledge of optics, nor the circulation without hydraulics, nor the bones and the muscles without mechanics: that metaphysics may have their

use in leading us through the intricate functions of the nervous system, and the mysterious connection of mind and matter. It is a truth; and it is a truth also that the whole circle of the sciences is required to comprehend a single particle of matter: but the most solemn truth of all is, *that the life of man is three-score years and ten.*

How has it happened that while, in other countries, the medical profession has been exhibited under every imaginable form of ridicule, here, in England, it has been so seldom chosen as a fit thing to laugh at? The truth is, that here no idea of ridicule was ever popularly associated with it; and to have exhibited it as if there were, would have been out of nature and unsuccessful.

A vain, pompous, counterfeit form of knowledge without, and a downright solid ignorance and incapacity within, made up a precious combination, which, not long ago, was found everywhere abroad. The mockery and fun that it excited were irresistible and inexhaustible; and Molière and Le Sage made the world ring with laughter at the expense of physic and physicians.

Depend upon it, what all men indiscriminately are told they *ought* to know, all men indiscriminately will soon *pretend* to know, be it never so extravagant; and when every medical man in every town and village throughout England, be he physician, surgeon, or apothecary, shall, in right of his profession, claim the homage due to vast learning and science, there will not be wanting some Molière or Le Sage to hold us all up to the just ridicule of mankind.

Let us take care then what we are about, and beware how we change the character of the English practitioner of physic. He is sound and unpretending, and full of good sense; what he wants is a little more careful and a somewhat larger instruction in what bears directly upon the practical part of his profession. Give it him (indeed we *are* giving it him), and he will become more trustworthy and more respected every day. But for all that is beyond this, we may recommend it, but we must not insist upon it; we must leave it for each man to pursue according to his leisure, his opportunities, and his capacity, and not exaggerate it into a matter of necessity for all. Where too much is exacted, too little will be learned; excess on the one hand naturally leads to defect on the other.

I know that much disquietude, if not unhappiness, has been felt by students, and especially by the best informed and best disposed, when, at the entrance of their profession, they have been met by obstacles which seem insurmountable. It is the special infirmity of ingenuous minds to reflect with too much anxiety upon their own progress in knowledge; to sit in judgment upon themselves, calculating whether they have made the best of all their opportunities, and wishing, vainly wishing, that their time might come over again, to enable them to supply this omission, or rectify that mistake. By many such, who are at all times too ready to deal hardly with themselves, every exaggerated statement of what they are required to learn is severely felt.

A well-weighed scheme of professional education, sound and practicable, comprehensive yet moderate in its requirements, and adapted to all, besides the many good purposes it would serve, would have the special benefit of satisfying the minds of students themselves that at each step of their progress they are in the right path. Such a scheme we are not likely to have soon. I will not presume to suggest what it ought to be; I would rather endeavour to show you, that, in spite of what you are perhaps at present inclined to fear, you may hope to earn a good reputation, to deserve and gain the approbation of mankind as others have deserved and gained it, although you do not possess a perfect literary and philosophical education.

Turn away, then, from the contemplation of this ideal perfection, which can only make you despair, and look to some real examples for your encouragement. But take care that they be *high* examples, and such as no small or ignoble efforts can imitate. I could choose them from among living men. For there are living men who would satisfy my own notion of what a physician ought to be. But I prefer to look for them among those that are gone, because there can then be neither offence nor envy in the selection. They shall, however, be such as are fresh in the memory of contemporaries, that I may have witnesses to the truth of what I say.

I will take Dr. Baillie and Dr. Babington; and I am content that the *general* estimate of the physician's character should be measured by what mankind at large thought of these two. Their intercourse with the world was large; and it is more

than probable that a great portion of the best informed men in their time did, in point of fact, form their notion of what a physician ought to be, from what Dr. Baillie and Dr. Babington actually were. I am content, also, that the *professional* estimate of the physician's character should be taken at the worth in which medical men held them.

Perhaps no men that ever lived were better understood, not merely in the general outline of their characters, but in all that nature, or erudition, or experience, or habit, or fortune, had made them, than were these two eminent physicians. They had both been public instructors in the lecture-room, and in large hospitals, and both had immense private practice. Of those who are now exercising their profession in the maturity of their age and experience, many were their pupils; and almost all the medical men of London have met them in consultation.

Now the reputation of Dr. Baillie and Dr. Babington would not be exalted by any lavish praise of them for qualities which they did not possess. They do not need our commendation for what they were not. Enough will still remain to make us content to be what they were, although it should be denied that either of them was remarkable for the extent or variety of his acquirements. We may still take them as our examples, although neither of them did, or ever pretended to, possess a knowledge of one-half the things now recommended to the medical student as indispensable. In truth, those who knew them best, and admired them most, will not say that their admiration of either was called forth by the many things they understood beyond all other well-educated physicians.

Dr. Baillie was an anatomist, and Dr. Babington a chemist. And thus each had chosen well a department of knowledge on which their minds seemed most capable of being exercised with a happy result. The minds of both had their natural bent towards matter of fact; and the favourite pursuit of each served, by its very use and exercise, to perfect their understandings according to the mould in which they were originally cast.

The one taught anatomy, the other chemistry, with great reputation and success; and both were deeply and experimentally versed in what they taught. But neither was anatomy, in the hands of Dr. Baillie, nor chemistry, in the hands of

Dr. Babington, made subservient to any great purposes of physiological or pathological discovery. Yet doubtless they *are* capable of such purposes, and to such they have been made to contribute by others. But education contemplates the ordinary fruits of knowledge. What is new exceeds its calculation. Men are not educated for discoverers; these stand alone; for they become what they are, beyond, and often in spite of, their education. They can never be quoted as examples.

But the fruit of their knowledge to the two eminent men in question was nothing more than the ordinary fruit; and, therefore, I choose them as examples. It was the ordinary fruit, but its measure was large. Their knowledge of anatomy and chemistry served them for all the purposes to which they are immediately applicable in the daily exercise of their profession; and, being in harmony with the natural bent of their minds, it still kept them striving after accuracy in all their investigations, and confirmed them in the habit and skill of appreciating the truth.

I am far from saying that in anatomy and chemistry consisted all the scientific knowledge they possessed, subservient to the uses of their profession. Herein, however, consisted the strength of their knowledge; and, for the rest, they were well-educated gentlemen. But I will venture to affirm, that had Dr. Baillie and Dr. Babington been constrained to accomplish half the course of literature and science now recommended for common professional education, they would, from the very texture of their minds, have been utterly spoiled as physicians.

My experience of human life has long since convinced me, that the number of truly learned and scientific men in the world is small. Therefore, real learning, and real science, must be things of difficult attainment, since so many are engaged in their pursuit. But be their *attainment* ever so difficult, it is not half so difficult as their *use*.

Knowledge may be an incumbrance as well as a help Many men know more than they are able to wield. There is a point (I believe) in the acquisition of knowledge (and this point varies infinitely in different individuals), beyond which, if more be acquired, the whole mass becomes useless to its possessor. I am acquainted with men who never have *done*, and never can *do* anything, because they *know* too much; and I am acquainted

with men possessing comparatively small knowledge, so dextrous in its use that they have ridden over the heads of others far, very far, their superiors in acquirement. Nothing is more common than to hear it said of some eminent and distinguished person, "Eminent and distinguished as he is, what would he not have been had he possessed the learning of such an one?" Whereas, if he had possessed one particle more of learning than he has, he would have been nothing at all; it would have weighed him down, and he would never have been heard of.

I am not speaking of pretenders, who succeed nobody can tell how—these are jugglers and mountebanks—but of those who succeed we *do know* how, and know it to be by dint of a knowledge not necessarily large, but solid and well chosen, which is (as it were) ingrained into their minds, and always at hand, and always producible for its purpose.

Fortunate, indeed, is the man who takes exactly the right measure of himself, and holds a just balance between what he can acquire and what he can use, be it great or be it small!

With all becoming deference to those who so loudly trumpet forth the praises of knowledge, and fright the trembling student with a portentous array of the wonderful things he has to learn, I would venture to crave some little regard for what is not so much as named by them, but what is pre-eminently more important than knowledge itself. I mean wisdom, as a thing distinct from knowledge, but not opposed to it; requiring, indeed, knowledge to work upon, but taking care to proportion that knowledge to the real end which itself (wisdom) has in view. I marvel that this wisdom is not enumerated among the ingredients of the physician's character, since it is conspicuously the chief of all, and was unquestionably that which gained for the two eminent men whom we have mentioned the praise which they justly merited from mankind.

It is, however, in its very nature, a thing too lofty for me to venture to describe. You shall learn what it is from one who well understood how distinct it is from mere knowledge, and who was himself endowed with it in a measure beyond ordinary mortals.

"Wisdom," says Robert Hall, "is to be distinguished from knowledge, to which it bears an affinity, but ought not to be confounded with it. Though wisdom necessarily presupposes

knowledge, and it is impossible to exercise it in things of which we are ignorant, yet it ought to be something more practical, and rather more comprehensive: it ever bears a relation to the end; and, in proportion as it is more perfect, to the highest and the last end the agent can be supposed to have in view. It first judges of the end fittest to be pursued, and next determines what are the most fitting and suitable means of accomplishing it. It being the province of wisdom to preside, it sits as umpire in every difficulty, and so gives the final direction and control to all the powers of our nature. Hence it is entitled to be considered as the top and summit of perfection. It belongs to wisdom to determine when to act, and when to cease; when to reveal, and when to conceal a matter; when to speak, and when to keep silence; when to give, and when to receive; in short, to regulate the measure of all things, as well as to determine the end, and provide the means of obtaining the end, pursued in every deliberate course of action."*

More simply, but with equal majesty and truth, spoke another of congenial mind upon the same high subject:—

> "Knowledge and wisdom, far from being one,
> Have ofttimes no connection. Knowledge dwells
> In heads replete with thoughts of other men;
> Wisdom in minds attentive to their own.
> Knowledge, a rude unprofitable mass,
> The mere materials with which wisdom builds,
> Till smooth'd, and squared, and fitted to its place,
> Does but encumber whom it seems to enrich.
> Knowledge is proud, that he has learnt so much;
> Wisdom is humble, that he knows no more."†

These words might have been spoken by an oracle, they are so suitable both to learners and teachers of our profession at the present day. We, of all men, ought to seek knowledge, not for the sake of being *knowing*, but for the sake of being wise; and those who are our masters, and are to tell us what we ought to learn, should not make a pompous announcement of all the wonderful things with which medicine may hold a possible alliance, but should prudently select and measure the knowledge required, with a view to the general capacity, and conformably to its end.

* Vol. V., Sermon xxiv. † Cowper's Task, book vi.

In laying down any scheme of education, you must take care to make it suitable to the majority of those who are to be educated. There may be circumstances in their condition and objects, rendering that education, which is the best in itself, not the best for them. Such circumstances belong, in an especial degree, to our profession. Very few enter it who are not to live by it: very few who are not required to exercise its practical duties *early*, from the necessity they are under of beginning as soon as possible to support themselves. So that the majority cannot wait to be made philosophers before they become practitioners.

These are homely considerations; but they are true, and most important to be borne in mind;—so important, that they, above all other considerations, ought to regulate the kind and extent of knowledge which should generally constitute the education of medical men in this country. Herein is involved a very urgent necessity. It is inherent in the state of society. You cannot alter it: you cannot evade it. Conform to it you must, if you desire to provide for the well-bringing up of those in whose assured competency to the fulfilment of their peculiar duties mankind have the highest interest.

This necessity, under which the majority find themselves, of exercising their profession *early*, requires that they should be made practitioners in the *easiest* and the *nearest* way. Their knowledge should be of things obviously and confessedly necessary, and this knowledge ought to be rigidly exacted, and nothing more; for if you go beyond this, you ruin the purpose you wish to serve.

There are, doubtless, many things *out of* the profession, by the previous knowledge of which the things *within* the profession are better understood. Such previous knowledge you may recommend, but you must not demand it. You may *recommend* that every man, before he enters upon the study of physic, should obtain the best general education within his reach: but you must specify nothing as absolutely necessary but what bears immediately upon professional use.

Now, that is *absolutely necessary*, and must be studied prior to experience, without which experience itself will hardly profit us when we have obtained it. And that is *absolutely necessary*, and must not only be studied prior to, but must be

still cultivated simultaneously with, experience, without whose constant help and guidance experience itself will be cramped, and hindered in its exercise, and cannot go on to perfection. Of processes of disease, and processes of reparation; of the great remedial agents, and the powers which belong to them, and their modes of acting, and modes of counteracting, and the methods of their application; of these some knowledge must be had prior to experience, and a greater knowledge must continually go along with it. Anatomy, and chemistry, and materia medica, are the keys to such knowledge. You have, therefore, a right to specify these as objects of study, to affirm that a competent knowledge of them is essential to the practice of physic, and to exact it.

The same necessity which the majority are under of practising their profession early, and of circumscribing their education within a short period, while it imposes some limit and selection of the things to be learned, requires also a prudent method of teaching them.

Upon subjects, the proofs of which are contained in specimens and experiments presented to the eye, instruction can only be had by resorting to the places where such specimens or experiments are exhibited and explained. Most properly, therefore, are chemistry, and materia medica, and anatomy, taught in lecture-rooms, where the proofs are continually ready at every stage of the instruction, and the instruction itself consists in little else than exhibiting them in their proper place and order.

But are not medicine and surgery conversant with objects presented to the eye? and ought not they, in like manner, to be learned by the contemplation of those objects? Yes.

But in the lecture-room these objects cannot be constantly present, so as to keep pace with the instruction, and be appealed to as proofs. Description, therefore, is made to supply their place, and become their substitute. Description, however, is a poor substitute; for it is absolutely unintelligible, except to those who have some acquaintance with the reality.

How, then, is medical and surgical instruction to be conducted, so as to make it answer its purpose more effectually? By keeping its real objects more, and as much as possible, in

view: and as those objects cannot be brought to the student, the student must be brought to them. As he cannot see them in the lecture-room, he must seek them in the wards of the hospital; and *there* he must seek his instruction too, if he is to obtain any. And even *there* he will find it difficult enough to learn, with the objects before his eyes.

I have long doubted whether systematic courses of lectures on medicine and surgery ought to be considered as essential a part of professional education as they are, and whether the rigid attendance upon them which is required does not stand in the way of other more indispensable means of obtaining knowledge; and whether they are not thus in danger of becoming a serious hindrance to the very purposes they are intended to promote.

You must go to the lecture-room, and see dissections, or perform them for yourselves, in order to learn anatomy: you must go to the lecture-room, and see experiments made, or make them yourselves, in order to learn chemistry.

And you must go to the wards of a hospital in order to learn disease and its treatment; for there only you can see the sick man, and inquire his symptoms, and give the remedy, and note its effects, and witness its success or its failure.

In the wards of this vast hospital, there are five hundred experiments continually going on, day and night; even the great experiments of appeasing pain, of repairing injuries, of controlling disease, of averting death. They are under the direction and superintendence of us, the physicians and surgeons. But we cannot take you into a room apart, and tell you what they are. They are open to your inspection. Go and see what they import. Your time is short. You will soon be engaged with the like experiments yourselves, and upon your own responsibility; and then, be assured, you will soon perceive that an acquaintance with all their particularities and details is essential to their success in your hands.

From what I have thought it my duty to address to you upon the subject of education, let no man impute it to me that I desire to degrade physic from the rank which has hitherto been conceded to it among the learned professions.

I believe (what my own observation has convinced me of)

that there is a mischief in putting forth a vast inventory of miscellaneous things, to be learnt by those whose time is hardly sufficient for mastering that knowledge which is obviously necessary for practical use. I believe, also, that it would be absurd to demand from students generally, whatever might be the time and opportunities at their disposal, a perfect literary and philosophical education, in order to the exercise of our own or any other profession.

But although I do not enumerate Latin, and Greek, and French, and Italian, and German, and insist that all these you must know, yet I by all means recommend you to get as good a literary education as you can. Let each man learn as many languages, beside his own, as are within the easy reach of his time, his opportunities, and his understanding. For every language will furnish him with a key to new stores of professional information. But to know a language, implies its ready use, not a dark apprehension of its words and phrases. Our own unassisted mother-tongue is a better way to be wise than this.

Again, although I do not enumerate every department of science that has obtained a name, and tell you it is indispensable that you should be conversant with all, yet I recommend you to get the best scientific education you can. Let each man, according to his time and opportunities, pursue that department, or those several departments, to which his mind inclines: but let him take care to feel his ground firmly established beneath his feet as he goes along. For *here* all half knowledge is no knowledge at all. Even homely common sense arrives at much safer conclusions in the things which belong to medicine, than any scientific principles half understood, and misapplied.

The medical men of England do, and will continue to, keep pace with the age in which they live, however rapidly it may advance in the course of improvement. They need not be trained and sophisticated according to any compulsory discipline in order to do so. By such accomplishments as are congenial with their professional studies, yet unforced and unpretended,—by such moral qualities as, however they are engendered, are, I am persuaded, refined and exalted by their daily habits and

avocations,—their character has been, and always will be, esteemed honourable, and their influence great.

Yes! their character is honourable, and their influence great; and yet they are little conversant with the vulgar means of popular credit. They are singularly abstinent from all passionate interference with subjects of mere temporary interest. No sect, no party in politics, has reckoned many of our profession among its clamorous advocates: but wherever there has been any association of good men for laudable ends—wherever any institution has sprung up having science or literature for its object—or any great scheme of benevolence been designed or perfected—medical men have been always found among their first, their most zealous, and useful promoters.

In England, too, there always have been among physicians those who have been upon an equality of education with the noblest and most learned of the land. They have been mixed with them at our public schools and universities; they have contended with them in honourable rivalry; and their minds have taken a congenial character from the similar studies of their youth. At length they have all separated to their several destinies for life. Some to the senate, some to the bar, some to the church, some to physic. These men, so educated, have ever afterwards looked with esteem upon each other, and each other's pursuits. And thus have the most illustrious men of every age, who could know nothing of physic as a profession, viewing it through those whom they have known engaged in it, been compelled to regard it with reverence and honour. In this way a credit has been derived to our common profession, in which every individual in every rank of it has partaken, when those whose good opinion is most coveted have taken their estimate of physic and physicians from contemplating the characters of such men as Dr. Heberden and Sir George Baker.

What has been will, I trust, ever continue to be; for I have a conservative jealousy of the rank due to my profession. I wish to see physicians still instituted in the same discipline, and still reared in fellowship and communion with the wisest and the best men; and that not for the sake of what is ornamental merely, and becoming to their character, but because I am

persuaded that that discipline, which renders the mind most capacious of wisdom and most capacious of virtue, can hold the torch, and light the path, to the sublimest discoveries in every science.

It was the same discipline which contributed to form the mind of Newton and of Locke, of Harvey and of Sydenham.

LECTURE II.

AMPLE MATERIALS AFFORDED BY HOSPITALS FOR THE CLINICAL STUDY OF MEDICINE.—REASONS OF ITS COMPARATIVE NEGLECT.—ITS INDISPENSABLENESS TO SUCCESSFUL PRACTICE.—ITS MORAL USES.—RELUCTANCE OF STUDENTS TO ENGAGE IN IT.—SURGERY, WHY MORE POPULAR THAN MEDICINE.—WHAT CLINICAL INSTRUCTION IS IN THE STRICTEST SENSE.—METHOD OF TAKING CASES.—REMARKS UPON AUSCULTATION AS AN AID TO DIAGNOSIS.

I HAVE always thought that hospitals are not converted to half the good they are calculated to serve as schools of medicine; and I think so still.

I have always thought that, in hospitals, knowledge is perpetually running to waste for want of labourers to gather it; and I think so still.

I have always thought that, in our schools, every mode of lecturing has been unduly exalted above clinical lecturing; and every place where knowledge is to be had, or is supposed to be had, has been unduly preferred to the bedside; and I continue to think thus.

With respect to clinical lecturing itself, custom has robbed it of its peculiar character, and, withal, of half its advantages, and half its popularity. It has been separated too much from the wards and the bedside, and has deviated into a discussion of abstract pathology and therapeutics. There may, indeed, be things which can be discussed with convenience and propriety only apart from the patient; and so let them be: but these bear a small proportion to the multitude of things which can only be learnt at his bedside, and in his very presence.

Here is a hospital containing 500 patients—a wonderful spectacle! Hither resort hundreds of students from every part of the empire. Here they see what the majority will never see again, after the period of their pupilage is over. They see

collected in one place every variety of disease, and every variety of injury, and numerous specimens of each. What an opportunity of instruction gained, if rightly used; what an opportunity lost, if neglected!

And which is generally the case? Is the opportunity, in fact, generally used or neglected? I speak from my own certain conviction, and I answer, that it is generally neglected. I know that five out of six of those who profess to attend the medical practice of this hospital (and it is the same at other hospitals) never watch a single case of disease through its entire course, during the whole period of their pupilage. I say this with great sorrow, and as a warning to those whose pupilage has yet to begin. This is what I mean by the materials of knowledge running to waste.

Now, seeing among the students of our profession that desire for knowledge which I do, I must be slow to charge upon them a systematic disregard of things most essential. May I presume rather to suspect that the discipline they are subjected to is a little faulty? I should be sorry to prejudice students against the course of instruction laid down for them: I would rather urge them to greater diligence, that so they might overcome any little impediments which lie in their way. Nevertheless, my situation of physician to this great hospital having given me some insight into the system of instruction pursued, and convinced me that it does not work so well as it ought, it becomes my duty to point out where I think the machinery labours.

I think, then, considering the limited period which the majority of students can devote to their education, a great deal too much is required from them as preparatory to their becoming practitioners. Among the multiplicity of things which they must bring certificates of having learnt, there is a fear that they learn some very imperfectly, and some they do not learn at all; and there is a chance that what they thus learn imperfectly, or not at all, may be the very things concerning which it is most important that they should be competently informed. And such is really the fact. So pressing upon the student's mind and time is the necessity of attending a multiplicity of lectures, that he has neither attention nor leisure left to bestow upon the observation of diseases and the effects of remedies.

But how are you to abridge the catalogue of lectures, and

what is there now taught which you could fairly exclude, in order to make way for a more ample observation of diseases in the wards of hospitals?

Anatomy must be learnt: the form, the situation, the structure of parts, must be known; even their intimate healthy structure should be much and often examined, by the medical student especially, that his eye may become skilled in detecting deviations from that structure, and tracing the visible vestiges of disease. Dissection, too, must be practised, by the surgical student especially, that his hand may be accustomed to the ready use of the knife. All the time that is bestowed upon it is, therefore, fairly due to anatomy.

Then come chemistry and the materia medica. And let no man who is making his entrance into the medical profession henceforth ever neglect chemistry. Chemistry was once thought to be conversant only with the physiology of external nature; but every day is bringing us to look more and more to chemistry to explain the physiology of our own bodies. It cannot, therefore, be suffered to become a less prominent part of medical education than it is. The same may be said of the materia medica. The articles of the materia medica are not likely, upon the whole, to increase in number; but those in use will require a more accurate study; more will be known concerning them, and more will consequently be to be learnt. Besides the natural history of many vegetables, there is also their chemical analysis. Chemistry has already detected, in several, the simple principle to which the whole plant is indebted for its medicinal virtue; and these simple principles are beginning to be largely and beneficially employed in practice. This, then, is not a time to abridge the study of the materia medica, when science is making in it new discoveries every day, and drawing from it more powerful and more convenient agents.

Then there are lectures upon botany; lectures upon midwifery, and upon the diseases of women and children; lectures upon forensic medicine. Now, I dare not say that the subject-matter of all these lectures is not of the highest order; and therefore I must not tell the student that *this* knowledge is less important than *that*, and that one lecture he may attend less diligently than another; I must only speak generally upon so delicate a subject, and contrive to intimate my opinion without

giving offence. The prudent householder, when he would furnish himself a house, sees well enough that some things are of mahogany, and some of rosewood, and others of ebony and gold. He sees much for beauty and much for use, and longs, perhaps, to possess all. But his question is, "Can I afford to possess them?" and his answer, "No, not at present; and I must wait until I can." So, when there is laid before the student this magnificent furniture of the mind, and he asks himself, "Can I possess it?" I will answer the question for him, "No, you cannot at present."—"But can I ever possess it?"—"Certainly you can."—"But how?"—"By diligence and by *time*. Your studies will not be limited to the period of your pupilage, and you will know all these things in *time;* but certainly not in the brief space of two or three years."

Observe, I am not captiously finding fault with these formal requisites of medical education. The things themselves are excellent. But I cannot help wishing, either that fewer had been demanded, or that more time had been allowed for mastering them.

But when all the lectures in question have had their share of attention, and you have brought away from each what information you can, your most important business, to which all these serve but as the humble instruments, is still to be performed: you have to learn disease, and how to treat it; and there are lectures immediately subservient to this purpose, viz. lectures on the principles and practice of medicine; also clinical lectures; also attendance at the bedside of the sick; also examination of the bodies of those who die.

Now, lectures on the theory and practice of medicine profess to teach physic systematically, and to give an entire view of the subject down to the present day. They are a kind of medical orrery, in which fevers and inflammations, exanthemata and hæmorrhages, profluvia and cachexies, are made to perform their circumvolutions with wonderful order and propriety. And, as the youthful astronomer needs to contemplate some mimic show of the heavens, before he can profitably scan the heavens themselves, so the youthful physician needs (it is thought) some orderly representation of the whole, to make him know and admire the extent and nobleness of his art, before he begins to deal with its important realities.

Beware, however, of mistaking the intention of these systematic lectures on medicine, or of allowing your minds to rest in them for purposes which they are not intended to serve. They are introductory, and only introductory, to knowledge which is to be acquired by other means. These means are necessary and indispensable—so absolutely indispensable, that, without them, there can be no knowledge. The knowledge in question is the acquaintance with diseases in all their forms, and the acquaintance with remedies in all their kinds, and all their modes of application; and the means in question are intercourse, continual intercourse, with the human beings who are the subject of diseases. Diseases are not abstractions; they are modes of acting, different from the natural and healthy modes —modes of disorganizing, modes of suffering, and modes of dying; and there must be a living, moving, sentient body for all this.

This body must be your study, and your continual care—your active, willing, earnest care. Nothing must make you shrink from it. In its weakness and infirmities, in the dishonours of its corruption, you must still value it—still stay by it—to mark its hunger and thirst, its sleeping and waking, its heat and its cold; to hear its complaints, to register its groans.

And is it possible to feel an interest in all this? Ay, indeed it is; a greater, far greater, interest than ever painter or sculptor took in the form and beauties of its health.

Whence comes this interest? At first, perhaps, it seldom comes naturally: a mere sense of duty must engender it; and still, for awhile, a mere sense of duty must keep it alive. Presently, the quick, curious, restless spirit of science enlivens it; and then it becomes an excitement, and then a pleasure, and then the deliberate choice of the mind.

When the interest of attending the sick has reached this point, there arises from it, or has already arisen, a ready discernment of diseases, and a skill in the use of remedies. And the skill may exalt the interest, and the interest may improve the skill, until, in process of time, experience forms the consummate practitioner.

But does the interest of attending the sick necessarily stop here? The question may seem strange. If it has led to the

readiest discernment and the highest skill, and formed the consummate practitioner, why need it go further?

But what if humanity shall warm it? Then this interest, this excitement, this intellectual pleasure, is exalted into a principle, and invested with a moral motive, and passes into the heart. What if it be carried still further? What if religion should animate it? Why, then happy indeed is that man whose mind, whose moral nature, and whose spiritual being, are all harmoniously engaged in the daily business of his life; with whom the same act has become his own happiness, a dispensation of mercy to his fellow-creatures, and a worship of God.

Such a man any of you may be; but you must begin by learning to stand by the sick bed, and make it your delight.

But the interest of attending the sick, I have said, seldom comes naturally; it begins in a sense of duty. All men, especially young men, have a repugnance to scenes of misery. A single object of wretchedness is enough to disturb one at first; but to find one's self at once transported into a throng of objects, where all are wretched, is apt to give a wrench to the spirits from which they do not always easily recover. It is here, then, just at the threshold of his practical studies, that the young man must rest upon his sense of duty. His sense of duty must rally him, and support him, and bring him back to the objects which he is so reluctant to face; and the interest will follow, if he is but just to himself.

I have now been a hospital physician many years, and many a succession of students has passed before me. I have not been an inattentive observer of their habits, and have remarked some things respecting the growth of this interest for the practical objects of our profession, which are really very curious.

At first all students are averse from visiting the sick; they have no fancy for the wards, either medical or surgical, and they especially shrink from the surgical. But when the repugnance is got over, and an interest begins to be felt, that interest is almost sure to be for surgery in preference to medicine; and yet, when they settle in life, their skill in surgery will be little called for, but nine out of ten of the cases which they treat will be medical.

Now, one reason why surgery is more popular than medicine is, that it is easier. Do not, I beseech you, imagine that I wish

to disparage surgery. In a profession like ours, nothing can show such bad feeling, or such bad taste, as purposely to let fall expressions which cast an imputation of inferiority upon those who happen to cultivate a different portion of the same field of science and usefulness from our own. And even here I will allow, if you please, that cases occur in the department of surgery, beset with difficulties and perplexities, which we in the department of medicine do not meet with, and which require information, and judgment, and skill of the highest order to surmount.

But I am now speaking of the ordinary routine of cases, such as we find them in hospitals; and, upon a comparison of such cases, surgery is certainly much easier than medicine; and students take to it the more kindly because it is easier.

Surgery, for the most part, requires fewer circumstances to bring you to a knowledge of its object than medicine does. In surgery there are prominent points of interest, which arrest and command the attention at once; in medicine the points of interest are to be sought after, and, being found, are to be retained and cherished by much labour of the understanding. External sores, external inflammation, and broken bones, require only to be seen and handled in order to be known. But the same knowledge which, in surgery, is obtained by the use of the senses, in medicine, which is conversant with internal disease, can only be acquired by a process of reasoning; and reasoning is more difficult than seeing and touching, and its conclusions are more uncertain, and much more liable to error.

Moreover, the adaptation of curative means requires more vigilance in medicine than in surgery. There is no end of the circumstances to be taken into consideration day after day, in order to practise medicine with tolerable success. A man has an *external* inflammation: the surgeon sees it, and is at once sure of its existence; he prescribes for it, and sees its gradual decline as plainly as he first saw its rise and progress. A man has an *internal* inflammation; but the physician, not seeing it, is obliged to come to the knowledge of its existence by a great variety of considerations: he prescribes for it, and is again obliged to enter into a variety of considerations before he can know that it has begun to decline or has ceased. The uncertainty of physic I readily admit; but I do not admit the vulgar reproach which has followed from it. There is nothing

absolutely sure but what rests upon the basis of numbers, or falls within the sphere of the senses. Where reasoning begins, there begins uncertainty; and on this account the highest and the best things in the world are all uncertain, and so is our profession. But from this very uncertainty those who practise it successfully claim their greatest honour: for where there is no possibility of error, no praise is due to the judgment of what is right.

Another reason why surgery is more popular than medicine is, that it is easier for pupils to make surgical cases a matter of discussion and conversation among themselves, and thus to convey an interest to each other respecting them. They can agree about the extent of this burn and that fracture, and understand each other when they talk about them; but concerning the progress of a fever, and all its circumstances—how they differ to-day from what they were yesterday, and what influence the means employed have had in determining the changes which have taken place—it is quite impossible that they should have any very general conversation. It is necessary to be in the presence of the patient to point them out. Language often fails of terms to designate them; and the most experienced often find a difficulty in making themselves intelligible to each other in speaking of them. There once flourished within these walls "The medical and Philosophical Society of St. Bartholomew's Hospital." I fear it exists no longer. The time was that it was attended weekly by at least an hundred students and others. There was often no lack of discussion, and good discussion too, upon professional subjects: but the subject was almost always a *surgical* subject. I have already shown the reason why it was so. It could not be otherwise.

Again: young men like to be doing something—something that shall be real employment. Thus they are gratified while they are plastering, and binding, and dressing, &c. They see and they feel that they are promoting some object daily and hourly, with their own hands, for the benefit of the sick. But in medicine, the quiet and almost passive manner in which they are engaged about the sick requires a state of mind which is seldom possessed in early life.

Why do I mention all these things? In order to show

you that I am well aware of all the circumstances which are apt to abate your interest for that department in which it is my duty and my desire to promote your instruction, and of all the difficulties I have to encounter, when I attempt to win you to it.

May I here be permitted to say a few words concerning myself? My office, as one of the physicians to this great hospital, makes it my first professional duty to further the studies of those who resort hither for instruction. A certain department is allotted me, and within that department I have, upon deliberation, chosen a certain course. If it be not essentially the best, it is at least that in which I feel myself to have the greatest capacity of usefulness. I desire that you should know what that course has hitherto been, in order that you may understand what it will be henceforward, and what you are to expect from me.

I have been physician here eleven years. Having no formal lectures to give, I have considered my business to be expressly in the wards of the hospital; and I have thought myself expressly placed there to be a *demonstrator* of medical facts. I use the term *demonstrator*, because it will at once carry my meaning to your minds; which is, that I have looked upon myself as engaged to direct the student where to look for, and how to detect, the object which he ought to know; and, the object being known, to point out the value of it in itself and in all its relations.

In prosecuting this my duty, I have betaken myself to the hospital at an early hour; and I have had a purpose in so doing. I have desired to meet the students before their minds were preoccupied with other things; that, among the interfering demands of other objects which arise in the course of the day, they should not have to catch a moment for that which, I consider, is the greatest of all—to steal a brief interval between lecture and lecture, and give it to that to which all lectures, and all the knowledge conveyed in all lectures, is but subsidiary and subordinate. I would not thank them for such an irksome wearied attention; I want them when their minds are fresh; and therefore I have always given myself to them when mine is fresh.

My visit to the hospital has occupied, generally, two hours; sometimes a little less, sometimes a great deal more. Half an

hour of that time would be sufficient for me to prescribe for my patients, as well as I could, and satisfy my conscience that I had done them justice. The remaining hour and half I have given to the duties of my office as a teacher of clinical medicine.

But in this business of clinical instruction, I have not been the only instructor, nor have the means of information been limited to what I say or I point out. Surely this would be a poor kind of schooling—a giving and taking of scraps of knowledge, where one mind receives just so much as another mind may have to bestow. No; it has been my chief care to put everything about the sick in the point of view most favourable for being well observed; that circumstances might become didactic; that they might give their own intimations, and speak to you themselves in their own tongues; and that thus you might accept knowledge neither from me nor from anyone, but gather it fresh from the reality. Such, I consider, is the true method of clinical instruction. In short, whenever I have entered my wards, I have been accustomed to regard myself in no other light than that of one who presides over a great solemnity, and is engaged so to manage all its circumstances that they shall produce their appropriate impression upon the mind of the spectators. You are those spectators; and the solemnity you witness has many scenes and several actors, and one main subject runs through the whole. The scenes are the diversified incidents of many diseases; the actors are the sick themselves, and those who minister to them—the nurse, the physician, and the physician's attendants; and the great subject of the whole is the life of human beings consigned to our hands for a time, and used and treated according to our pleasure, and always for purposes of good. This life is by all means to be saved; its diseases by all means to be alleviated or cured; and the arts and methods of saving, and curing, and alleviating are to be so displayed, that the benefit and blessings of individuals may be multiplied infinitely.

But how multiplied infinitely? Even through you. Recollect you are the spectators; I am but the actor. For this is a case in which the spectator's place is a thousand times more important than that of the prime agent, if the measure of things be calculated by the result. My business is with the few indi-

vidual patients before me; and whatever good or whatever evil I do, would be strictly limited to them, but for your presence. Yes, you are there to take note of the errors into which I may fall, that you may avoid them, and so restrict the mischief within its present sphere; and you are there to take note, also, of the good which I may do, and learn the method of doing it, and make it your own, and carry it abroad with you, that it may bear fruit an hundred-fold, and be multiplied among all mankind.

You will perceive, then, that with me clinical instruction is, as little as possible, a matter of formal lecture. I will tell you the manner of my proceeding.

Upon the admission of a patient, my first object is to learn the exact nature of the disease I have to deal with; and this is done by my own observation, and by inquiries to which the patient himself or his friends make answer. This is taking the case.

Now, in taking the case, I desire always to proceed after a certain method; and, when I am able to pursue that method, all the circumstances which I seek to know unfold themselves naturally and easily, and then it is a simple, agreeable, and interesting employment.

But often, very often, I am driven from all pretence of method in taking the case. The poor patient is embarrassed by the novelty of his situation, or he is deaf, or his disease incapacitates him; and he hardly understands your questions, and gives you strange answers. Thus things drop out confusedly one after another, and you must be content to accept them as they come, and join them together as you can. But, upon these terms, taking a case becomes a very irksome, disagreeable business.

In taking the case, however, if I am able, I always proceed thus:—

The patient being placed before me, I ask him no question until I have learned everything worthy of remark which my own eyes can inform me of. His physiognomy; his complexion, whether florid, pale, or dusky; the general bulk of his body, whether large and full, or spare and wasted; the condition of particular regions, whether swelled or attenuated; and of the surface, whether there be any eruptions or sores upon it, and

what is their character; and, lastly, the power of locomotion, whether he have free use of his limbs or not.

All these are most important particulars, and we ought to make much of them. There can be no doubt concerning them; they are objects of our own observation, and come to us authenticated by the testimony of our own senses. One step securely ascertained leads to another; and from what we see upon the exterior, we obtain a clue for directing our inquiry to the seat and centre of the disease within. If locomotion be hindered, we look well to the brain and spinal marrow; if there be the livid lip and dusky skin, we scrutinize particularly the condition of the heart and lungs; if the whole body, or some of its parts, be attenuated, we examine well the organs of nutrition.

Having thus learnt all I can with my own eyes, and felt the pulse and seen the tongue, I next proceed, in taking the case, to that further inquiry in which the patient takes a part: and first, I ask him concerning his general sensations, especially whether he be hot or cold; and I endeavour to learn whether his heat and cold occur under conditions which constitute fever.

Next, I inquire into the state of particular organs; and, beginning with the head, I ask after pain, vertigo, and sense of weight, the sight and the hearing, and sleep and wakefulness. Many of these things are only glanced at, or perhaps passed over altogether, if there be no reason to suspect disease of the brain.

Then, passing to the chest and respiratory organs, I ask concerning pain and cough and expectoration, and the state of the breathing under various conditions of exertion and in different postures of the body; and I learn the force and extent of the heart's pulsation.

These things are hardly dwelt upon, or soon despatched, if there be no suspicion of disease in the chest; but if there be, not all we can learn by simple inquiry is enough to ascertain its nature. The patient must, moreover, be submitted to the process of auscultation. This process, however, in order to avoid interruption, I postpone until other inquiries are finished.

Lastly, proceeding to the abdomen, I ask here also concerning pain and uncomfortable sensations, the appetite, the digestion, and the evacuations, their frequency, quantity, and appearance; and then I ascertain with my hand its form and

fulness, the possible enlargement of particular viscera, the effusion of fluid, or the existence of pain upon pressure.

Here the examination of the patient ceases, as to his actual condition; but the history of his complaint remains to be learnt, its origin and its progress hitherto, and its probable exciting cause.

Perhaps it would seem more in the order of nature, and therefore the best method, to take the history first of all. Formerly I used to do so, but I found it practically inconvenient. If you first learn the existing complaint, you know how much of its previous history you will require to illustrate it; but if you first inquire the history, since you do not yet know what it is to illustrate, you cannot tell how much of it you shall want, and must allow the patient to tell what he thinks fit; and, since every person's complaint is interesting to himself, he is apt to discourse about it rather too much at large, and too little to edification. Therefore it is, that I now always inquire the history last, inverting (if you please) the order of nature; and I take care to make the patient answer express questions rather than leave him to expatiate at his own discretion.

And now the case is taken and recorded in a book by the clinical clerk: not that I deliver over to be recorded all the circumstances that come out in the progress of the examination, but only such a selection of them as may serve to declare the disease, and furnish guidance and direction in the treatment of it.

The case, I say, is now taken, provided there be no suspicion of disease in the chest. But if there be, the patient must be submitted to the process of auscultation.

What auscultation is, and the philosophical principles which recommend it as an instrument of diagnosis, it belongs to the lecturer on the principles and practice of medicine to teach you. But as, in the course of my clinical instruction, I shall lay great stress upon it, and at every visit shall present you with instances of the necessity of using it, and shall invite you to give much time (for much will certainly be required) in order to learn to exercise it skilfully, you have a right to expect from me, who have employed auscultation in this large hospital eleven years, some observations concerning it, and some estimate of its value.

The more accurate physicians of our own times have not disdained the guidance of another sense in the investigation of disease. They make use of the hearing as well as the touch and the sight; and in those things which are more fitly and naturally subjected to it, they have found it not an unfaithful interpreter of the truth.

Auscultation, as it is called, professes to furnish important aid in the diagnosis of diseases appertaining to all the organs within the chest. Its use, however, has not yet become popular in this country, nor is its value ascertained.

There are those who condemn it as absolutely worthless, and there are those who commend it as infallible.

Its vehement and unqualified condemners, judging from what they write and say, are absolutely ignorant of, and unpractised in its use; and its unqualified commenders are probably of that happy temperament which is naturally averse from admitting the real difficulties of any subject, and therefore find none in medical diagnosis, which is of all things the most difficult, whatever be the means employed for its illustration.

But there are many sober, well-informed men, who, having the opportunity, have thought it their duty not to spare the necessary pains of practically acquainting themselves with a method of inquiry which comes recommended to the world by one of the soundest pathologists that ever lived: and among these there will not be found one who does not attach some (and that a very considerable) value to its use.

A priori, it would not have been believed that the pulse could ever teach us what it does. Sir John Hawkins, in speaking of a visit he paid to Dr. Johnson one day during his last illness, says, "Before my departure, Dr. Brocklesby came in; and taking him by the wrist, Johnson gave him a look of great contempt, and ridiculed the judging of his disorder by the pulse."

If we had heard, for the first time in our own day, of some physician going about this town, and putting his fore-finger upon the wrists of his patients, and professing to know, from something he found there, that this man had an inflammation of his lungs, and that man of his bowels, and presuming to prescribe bleeding and other gigantic remedies, simply from faith in his own infallible fore-finger, grave men would denounce

him as a dangerous quack, and pleasant men would hold him up as a fair subject for ridicule.

Yet use has so educated the fore-finger of us all, that this is the very thing we are now doing every day of our lives. When, therefore, so much is confessedly learnt by one sense, it is rather hasty to conclude that nothing whatever can be learnt by another. When, by touching an artery, be the disease what it may, and seated in whatever part, we seldom fail to gain some knowledge concerning it, and some suggestion how to treat it, why should it appear incredible that two particular organs only, the lungs and the heart, should submit some of their diseases to the cognizance of the ear?

Concerning the sense of hearing, in relation to its proper objects, and in relation especially to diseases of the heart and lungs, no man can learn from another the kind of information which it is able to convey; everyone must teach himself. In this respect it is with hearing as it is with touch. You may talk of a hard and a soft pulse, of a full and a small, of a quick and an irritable pulse; but be assured you thus convey no intelligible idea, except to those who are by practice as conversant with the pulse as yourself. So, too, with respect to the heart, you may talk of its sound being clear or dull, near or distant, limited or diffused; and, with respect to the lungs, you may talk of the bronchial and vesicular respiration, of the bronchial voice, and pectoriloquy, of rhonchus and sibilus, and large and small crepitation; yet none can understand you but those who have given much time and pains to the exercise of auscultation.

Let us recollect that the pulse submits none of its qualities, but those which respect its number, to actual measurement; all the rest are determined according to the perceptions of the person who feels it. Yet, concerning these, there is a tolerable agreement among medical men. It is the same with the heart. The number of its contractions may be counted, and therefore never can be doubtful; but the modes and qualities of its contractions, which are many, are determined according to the perceptions of the person who hears them. These, however, like the kindred qualities of the pulse, are accustomed to strike all who habitually attend to them in the same way.

Summarily, then, concerning auscultation, my experience (I think) warrants me in saying thus much:—1. That there are

some diseases of the chest which in their *kind* entirely elude it; 2. That there are some which elude it not in their kind, but in their *situation;* 3. That there are some in which auscultation is only a help to diagnosis, but still a very great help; 4. That there are some (and perhaps the larger number) in which the conclusions of auscultation are as unerring as those of sight itself.

Certainty is a big and portentous word, applied to any the smallest portion of our art. Yet still there is a small district of the whole field of diagnosis, but a large district as it respects particular organs, which auscultation has rendered absolutely certain.

With auscultation I almost always use percussion; and the results of the one perpetually correct or confirm the results of the other, and strengthen the diagnosis.

But does all clinical instruction consist in directing the mind how to ascertain mere particulars, whether by auscultation or percussion, or by whatever other method is adopted for their discovery in different organs? No. Clinical instruction is not merely occupied in directing observation to facts, but it assists the mind in estimating their value. Thus, when the record of the case has been read aloud, I admit you to share in my deliberation upon all its particulars, while I endeavour to assort them and bring them together, and make them yield all the light they are capable of throwing upon the nature and seat of the disease. Sometimes I can at once come to a confident diagnosis; and when I can, I at once announce what it is, and give my reasons for it.

Sometimes, after great pains of inquiry, I am still in the dark; and when I am, I say so, and desire to reserve the case for future examination.

Sometimes, perhaps most frequently, I feel that I have a tolerably right notion of the complaint, but require some circumstances to be more clearly made out, before I can be absolutely certain. And then I state what are the circumstances which give me the notion that I have, and what I still desiderate to bring me to a more confident conclusion.

The case still remains to be prescribed for. In prescribing, I endeavour to be as simple as possible, to make the indications of treatment as intelligible as possible, and the object I have in view clearly seen.

The case thus examined, and commented upon, and prescribed for, I then commit to your future observation as a medical study.

In the further progress of the case I am still present, not to give formal lectures, but to take care that none of the circumstances which continue to develop themselves may fall to the ground profitless; and that you may be tutored by *them*, and not by *me*.

Not one only, but many cases, I shall thus consign to your study with the like care; and in all I shall be continually at hand to render you my assistance. That assistance, however, I shall never interpose, so as to hinder the exercise of your own independent observation, and mar the very purpose of clinical instruction.

With respect, however, to formal lectures, as a part of clinical instruction, I will say this: that the teacher would do well occasionally to call his pupils together, and state to them his own views (for every man *has* his own views) of certain leading points of pathology; for these views, he must be conscious, give a bent and direction to every remark he makes upon the objects which he and his pupils are daily contemplating together; and it is desirable that they should have the best key to interpret him by. This would require a formal lecture.

Again, for the same reasons he would do well to acquaint his pupils with any general principles at which he may have arrived in the treatment of disease. This, too, would require a formal lecture; and such lectures I am almost tempted to promise you, if I was sure they would be acceptable. But it must not be yet; for I am not certain that we should at present quite understand each other upon these interesting subjects.

The progress of the human mind is evermore from particulars to generals; and he that would inform others must be careful, in the manner of his teaching, not to transgress the order of nature. Full of this important truth, I must first seek to rivet you to the contemplation of individuals, and only venture to unfold to you any general principles, which I may conceive myself to have reached, either of pathology or practice, in proportion as I judge you able to authenticate them by your own growing experience.

But it is *your* present duty to exercise your observation

carefully and unremittingly ; and it is *my* present duty to point out the fittest objects, and place them in the light in which they can be most profitably seen.

If ever the desire to view the beauties and sublimities of nature has led you to ascend some lofty eminence, you have probably taken with you one more familiar with the scene than yourselves, as a guide; but you have still trusted to your own eyes and your own feelings, to fill you with the delight of the prospect, and tell you what to admire and wonder at ; and you have required no more from the guide than to point with his finger, and say, "See here, and see there."

So in entering this place, even this vast hospital, where there is many a significant, many a wonderful thing, you shall take me along with you, and I will be your guide. But it is by your own eyes, and your own minds, and (may I add) by your own hearts, that you must observe, and learn, and profit: I can only point to the objects, and have little more to say than "See here, and see there."

LECTURE III.

FURTHER REMARKS UPON TAKING CASES.—SUGGESTIONS AND CAUTIONS IN THE READING OF BOOKS.—SYSTEMATIC, NOSOLOGICAL, PRACTICAL BOOKS.—THE DEGREE IN WHICH THEY ARE VALUABLE TO THE STUDENT.—DR. CLUTTERBUCK.—M. BROUSSAIS.—MR. ABERNETHY.—DIVISION OF PRACTICAL MEDICAL LITERATURE INTO THAT WHICH REGARDS WORKS OF OBSERVATION SOLELY, AND THAT WHICH REGARDS WORKS BOTH OF OBSERVATION AND RESEARCH INTO MORBID PROCESSES.—THE LAST PROPERLY CALLED THE PATHOLOGICAL, AND ESPECIALLY RECOMMENDED TO THE STUDENT.

THE student often asks me such questions as these,—"Would you recommend me to note cases down for myself in writing? What books should I read, or what studies should I pursue, in aid of my daily observation of disease?" These are certainly very important considerations, and I will now give you the best advice concerning them I am able.

I often see young men, at the very commencement of their attendance upon medical practice, taking cases; and when I do, I always dissuade them from doing so. At present, it is quite impossible that they can do what they desire: they literally cannot take a case. Their present business is to observe. They must learn to know the things themselves before they can put them down and set them in order for use and reference.

I will venture to give this general advice. During the first three or four months record nothing; use your observation to the utmost; be continually in the wards, looking at the sick and asking them questions; be inquisitive about the effects of medicines; be listening perpetually, with your bare ears or with the help of the stethoscope, at the chest, that you may become familiar with the sounds of healthy respiration and the healthy contractions of the heart; and then try to use the same means for the detection of disease. Accustom yourselves to feel

the pulse; the number of its beats is easily measured, but it has qualities which are referable only to the sensations of him who feels it, and you must educate your touch to the discrimination of them; for these qualities, much more than its mere number, serve to guide us in the detection of disease and the method of treating it. The tongue, too, must be often looked at, before you will be able to detect upon it the marks which are morbid. There are certain secretions, also, the different morbid qualities of which you must learn by frequent examination: the expectoration, the urine, both have qualities upon which may depend the diagnosis of disease and the choice of remedies. I am not now making any orderly enumeration of symptoms, but merely instancing a few cardinal points with which habit must render you a little familiar, and enable you to appreciate the information they are calculated to convey, before you can take cases for yourselves with any promise of advantage.

Let me also mention the physiognomy of disease. This can never be adequately described, and I urge you always to remark it and to dwell much on it; for some acute observers have drawn such secrets from the expression of the countenance, that it has been to them in the place of almost all other symptoms.

I would recommend, then, that for three or four months the student should allow his curiosity to range discursively over every variety of disease, familiarizing himself with the great signs which belong to all, before he binds down his mind to the rigid contemplation of particular cases. When I say discursively, I still mean diligently, and with an earnest purpose of improvement; and, in the course of three or four months thus employed, you will pick up much real knowledge, you hardly know how; but you will find it such as will stay by you.

And now you may begin to take cases: but take only a few at first, and be discriminate in your choice; let them be instances of well-marked acute disease; and, when you enlarge your number, I would advise you to employ yourselves upon several of the same denomination at the same time. Take three or four cases of dropsy, or of fever, or of rheumatism. Thus you will learn, by the benefit of comparison, what can be learnt in no other way. You will see shades of difference in the diseases themselves, arising from external circumstances or from

the different constitutions of those who bear them, and a consequent variety in the modes of treatment required.

Take your cases in one of two ways. Either take them altogether independently for yourselves, or copy them out of the book kept by the clinical clerk, adding any particulars of observation or comment which may have occurred to your own minds during their progress. The last is the least laborious way, and for a beginner (I think) the best, provided you make a point of never copying cases which you have not attentively watched during their whole course. At all events you might trust to the clinical book for furnishing you their framework or leading facts, and their history; and thus leave yourselves more at leisure to note down your own remarks from day to day, as the cases proceeded. If, in this employment, you make a good selection, and do your work carefully, you will, at the end of your hospital attendance, take away with you a little body of practical medicine founded on your own experience, which will be useful to you, very useful, as long as you live.

Do not let a suggestion which I am going to add seem trifling or impertinent. I would wish to see the freest intercourse between pupils, with a view to mutual instruction. I would rather find two or three taking the same cases together, than one so employed alone. You have it in your power thus to give infinite help to each other. Of all modes of instruction that is the most agreeable, and often the most valuable, where one, a little senior, or a little more advanced in knowledge, communicates information to another not quite so forward. There are, besides, many little difficulties which no man can tell you better how to surmount than he who has just succeeded in surmounting them. At this day I continue to feel gratitude to two or three individuals a year or two senior to myself, whom I found at this hospital when I first became a student.

Then comes the important question, What books the student should read, and what studies he should pursue, simultaneously with his attendance in the wards of the hospital, in aid of the objects he has now in view?

Perhaps it may appear very strange to you, that, while you are intent upon observing the symptoms of diseases and the effects of remedies, I should advise you to be very sparing in reference to books which treat expressly of such matters. You

see the things themselves; then why learn them at second hand? I do not know that I have any objection to certain elementary books—"Vade Mecums," "Practical Manuals,"—provided they are short. Such books are to the medical student what maps are to the traveller. They give a succinct summary account of the whole subject according to the last survey; they help him to explore the country; but no man can be said to know a country who has gone over it only on the map. The map may have given him his first general notion of it; but his more intimate acquaintance with it, that sagacity which enables him (if I may so say) to take the right turn in the dark, can only come from the habit of perpetually traversing it. Such are elementary books of practice to the student of physic. He wants them to tell him where he is, and just to give him a start; but he must never trust to them for anything beyond this. The misfortune is, that these books are too often read, not to assist, but to excuse the labour of practical observation. Many a young man has preferred to sit by his fireside and read "Thomas's Practice of Physic," to a diligent attendance upon the sick in the wards of a hospital; and the consequence has been, that he has started into practice with Thomas under his arm, and nothing else, and Thomas and he have been companions through life, and he has never been able to do without Thomas to his dying day; seeing and reading all things through Thomas's spectacles.

Then there are books which give definitions, or succinct descriptions of diseases by name. Not long ago, Cullen's "Nosology" was the most popular, indeed the only, book of the kind in use. But of late there has been a larger demand for such books, and the supply has equalled the demand. Nosologies are resorted to by the student to enable him to learn "Practice" (as it is called) in a much shorter time than his observation of actual diseases and their treatment in the wards of a hospital can possibly teach it him. It would take him two or three weeks at least to watch any actual case of fever through all its stages, while *by his book* he gains an acquaintance (such as it is) with all the kinds of fever that were ever heard of in half the time.

But Nosologies teach the student "Practice" in no other sense than that of enabling him to seem to have a knowledge

which he has not in reality. They qualify him to pass his examination, not to understand and to treat disease.

This examination is an important concern; and the majority of students have unfortunately found out that they can fit themselves for it by other means than those which they are enjoined and invited to pursue. But it is my duty to tell you, that, while you are committing to memory these technical arrangements of diseases, and absenting yourselves from the hospital, and fancying that you thus gain time in respect of one particular purpose, you lose time wofully, perhaps irretrievably, in respect of the great business of your lives. Nor is the loss of time the greatest evil: your minds are led astray from their proper object; for, while you are learning a Nosology by heart, you are no more studying Physic than if you were digging and delving in a field.

It has often grieved me to see young men saunter about the hospital square, with a little book in their hands, *grinding* a Nosology, which they are sure to forget in a few months, instead of going from bed to bed, full of interest and alacrity, and gathering knowledge which would become their own, and remain with them as long as they live.*

* It would be happy for those who have but a short time to bestow upon the study of their profession, if that method which alone is calculated to make them good practitioners was the only one they could pursue respective to their examination. I believe that things might be so ordered as to produce this result. I offer the following remarks upon the subject.

Every one should be examined upon what he has seen, and that only; and he should be expressly asked what that is before he is examined. He should be asked what diseases he has actually observed during his hospital attendance, and then made to give a strict account of them and their mode of treatment. An attendance upon medical practice during a whole twelvemonth, in a large hospital, would bring him acquainted with an immense variety of diseases; so that there would be no fear of the examiner's interrogatories being cramped within too narrow a sphere.

What encouragement would it be to a zealous pursuit of practical medicine, if the student knew that this would be the form of examination to which he would be ultimately subjected!

But even if this were the case, it would, in my notion, still fall short of what is both desirable and attainable. I believe we have been all hitherto wrong; or, to speak more boldly still, I believe the best schools never yet were right, in the prescribed modes of imparting, and acquiring, and ascertaining the knowledge of practical medicine. The very examinations themselves should be conducted in the wards of the hospital. The

There is another class of books, not systematic, yet purely practical, which professedly discuss the treatment of diseases, and their nature and essence, entirely with reference to their cure. They are generally written upon some one particular disease, or upon several diseases of a like character. Some of them you will feel a great temptation to read.

Among this class are found the great treasures of medicine; and among the writers of them are found the great benefactors of the human race. The writer of a good practical book on medicine, who tells the world something which it did not know before, something of large application in fortifying or restoring the health, strength, and comfort of man's body and mind; or who, if he tell nothing new, yet wisely sets in order what is already known, and gives it a better and more convenient adaptation to the same high purposes; such a writer, in all just estimate of things, is second, and second only, to the great expounders of moral and religious truth.

But, unhappily, among this same class of books is also found

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presence of the patient is necessary at every step, for teaching, for learning, and, finally and most of all, for examining. Surely it is not possible to tell whether a man knows disease unless you see him in the very act of searching after it and finding it; or whether he can treat disease, unless you see him while he is applying his remedies to it.

But (it would be said) the arrangements of a hospital would not admit of all this. Nothing would be easier. This great hospital can provide for anything which is manifestly conducive to the public good. I would only ask for a small ward containing half a dozen beds; and these beds should be occupied by half a dozen well-chosen cases, drafted from the rest of my patients.

This ward should be my clinical school, and into it none should be admitted but myself and half a dozen pupils. These I would have under examination for a month, and then the same number should succeed them; and so on, month after month, all the year round.

But how should I thus have them under examination for a month? By making them act before me, for a month together, the very part they will have to act, for good or for evil, as long as they live. They should have the cases under their own care and treatment, but strictly under my superintendence; for this superintendence would constitute the examination. They should question the patient before me, and apply whatever means they thought fit for the detection of his disease, and give their reasons for whatever notion they might form of it. Then they should prescribe before me, and make choice of their remedies, and give their reasons

everything that is wretched in the literature of our profession; and the bad practical works have a mighty predominance of quantity over the good.

In the shape of practical treatises our own age and country has bred, and is breeding, and the press is assisting at the birth of, the oddest and most worthless trash; and this often obtains a wide circulation, and a strange popularity.

While you are watching various diseases, you cannot help feeling a desire to know what they have said concerning them, who have expressly written about them; and in your wish to read something, you are, without direction or warning, as likely to lay your hand upon a bad book as a good one; nay, more likely, for a bad book is generally a very easy book, having been composed by its author with no labour of mind whatever; whereas a good book, though it be not necessarily a hard one, yet, since it contains important facts, duly arranged, and reasoned upon with care, must require from the reader some portion of the same attention and study to comprehend and profit by it,

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for whatever indications they thought fit to follow in the treatment. In short, every day they should give the same sort of little lectures as I am accustomed to give upon each patient as I go round the wards.

At the end of the month, I would give a certificate of competence to those whom I thought deserving of it.

This method, besides being the best possible test of the knowledge which a man had already acquired, would also be a lesson of instruction in the use of it. Such an examination, or rather such a practical exhibition of knowledge, in its use and exercise for a month together, would have none of the annoyance, either actual or prospective, which belongs to examinations as they now are. As this would be the most profitable, so it would be the most pleasant part of the student's professional education. There would be none of the posing and puzzle of formal interrogatories, but, instead of them, there would be the ease and comfort of the most unreserved communion between pupil and teacher, upon the subjects most interesting to both.

After a student has gone through his prescribed course of education, and been examined, he is ready to practise as soon as anybody will employ him; and I would ask any person of common sense to which of the two he would submit his body with the greater confidence—to him who had Vogel, Sauvages, Cullen, or any other nosologist, by heart, or to him who, having spent a twelvemonth in the diligent observation of a great variety of diseases in a large hospital, had brought his knowledge to the test of practice during a whole month under the eye of the physician?

as it required from the writer to compose it. A good book, at all events, is never a very easy book, and never suddenly popular.

What books of the practical kind you should read, I will tell you presently; what you should not read, I will tell you now.

Never read any book that bears internal marks of being addressed more to the public than to the profession. They are all bad, and many dishonest.

Mind that you are not betrayed to commit yourselves unwarily to books (especially of modern date) upon diet and digestion, upon the liver, and the stomach. Unfortunately, the public is well understood to have such a relish for reading upon these subjects, that new motives have been thus let in for medical authorship, which are not very creditable. There is a demand for books of the kind; and if they are executed with some plausibility, they have a certain sale, and a certain kind of reputation is gained by them. Any of you, who may feel himself a little sharp and clever, might write such a book tomorrow, with a tolerable chance of all its attendant advantages. There is not a medical publisher in this town who would not give you something handsome for a book "upon the stomach."

But there are books upon practical medicine, written in our own times, much talked of, and containing much that is good; which, nevertheless, a student just beginning to observe for himself would do well to avoid, for they are sure to give an undue bias to his mind. Books, I mean, in which you find some strong predominant theory; as where numerous diseases, apparently different, are uniformly ascribed to some single cause, and an uniform practice recommended in conformity to the theory: where, for example, every sort of pain, in whatever part, or every species of nervous complaint, is attributed to plenitude or emptiness of the blood-vessels, or to errors in the functions of the liver, or the stomach, or the duodenum, or the bowels generally; and bleeding or leeches, or mercury or purgative medicines recommended accordingly.

I do not mean to say that such books may not be read with profit; but they can only be so read when the reader is able to guard himself with the checks and reserves of his own experience. They have, for the most part, been written by men of talent;

and, in attempting to show that all, or nearly all, diseases are cured by the pursuit of one indication of treatment, while they fail of establishing the point they intend, they succeed in establishing something short of it. They often show that the indication in question is just in many cases, and that it is one which deserves to be borne in mind in all.

The seat of fever is placed by one modern author, whom I greatly respect, in the head; and by another, in the abdomen. According to the one, all febrile movements radiate from inflammation of the brain as their centre; according to the other, from inflammation of the mucous membrane of the bowels. The doctrine of both, as a piece of philosophy, is untrue; but still both Dr. Clutterbuck and M. Broussais have deserved well of the profession, in so far as they have contributed to establish two paramount indications in the treatment of fevers—by showing, that in numerous cases our success will entirely depend upon the undeviating steadiness with which we address our remedies to the head or the abdomen. Hereafter you may read these books, but not at present.

I think I can illustrate all I have to say by one great example. Recollect, I am cautioning the student, the medical student especially, against trusting his mind to the fascination of any tempting theory, before he has put it fairly upon its guard by much independent observation of his own.

The work of Mr. Abernethy, upon "The Constitutional Origin of Local Diseases," has extraordinary merit and originality. The substance of the whole is this,—that local diseases are rather symptoms of a disordered constitution than primary and independent in themselves; and that they are to be cured by remedies calculated to make a salutary impression on the general frame, not by topical dressings or any mere manipulations of surgery. All this is good, and entirely justified by experience.

Next, that this disordered state of the constitution either originates from, or is rigorously allied with, derangements of the stomach and bowels; and that it can only be reached by remedies which first exercise a curative influence upon these organs. Even thus far it is a beautiful theory, and I am not disposed to deny it a large share of truth.

Then come to be considered the real nature of these visceral

derangements, of which little distinction is made, and the remedies proper for their cure, which lie in the small compass of a blue pill, or a compound calomel pill, at night, and a mixture of gentian and senna in the morning.

Practically the sum of all is this: that be the local disease what it may, the constitutional ailment what it may, and the derangement of the stomach and bowels what it may, this one method of treatment is at all times applicable.

What a tempting theory! and what a still more tempting practice! As soberly set down in print, the student can hardly help receiving them; for, being once faithfully received, what a world of tedious study and observation must they save him!

You, who never knew Mr. Abernethy, and have only read the doctrine which I have endeavoured to sketch, as it is carefully and beautifully developed in his book, have no notion what he made of it before his pupils in this room. A vein of it ran through every lecture that he gave. In his book it stands as a suggestion to surgeons, concerning the constitutional origin and treatment of local diseases; in his lectures it acquired an amplitude and extent which embraced every kind of disease incident to man.

You, who never knew Mr. Abernethy, have no conception of his powers as a lecturer. He so eloquently expounded some of the highest truths; he so nicely disentangled the perplexities of many abstruse subjects; he made that so easy which was before so difficult, that every man who heard him feels perhaps to this day, that, for some important portion of his knowledge, he is indebted to Mr. Abernethy. But he reserved all his enthusiasm for his peculiar doctrine; he so reasoned it, so acted it, and so dramatized it (those who have heard him will know what I mean); and then in his own droll way he so disparaged the more laborious searchers after truth, calling them contemptuously "the Doctors," and so disported himself with ridicule of every system but his own, that we accepted the doctrine in all its fulness. We should have been ashamed to do otherwise. We accepted it with acclamation, and voted ourselves by acclamation the profoundest of medical philosophers, at the easy rate of one half hour's instruction.

The great Lord Chatham, it is said, had such power of inspiring self-complacency into the minds of other men, that

no one was ever a quarter of an hour in his company without believing that Lord Chatham was the first man in the world, and himself the second; and so it was with us poor pupils and Mr. Abernethy. We never left his lecture-room without thinking him the prince of pathologists, and ourselves only just one degree below him.

Now that an important practical doctrine had been unfolded, is most true; and that it had been carried to an unwarrantable extent, is most true also: but how far to accept it, and how far to reject it, were questions for the sober judgment of a matured experience.

Therefore I do say, that this great teacher, in so far as he taught an exclusive doctrine, and claimed for it an almost universal application, and won an acceptance for it by the fascinations which genius, and fancy, and eloquence can command,—gave a hurtful bias to the mind of the student, and indisposed him to the indispensable task of observing for himself. For how is it possible that the mind, the youthful mind especially, can bind and buckle itself to the labour of getting possession of knowledge in the hardest possible way, by sifting every particular, and by patiently observing at the bedside, when it believes itself already furnished with all the wisdom which such laborious and *jealous* processes can ever teach? Yes, observation of disease is not only a laborious but a *jealous* process: it allows nothing to pass but under the warrant of the most cautious reasoning, or of the senses themselves; for these are the natural sentinels of the truth.

Summarily, then, I will venture to say, of all books which enter minutely into the practical examination of particular subjects, and those especially which open peculiar views, that it requires much personal experience to form a correct judgment of them, and to profit by them. While you are yet inexperienced in the subject-matter, you may be pleased with them as an argument, or a process of reasoning, and thus they are likely to make an undue impression upon your mind. But you will be at a loss about the simple conceptions, which are the pith and marrow of the whole. Under such and such conditions, and on such and such emergencies, says the writer, I reason thus; and this is my view of the case; and this is my practice. But to estimate the justness of his views, and the propriety of

his practice, you must first be familiar with the conditions and emergencies he speaks of.

I fear that by this time you are beginning to fancy me possessed of some strange prejudice against books; for that, whether they be good or bad, I still find some reason for advising you not to read them.

Indeed, it is not so. In what I am now saying, I am only endeavouring to explain myself a little more at large concerning a subject upon which I have occasionally conversed with you in the wards, when (as often happens) some one has asked me what books he should read upon this or that disease, which has at the time been the subject of observation. In truth, I want to excuse you to yourselves from any misgivings you may have, that you are not doing all you might for your own information, when you are *not* reading about every disease you see; for I am persuaded of nothing more certainly than this, that there is a previous necessity of disciplining our own mind by an independent course of observation, in order to fit it for anything like profitable instruction by the teaching of other minds, or, indeed, to furnish us with any tolerable security against being deceived instead of being taught.

They only who are practically informed can read good books with profit, or bad books without injury.

But still the literature of our profession, in its direct bearing upon practice, is a matter of the highest concern to you all. It is true that you cannot make any great acquisitions in it at present, but you ought to begin even now; not by running from this writer to that for a scantling of knowledge concerning each particular disease as it presents itself, but by seeking an acquaintance with those great writers who hold the keys to the just knowledge of all diseases, and the just administration of all remedies. Simultaneously with the observation of cases in the wards of the hospital, you must begin to learn the nature of morbid processes; and the study of these, and the observation of symptoms, begun together in this place, must never be separated in your minds as long as you live.

Hitherto I have spoken of books generally only, and as they may chance to fall in the way of the student; but now I would lead you to view a little more closely the character of medical literature in its bearing upon practice, in order that

you may judge whether mine is reasonable and sound advice, when I desire you to direct your studies to that part of it especially which is pathological.

Practical medical literature may be divided into that which is purely the result and product of observation of the living body, and that which is the joint work of such observation and of research into the nature of morbid processes.

I speak of that only as literature, whether ancient or modern, which is generally known and used and referred to, and has thus become classical by common consent. The part of medical literature bearing this character, which is purely the growth of observation, has many times struck me with wonder. For when I see that observation, exercised upon mere signs and external things, has so assorted and arranged them, so ascertained their import and described their succession, as if it possessed an insight into the inward processes out of which they arise, yet really possessing none; and when I see that, still guided by mere signs and external things, it has often given to powerful remedies the safest and the best direction; and that, concerning the event of diseases and the issues of life and death, it has been able to see clearly, and discriminate nicely, and prognosticate truly,—I feel assured, that from the records of practical medicine may be adduced the highest instances of human sagacity and prudence. I have lately been turning over the Prænotions of Hippocrates, and the Epidemics of Sydenham, and this is the impression they have left upon my mind, concerning a class of writers of which they are pre-eminently the chief.

This part of medical literature, with which pure observation is conversant, receives few accessions from time to time; and this will hardly seem strange, when it is recollected that the same, or nearly the same, things which we now observe, have been observed by others, with the purpose of turning them to the same account, for more than two thousand years. The field of observation was well cultivated at an early period, and few names stand forth in any particular age, at subsequent periods, who have been really eminent in this department; and these have become fewer and fewer as the world has grown older.

Now, when I desire you to reserve the study and perusal of these writers for some future period, even until the time arrive when you have taken the treatment of diseases upon yourselves,

do not conceive me to intimate that they are above the reach of your abilities. The truth is, that at present you have to learn their *language*. I do not mean the language of their words and phrases; these, indeed, are common words and phrases, but they intend something beyond the common meaning.

This meaning you can only gradually pick up, by living in the same region where they lived—by seeing the same things, and conversing with the same objects, that they were conversant with. This region is the region of observation; and they who live in it, and they who live out of it, cannot understand each other. They can construe each other's phrases, but they have a very dark apprehension of each other's meaning.

Do not imagine that I am forming an exaggerated estimate of this class of writers, because you do not *now* hear of their being much read by medical men at any period of life. I know they are not much read, and I will tell you why: it is because the majority of medical men have no real love for the practical part of their profession. It is a labour to them to observe; therefore they are no observers. They cannot see clearly what they must strain their eyes to see at all; and I will tell you the reason of this also: it is because when they were students (pray take warning from what I say) medical practice was unpopular, and they never attended to it; and they never were able in after-life to learn what they ought to have learnt in their youth. Their very faculty of observing was sound asleep when it should have been wide awake, and it could never afterwards be roused to discern more than the most obvious forms of things. No wonder, then, that the highest excellence in this same department of observation should have found few to appreciate it, and few to admire it.

But I hope better things from you. Only be diligent, and, at your time of life, and in so vast a field as this hospital, the very use and exercise of observation will naturally produce a taste and tact for observing; and then whatever you see in after-life you will see with profit, and draw sound experience from it; and not only so, but you will find yourselves of kindred minds with the great masters of our art,—reading them, relishing them, and improving by them.

But there is another part of practical medical literature, viz. that which is the joint work of observation and of research

into the nature of morbid processes—in a word, the pathological.

Observation, and mere observation, had been at work for ages, and the extent to which it had penetrated into the nature of diseases does, I confess, appear to me quite wonderful. But it could go no further alone; and it was obvious that, if diseases were ever to be better understood and better treated, observation must be aided by some new method of inquiry. That new method, in the course of time, was introduced, and is now popularly employed; it consists of research into morbid function and morbid structure, and is based upon the knowledge of healthy function and healthy structure. It is pathology founded upon physiology.

By the combination of these two methods, observation of symptoms, and a rigid research into the nature of morbid processes, the face of practical medicine has been completely changed, even in our day.

The advantage which the physician now has over the physician of old is this: he has the same observation to guide him, and he has, moreover, a previous knowledge of the real condition of things, from which the immediate objects of observation derive themselves; and, coming to his work of observation with that previous knowledge, he is able to make observation itself go as far again as it would go alone.

The fever, the cough, the sputa, the laborious breathing, the wasting of the flesh—these are the immediate objects of clinical observation, and they at all times intimated fearful things to the physician of old, concerning diseases of the lungs. But effusions and congestions, and softening and hardening, and tubercles and vomicæ—these are the real things from which the fever, the cough, the sputa, the laborious breathing, and the wasting of the flesh, all derive themselves; and the physician now knows them all, and what they are in their origin, in their progress, in their termination, and which are capable of reparation and which are not; and, knowing what they are, he has taxed his observation for the detection of them in the living man; and, having detected them, he has further taxed his skill for a more exact application of remedies for their cure. And unquestionably he has often succeeded, both in detecting and curing, by the aids of this knowledge, what would have gone

undetected and uncured if he had still worked by clinical observation alone.

Every day I go round the wards with you, I talk of things which must be quite unintelligible, if you are ignorant of morbid processes. There are forms of symptoms I am perpetually pointing out, which cannot be estimated except in their exact connexion with certain forms of disease previously understood. All the principal changes of structure which the lungs or the heart are capable of undergoing, must be well understood before you can appreciate any of the signs derived from auscultation or percussion. You may listen to the chest for ever and be no wiser, if you do not previously know what it is you are to hear. You may beat the chest for ever, and all in vain, unless you know what it is that is capable of rendering it now dull and now resonant.

The aid derived to practice from a knowledge of the means and agencies by which the disease is carried on is not always the same; but still there are few cases in which it does not contribute either to enlarge, or refine, or verify our observation, and to direct our treatment with a surer aim.

But this knowledge is not to be learned only in the wards of a hospital, or in any one particular way. The sources of it are various, and so too are the methods of obtaining it: lectures, books, museums, dissecting rooms, and experiments upon the living or the dead body. It is conversant for the most part with demonstrable objects, which are capable of being measured, and weighed, and delineated. It is beginning to take the form of a science, and to be governed by certain rules.

In as many places, then, and in as many ways as it is capable of being learned, you are at liberty to learn it.

And, howsoever and wheresoever you learn it, you must bring it with you into the wards of the hospital; and your observation will there breathe a spirit into it and apply it to its proper use.

Thus an acquaintance with the means and agencies by which disease is carried on will give greater effect and certainty to clinical observation, each testing and confirming the truths of the other, and both working together to the same end, and producing a surer diagnosis and a safer treatment.

Such are the things which I have now called you together to communicate; and, simple as they really are, they have cost me some thought; and it is with some feeling of responsibility that I offer them, when I reflect that the advice which I am giving you now, at the entrance of your practical studies, according as it is good or bad, may lead or mislead you for life.

It is a matter of no trifling concern to the well-disposed student, that he should be put in the right way of using his own observation, and that he should be well aware of all the means which are calculated to aid or hinder him in his task.

With respect to the taking of cases, which is one chief mode in which the observation is exercised, I have advised that the student should not begin to take them too early, and before he has got a clear notion of the great cardinal symptoms which are the guides to diagnosis and treatment: yet that he should not begin to take them too late; that, after he understands the import of symptoms, he should not allow his mind to rest too long in the abstract contemplation of them, but apply his knowledge to its use and exercise upon particular instances.

With respect to books, which are the chief aids or the chief hindrances to clinical observation, I have told him what he must read with caution, and what he must not read at all; what he cannot read *now* without an injurious bias, but may read hereafter with profit; and what he may read *now* without harm, but hereafter with more certain advantage. But, of all books and all studies, those, I have told him, are best calculated to promote the business of clinical observation which are especially conversant with the nature of morbid processes.

Medicine thus pursued begins to assume the form of a science. We call it pathology.

Now pathology is a study for your whole life. But it must be begun here, and it is important that it should be begun in the right way; and I am interested in seeing that it is so, because every right notion of pathology will be a great assistance to you in the acquisition of that knowledge which is to be gained in the wards of the hospital, and every wrong notion a serious hindrance.

LECTURE IV.

PATHOLOGY.—WHAT ARE ITS ELEMENTS.—HOW ANATOMY CONTRIBUTES TOWARDS IT—HOW CHEMISTRY—HOW EXPERIMENT—HOW CLINICAL OBSERVATION.—ILLUSTRATIVE INSTANCES IN ACUTE INFLAMMATION OF THE LARYNX—IN DISORDERED CONDITIONS OF THE URINE—OF THE BLOOD.—DR. PROUT—DR. STEVENS.—THE KNOWLEDGE OF LOCAL MORBID PROCESSES ONE ELEMENT OF PATHOLOGY.—INFLAMMATION. — ITS VAST EXTENT AS AN OBJECT OF INQUIRY.—ITS GENERAL LAWS.—ITS MODIFICATIONS IN DIFFERENT STRUCTURES. — SPECIFIC DISEASES. — SCROFULA. — CANCER, ETC.— DROPSY. — SPONTANEOUS HÆMORRHAGE. — SURGERY PROPERLY INTRODUCTORY TO MEDICINE IN THE ORDER OF PURSUIT.—A RECOMMENDATION TO STUDY DISEASES OF THE EYE.

WHAT is pathology? It would seem to imply whatever, either of discourse or reasoning, has any reference to diseases. But this is much too large and loose an acceptation; yet I cannot determine the exact compass of its meaning, so as to bring it within the limits of a definition.

For popular uses it is often well to lean to the popular sense; and the popular sense regards pathology as conversant with *explaining* the phenomena of diseases, not merely with *observing* them. This is just an intimation of the truth. But we must take a nearer view of the matter, and guard against any mode of expression which may betray us into error at our setting out.

True it is that pathology is "conversant with explaining the phenomena of diseases, not merely with observing them." But it is also true that, without observation of the living body, there can be no pathology. Observation needs certain helps to give it a pathological aim; but these are only subordinate; and it still belongs to observation to concentre all that they are capable of teaching in the real knowledge of disease.

This should be clearly understood. Observation, working by itself, was able to win from the waste a large field, and to bring it into cultivation, and to reap from it a wonderful harvest. But the cultivation was expended upon the surface, and did not go deep enough into the soil.

When anatomy betook itself to investigate morbid structures, and chemistry to analyse morbid fluids, and experiments of various kinds upon the animal body pushed their researches in their several ways, a number of new facts were brought to light; and diligent men made an inventory of them, and clear-sighted men gave them an order and arrangement. But neither was *this* pathology.

The truth is, that not one of these, taken separately, can arrogate to itself the name and character of pathological; but all taken together, and brought within the sphere of mutual illustration, furnish the full amount of our knowledge concerning the nature of diseases. Therefore, whatever is learned by dissection, concerning forms and structures; whatever by chemistry, concerning elementary constituents; whatever by experiment, concerning the appearance and behaviour of parts and organs, under any new conditions in which they are artificially placed; and, finally, whatever is learned concerning the actings and sufferings of disease in the living man; all these, in their sum and aggregate, must be deemed to constitute one pathology.

Now, believe me, you are never *more* engaged in studies strictly pathological than when you are busied about the sick in the wards of the hospital; when you are observing external signs, indeed, but seeking to penetrate beyond them, and endeavouring, through them, to come at the actual procedure of the disease itself. And believe me, also, that you are never *less* employed in pathological studies than when you are dissecting, or analysing, or experimenting, if the facts thereby adduced are suffered by you to remain inert and useless, and dead, and are not delivered over to the observation, that it may turn them to good account.

Anatomy, and chemistry, and experiment, by their own authentic facts, are most necessary guides and safeguards to the knowledge of disease by observation of the living body. But these have not so much enabled observation to enlarge

its proper territory, as to penetrate deeper into the same soil.

I have seen a man, young, and full of flesh, and with the form and plumpness of health, laid out dead. And I have scrutinized all his organs thoroughly and carefully; and all were healthy and perfect, save the margin of that little chink which conducts to the larynx. And here there was a slight swelling, partly of the membrane which invests it, and partly of the cellular substance beneath; but there was no ulceration, no breach of surface.

And could *this* occasion death? Why, there was hardly a perceptible narrowing of the passage. And could this, I say, produce death? Yes! indeed could it. Truly this little swelling is a mighty disease. In two short days it had subdued and annihilated this very man. Not all the force of remedies, or all the vigour of his own frame, could save him. I had seen him with all his might fighting for breath, but in vain, for he died strangled.

But whence do we chiefly learn the pathology of this disease? In the corpse, or in the living man? Why did the little lymph and serum *here* effused become a fatal mischief? The corpse did not, and could not, tell us. For anything it disclosed, he might still have lived; for after death the glottis was open, and air was made to pass freely through it to the lungs.

But what the corpse could not teach, the acting and suffering of the living man declared intelligibly enough. He spoke, and coughed, and breathed hardly and convulsively, and in an agony, and with a loud scream, or croupy noise; and he could not swallow. At length, voice, and cough, and breath, were all suppressed, and he died.

After death the glottis was open; but what was its state during life? Unquestionably it was greatly narrowed, or nearly closed; all that the patient did or suffered gave proof of the fact.

But what *can* narrow the glottis, if it be not narrowed mechanically? Surely nothing but the *vital* action of its own muscles.

Behold, then, the whole pathology of the disease! Those tiny muscles, which move the arytenoid cartilages and the vocal

cords, could not bear the contiguity of the disease of the mucous membrane. It irritated them into a mighty spasm, which no effort of the will, no struggle of the whole body, could arrest or control; and, acting beyond their natural sphere, they dragged into a forced approximation every part which they could move, and nearly closed the glottis.

Here is a disease of which the pathology is complete, and so clearly and intelligibly made out by dissection of the dead and observation of the living body, that it would not be difficult to assign exactly how much is due to one and how much to the other. The material change of structure, in its kind, its seat, and extent, is disclosed by dissection after death. This is the point of departure for the whole disease, and small enough it seems. But the disease, in all its magnitude and terror, and the very means and agents of its peril and fatality, become known by observation of the living body.

It is useful sometimes thus to analyse the sources of our knowledge, that we may apply to the same in fuller confidence when we desire its increase.

But I have not done with this beautiful instance, which has exhibited a perfect piece of pathology, as the conjoint work of clinical observation and of dissection. I will still make use of the same instance—this acute inflammation of the larynx,—in order further to exhibit to you how pathology can add new and wonderful resources to practical medicine.

As I was going round the hospital one morning, a dying woman was carried in and laid upon a bed. What a frightful picture she was! Cold, and livid, and pulseless; her eyes starting from their sockets; her mouth wide open, and lips, and tongue, and teeth, black with sordes; and breathing convulsively, and with a kind of scream. With what agony she struggled for life! and what force she used to preserve it! Tossing about her arms, striking aside all who came near, for they kept the air from her; and dashing away a cup of water that was offered, for she knew a single drop would suffocate her.

What was to be done? All I could learn was that a few days ago the woman was well. She got wet; and in consequence she had sore throat and hoarseness. She had been bled, without relief. Symptom after symptom arose rapidly

and uncontrollably, until they reached their present awful consummation.

This was quite enough to know. I ordered her trachea to be opened. Mr. Earle was at hand, and did the operation at once. The relief was complete, and she sank into a calm slumber.

For two weeks she breathed through the wound entirely; then partly through the wound, as it began to heal, and partly through the glottis, her voice beginning gradually to return. At the end of three weeks she breathed entirely through the glottis, and in six weeks she was discharged well. I have twice since, at distant intervals, met her in the street, and she has recognised me with a smile.

Now, do you ask what it was that called for the use of this extraordinary measure, and what was the manner of its success? Revert to the pathology of the disease, and you will see.

The disease was acute inflammation of the glottis. But dissection finds nothing in inflammation of the glottis which is *peculiar*. Dissection does not discover why it is not just as curable as inflammation of any other organ. But recollect, not more than half its pathology can be learned by dissection. For the rest, we look to clinical observation; and clinical observation teaches, that all that is peculiar and intractable in inflammation of the glottis is derived, not from its own nature, but from the part it occupies. In its own nature it is as curable as inflammation of any other part; but the glottis, from its essential irritability, will not suffer inflammation to abide upon it long enough to go through the process of a cure. The muscles of the larynx, if they must act, will *now* act convulsively; and act they must; for the larynx is an organ of perpetual and vital use, and in that use the muscles are engaged.

Hence the necessity of placing this organ under some artificial condition, which would enable the constitution to dispense with its use for a time. This is effected by opening a new passage for air through the trachea into the lungs, whereby the larynx is left at rest, and its inflammation brought within the same possibility of cure as that of other parts.

Thus we have seen how clinical observation, guided by a well ascertained anatomical fact, was able to concentre a complicated series of morbid actions and sufferings in one point, and arrive at a consistent pathological result. And we have seen, also, how that result, leading to a new and successful method of treatment, obtained thereby the best confirmation of its truth.

In like manner, chemistry, by giving the aid of its authentic facts to clinical observation, has led the way to large and consistent views of pathology, which *alone* it could not have enabled us to reach. In the hands of Dr. Prout, chemistry has become a key to pathology. As a chemist, he has pushed the analysis of the constituents of unhealthy urine much further than his predecessors. As a physician, he has turned both their discoveries and his own more largely and successfully to the uses of pathology. While he has given his own peculiar skill and genius to the work of chemical analysis, he has still adhered closely to clinical observation; and thus he has detected, in the disordered actions of different parts, and of the constitution at large, a manifest pathological alliance with the morbid products of the kidneys.

Read his chapters, especially upon "The Lithic Acid Diathesis," and upon "The Phosphatic or Earthy Diathesis," and you will see, not only how the characteristic constituents of the urine in the one are opposed to those in the other, but that the lithates have a peculiar kind of constitution to which they are allied, and peculiar forms of disease with which they are apt to be accompanied; and so have the earthy phosphates; and that these are as much contrasted with each other as the characteristic constituents of the urine itself.

I recommend Dr. Prout's book upon Diseases of the Urinary Organs to you, for the sake of the important information which it contains; and, moreover, as the best specimen of that method of philosophizing which medicine requires and admits. For if we consider the peculiar place which medicine holds as a department of knowledge, and how many things may be made to bear upon it which seem hardly to belong to it, no work can be too much prized which will teach us how to reason upon medical subjects, and especially how to unite the conclusions of any demonstrative experiment with the results of clinical

observation, so as to render them both subservient to an explanation of diseases.

When we speak most modestly of medicine, we call it nothing more than a conjectural art. But this conjectural art so closely borders upon the neighbourhood of the sciences, and draws so much from their principles and discoveries, that we may be pardoned for sometimes calling it, and even believing it to be, itself a science.

Dr. Stevens, by experiment as a chemist, found that there was a condition of the blood in which it lost its due proportion of water, and its due proportion of neutral salts, especially common salt. And Dr. Stevens, by observation as a physician, learned that this condition of the blood was associated with the malignant symptoms of yellow fever. The contemplation of these facts led his mind to the employment of a new practice, the object of which was to give back its defective ingredients to the blood by the administration of salt and water; and thus he succeeded in curing an enormous proportion of those who, by any other method of treatment previously known, would have been thought incurable.

Here chemical experiment and clinical observation, leading (as it were) each other by the hand, proceed together, and arrive at the seminal principle of the disease. Passing by this organ and that, and this function and that secretion, they penetrate to the spring and source of all, even to the blood itself, and *there* they find it, and apply a remedy which is able to reach it *there*.

Truly these things are calculated forcibly to arrest the attention of every philosophical physician. Are we upon the verge of a great pathological discovery? We know how much belongs in common to all diseases called febrile. Dare we presage that the worst, and hitherto most fatal symptoms of all fevers, will soon be shown to have one origin? that a pravity, or deficiency in the constituents of the blood, is the cause? that this is demonstrable? and that it is remediable by the simplest means, which are always at hand?

Thus far I have endeavoured, by suitable instances, to show you the elements of pathological knowledge in actual operation, and how they work their way to the rational explication of the disease, and to the successful remedy.

But these elements must be possessed before they can be used. And, besides what results from clinical observation, which is one element, there are others which, you have seen, are supplied by anatomy, and chemistry, and experiment; and by these means you must acquire them, or by the instruction of those who already understand them.

I must presume that you are already tolerably acquainted with the structure and functions of the body in its healthy state; for otherwise you have a slender chance of comprehending its diseased conditions. The same blood-vessels, the same absorbents, and the same nerves, which are the agents of health, are also the agents of disease. The blood-vessels supply the pabulum by which the whole body and all its parts live and grow and perform their natural functions, and the blood-vessels also supply the pabulum from which every morbid structure and every morbid secretion is furnished and maintained. The absorbents bring in from without whatever is capable of assimilation and conversion into blood; and thus furnishing the materials by which the blood itself lives, they become, in the first and highest sense, the very springs and fountains of the body's nourishment. But the absorbents, which have assigned to them the process of ulceration in all its degrees and extent, become also, in an especial manner, the instruments of the body's destruction. The nerves impart pleasure, and the nerves impart pain. They regulate motion according to the will, and they withdraw it from the dominion of the will, rendering it convulsive and disorderly. Thus are the conditions of health made to give place to the conditions of disease by the instrumentality of the same agents, but by other modes of action.

You must seek to understand these things. As soon as you enter upon the business of clinical observation, you must have some right conception of them, if you would observe usefully; and ever afterwards, while your practical experience increases, you must take care that your knowledge of morbid actions keeps pace with it; that as your views become enlarged, they may be still precise, still *pathological*.

A discouragement often attends the first inquiry into morbid actions, from a belief that they are, in their very nature, so irregular that no clear notion can be obtained concerning them. But, although morbid actions, in comparison with healthy

ones, may be called irregular, they are not irregular in themselves, but capable of being reduced to laws, and conversant with principles.

For this reason, your inquiry into the nature of morbid processes should be careful and wary at every step. For here, if anywhere in the whole range of your professional studies, you must clearly understand each particular as you proceed, until you reach the point at which you discern the proof of a general law, and from that point you will advance rapidly and agreeably into a larger field.

Now the study of morbid processes begins with inflammation. And even popular opinion has learned to associate many portentous things with the notion of inflammation. And justly, because the world finds us perpetually talking about it, and perpetually dreading it. Practically, inflammation is never absent from the minds of medical men. Wherever an organ labours, wherever there is pain, the first practical question which we seek to determine is, whether there be inflammation present.

Inflammation is unquestionably the most capacious of all medical subjects; and fortunately it is that to which the best minds of our profession have been especially directed; and, more fortunately still, which they have best succeeded in illustrating. We are, therefore, sure of the best guides to assist us in the knowledge of it.

And since the knowledge of inflammation consists in great part of demonstrable facts, it is the more valuable on account of its certainty. And, moreover, since it is in a peculiar manner fundamental of almost all other knowledge in pathology, it is manifestly indispensable.

You must study inflammation as if it were a subject of rigid philosophy, carefully and patiently, and with the purpose of understanding every stage and step of it as you go along.

In inflammation there are numerous processes included; these may either be considered as parts and parcels of one inflammation, or some only as properly constituting the inflammation, and the rest as its products or consequences.

There is the *vascularity*, in which the blood-vessels act an important part within themselves prior to any change in the condition of parts without.

There is the *effusion*, in which the contents of the blood-vessels escape into the surrounding textures; these are serum, or lymph, or blood.

There is the *suppuration*, in which a new and peculiar fluid is formed.

And, coincident with these processes, there are adhesion, ulceration, granulation, gangrene. Of which some are destructive and some reparatory.

Now the several processes have their own physical conditions, which separate them from each other. And thus they require a separate study; by which you may know the very channels and agents of each according to its kind, and what the arteries, what the veins, what the absorbents do, and what the nerves.

But there is nevertheless a strict physical alliance between them; and, therefore, they must also be studied collectively. One does not merely precede the other, but naturally conduces to it; another does not merely follow, but naturally germinates from, its antecedent.

But there is no such thing as inflammation in the abstract. It must belong to some part or structure. Yet, as soon as you begin to contemplate it in one structure, you must not imagine that you are to find it in all other structures strictly the same. It is the same in kind; but it has different forms and modifications, according to the part it occupies. You may first study inflammation in the subcutaneous cellular membrane. I would advise you to do so, because here it exhibits the plainest example of itself, and all the processes which it includes here display themselves prominently and in a regular succession. But beware of calculating the progress of inflammation in the brain, the lungs, or the spleen, by what you have seen of it in the subcutaneous cellular membrane. What in this case is the commonest process of inflammation, viz. suppuration and abscess, those organs very seldom admit.

Some or other of the processes enumerated occur in all organized tissues, whenever they are inflamed. But different organs are more ready (if I may so say) to accept this and to refuse that, as they are induced by peculiarities of their own structure. Perhaps there are no two organs of the body which exhibit inflammation under exactly the same aspect, and the

variety is owing (as far as we know) either to the different tissues of which they are composed, or to a different arrangement of the same tissues.

Consider, then, that concerning inflammation you have two great objects of inquiry. The first embraces what it is in itself, the rationale of its several processes, and the general laws which govern it, wheresoever it is found. The next embraces what it is under all the modifications with which it is capable of being impressed by the various structures and organs which it occupies, its general laws still remaining inviolate—what it is in the brain and spinal marrow, in the lungs and in the heart, in the liver and spleen, in the complex structure of the joints, in every coat of an artery, in every vestment and membrane of the eye, and in the walls and marrow of the bones.

But this immense subject of inflammation, in all its details, surely cannot be mastered by the student during the brief period of his pupilage. Nevertheless he may make his entrance upon it, and may proceed so far in it as to reach some just conception of its general laws. Besides, now is the time when he has peculiar helps at hand which will enable him to prosecute the inquiry in the right way. What these helps are, I will tell you presently.

Thus the knowledge of inflammation may be regarded as the groundwork of all pathology. It is the commonest as well as the most comprehensive of morbid actions. I call it *common*, because it seems to arise inevitably in every man and in any part of the body under certain circumstances; you may even produce it at will.

Besides inflammation, there are other morbid actions, the processes and products of which require your study. These are not *common*, in the sense which has been just explained. Scrofula, or cancer, or fungus, or hydatid cysts, cannot be produced at will; neither are they incident to all bodies; neither do they inevitably result from any known conditions. These diseases are called *specific*, as contradistinguished from inflammation, which is *common*.

Concerning specific diseases, we have not the same amount or the same certainty of knowledge that we have concerning inflammation. We have a large inventory of facts, but not a clear insight into the general laws which influence their pro-

duction; and knowledge, as long as it falls short of this, is still uncertain and precarious.

Besides these, there are certain other diseases which require to be studied in the very processes out of which they arise. They are not so mysterious as specific diseases, and yet not so *common*, or so well understood, as inflammation. Many conditions of their production we profess to be acquainted with; but still we cannot produce them at will. Dropsies and hæmorrhages are of the class I mean. Each of them is a very large subject.

I have been speaking of all these diseases—of inflammation, and dropsies, and hæmorrhages, of scrofula, cancer, &c.—as *local;* that is, as having a place and locality in which the special morbid processes of each are carried on; for it is to such processes that I desire now to direct your attention, the knowledge of them being one of the elements of pathological medicine.

Not that each has not more belonging to it than its local process which deserves inquiry—some disorder of parts remote, or of the general system, out of which it may primarily arise. But, however this may be, they none of them, whether inflammation, or dropsy, or hæmorrhage, or scrofula, or cancer, have any *demonstrable* existence, until they declare themselves by modes of action and modes of suffering, *in the part*, which are beside the uses and conditions of health; by something added to, or something deducted from, or some change wrought upon, its constituent structures.

Now I have told you, that, during your pupilage, and at this hospital, you have peculiar helps, enabling you to make a successful beginning in this element of pathology, the knowledge of local morbid processes. You have lectures both medical and surgical. Of surgical lectures, a large portion is always occupied in describing and explaining the processes of inflammation, and in illustrating them as the results of injury, or accident, or disease, in parts and organs which fall within the special province of the surgeon. The medical lectures, also, are largely explanatory of inflammation, what course it takes, and what termination it has in different internal organs; how this course and termination vary in serous, in mucous, and in fibrous membranes; how in the pleura, the pericardium, and the peritoneum, they are of one sort; in the lining of the trachea

and bronchi, stomach and bowels, bladder and urethra, of another; and still of another in the dura mater and pericranium; and how each circumscribed viscus of the body shapes and qualifies its inflammation in its own manner, and to a particular end.

Concerning dropsies, besides their remote cause, medical lectures dwell upon their cause *in the part*, or the very process of the effusion; and show why one organ or structure should more readily accept it than another.

The subject of hæmorrhage—I mean that hæmorrhage which is independent of injury or accident from without—is laid before you in the same lectures; and the process of its production shown, as it occurs in the brain, in the bronchi and lungs, in the stomach and bowels, in the kidneys, bladder, and uterus.

Of specific diseases, take cancer and scrofula as examples from among many which bear different names, and are formed (perhaps) by different processes. These are diseases with which both medicine and surgery are equally concerned; and the teachers of each have most curious and interesting information to give respecting their growth, by a well-defined succession of morbid processes. Of cancer, the female breast and uterus, and the stomach, are the most frequent seats, while hardly any part is exempt from a liability to it. Of scrofula all parts are the seat. It is enough to say, that it is the essence of that all-pervading disease, pulmonary consumption, in order to engage every one who intends to practise physic in the endeavour to learn all that is known about it.

Concerning these several orders of disease, inflammation, dropsy, and hæmorrhage, and specific complaints such as cancer and scrofula, let me further add, that, besides the study of each as it is in itself, in its own morbid process, there is a study of them also in combination, which belongs to the knowledge of pathology; for they occur as frequently in combination as alone, one process running into another, or one process exciting other processes all around it. Thus inflammation will run into dropsy or hæmorrhage; and it will be so blended with scrofula, as to be called *scrofulous inflammation*. Cancer can hardly reach its fatal consummation without inflammation and hæmorrhage; and scrofulous tubercles of the lungs often excite at the same

time inflammation of the surrounding pulmonary structure, hæmorrhage from the bronchi, and dropsy into the cavity of the pleura.

You really must have some correct notion concerning all these things before you can derive the profit which you ought from clinical observation; and, as I know you cannot go through a laborious investigation of them at present in all their accurate detail, I must refer you to the sources of information nearest at hand. I must advise you, for you have all attended medical, and most of you surgical, lectures, and have taken and preserved your own notes of them,—I must advise you, to review all those parts of such lectures which expressly treat of morbid processes. Those of you who are already conversant with surgical practice will do well constantly to bear in mind what you have seen upon the exterior of the body. You will find it of daily use in every medical case you observe. Processes of disease within and without the body are of the same essence. Their forms only are influenced by structure, and are different in different parts.

Unquestionably, there is no better introduction to the practice of physic than the practice of surgery. It is a course which I strenuously recommend to all those who have time to carry it into effect. The best and most highly instructed men feel the necessity of it, and do not shrink from it. I believe I am correct in saying, that of men educated at the universities, and then resorting to St. Bartholomew's Hospital with the view of becoming physicians, during the last sixteen years, there has hardly been an individual who has not gone through the entire business of a surgeon; not as a mere looker-on, but as a dresser, for as long a period as if he was to practise surgery all his life. I mention it to their honour; for an admirable race of physicians has been produced by this system. Thus it has become the established practice here for all who intend to be physicians to begin with surgery. Professor Haviland of Cambridge first brought the practice into fashion by his judicious advice to all within the sphere of his influence; and I have done all in my power to second the recommendation of my excellent friend. The benefit consists mainly in this, that it makes you familiar with the visible processes of disease and reparation while they are actually going on.

There is yet another recommendation which I would offer to you; and let it not seem a strange one, whether you are to be physicians or surgeons. If you desire to make pathological knowledge the groundwork of your credit and usefulness through life, let me advise you not to allow the period of your pupilage to pass by without making a special study of the diseases of the eye. Here you see almost all diseases in miniature; and, from the peculiar structure of the eye, you see them as through a glass; and you learn many of the little wonderful details in the nature of morbid processes, which, but for the observation of them in the eye, would not have been known at all. Let every one of you who has a few months to spare give them to the Eye Infirmary.

Now, after what has been stated, you will perhaps be ready to ask me, Whether it be indeed true, that all this sort of knowledge is required to fit a man to practise physic? and I will answer you honestly, That it is not required. Many a clever man practises physic with tolerable success, who has never troubled his head about morbid processes, and who has not the remotest notion how those things come to pass which he has been witnessing, in their effects or their symptoms, all the days of his life. A man may practise physic without it, but he cannot be a *first-rate* practitioner without it. In the treatment of diseases we often minister to the symptoms, and the symptoms only; but, in the treatment of diseases, we often minister partly to the symptoms, and partly to the very processes of the diseases themselves. This he only can do who knows what they are. Believe me, he who would be a first-rate practitioner must lay his foundations broad and deep in the knowledge of morbid processes; otherwise, although he may sometimes prognosticate truly concerning life and death, he can *never give* an accurate diagnosis concerning the nature of diseases of which he can understand nothing. Above all, he must never hope to benefit mankind by advancing the knowledge of his profession a single step.

LECTURE V.

ON THE PROPER OBJECTS OF MEDICAL INVESTIGATION.—WHAT MEDICAL FACTS ARE, AND WHAT THEY ARE NOT.—THE OBSERVATION AND COLLECTION OF FACTS.—THEIR ARRANGEMENT, ACCORDING TO ANALOGY OR RESEMBLANCE—ACCORDING TO THE RELATION OF CAUSE AND EFFECT.—WHAT IS MEANT BY THE RELATION OF CAUSE AND EFFECT BETWEEN MEDICAL FACTS.—PECULIAR DIFFICULTIES IN THE WAY OF ASCERTAINING THAT RELATION.—THE TASK OF ASCERTAINING IT NECESSARY TO OUR KNOWLEDGE OF THE SOURCES OF DISEASE—OF THE INFLUENCE OF REMEDIES—AND OF THE CONNEXION BETWEEN THE DISEASE AND ITS SYMPTOMS.—THE SOURCES OF DISEASE, HOW EASILY AND QUICKLY ASCERTAINED IN SOME INSTANCES—HOW TARDILY AND DIFFICULTLY IN OTHERS.—THE INFLUENCE OF REMEDIES, HOW LIABLE TO DECEPTION, FROM A VICIOUS CREDULITY ON THE ONE HAND, AND A VICIOUS INCREDULITY ON THE OTHER.—THE NATURE OF GENERAL PRINCIPLES IN MEDICINE, AND HOW THEY ARE REACHED.

CERTAIN observations which I formerly made* must be considered to comprise the *method* only in which the particulars of a case may be most conveniently surveyed: but more remains to be considered than the mere form of case-taking.

You may adopt this mode of case-taking, or any other which your own experience may make most convenient to you: but no method of recording the particular facts will be of any use to you, unless you have right notions concerning the *facts* themselves. The facts, in truth, *are* the case; and it is only for the sake of getting *sure* possession of them that we adopt any form of (what is technically called) case-taking.

Now the whole business of your lives will be the business of taking cases; not necessarily with pen, ink, and paper, or after any technical form. But still it will be the business of

* Vide page 29.

your lives really and essentially, and in the justest sense; for each case is made up of its own facts, and the facts alone must teach you the nature of the disease, and suggest the remedy in every patient you see. Therefore, as long as you live, you will be evermore conversant with facts; learning and collecting them, arranging and combining and separating them, tracing their relations, and through them arriving at general principles.

There are peculiar causes which will ever prevent medicine from arriving at the certainty of purely physical science. But in so far as it is *certain*, in so far as it has taken the form of a science at all, it is built upon the same foundation with all other sciences; namely, upon facts: and in so far as it is uncertain, beyond what, in its own nature, it ever need to have been; in so far as it has not deserved the name of a science, it is raised upon a foundation which never would have been deemed sufficient for any other department of human knowledge.

It is important, then, to the right judgment and the right treatment of every individual case that we see—it is important for the sake of preserving to medicine whatever claim it may have to the name of a science, and (I will add) for the sake of our own credit and satisfaction, that we should utterly reject all other foundation of professional knowledge except matters of fact.

I desire to convince you of this, by showing you, first of all, what matters of fact in medicine really are, and what they are not.

Every man's notion concerning any department of knowledge is the popular notion, until it is rectified by further inquiry. The popular notion concerning medicine is, that diseases are separate essences, and that an idea can be formed of them apart from the living being in whom they occur; that a fever, or a pleurisy, would still be something real, although there were no living beings in whom they could manifest themselves.

But diseases are not abstractions; they are (I have said) "modes of acting different from the ordinary and healthy modes—modes of disorganizing, modes of suffering, and modes of dying; and there must be a living, moving, sentient body for all this."

Endeavour to understand this truth; and at the very entrance of your professional studies get rid of all abstractions,

that you may never record them and use them (as they are and have been recorded and used) as matters of fact.

You may record that this man has a hot skin, a dry tongue, and a frequent pulse, and call his disease a fever; that this man has a pain in his side, difficult breathing, and a hard pulse, and call his disease a pleurisy; but beware of taking this fever or this pleurisy for more than they really are. The fever is nothing, and the pleurisy is nothing, but the complex of the several facts which you have recorded under the head of each: separate from them, they are mere names. The fever has no treatment, and the pleurisy has no treatment, but what is suggested by the facts included under each. You prescribe for the hot skin, the dry tongue, and the frequent pulse, and you bring back their condition to what is natural, and so cure the fever; and you prescribe for the pain in the side, the difficult breathing, and the hard pulse, and so cure the pleurisy. But if, without regard to the facts apparent in the individual patients, you pretend to address your remedies to the fever *itself*, or the pleurisy *itself*, you take aim at an absolute phantom.

Bear in mind, then, that abstractions are *not facts;* and next bear in mind that *opinions* are not facts. To record that a patient is *better* to-day and *worse* to-morrow; that he is at one time doing *well* and at another doing *ill;* is to give a *summary opinion* upon the facts, not the *facts* themselves.

I do not mean to say that such summary opinions should not be formed, or that they should not be announced. We always *do* form them, and the patient, or the patient's friends, always require us to communicate them; and most justly. But to ourselves, and in communication with other medical men, either for the sake of marking the exact state of the disease or suggesting the remedy, they are utterly useless.

If, in going round the wards of the hospital, I had it punctually recorded that this, that, and the other patient had a peritonitis, a nephritis, or a hepatitis; and that day after day this, that, and the other patient were better, worse, or just the same; and if, day after day, I were to order bleeding, blistering, or purging, as the case might require, and thus you were to witness numerous instances of recovery, you would not reap the slightest benefit from me and my pretended instruction, although you went round with me for a twelvemonth. For this would

be to keep industriously from your notice everything in the shape of a *fact* by which you could estimate the nature and progress of the disease, or the operation and effect of the remedy.

In medical science, the only materials of our knowledge are those things which are referable to our sensations and perceptions: matters of fact. Such are the temperature of the skin; the number and qualities of the pulse; the quantity and qualities of secretion; functions and modes of action in the several parts and organs of the body; and all their cognizable deviations from what is natural: also pain; for although pain, as to its actual occurrence in a particular case, must be taken upon the testimony of the sufferer, each man's own experience must some time or other have convinced him that pain is a *fact*.

Such, too, are the conditions of parts discoverable after death; their increased or diminished bulk; the changes of structure and injuries they have undergone, and the morbid products to which they have given origin. These are the sort of facts with which we have to do, if we would know diseases and how to treat them.

Such being the materials of our knowledge, it becomes important to consider how we can use them best, and abuse them least; for the materials of our knowledge require to be hewn and squared, and fitted to their place, with much care, and skill, and diligence; otherwise, what might have been a seemly edifice may chance to be no better than a heap of rubbish.

First, then, as to the *simple reception* of medical facts, there is a good deal to be learned.

All facts are not of equal value: some are trivial and accidental, some important and essential to the subject. In your inquiries at the bedside, you will have to select and to reject cautiously and discriminately. Patients themselves are apt to press upon their medical attendants symptoms (generally consisting of strange sensations) which are irrelevant to their present disease. Again: in your examinations by dissection, you will have to use much care, lest you should admit appearances as indicative of disease which are accidental, or may have arisen during the process of dissolution, or from the position of the body after death.

But in this sifting and separating of facts, be cautious of throwing away anything that is really valuable. Beware of rejecting facts of which you do not, perhaps, comprehend the import, and because you do not comprehend it; but rather reject those which you *do* comprehend, and know them to be trivial. Be careful especially of not allowing their due weight to facts which appear contradictory to each other; rather examine them more scrupulously because they are so. For instance, a patient may have a severe pain in the side and a frightful dyspnœa, and the pain and the dyspnœa may have arisen suddenly. Such symptoms at once carry with them the suspicion of active inflammation, and our thoughts may immediately run upon copious bleeding. But the same patient may, in the meantime, have his skin cool, and his pulse soft and tranquil, and not more frequent than the pulse of health. Cases of this and of the like kind are not uncommon, in which symptoms of a contrary character neutralize the import of each other, disprove the suspected existence of a dangerous disease, and forbid the needless adoption of a severe remedy

The more you exercise yourselves in the observation of medical facts, the more you will understand the sources of error to be avoided in the reception of them. Time and diligence, and constant intercourse with the sick, if you have but *an impartial and honest* mind, will enable you to lay up a large and useful store of genuine facts, and to draw from it as the treasury of your future knowledge.

I say an *impartial* and an *honest* mind, because it is remarkable how apt some little favourite theory is to get early possession of the student's imagination, rendering him dishonest (perhaps unconsciously) in the simple reception of facts. It is like some little favourite sin in our moral nature, which taints the character of the whole man.

A premature desire to generalize, an eagerness to arrive at conclusions, and a readiness to rest in them, are very common infirmities, and they offer very serious hindrances to the right acquisition of facts. For, if the early habit of theorizing do not estrange the mind of the student from the wish to observe altogether, it may so pervert the faculty itself in its very use and exercise, that, be his wish what it may, he cannot observe honestly. He gives an undue weight to the facts which accord

with his assumed principle, and no weight at all to those that conflict with it: habit forces him to do so, and he cannot help it.

A very good and wise man has explained this matter by an illustration, which is so beautiful and so true, that I must recite it to you:—"A watchmaker told me that a gentleman put an exquisite watch into his hands that went irregularly. It was as perfect a piece of work as was ever made. He took it to pieces and put it together again twenty times. No manner of defect was to be discovered, and yet the watch went intolerably. At last it struck him that the balance wheel might have been near a magnet. On applying a needle to it, he found his suspicions true: here was all the mischief. The steel work in the other parts of the watch had a perpetual influence on its motions; and the watch went as well as possible with a new wheel. If the soundest mind be *magnetized* by any predilection, it must act irregularly."*

In the reception of facts, then, it is essential, first, that they be fully ascertained—*i.e.* upon sufficient observation; 2ndly, that they be fairly and honestly represented, without disguise, modification, or omission, to make them suit particular theories; 3rdly, that they be important and essential to the subject—not trivial and merely incidental to it.

Next comes the arrangement of facts. Now, facts may be arranged according to certain characters of agreement observable between them. There is an agreement of analogy, and an agreement of resemblance. *Analogy* is a loose sort of resemblance, in which points of agreement and points of difference are mixed up together. By separating twenty points in which they differ, and retaining the two or three in which they agree, multitudes of things may be brought into this loose kind of resemblance and classed together. Thus, *analogy* is enough to bring diseases together in the same nosological order, as Fevers. Then, again, by calculating what individuals of the general order have most points of agreement, and distinguishing them accordingly, we come to divide orders into genera; and by still further selecting the individuals of the genus which have most points of agreement, we find the species; and by dealing with the species in like manner, we may divide it again and again,

* Cecil's Remains—on judging justly.

until thus, from the relation of mere analogy, we reach that of a tolerably strict resemblance. This is the scheme upon which nosologies are constructed; and nosologies have their use, until we can arrive at something better. It is better to arrange facts according to the relation of analogies and resemblances than not at all.

But it is far better still, if it be possible, to arrange them according to their natural sequences, *i.e.* according to the relation of cause and effect. Now, "our knowledge of cause and effect, in reference to any two particular events, is founded entirely upon the observation of a uniform sequence of the events; or of the one following the other in a uniform manner in a great number of instances. The greater the number of instances is in which the sequence has taken place, with the greater confidence do we expect it to take place again under similar circumstances; and every single instance in which it does not occur weakens our confidence, unless we can discover some adequate cause by which the sequence was interrupted. The result of this confidence is, that when we observe the first of two such events, we expect the second to follow it; and that when we observe the second, we conclude the first has preceded it. The first we call cause, the second effect."*

The philosophical physician is evermore studying how, upon adequate grounds, he can assign to medical facts this relation. But he knows in how delicate and difficult a task he is engaged. He is obliged to wait upon experience, and to attend to phenomena as they happen to occur. He cannot bring them together at will, and vary and transpose them as he likes, so as to learn their connexion. He envies the ease with which the chemist can bring any substance within the sphere and influence of as many others as he pleases; and the accuracy with which he can then ascertain the degrees of affinity it bears severally to each,—an accuracy so precise, that he can express them by numbers.

Further: when the physician has ventured to draw such

* Abercrombie; Intellectual Powers, 390.

Whoever is interested in following the right path of medical investigation, would do well to acquaint himself with that pointed out by Dr. Abercrombie. I recommend his book to the careful perusal of every student.

conclusions as his long observation seems to warrant, he cannot test their truth by any simple experiment. He has no litmus or turmeric paper to tell him whether the blood-vessels or the nerves are the prime agents in producing a certain form of fever; but what his long observation seems to have taught him, he must still wait for the same long observation to confirm or to confute.

Unfortunately for us, the nature of medical causation is such, that it takes as much time and trouble to rectify an error as to establish a truth. Thus it may require the experience of one man's life to arrive at some plausible theory, and the counter-experience of another man's life to show that it is false.

The vast experience required to establish a uniform sequence of events, and the impossibility of applying any summary test to the truth of our conclusions, are difficulties inseparable from the nature of medical causation. They lie at the very root of the matter.

And beside these, there are others, from sources impossible to enumerate, which perpetually beset and waylay our path of inquiry, breaking in upon the uniform sequence of events, and disappointing the best conclusions of experience. Such are the influences of places, and seasons, and climates, and the wills, feelings, and propensities, and all that is understood by the constitution, corporeal and mental, of human beings.

But however hard the task may be, we must still try to know the true relation of the things which concern the ordinary practice of our profession; we *must* trace the influence of external agents as causes of disease; otherwise we can do nothing for its prevention. We *must* trace the influence of external agents as remedies; otherwise we can do nothing for its cure. We *must* trace, too, the connexion between certain symptoms and certain morbid processes going on within; otherwise we can adopt no rational treatment of individual cases.

Respecting the external causes of disease, you must make them your study as opportunities may present themselves to you. But there are some so important, that their investigation has become of itself a department of medical education; I mean what is called "forensic medicine." And, indeed, I know no department of public teaching which, if it be entrusted to good hands (and I am sure it is so in this hospital), promises more

benefit to medical science and medical practice than this. It undertakes to illustrate the modes in which injury, disease, or death, arrive from those external agents or accidents that are most signally hurtful to animal life; from poisons of every kind; from lightning; from hanging and drowning; from corruption of the air; and from every method of simple violence. Thus forensic medicine is conversant with all the highest points of physiology and pathology; and its very purpose requires the greatest exactness in the nature and display of its proofs. It requires, in truth, that they should be so made out as to be obvious to such understandings as ordinary men are accustomed to bring with them into a jury-box.

Here, then, is provision made, within a large and interesting field, for demonstrating to you the effects of external agents as the causes of disease; and all *matters of fact*.

The display of the fatal effects resulting from the causes enumerated to several vital parts and organs, furnishes so many demonstrations of the possible ways in which the same parts and organs are capable of suffering from causes less hurtful. Not long ago, poisoning was an affair of the utmost darkness and mystery; but now, the rationale of poisoning in its several kinds is so well made out, that I am able to refer to it for the best instances which pathology affords of cause and effect in the manifest influence of external agents and accidents for the production of disease.

Poisoning, and the severer injuries, are a sort of pathological experiments. They produce upon this or that organ all the phenomena which any conceivable disease can exhibit; and they produce them in the greatest simplicity, because the subject is often previously in a state of health. Thus it is that they furnish an admirable introduction to the study of what is called spontaneous disease in the same organs.

Very acute inflammations arising suddenly in a healthy body afford the *next best* instances of the effects of external agents as causes of disease. I am accustomed to regard them in the light of medical accidents; for although the inflammation come from no injury in the ordinary sense, yet it will generally be found to have followed the exposure of the body to influences which act with a kind of violence,—such as sudden changes of temperature, or the use of meats and drinks which, from their

preposterous kind or quality, have become in a manner poisonous. Thus, the external cause productive of inflammation, and the time of its application, are sometimes as accurately determined as a fall or a blow.

In proportion as diseases are more chronic, they are, upon the whole, less easily assigned to their external exciting causes. With respect to them, a longer experience and inquiry are needed to establish a uniform sequence of the events. If you are attentive observers in the wards of this hospital, it will require a brief period only to convince you that the sudden impression of cold is among the undoubted causes of acute inflammation in various vital organs. But you cannot know that the habitual indulgence in spirituous liquors is undoubtedly productive of congestion of the liver until after years of experience. For the present, you must be content to take this fact upon trust from the testimony of others.

The question of malaria, of contagion, and of animal poisons, the certainty of their influence as causes of disease, and the mode, the sphere, and condition of their operation, are all questions of the highest importance, and involving facts of great interest. But I would advise you not to meddle with these questions at present; for *at present* you can only do it *speculatively*: therefore it is better that you should wait until a sufficient number of facts have fallen under your notice, either to enable you to form some conclusions of your own upon these subjects, or to have a sound judgment of the conclusions which have been formed by others.

With respect to the influence of external agents as remedies, I would recommend you to be most jealously observant of every circumstance connected with the treatment of individual cases. A *mere sequence* of events is not a *necessary* sequence. The remedy may be administered, and the disease may cease; and yet the treatment and the cure may not be cause and effect.

The remedy may have been really *inert*, and the spontaneous powers of reparation in the constitution or in the part may have been enough to surmount the disease; or the remedy may have been *active* in the wrong direction, and the powers of reparation more than enough to surmount the disease,—enough both to surmount the disease and to render the remedy harmless.

Your only safeguard against such deception lies in the most jealous and scrupulous observation at the bedside. Turn your attention, first of all, to well-marked instances of disease which is acute and rapidly progressive, where the remedies must be of equal force with the disease, and must operate with equal rapidity, and from which, if a curative impression follows, it must be a *sensible* impression. Watch the treatment of inflammation in various vital organs by the different modes of bleeding, by calomel, by tartar emetic, by colchicum; and the treatment of certain convulsive and painful disorders, and certain forms of delirium, by opium.

Beware of mistaking the nature of the disease, and then believing that the remedy has cured what in fact never existed. I lately read a book in which a certain remedy was recommended as infallible for diseases by dozens; tic douloureux, ovarian dropsy, disorganization of the heart, paralysis, and I don't know what besides, were all cured by it. Of these diseases no description was given, but only the name; so that there was not the slightest evidence that any such diseases existed as those that were said to be cured.

In like manner there have been remedies for cancer, remedies for consumption, remedies for stone, all owing their reputation to the nature of the disease being mistaken.

This is the field in which ignorance and imposture reap their golden harvests. Not that it is impossible for those of good intention and good information to mistake the character of a disease, and so ascribe an efficacy to a remedy which is not its own; but those have the best security against this error who have taken the most pains to acquire a habit of faithful and jealous observation.

But there is an opposite infirmity not uncommon among medical men, which is just as much to be deprecated as the easy credulity of which we have been speaking,—a scepticism in regard to the influence of medicine—a stubborn reluctance to admit the relation of cause and effect between remedy and cure. Surely it is not at all less hurtful to take up a notion that a number of diseases, from their nature or seat, are beyond the reach of all remedies, than to believe that any or every disease that gets well after the use of any remedy is necessarily cured by it. In the one case things are linked together as cause and

effect, which bear no such relation; and in the other, things which really bear that relation are perversely dissevered.

Many men pride themselves upon this vicious scepticism, and wish to be thought to exercise a philosophical caution. Voltaire, who was upon the watch for every ludicrous infirmity of human character, was sharp enough to discern this in physicians, and has made excellent sport of it.

He makes a physician of renown come from Memphis to cure Zadig of a wound in his *left* eye. The physician, however, affirms it to be incurable, and predicts the very day on which Zadig is to lose his sight, regretting at the same time that the accident had not befallen the *right* instead of the left eye, for *then* he would have promised his cure; wounds of the *left* eye being in their very nature irremediable. But Zadig gets well; and the physician writes a book to prove that he ought nevertheless to have lost his sight.

But much better than the fiction of Voltaire, and very much more to the purpose, is what really happened, and stands authenticated in a Scotch law book. Three physicians and two surgeons made oath in a court of justice, that, "by the rules of their prognostics," the wounds received by one James Houston were mortal wounds. But James Houston was still alive; and, to the honour of the three physicians, the two surgeons, and "the rules of their prognostics," was plaintiff in the very process wherein they had so memorably deponed.*

With respect to symptoms and their connexion with morbid processes going on within, the subject is too extensive to allow me to enter upon it at present. Indeed, it is so extensive and so important, and so full of its own difficulties, and yet a subject wherein right views are so expedient, and wrong views so perilous, that I have thought it would not be altogether a profitless task either to you or to me, if we were to undertake together a patient and comprehensive examination of this very subject. Therefore, in connexion with the proper business of the wards, and the observation of cases, and (as I trust) for your help and my own, and for our mutual guidance at every step of our practical studies, I will endeavour, in a series of lectures, to give what illustration I am able to the doctrine of symptoms.

* Edin. Med. and Surg. Journal, vol. i. p. 339.

It remains that we notice another, and (philosophically considered) a higher purpose, which concerns us in the contemplation of medical facts—viz. the discovery of general principles from them.

A principle (as the name imports) is a beginning. A principle is some matter of fact to which numerous other matters of fact are traced as to a common source; and when we speak of discovering a principle, we mean the business of analyzing or decomposing compound matters of fact, into those which are simpler, until we come to one which is simpler still, and more general and elementary; and, being unable to go beyond it, we regard this as an ultimate fact, or a principle. A principle is an ultimate fact, and a universal fact, and true, without a single contradictory instance.

Now it must be confessed that there is no fact in medicine (*i.e.* no fact respecting the animal body, its actions upon itself, or its obedience to other influences) which has the same character of universality with certain facts respecting the external world. In medicine we have no fact so universal as that all bodies unsupported fall to the ground, no principle so sure or irrefragable as gravitation.

Nevertheless, in medicine we talk of principles, and we are continually striving after them; but in the strict philosophical sense, have we ever really compassed them?

We have reached *forms* of principles (if I may so say) rather than principles themselves. By *forms* of principles I do not mean things fictitious, or things purely imaginary, but facts tested by observation, and carefully analyzed, and very comprehensive, but not universal: facts true in a vast number of instances, but not true in all.

In medicine there have, indeed, been facts, which for a time have passed for universal, and for a time have held the place of principles; but larger experience has shown that their title to it was not a just one.

There is a certain order of symptoms constituting what is called "angina pectoris;" and angina pectoris was for years, by the common consent of medical men, drawn from extensive observation and dissection, universally ascribed to ossification of the coronary arteries of the heart. But more enlarged experience has found angina pectoris to exist where there has been no

such change of structure, but another form of disease, viz. dilatation of the origin of the aorta; and *still more* enlarged experience has found it where there has been neither one nor the other.

The disease produced by the vaccine virus gave proof, by instances almost innumerable, of imparting to the constitution a protective power against small-pox. Accordingly, this power was believed to be absolute and universal. At length contradictory instances arose and multiplied; and the protective power of vaccination was now no longer a law or a principle. In the present state of our experience we still ascribe to vaccination a protective power against small-pox, but one which is *only highly probable*, not certain. We ascribe to it, moreover, when its protective power fails, a mitigating power; but this, too, is only highly probable, not certain; for assuredly both its protective and its mitigating influence sometimes fails, and small-pox after vaccination sometimes goes through all its stages *unaltered*, and is in all respects the *same small-pox*, as when no vaccination has taken place.

After the history of vaccination we know not what number of concurrent instances is enough in medicine to prove a fact universal, or when we can ever be safe against the intervention of contradictory instances, and venture to rely upon any fact as a doctrine or a principle.

But we are not, therefore, to abandon our search after principles; and the same method which in other departments of natural knowledge has alone led to their discovery, we must still employ in ours; for although in our hands a less eminent success has hitherto attended this method, no success whatever has attended any other. This method imperatively requires that the principle sought be a matter of fact.

If, while we properly restrict ourselves to matters of fact in every other stage of our investigations we yet take a fiction for our principle, medicine will never improve as a science in our hands, and ancient errors and follies will only give way to new ones.

The fault of physicians has not so much been, that they have shown a general disregard of matters of fact, as that they have lost sight of them just when they ought especially to have kept them in view—when they were concerned with principles.

Hence the mischiefs that have arisen to physic in the shape of so many renowned theories, either shown to be false, or not shown to be true. Take any of these false or unverified theories you please, and you will always find it derived from some principle gratuitously assumed, a principle which is either no matter of fact at all, or incapable of being shown to be a matter of fact.

The principle assumed may be some physical process or property, such as a spasm of the extreme vessels, or a peccant matter in the blood; things which possibly may be, but which are entirely without proof, and even too subtle to admit of any; possible facts, but facts quite unascertained and gratuitous.

Or the principle may be neither process nor property, nor anything that has a physical existence, real or possible, but a mere figment of the mind. The Zoonomia of Dr. Darwin abounds in principles of this kind. Even Mr. Hunter, with all his wariness and penetration in search of truth, admits what he calls "the stimulus of necessity," as a principle or element engaged in the production of diseases before they are yet apparent in their phenomena. This is to escape from physical inquiry into the region of pure fancy.

What has been said of principles in medicine, whether erroneously or legitimately pursued, may seem to offer small encouragement to physicians to engage further in the search after them. Many of the most celebrated that have given renown to schools and universities have been abandoned altogether —abandoned because they have been erroneously pursued, and have not possessed the essential character of matters of fact. Many, again, being real matters of fact, and legitimately sought and accepted as principles for a time, have at length been abandoned, because they have been found not to possess the essential character of universality.

But we must still concern ourselves with principles; we cannot help it; all men do it in some sort or other; for the mind is not able constantly to keep in view all the particulars of its own experience. It must needs reduce them within a narrower compass, and contemplate them (so to speak) in some representative. Thus a law or a principle must be set up, right or wrong. Some forge a maxim, and some forge a fact, and soon find it conspicuously illustrated in every instance of disease

they meet with. They find *irritation* in everything, or *spasm* in everything, or *bile* in everything. While others, after having gone on observing and collecting facts, and cautiously arranging them according to their natural relations, venture at length to rest in one which seems to have every characteristic of a principle, and yet in process of time may turn out to be no principle at all: witness the protective power of vaccination.

The truth is, something must be conceded to physicians in respect of the very nature of the subject on which they are engaged. Let the principle be ever so legitimately reached, we are only answerable for it as a law explanatory of the facts *already known*. But in medicine new facts are continually presenting themselves. These may be still comprehended within the same principle, or they may not. If they are, they furnish a stronger attestation to its truth. If they are not, they weaken or destroy the principle altogether. But it is no disparagement to us that our principle has failed. We are only concerned that the method by which we reached it is the right one; and then, though it fail, we are at liberty to arrive at a new principle, if we are able, by the same method; that is, to find some other matter of fact comprehensive of the newly discovered particulars, and to concede to it the character of a principle.

Such is the nature of medicine, that things which we have laid up in our minds as settled truths often require to be modified by our future experience, and come at last to be rated many degrees below the value at which we originally prized them.

Nevertheless, we do not claim for medicine a liberty to transgress any of those landmarks which philosophy has set up to indicate the path of truth. Let it have no other principles, and no other method of arriving at them, but such as philosophy approves; only let no disparagement fall upon it (considering the nature of the things with which it is conversant), if, for just reasons, it be sometimes dissatisfied with principles which it once embraced, and seek to discover new ones.

Finally, then, as to general principles in medicine, let it be remembered that the mind must always seek to arrive at a matter of fact, and *there* only be content to take its rest. But it need not settle there longer than until the clear discernment of some other fact, more general and elementary, opens the way to a safe progression beyond it. Then the fact last discovered becomes

the principle, and the other is only one of the several stages conducting to it.

In this mode of proceeding our knowledge may be *incomplete*, but it is never erroneous. The mind advances from fact to fact, resting on one as the stepping-stone to another, and feeling safe in the possession of the truth, although it may not be *all the truth* that is capable of being ascertained.

Every fact from which another fact is derived is in some sort a principle. To us it is a *first* principle as long as we are obliged to rest in it; but as soon as another fact is discovered which is prior to it, it loses its character of a *first* principle; and, if it be a principle at all, it is only an *intermediate* one, the first being always that to which we know nothing prior.

LECTURE VI.

ON THE DOCTRINE OF SYMPTOMS.

GENERAL NOTION OF SYMPTOMS.—HOW THEY DIFFER FROM MERE SIGNS.—THE RELATION OF SYMPTOMS TO DISEASES NOT THE SAME IN ALL CASES.—SYMPTOMS ARE DIRECT OR INDIRECT.—CHARACTER OF EACH.

DIRECT SYMPTOMS RESPECT THE SENSATIONS, FUNCTIONS, AND STRUCTURE OF THE PART AFFECTED:—

1. SYMPTOMS WHICH RESPECT SENSATION.—PAIN—ITS DEGREES—ITS QUALITIES.—AMOUNT OF INFORMATION DERIVED FROM PAIN AS A SYMPTOM.—SOURCES OF DECEPTION ARISING FROM IT.

2. SYMPTOMS WHICH RESPECT FUNCTION.—AMOUNT OF INFORMATION DERIVED FROM THEM, AS COMPARED WITH THAT DERIVED FROM SENSATION.—AMOUNT FROM BOTH TAKEN TOGETHER.

3. SYMPTOMS WHICH RESPECT STRUCTURE.—THE INFORMATION DERIVED FROM THEM LIMITED TO PARTS WITHIN REACH OF THE SIGHT AND THE TOUCH, UNTIL AUSCULTATION BROUGHT THE DISEASES OF CERTAIN ORGANS WITHIN THE SCRUTINY OF THE EAR.

In going round the hospital my mind often reverts to the time when I was a mere beginner like yourselves; and I remember how strange and puzzling to me was everything that I saw; how I thought I never should be able to distinguish diseases, one from another, as long as I lived; and, as to treating them, I could not look forward with the hope that my conscience would ever allow me to attempt any such thing.

Above all, I was perplexed with the number and variety, and (as I humbly thought) contradictory nature of symptoms. It seemed to me, that if I could ever succeed in learning them

all, it would be to no profit; for the same symptoms appeared sometimes to import one thing, and sometimes another.

There was a patient, perhaps, suffering convulsions; and the physician evidently thought the case most grave and perilous, for he employed several remedies of the most gigantic power, and succeeded in saving him. But there was another patient suffering convulsions no less severe, and to my apprehension just of the same kind, yet so far was the physician from thinking seriously of this case, or treating it severely, that he just looked at the patient and smiled, ordered some cold water to be thrown in her face whenever the convulsions returned, and said that would cure her; and, sure enough, he was right. Moreover, many died who seemed to me to have little or nothing the matter with them, and many recovered whom I did not hesitate to condemn to death at first sight.

Thus health and sickness, and life and death, seemed the most mysterious things in the world; and the symptoms which were said to indicate them were to me a long while unintelligible.

These recollections, at this day so often present to my mind, enable me to place myself in your situation. They serve the good purpose of making me feel, that just the same difficulties which are yours now were once my own, and of making me wish to aid you by my little experience in removing them.

Yet the very objects which have puzzled me, and are, perhaps, now puzzling you, do in fact contain infinite instruction. It is by symptoms, and by symptoms only, that we can learn the existence, and seat, and nature, of diseases in the living body, or can direct and methodize their treatment. But, first of all, symptoms themselves must be understood, before we can make the proper use of them for gaining the instruction which they are calculated to convey.

It is important for us to understand that the symptoms or signs of diseases are never to be taken in the like sense with that in which the signs of external things are often regarded. The buoy which, floating in the river, marks its navigable tract; the bell which, by striking, denotes the lapse of time; the stone by the wayside, which tells us how far we have come, and how far we have yet to go; these are most important *signs*, and of indispensable service to us all, but they have no *natural* connexion with the things they are made to indicate. They are

mere expedients, of conventional meaning and use. Navigable rivers, and time and space, would still exist, though there were neither buoy, nor bell, nor milestone, in the universe.

There is nothing that we call the symptom of a disease, which does not contain within itself much more than a mere sign. Heat, pain, redness, swelling, are called the signs of inflammation; but nature does not intend by them barely to intimate that inflammation exists; they are essentially connected with the processes she is carrying on.

Thus at early dawn we point to the first glimmering in the east, and call it a *sign* of the rising sun; but it is more,—it is an emanation from his beams. We look at the cloud above our heads, and say it is a *sign* of rain; but it is the gathering of the waters themselves.

Concerning symptoms I would nevertheless remark (what is very important to be borne in mind), that they stand in different relations to the diseases to which they belong. They may flow out of the disease, so as, in idea at least, to be separable from it; or they may be involved in the disease, so as to be identical with it. The difficult respiration, the cough, the sputa, the emaciation, the hectic, are the symptoms of phthisis, and are distinguishable from the disease itself. They are the signs of something beyond themselves, which we do not see—viz. tubercles of the lungs. But the symptoms which denote an intermittent fever are the same which constitute the disease. We have no idea of an intermittent apart from the rigor, the heat, and the perspiration. The same may be said of other fevers, and of almost all diseases not organic, in which, if you seek to separate the symptoms from the disease, you must resort to theory for the purpose, and conceive an action of a certain kind prior to the *actions* which constitute the symptoms, and productive of them.

There are then symptoms which, in the plain and intelligible sense, are signs and tokens of the disease that exists separately and distinctly from them; and there are symptoms which, however they may be spoken of as signs merely, are, nevertheless, all *that we know* of the disease itself. The disease is the symptoms; and the symptoms are the disease.

True: but it can hardly be conceived that they are in reality the same. Yet it is better that they should be so regarded,

than that we should go beyond our knowledge in attempting to distinguish them. Better to see in fevers only a certain combination of symptoms, than to run wild about a debility of the nerves, a spasm of the extreme vessels, or a peccant matter in the blood.

Again; I would remark that it is often most difficult to draw the line between what is disease, and what is symptom; and that the same conditions may, in different cases, become now one and now the other. A dropsy, or an hæmorrhage, are sometimes primarily and essentially *the disease*. Sometimes they are secondary, and incidental to *the real disease*, and are themselves only symptoms.

How impossible, then, must it be to give any definition of a symptom which shall be *philosophically* true, and at the same time satisfy every sense in which it is practically regarded!

From the imperfection of our knowledge the whole subject of semeiology is beset with philosophical difficulties; and no advantage will be gained from conducting our inquiry concerning it in a stricter method than its own nature will bear.

Whatever it is that bespeaks the presence of diseases, or denotes their nature or their seat, or, moreover, whatever indicates the proper method of treating them, may be equally regarded in the character of a symptom. At present I shall restrict myself to the symptoms which denote the disease, reserving those which indicate the remedy for future consideration.

There are certain popular tokens of disease which all the world is acquainted with. A man is generally known by his friends to be ill before he submits his case to a physician. They judge, and judge rightly, from his complexion, his aspect, his voice, his gait, or something unusual and unsatisfactory in his whole form and behaviour. And these circumstances, which strike everybody, are not unobserved by us. All can gather from them that disease is present somewhere; but *we* can often draw from them intimations of its very nature and seat.

But how much soever physicians may learn from what constitutes the physiognomy of diseases in its largest sense (and, indeed, they may learn a great deal), their more accurate knowledge is derived from symptoms which admit of a more exact analysis. Upon such I propose principally to dwell.

Of symptoms some belong immediately to the part affected. They proceed immediately from it, and are referred immediately to it. We will call these *direct symptoms*. Others, belonging originally to the part affected, declare themselves through the medium of other parts, or through the medium of the constitution at large. We will call these *indirect symptoms*.

To begin with direct symptoms, there are those which respect the sensations of the part, in whatever way they may differ from what is natural.

Now, concerning unnatural sensations as evidences of disease, to what extent they exist, or whether they exist at all, we are not competent absolutely to determine. In matters of feeling we must depend entirely upon what our patient tells us. Every man smarts with his own pain; himself, and nobody else, can say how much. We must presume, therefore, that our patient has no disposition to deceive us; and, giving him credit for the correct expression of his own feelings, we must act upon his information concerning them.

Pain and uneasiness! These are general terms. But there is more to be learnt upon the subject of morbid sensations than what these terms are calculated to convey.

There are qualities and peculiarities of pain arising from parts which are disordered, diseased, or injured, as there are qualities and peculiarities of sensations arising from parts which are healthy.

Ask the man whose leg has just been amputated, and he will tell you that he suffered one kind of pain when the knife divided the skin, and another when it cut through the muscles; and that sawing through the bone gave him still a different pain. Ask the man who has just suffered the operation of a violent purgative, and he will tell you, that after taking it he first felt oppression and nausea; that presently the nausea became fainter and fainter, until it was exchanged for real pain; twinges and griping arose, and became more and more acute, until they were relieved by an evacuation from the bowels.

Here we see various peculiarities of uneasiness arising from the same mode of irritation applied in succession to the different parts of the intestinal canal, and the different structures which go to the formation of the leg.

Pain in vital parts is different from common pain; and the

pain in one vital part is different from the pain in another. In the brain it is heavy and stupefying; in the heart and lungs, and contiguous structures, it is apt to be acute, and generally much circumscribed, or confined to a spot; in the liver, the uterus, and testicles, it is oppressive and sickening.

Pain varies also in parts not vital. In skin, cellular structure, and muscle, it rouses and excites; in tendon, ligament, and bone, it rather oppresses; in nerve, it is numbing, prickling, or intolerably acute, and often runs along a string (as it were) to a considerable distance. Witness tic douloureux; witness the sciatic affection.

But, besides the qualities and peculiarities of pain belonging to several parts, and denoting *generally* their unhealthy condition, there are those which belong to the same parts under different states of disease, and are thus expressive of *the kind* of morbid action which produces them. There is what is called throbbing pain, in which the patient, simply by attending to the part affected, may count his own pulse. This throbbing pain is characteristic of inflammation just at the point when it is passing from the adhesive to the suppurative stage. There is a pain which is called lancinating, almost the constant concomitant of cancer, and very different from the pain which would attend common inflammation in the same parts of the body.

The sense of pain is in proportion to the magnitude of the disease, only within certain limits. The extremity of the disease may abate or even abolish the sense of pain altogether. Thus, there are circumstances in which it is in vain to seek to learn the existence of pain by interrogation merely: the sense of it must be awakened by hard pressure or rough handling of the part in which the disease is thought to reside; and sometimes even these are insufficient for the purpose. The sense of pain is irrecoverably gone. Surely there is a benevolent intention conspicuous in all this. The way of death is often smoother than the path of life; and great bodily anguish (there is reason to believe) does not often enter largely into the process of dissolution.

But I do not wish to insist upon pain, its quantity, and peculiarities, as infallible criteria by which to detect what is the seat of the disease, and what its kind. It is possible to speculate too curiously upon morbid sensations; to speculate

even so far as to deceive ourselves respecting them. Besides, the patient himself, by an over anxious and over constant attention to what he feels, is liable to miscalculate the kind and quantity of his own sufferings, and thus to mislead you by exaggerating every little ache into an intolerable grievance. Again; the patient himself, by habitual disregard of what he feels, is liable to miscalculate in the opposite way. He either has no pain, or he owns to none, where another would complain of a great deal; and thus he misleads you by extenuating a real grievance, or entirely passing it by. Neither of these can be safely trusted for a correct interpretation of their own sensations.

It is an important practical truth, well worthy of being remembered, that diagnosis is capable of being greatly aided or greatly obstructed by the *personal character* of the patient. Education, and the better habits of civilized life, render men more rationally attentive to their internal sensations and better able to describe them; whereas over refinement engenders such excessive care and regard of the feelings, that it contrives to sophisticate and spoil them; and barbarity acts so much in spite of them, that it blunts or nearly abolishes them altogether.

Plain sensible men, who feel just what they ought to do, and tell just what they feel, are the most agreeable patients to attend. You make out their complaints easily and satisfactorily; they have noticed the first *real* deviations from healthy sensation, and can describe them intelligibly; and they obtain from you an earlier and more certain relief. But soft, delicate, nervous persons, who feel extravagantly, and still exaggerate what they feel, are very troublesome to deal with. You are not certain that they do not deceive both you and themselves; and such a perplexity is cast over their complaints that you can neither understand them nor treat them properly. Again; the stupid and half civilized, who are often literally *insensible* to their disease until it has endured a long time, and made a considerable progress, and done formidable injury to the parts concerned, can hardly give you any help to the knowledge of their complaints by their own description of their feelings.

I have often remarked, in the victims of extreme intemperance, that they have little or no consciousness of the pains

and disordered sensations proper to the diseases which they suffer. This strange want of correspondence between the symptoms of disease derived from other sources, and those derived from the sensations, is a subject of very curious speculation medically, and of very melancholy interest morally. For the chief cause of the anomaly I believe to be really that to which I have alluded. There are whole classes of society in London who are never really sober for years together. The sensations proper to health and to disease are alike unknown to them. In health, the stimulus of spirits renewed day by day and hour by hour, gives them feelings and excitement which are unnatural; and however they may be mistaken for those of health, do in truth not at all belong to it. They are better, perhaps, and more pleasurable, than any that health has to give; and they have superseded them. In disease (disease which it has itself produced), the stimulus of spirits gives them feelings and excitement which are still unnatural, and disguise or supersede the sensations which they then ought to have.

People are frequently brought into the hospital just ready to perish of complicated visceral disease, yet declaring that they never suffered ache or pain in their lives until a few weeks ago. Their liver, spleen, kidney, and heart, and blood-vessels are all disorganized. They are breathing, perhaps, with one lung; and the cellular structure and some cavities of the body are distended with fluid. Here is a disease which must have been the growth of years; yet true it is, as they say, that they have felt neither ache nor pain until within a few weeks. Spirits—spirits more and more recklessly taken—have sustained and excited, and cheated them, with false strength and false feelings, till fluid has gushed out everywhere, and vital organs have been suddenly oppressed, and down they have sunk at once, and irretrievably.

Nevertheless, the inquiry into morbid sensations is most interesting in itself, and most proper and necessary to be pursued for pathological and practical purposes.

There are complaints of sensation, and sensation merely. People feel burning heat and pinching cold, in opposition to the indications of the thermometer, and in opposition to the perceptions of the physician, who applies his hand to the person of his patient without being able to confirm the fact by his own

feeling. People will complain of severe pain upon some external surface, which exhibits no visible marks of disease.

These complaints of sensation, and sensation merely, often occur in those whom you cannot suspect of any intention to deceive you. They are often real diseases, and being such, are generally most difficult to cure. But these complaints of sensation, and sensation merely, are those which people most frequently counterfeit when they have an interest in being believed to be ill; and they often counterfeit them successfully, owing to the extreme difficulty of detecting the deceit. Yet even here the physician would make the probability of deception less, if he were acquainted with the kind of pain which is usually felt in that part to which the patient refers it.

But, although we may learn from this order of symptoms all that they are in their own nature capable of teaching us, yet, in almost every case we meet with, we shall find a necessity of inquiring into other symptoms, if we would know the real condition of the part which falls under suspicion of disease. The majority of complaints are not such as declare themselves by *this* or by any one order of symptoms only. Diseases of *mere* sensation are very few.

Now there are other direct symptoms (symptoms immediately referable to the part affected) besides those which respect its sensations; and these are in truth more important, on account of the more certain information which they are calculated to convey. These other direct symptoms respect the functions of the part.

If the patient own to pain in a part, we suspect that part to be the seat of some morbid affection; but we are not satisfied that it is so, nor can we tell what the morbid affection is, until we have made further inquiry.

Suppose a man complains of pain in the head. It may be a mere nervous pain; it may be a sick headache; or it may be a symptom of inflammation of the brain. But we cannot tell what it is, and (what is worse) we cannot prescribe with any reasonable chance of procuring relief, until we have ascertained many more particulars concerning it.

Or if the patient own to no pain, yet, if a part has fallen under a suspicion of disease, we cannot be satisfied that it is healthy until we have made other inquiries. I have known

people die of diseases of the brain, of the lungs, of the heart, who have suffered no pain whatever.

It appears, then, that whether we learn much or little, or nothing, respecting the sensations of a part, there is always a necessity for further inquiry, if we would know the nature of its complaint.

Our further inquiry is still into direct symptoms, viz. those which respect the functions of the part.

The symptoms which respect function are of much more practical value than those which respect sensation; and for this consideration especially, that the knowledge which they convey is less fallible in itself, being the result, not of what we ask and another tells us, but of what we see and note for ourselves. In obtaining it, we depend not at all upon the representations of the patient, but entirely upon our own observation and reasoning.

But, concerning the direct symptoms which respect the functions of parts, and which consist in the various deviations of those functions from their healthy state, let this especially be borne in mind, that valuable as they are themselves, to *us* they will be of much, or little, or no use, according as we take much, or little, or no care, to prepare and capacitate ourselves for understanding them. *Everybody* cannot tell when and how the functions deviate from what is natural. A competent acquaintance with physiology must precede and prepare us for such knowledge. We must begin with what is natural and healthy, and afterwards inquire into what is unhealthy and disordered; and thus learn the latter by comparing and contrasting it with the former.

As the anatomy of healthy structure must always be the beginning and foundation of morbid anatomy, so must the physiology of healthy function be always the beginning and foundation of morbid physiology; for by this name of morbid physiology I will venture to call the knowledge of all the various ways in which the functions of the living body and its several parts are capable of being perverted and deranged.

Some interruption or derangement of their ordinary functions probably always attends disease or injury in every part and structure of the body; such interruptious or derangements, being discovered, are the symptoms which bring us home to the seat of complaint more surely than any other.

But there are parts in which they are not discoverable; namely, those of which the ordinary functions are unknown, as the spleen. And there are parts in which I will not say that they are not discoverable, but only that they are never discovered—namely, those whose functions are so mixed and blended with the functions of other parts, that it is impossible to determine how much belongs to them and how much not, either in health or in disease. Who shall say when the pancreatic secretion is redundant or defective, or of an unhealthy quality?

But the brain and the nerves; the heart and the blood-vessels; the lungs, the liver, the stomach, and the kidneys; all give direct intimations of their diseases, by the interruptions and derangements of their ordinary functions.

The brain and nerves exhibit direct symptoms of their diseases in every manner and every degree in which sensation or voluntary motion, the senses or the intellect, are capable of being impaired or perverted. The heart and blood-vessels exhibit direct symptoms of theirs, in the strength or weakness, the unusual extent, and the irregular succession of their pulsations, and the sounds accompanying these; also in many qualities and varieties of the pulse, and in the unequal course and distribution of the blood itself.

All are acquainted with the direct symptoms which impute diseases to the lungs, when, instead of a respiration which should be easy and uninterrupted, there is panting and wheezing, and stertor, and cough; and when, instead of the humid vapour which in health is separated by the bronchi, and mingles and glides forth with the breath, there is a hard and difficult expectoration of phlegm, of mucous or purulent secretion, or of blood.

But the direct symptoms which impute diseases to the lungs most unequivocally, and which make the most precise discovery of their nature and seat, are not of common or popular apprehension. There is a method by which the entire lungs, and each separate portion of them, can be scrutinized, and by which we can learn where respiration is perfect, and where it labours. It is the method of auscultation. And this method of auscultation does not merely discover a defect or failure of function in the lungs at this part or that, and so leave us to infer, from reasoning or from other circumstances, the exact nature of the disease

(this indeed, would be a great deal, and as much as the direct symptoms which respect the functions of parts are generally able to do); but it often leads to more—discovering not merely the symptom, but the disease itself.

The excess and defect of bile, and various qualities of that secretion different from those of health, are the *direct* symptoms by which the liver shows itself morbidly affected. And the common consequences that immediately flow from impediments to the digestive function are the direct symptoms by which the stomach declares its complaint. Such are distensions, eructations, and rejected food, which has undergone the process of fermentation, and become acid and putrid: for these plainly show that the substances submitted to the stomach have been left to suffer the chemical changes to which they are naturally obnoxious, the organ having lost its controlling power over them.

So, too, in the various changes which the urine is apt to suffer, in its excess and its defect, in the predominance of an acid or an alkaline quality, in its amorphous sediments and its crystalline deposits, we have the direct symptoms, which lead us to search for disease in the kidneys.

But each of these systems and organs requires from the student an express acquaintance with its natural functions, before he can be prepared to examine and appreciate their errors and defects; and then these errors and defects themselves he must expressly study, before they can yield him all the information which, as direct symptoms of disease, they are calculated to convey.

Now, whatever part be affected, when we bring our own knowledge of its disordered functions, and add it to what the patient tells us of its disordered sensations, we shall generally come somewhat nearer to a right notion of the seat and nature of the complaint; sometimes, indeed, to a *perfect* comprehension of it, so far as it is capable of being ascertained at all; inasmuch as there *are* conditions of disease into which no further inquisition can be made when we have learned the feelings and the functions of the part to which they belong.

Many of the local complaints which we are called upon to witness and to treat, are *not* of a nature to affect the structure of the part. In them it is not clear that there is anything to be ascertained beyond the symptoms which respect its sensations

and functions; whether, if the part were laid bare to us, and we could see and handle it, we should have any better notion of its complaint, or how to treat it. A man has a pain in his stomach, and he cannot digest, and yet he has no organic disease; and this being the case, I really do not comprehend how we are more likely to learn the cause of the pain and the indigestion, or the proper method of its relief, seeing the stomach, than seeing it not.

But with all the knowledge of disordered functions which our best observation can furnish us, and all the knowledge of disordered sensations which the patient's faithful interpretation of his own feelings can supply; with all the light which sensation and function can, as direct symptoms, throw upon the disease of the part affected; still we often need *other* symptoms and *more* light to inform us what the disease really is, and what its treatment should be.

There is yet another order of direct symptoms besides those which *immediately* respect the sensations and functions of the part—viz. those which *immediately* respect its structure.

When parts are within reach of the sight or the touch, we can often judge whether their forms and structure be different from what they ought to be, and thus obtain direct evidence of their disease: and this evidence may be all that we require. What we see, or what we feel, may convey to us a complete knowledge of the disease, and render all consideration of other symptoms unnecessary. Sometimes these visible or tangible deviations from healthy structure are not only the direct symptoms expressive of the disease, but the disease itself.

In the morbid affections of external parts we can examine at once their state and structure, and learn in what respect they differ from what is natural; and the observation of this *direct symptom* almost supersedes the necessity of inquiry for any other; for herein we witness the very manner and process of the disease itself. We see the increased vascularity and tumescence which constitute inflammation, the lymph which is the material of adhesion, and the fluid which is the essential product of suppuration. We see all those sensible changes in the condition of the skin and cuticle which constitute the many orders and varieties of cutaneous diseases—rashes and vesicles, and pustules and scales; in which it is obviously impossible to

distinguish the symptom from the disease, or the disease from the symptom.

Again: we learn by the touch that the os uteri is changed in structure—that it is scirrhous, or that it is ulcerated, or that a polypus passes through it or grows from it; and here we have not only the palpable evidence of the diseases, but the diseases themselves.

But our business, as physicians, is chiefly with internal parts and organs; all of which are beyond the reach of our sight, and but a few perceptible to the touch; and these few only under certain conditions of disease.

Some organs of the abdomen, when disease has produced an increase of their natural bulk, become palpable through the integuments, and allow an examination of their shape and dimensions. But, after we have in this manner obtained such information as is possible respecting the structural condition of an internal organ, let us be careful to estimate it properly, and not to value it for either more or less than it is worth.

To ascertain by the touch that certain organs within the cavity of the abdomen have undergone an augmentation of their natural bulk—that the liver, or the spleen, occupy a space far exceeding that which nature has allotted them, beneath the cartilages of the ribs on either side—is, without doubt, to fix disease upon them. But it is not to determine the nature of the morbid processes which either are or have been in action within them. Finding the spleen enlarged, or the liver enlarged, we have the palpable result of some morbid action; but what that morbid action has been, and whether it is still in progress (the only questions which are pathologically or practically important), we must seek to discover by other symptoms. The increased bulk, then, of an organ, is a symptom, a direct symptom, of great value and certainty in fixing the *seat* of disease, but nothing more.

But it is by another sense that we are admitted to a much more intimate scrutiny of an internal part than any which the mere touch can afford; and thus obtain *direct symptoms* of its disease which are often as infallible as those derived from sight itself.

By auscultation we not only become acquainted with the remote effects or ultimate results of morbid action, but often,

when the disease is just doing its first rudiment of injury, the secret of its proceeding is betrayed to the ear.

The ear not only discovers the vomica or cavity, which is the last of many changes wrought by tubercular disease upon the structure of the lungs; not only finds their permeable texture converted into a solid mass by the gradual deposition (it may be) of tubercular matter, or (it may be) of inflammatory lymph; but it can detect the first effusion produced by inflammation into the vesicles of a single lobule: it can detect pneumonia almost, perhaps altogether, simultaneously with its beginning to exist.

LECTURE VII.

ON THE DOCTRINE OF SYMPTOMS.

DIRECT SYMPTOMS OF DISEASES AFFECTING THE STRUCTURE OF THE THORACIC ORGANS, KNOWN BY MEANS OF AUSCULTATION.—MODES OF AUSCULTATION.—PRELIMINARY ACQUAINTANCE WITH MORBID PROCESSES ESSENTIAL TO ITS SUCCESSFUL USE.—PRECISE NATURE OF ITS OBJECTS IN RESPECT TO DISEASES OF THE LUNGS.—GENERAL DIRECTIONS FOR ITS USE.—AUSCULTATORY SIGNS OF HEALTHY LUNGS.—AUSCULTATORY SIGNS OF DISEASED LUNGS—KNOWN IN THE ACTS OF BREATHING, SPEAKING, AND COUGHING.—THESE SIGNS CONSIST IN DRY SOUNDS AND MOIST SOUNDS.

RECOLLECT for a moment the inquiry in which we were engaged, and how far it has proceeded. It is an inquiry into the nature of symptoms; and hitherto we have spoken of symptoms *directly* referable to the part affected; those which regard its sensations, and those which regard its functions; and we have endeavoured to estimate the information which these are respectively calculated to convey. We have spoken also of those which directly regard its structure; and are now considering the information to be gathered from them.

This information, it should seem, is necessarily very limited in regard to internal organs in general; but the thoracic organs are excepted from the rest, because, being within the cognizance of another sense, and thus subjected to a method of investigation peculiarly applicable to themselves, all their actions and conditions are more clearly perceived and known. This is the method of auscultation.

There are different modes of performing auscultation. In one mode we apply the ear itself to the surface of the chest: this is (what is called) immediate auscultation. In another, we apply the tube to the chest, and the ear to the tube: this is mediate auscultation. In another, without applying the ear to

the chest either immediately or mediately, we strike its walls with our fingers, and listen to the sounds which result: this might be properly called auscultation by percussion. But percussion and auscultation are often spoken of, as if they were different things, whereas they are only different modes of appealing to the same sense; for we gather our information equally from what we hear, whether we strike the chest or apply our ear to it, or use the instrument.

I have often taken occasion to point out to you the importance of pathological knowledge to the just diagnosis and the successful treatment of disease; and, as a part of pathology, I have laid especial stress upon the knowledge of morbid processes. Now the use which you will or will not be able to make of auscultation, will depend upon your knowledge of the pathology of those organs to which it is applied.

The sounds which reach the ear through the walls of the chest during breathing, or speaking, or coughing, varied and modified by divers diseases of the organs within, are easily discriminated. Any person not deaf will soon learn that there is some distinction of these sounds. But we may distinguish them correctly, and call them by right names, and make a musical scale of them, if we please, and still know nothing of the morbid conditions which they indicate, and out of which they arise. These cannot be discovered by a discriminating ear *only*; they must first be known what they are in themselves. By means of auscultation, various diseases of the heart and lungs are capable of being detected with wonderful certainty; but the power of so detecting them belongs to those only who have studied these diseases in all the processes of their formation, and progress, and results.

It is with hearing as it is with the other senses. When they are taxed to give intimations to the mind concerning the objects by which they are impressed, it is necessary that the mind should have a previous knowledge what those objects are. Place a man within sight of London, and give him a telescope, and tell him to look for St. Paul's. St. Paul's he will undoubtedly see, and many a striking object besides; but he will not be able to distinguish it from Westminster Abbey or the Monument, unless he is previously instructed what sort of building St. Paul's is.

A child will at once perceive a difference between the fragrance of the violet and of the rose; but it must know the violet, and know the rose, and smell them both singly, and by turns, before it can assign to each its peculiar sweetness.

Any man can discern a difference between the sound of a trumpet and of a drum; but he must have been where trumpets have been blown, and drums been beat, ere he can tell which sound belongs to each. My voice is different from yours; but a man must be familiar with you and with me, and have heard us speak a hundred times, before he can distinguish us by our voices.

So diseases of the chest have, as it were, different voices; but we must first be familiar with the diseases themselves, and then be accustomed to hear them speak, ere we can tell one disease from another by its voice.

What *are* diseases of the chest? Pneumonia, pleurisy, phthisis. And *do* we mean that auscultation can distinguish each of these from the other? Yes, truly; and we mean more than this—much more.

Pneumonia, pleurisy, and phthisis are only the complex of several morbid processes and results. There is no such thing as a pneumonic, a pleuritic, or a phthisical sound. Pneumonia, pleurisy, and phthisis have no sounds that are peculiar to themselves *as such:* but the sounds that we hear in these diseases result from certain morbid processes going on, and certain changes wrought upon the structure of parts; which processes and changes make up the complex to which we give a name. We hear the sounds denoting that this part of the lung is loaded with fluid, that part condensed with solid matter, and another hollowed with cavities. Thus we get at inflammation; thus we get at phthisis. We anatomize by auscultation, if I may say so, while the patient is yet alive, the very processes and changes of structure of which inflammation or phthisis consist: and so of other diseases.

Auscultation professes to make us acquainted with the actual condition of the lungs in many of the most important diseases incident to them; their actual condition at any *particular time;* and their changes from one condition to another *from time to time.*

I am not aware that, before auscultation lent its aid to

diagnosis, we could do more than speak generally concerning the diseases of the lungs during the life of the patient. We could affirm generally that the lungs were inflamed; and, knowing, from our acquaintance with morbid processes, that it was the tendency of inflammation to produce such and such changes of their structure, we were aware what perils it involved, and could anticipate with tolerable accuracy what we should meet with when the patient died. So, too, we could affirm generally that there were tubercles or vomicæ in the lungs; and, understanding the forms and processes of phthisical disease, we could foretel in the main what we should find after death.

But auscultation anticipates the disclosures of morbid anatomy. Nearly all that dissection can unfold, it tells while the patient is yet alive. It does more: it brings us acquainted with diseases long before they have reached their fatal stage. By dissection we come in with our knowledge *at last*, and gain assurance of the disease from its ultimate results. By auscultation we are often—very often—enabled to make our knowledge keep pace with the disease from its least and earliest beginnings, through all the stages of its progress to the end. By auscultation we contemplate a living action going on, and have cognizance of it while it is yet at work. By dissection we contemplate the ruin as it is left, when all action has ceased.

I am not going to give you a regular didactic discourse upon auscultation: you can only learn it for yourselves, by the use of your own ears, in the wards of the hospital. And even by your own ears it is hardly possible to learn it anywhere except in the wards of a hospital; for you must have many patients to practise upon at the same time; and, moreover, you must have many fellow-students engaged at the same time in making the same observations with yourselves, that you may compare notes together, and agree about what you hear. I am quite sure that no man can arrive at any useful or safe conclusions from auscultation, if he studies it alone. I speak from experience when I say this. When I first turned my attention to auscultation, I found so many sources of deception connected with it, that I determined to admit no fact which was not attested by others besides myself; and I would advise you to

proceed at first with the same scrupulous care. "That everything is easy when you know it," sounds like the simplest of truisms; but, indeed, it is a very wise apophthegm. It imports that, be a thing ever so difficult, you may, by taking the necessary pains, obtain such a mastery over it, as to be surprised that you ever thought it difficult at all. Auscultation surely is not the most difficult thing in the world; neither is it the easiest. It is beset with many perplexities, and requires much time, and labour, and patience, and caution, to master it perfectly; but, being mastered, it becomes the safest, simplest guide, within its proper sphere, to a just diagnosis.

But auscultation, I have said, can only be learned within the walls of a hospital. Yet, perhaps, I may be able to give you some general directions how to proceed, which may be of use to you; and I wish to speak of auscultation at present as it respects the lungs only.

Now before you seek to acquaint yourselves with the sounds which indicate diseases of the lungs, you must learn those which are expressive of their healthy state: for the healthy sounds must be your standard of comparison in judging of the unhealthy.

It is useless for me to attempt to describe (what is called) "the healthy respiratory murmur;" I could only tell you that this sound is like some other sound with which you might be more familiar. But in a few weeks you will know the respiratory murmur so well by experience, that it will itself become the most familiar of all sounds. The pure perceptions of sense cannot be made clearer by descriptions and similitudes.

I would recommend students to practise auscultation upon each other, for the sake of learning what the healthy respiratory murmur is; and to do it often, and upon many individuals. The respiratory murmur is, I believe, the same in kind in all men who have healthy lungs; but it has differences of degree belonging to it in different men, which are somewhat puzzling at first. It does not reach the ear with the same clearness and loudness in the fat and the lean man. Fat and muscle damp the sound, where they abound above measure, as effectually as coats and waistcoats. Ausculting a man who is very fat and muscular, is like ausculting a man with his clothes on: you must make the same allowance in both cases.

But still the reason why the healthy respiration is more or

less audible cannot always be found in the integuments of the chest. It often happens, that in a thin spare man, whose lungs are perfectly sound, you can scarcely hear it at all, while in a fat man you hear it most distinctly; and, what is more remarkable, in a fat woman, even through the mamma.

People seem to me to differ very much in the mode and intensity of their breathing: some fill their lungs at every inspiration: the air appears to go further, and to dwell longer, within them. They breathe as if they had a luxury in breathing; and your ear seems to follow the air through every cell and vesicle as it goes in and out. Some, on the contrary, let the air just enter into their lungs, and come back again. They breathe as if they were afraid of breathing; and your ear can hardly detect any respiratory murmur except when they breathe with a forced effort.

It is probable that these diverse modes of breathing, in people perfectly healthy, are required by peculiar states of the circulation; and that they are natural and necessary provisions, not only consistent with health, but essential to it.

In children the respiratory murmur is far more audible than in adults; and on this account it would be well for those to whom auscultation is new to make their first trials upon children, that they may know what the healthy respiratory murmur is in its full and complete development.

That the parietes of a child's chest are thinner, there can be no doubt: and this *may* be one cause why its breathing is more audible. But the mode and intensity of the breathing itself is the chief cause; and this peculiar breathing of a child is in obedience to some natural necessity, and that necessity is probably respective to its circulation.

In adults (even in fat and muscular men and women) the respiratory murmur is sometimes as loud as in a child. But then it is generally in *some part* only of the lungs that it is so; and when this is the case, it is the result of disease, and the disease is demonstrably of a nature to require that a larger quantity of air should be received into that portion of the lungs whence the louder respiratory murmur issues, and that there should be a more energetic act of respiration.

All this you will soon be able to verify for yourselves, by numerous cases in the wards of the hospital.

Having learned the natural respiratory murmur, the sound which indicates that the lungs are healthy, you have got your standard of comparison, and are now prepared to judge of the sounds which denote their disease, as far as they are connected with the respiration. But you have got more than a mere standard of comparison by which to try the quality of other sounds. You are enabled to appreciate *simple* defects and failures of the respiratory murmur itself; and, indeed, it is as important a part of the business of auscultation to learn the extent to which the respiratory murmur is absent, as to discriminate the kind and character of the *new* sounds which are present and have superseded it. Besides, the diseases of the lungs are neither few nor inconsiderable in which auscultation finds no new or unnatural sounds whatever, but only the natural respiratory murmur abated, or abolished; and these *privative* signs are as valuable helps to the diagnosis of pulmonary disease, as any that are most positive and real.

But, after all, let it be borne in mind that the auscultatory signs of pulmonary disease are not all developed in the act of breathing: many occur in speaking or coughing; as will hereafter be shown.

It seems to me that it would be enough for all practical purposes if the unnatural sounds referable to the lungs, whether in breathing, speaking, or coughing, were divided *generically* into two; into *Dry* sounds and *Moist* sounds.

By dry sounds I mean those which result when bronchi, vesicles, or pulmonary cavities, present impediments, or rebounding surfaces, to the passage of air, and thus become sonorous or vocal from reverberation. By moist sounds I mean those which result, when bronchi, vesicles, or pulmonary cavities, present fluid to the passage of air, and thus yield a crackling or bubbling noise from the mingling of air and fluid together.

Of these sounds, the dry and the moist, I will point out such well marked varieties as (I conceive) need to be understood, and will endeavour to give to each an appropriate name; taking care in the meantime to treat the subject as little artificially as possible, while I state fairly and faithfully, from my own experience, *how* I have used, and *what benefit* I have derived from using, this newly invented key to the diagnosis of thoracic diseases.

LECTURE VIII.

ON THE DOCTRINE OF SYMPTOMS.

RHONCHUS AND SIBILUS—WHERE AND HOW PRODUCED.—HOW THEY INTERFERE WITH THE RESPIRATORY MURMUR.—IN WHAT SENSE THEY ARE DRY SOUNDS.—THEIR PATHOLOGICAL IMPORT.—CONDITIONS UNDER WHICH RHONCHUS OCCURS—CONDITIONS UNDER WHICH SIBILUS OCCURS—ILLUSTRATED BY COMMON FORMS OF BRONCHIAL DISEASE; BY ASTHMA, AND BY A PECULIAR FORM OF ACUTE BRONCHIAL INFLAMMATION.

Before I employ any terms to designate particular sounds, I would remark that the language of auscultation is not yet uniform. All writers do not use the same terms to designate the same things; and, until they do, some inconvenience must continue to be felt. Under these circumstances, I shall take the liberty of using those which have become current in St. Bartholomew's Hospital, and have had here a certain meaning attached to them.

In considering the direct symptoms of diseases of the lungs derived from auscultation, I will begin with that part of their structure which is most obnoxious to disease; for there *is* a part in which disease is found most frequently to begin, and to which, wherever else it may begin, it is almost always found ultimately to reach. This is the mucous membrane of the bronchi and their ramifications.

It is essential to the healthy respiratory murmur, not that the bronchi and their ramifications be merely free and pervious in every part, but that their surface be equal and smooth, and lubricated with moisture, and that the moisture be not in excess. If the surface be unequal, rough, or unlubricated, dry sounds reach the ear in the act of respiration; if there be excess of moisture, the sounds that reach the ear are those of air mingling with fluid. The dry sounds thus proceeding from the

air passages I will call *Rhonchus* and *Sibilus*, and the moist sounds *Crepitations*.

This Rhonchus and Sibilus, and these Crepitations, are always produced in breathing, not in talking or coughing. And first I wish to speak of what they are in themselves, and of how they interfere with the healthy respiratory murmur; and then I will endeavour to estimate their pathological import.

The terms Rhonchus and Sibilus are perhaps as intelligible in themselves as they can be made by further description. Rhonchus is the larger and hoarser sound; Sibilus the smaller and shriller. And, from what you must familiarly know of the sounds produced by blowing into a pipe of larger or smaller size, you will readily conceive that Rhonchus proceeds from the bronchi in their first divisions, and Sibilus from them in their minute ramifications, or from the vesicular structure of the lungs.

Rhonchus often occurs alone. It is often the only unnatural sound that is heard; and then the affection is of the bronchi in their first or larger divisions exclusively. In this case, to whatever degree the rhonchus supersedes the healthy respiratory murmur, it does so, not in the sense of preventing it *from taking place*, but in the sense of preventing it *from being heard*. The Rhonchus overpowers the respiratory murmur. The greater sound overpowers the less; but the less is extant notwithstanding.

The reason is, that the bronchi in their first divisions have nothing to do with producing the respiratory murmur; it does not arise in *them*, but in the lesser ramifications and vesicular structure beyond them; therefore they have no power to hinder the respiratory murmur, except when they suffer such impediments as absolutely preclude the access of air even to themselves, and consequently must prevent its further progress.

But, in point of fact, it seldom happens that the Rhonchus is loud enough to overcome the murmur altogether; and while they exist concurrently the ear has often a distinct perception of both. There is a loud hoarse sound in several parts, and there is also, perhaps even in the same situations, a clear respiratory murmur. The murmur is, as it were, heard through the rhonchus. In such cases some of the *larger* bronchi contain the cause productive of the dry sound, but offer, nevertheless,

hardly any impediment to the free passage of air; which, reaching the lesser bronchi and vesicles of the lungs, and finding them healthy, glides through them, and produces as it goes the murmur which is the best evidence of health.

So, too, Sibilus often occurs alone, and is often the only unnatural sound that is heard during respiration; and then the affection is of the bronchi in their lesser ramifications, or of the vesicles of the lungs. But in this case, to whatever degree the Sibilus supersedes the healthy respiratory murmur, it does so not in the sense of preventing it from being heard, but of preventing it from taking place. And the reason is, that *the parts* which produce the Sibilus and the respiratory murmur are the same; but the *conditions* under which they produce them are different. Consequently the sounds themselves are incompatible with each other, and cannot co-exist.

But Rhonchus and Sibilus, though each often occurs alone, do just as often occur both together. And nothing more is wanted to this event than that a bronchus, through its several divisions and ramifications, large and small simultaneously, should contain the cause capable of modifying the vibrations of the air in its passage.

The moist sounds occasioned by the mingling of air and fluid in the bronchi and their ramifications during the act of breathing, which have been variously denominated, I call by one name, Crepitations; and of Crepitations I only make the distinction of *Large* and *Small*.

Sounds so produced are ever without variety, and can only differ in being greater or less. And according to their largeness or smallness, and the space to which they are extended or confined, they become important signs in all those diseases where a separation of fluid from the mucous membrane of the air passages is a pathological ingredient.

The *Large* Crepitation is occasioned by the mingling of air with fluid in the first divisions of the bronchi. It arises from the same parts as the Rhonchus, but results from a different condition.

The *Small* Crepitation is occasioned by the mingling of air with fluid in the lesser ramifications of the bronchi, or in the vesicles of the lungs. It arises from the same parts as the Sibilus, but is owing to a different state of those parts.

The large and small Crepitation have the same effect of suppressing or superseding the respiratory murmur that the Rhonchus and Sibilus have; and each after its own manner respectively. The Large Crepitation, proceeding from the same parts as the Rhonchus, may overpower the murmur, but cannot prevent it from taking place; whereas the Small Crepitation, proceeding from the same parts as the Sibilus, is instead of the murmur, which it abolishes altogether.

All the bronchi in their primary divisions may contain an excess of fluid, and *Large* Crepitation may be heard over every part of the chest; and yet, through that *Large* Crepitation, a practised ear will be able to detect the respiratory murmur; obscured, indeed, by the louder sound, but itself genuine and healthy.

So, too, all the bronchi in their lesser ramifications, and the whole vesicular structure of the lungs, may contain an excess of fluid, and *Small* Crepitation may be heard in every part of the chest; but, in the meantime, no ear is subtle enough to catch the natural respiratory murmur, for no such murmur exists.

The Large and Small Crepitations may co-exist together in every variety of combination. You may have both Large and Small in every part of both lungs; or Large in one lung, and Small in the other; or Large and Small in different parts of the same lung. And with Large and Small Crepitation thus differently combined, Rhonchus and Sibilus may be still intermingled, and some natural respiratory murmur be here and there distinguishable among all the rest.

I wish now to consider the pathological import of the several auscultatory signs which have been specified, before I proceed to others; for they are practically the most momentous of all. They are few; but they convey vast information according to the manner and combinations in which they occur. Do not be surprised at this. There are but twenty-six letters in the alphabet; yet these compose all language; and language conveys all knowledge. Think of knowledge, its vastness, its variety, its multitudinous particulars! Yet language has compassed it all; language has delineated it all; and language is daily furnishing to you and to me little pictures of such portions of it as we desire to survey. Still the wonders of language are comprised in twenty-six letters.

Let it not, therefore, seem strange that many conditions of disease are signified by a few simple sounds.

I have called Rhonchus and Sibilus *dry sounds*, because I thought it particularly important to distinguish them from others which, arising from air and fluid in the act of mingling together, are properly denominated *moist* sounds. But I must warn you against inferring, from my designation of them as *dry* sounds, that a preternatural dryness of the air passages is essential to their production; for such is not the case. Indeed, respective to the conditions out of which they arise, I would rather say, of Rhonchus and Sibilus, that they were *not moist* than that they were dry. And if you like it better, there is no objection to your speaking of sounds that *are moist* and sounds that are *not moist*, instead of sounds that are *moist* and sounds that are *dry*.

Rhonchus is the most fluctuating and inconstant of all sounds that belong to the lungs. It arises out of various pathological conditions, and out of some that do not deserve to be called pathological at all. It would be affectation to pretend to speak of them all with precision.

People in perfect health are apt to have Rhonchus mixed with the ordinary respiratory murmur: they strive to clear their throat by a forced effort of expectoration, and sometimes bring up a little phlegm and sometimes not; they only displace it. Hereupon the Rhonchus ceases, and the respiratory murmur remains alone. Again, people suffering disease of the lungs are apt to have Rhonchus mixed with all sorts of unnatural sounds. They make an effort to dislodge or reject a little phlegm and the Rhonchus disappears, but all the other unnatural sounds remain.

In these cases the cause of the Rhonchus is evidently secreted matter from the surface of the bronchi; but why does it not produce crepitation and not rhonchus? Simply because it is *not fluid* enough to allow the air to penetrate it, mingle with it, and pass through it. The air, in going in and coming out, passes by it and impinges against it, and suffers a vibration from it; and this yields the sound.

This Rhonchus, which is owing to tough and viscid phlegm clinging to the part upon which it is deposited, is sometimes propagated over the whole, or a considerable part of the chest;

but then the extent of the Rhonchus is no just measure of the quantity of the phlegm. The phlegm may still be very small in quantity; but being lodged in the trachea, or the first divisions of the bronchi, where they are very large and free, it vibrates, like a harp-string, to the impulse of air, and diffuses the Rhonchus far and wide through the chest.

But, whether this explanation be satisfactory or not, it is perpetually happening that a Rhonchus, heard in every part of the chest, is made to cease at once by a stout voluntary effort of expectoration. Indeed I may venture to say, from my own observation, that a Rhonchus may *almost always* be thus got rid of, whether it occur alone or be one amongst other unnatural sounds. And hence I infer that the cause of Rhonchus is *almost always* a small tough moveable piece of phlegm, adhering to the trachea or first divisions of the bronchi.

The cause is, however, sometimes immoveable and permanent, and quite of another kind. Anything, from within or from without, that has the effect of narrowing or obstructing a bronchial tube, in ever so small a space, may occasion the same sort of sound. Changes of structure within the parts themselves, such as a thickening of the mucous membrane, or the ossification of a cartilaginous ring; or morbid growths from without, such as a bronchocele, an aneurismal sac, or a tubercular mass in the bronchial glands, or in the lungs themselves,—have so compressed or distorted the trachea, or certain bronchi, that the air could not force its way through them without continual vibrations, and without the respiration being constantly accompanied by a hoarse unnatural sound—by Rhonchus.

It is not easy to determine the conditions which are essential to the production of Sibilus. In seeking to assign them, I am left to my own reasonable calculation of what they *possibly may be*, having no direct means of proving what they actually are. I never knew any person die whose only auscultatory symptom was a Sibilus. Yet I believe people may and do die, and I have myself seen several in imminent peril of death, from disease of the lungs, whose only auscultatory symptom referable to the lungs, has been a widely diffused Sibilus. Of such cases I will speak presently: they are of great importance, and well deserve to be pointed out.

Sibilus, like Rhonchus, may occur alone, or in combination with other auscultatory signs. But whether alone or mixed with others, it cannot, like Rhonchus, be got rid of by an effort of expectoration. Yet the cause of both may be the same in kind, and different only in situation. A secreted matter, not fluid enough to admit air to mingle with its particles and thus produce a *moist* sound, but so consistent as to present a reverberating surface, and thus produce a *dry* sound, may be equally the occasion of Rhonchus and Sibilus. But in Rhonchus this matter is within reach of a voluntary succussion of the trachea and bronchi to expel it; in Sibilus it is beyond it. In Rhonchus it is contained within the first divisions of the bronchi; in Sibilus, within their lesser ramifications.

Sibilus, whether alone or in its combinations, cannot, like Rhonchus, ever be regarded as an innocent symptom. It is a more unquestionable evidence of disease than Rhonchus, in whatever that disease may consist.

Sibilus is almost always mixed with Small Crepitation. They are united together in the same individual, and often succeed and supersede each other, as if they were contending together for the mastery; now one and now the other being predominant.

This mixture of Sibilus and Crepitation, and the predominance first of one and then of the other, are chiefly seen where both are largely diffused throughout the lungs; and in such cases one may often remark a fluctuation of the general symptoms answerable to this fluctuation of the auscultatory signs; that these symptoms, in their aggregate, become more inflammatory when the Sibilus increases, and less inflammatory when the Sibilus declines; also that the remedy which abates their general inflammatory character commonly abates the Sibilus; also when the expectoration is freer the Sibilus is less, and when the expectoration is scanty the Sibilus is more.

Now these are, indeed, great practical points, if they be true; and there are always examples enough to be found in this hospital by which you may test their truth. There are plenty of patients who have been suffering cough, expectoration, and dyspnœa, long and habitually,—some from disease which belongs primarily to the lungs—some from disease which is derived to the lungs from the heart. Watch these patients well for a few

weeks together; mark the auscultatory signs and their fluctuations; mark the general symptoms and their fluctuations also; mark the treatment, and its adaptation to, and influence upon both; and then judge whether the practical points, which respect the particular auscultatory sign now in question, are really such as I have represented them.

These cases of *chronic* bronchial affection are the most favourable for teaching the character of Sibilus, and how it stands related to other auscultatory signs, and to more general symptoms, and how it is amenable to methods of treatment. Such cases tell their story (if I may say so) more leisurely: they tell it over and over again, and with many interesting variations and thus give you time to dwell upon it and understand it.

But still of Sibilus, that is thus mixed with Crepitation, now superseding it, and now superseded by it—becoming less as the expectoration is more, and more as the expectoration is less—increasing as the general symptoms are more inflammatory, and yielding to the same remedies that they yield to;—of this Sibilus I do not pretend punctually to know the local efficient cause, or the exact pathological condition of the parts out of which it immediately arises. I am content to believe (without any curious speculation upon things which I cannot prove) that, upon the access of a more inflammatory action, the secreted matter in many bronchial ramifications becomes more scanty and less fluid, so that the air that is breathed cannot freely mingle with it, and thus Crepitation yields to Sibilus; and that, upon the subsidence of the inflammatory action, the secreted matter becomes more copious and more fluid, so that again the air freely mingles with it, and thus Sibilus in its turn yields to Crepitation.

But Sibilus does not occur under these circumstances only. It does not merely go and come, or occasionally intervene in chronic bronchial disease, of which the auscultatory symptom that is most characteristic and abiding is of another kind. There are cases in which Sibilus is itself the most characteristic auscultatory symptom,—cases in which for a long period, and cases in which even from first to last, there is no other auscultatory symptom whatever but Sibilus.

There are cases of (what I suppose would be called) genuine

asthma, that present some such symptoms as these: dyspnœa, or rather an agony and fighting for breath; livid lips; cold and livid extremities; and a dry ineffectual cough, terminated and relieved, after an uncertain interval, by a copious puriform expectoration. Here, during the agony or paroxysm (and unfortunately it often continues long enough to allow a very leisurely examination of the chest by the ear—sometimes many days, sometimes a week or two), the sole auscultatory sign is a Sibilus pervading a larger or smaller portion of the lungs, according to the severity of the case. And, as the agony lessens, and the expectoration begins to appear, Crepitation is found mingling itself with Sibilus; and, when the agony has *entirely* ceased, and the expectoration become more copious and free, Crepitation, and Crepitation alone, is then heard in the same situations, and to the same extent, that Sibilus, and Sibilus alone, was heard before.

I have witnessed instances of asthma in several individuals, and several attacks of asthma in the same individual, where the auscultatory signs have had as strict and definite a correspondence with the stages, progress, and prominent symptoms of the disease, as that which I have here described.

Now, if absolute Dryness can be ever safely predicated of the respiratory passages, and can be ever safely reckoned among the pathological ingredients of their diseases, and ever clearly notified by one express symptom, it is in spasmodic asthma, of which it seems the chief pathological ingredient during its first and often most protracted stage, and is clearly notified by a widely diffused Sibilus.

I am persuaded that the natural moisture of the respiratory passages is *then* really in defect, and that Sibilus is really an index of the fact. Sibilus may then, if ever, be truly called a Dry sound. But I am not sure that the Sibilus directly results from the mere condition of Dryness; I doubt whether simple Dryness alone would naturally produce it. In consequence of its Dryness the mucous membrane may lose its elasticity, and become to a certain degree unyielding; or it may undergo wrinklings or puckerings at various spaces, or its general tumefaction may produce a narrowing of the smaller tubes, and thus present obstacles to the passage of air, and impart to it new vibrations; and hence the Sibilus.

Nevertheless, whether Dryness of the respiratory passages, or other conditions necessarily resulting from it, give immediate occasion to the Sibilus, the Sibilus may be properly regarded as the auscultatory symptom of the former. Dryness of the mucous membrane bespeaks a well-known pathological change; the other conditions are mere matters of conjecture.

Thus far I have spoken of Sibilus occurring in two forms of bronchial disease. 1st, As it intervenes among the auscultatory symptoms of certain chronic affections, characterized by dyspnœa, expectoration, and cough, instances of which are perpetually at hand to enable you to verify the fact. 2ndly, As it presents itself as the sole auscultatory symptom that attends the paroxysm or agony of an asthmatic attack, when it is so marked that its presence can never be doubted. In both these forms of disease Sibilus conveys pathological and practical information of great importance.

But does Sibilus ever occur in acute bronchial or vesicular inflammation? And does it ever *so* occur as to throw essential light upon morbid processes going on, and essential light upon modes of treatment?

Inflammation of the bronchial ramifications perhaps never exists without the natural secretion of their mucous surface being either diminished or increased, and, consequently, without the accompaniment of those sounds which indicate its defect or excess; *i.e.* without Sibilus or Crepitation.

Sibilus is apt to occur at the beginning of such inflammation; and thus it corresponds with the pathological condition out of which it arises, the mucous membrane, when it is inflamed, becoming drier than ordinary before it yields a more abundant secretion.

Sibilus, too, after it has arisen, is apt to be of short duration, seldom abiding long as the *sole* auscultatory symptom of such inflammation. And herein also it corresponds with the pathological condition from which it proceeds; for the dryness of the mucous surface generally soon gives place to moisture.

Hence it happens that Sibilus is so seldom met with in practice, except with some mixture of Crepitation. The inflammation is, in truth, not submitted to our observation until the stage of *dry* sounds is passing, or has already passed, into the stage of *moist* sounds.

Nevertheless, there are cases in which Sibilus is the sole and abiding symptom derived from Auscultation, and a dryness of the air passages the sole and abiding morbid condition. They are cases distinct from asthma—cases of genuine inflammation, and so remarkable as to require an especial notice.

I have met with a frightful affection in children; but what its nature was I could never tell, until auscultation enabled me to unravel it. It commonly passes for inflammation of the lungs. But, when children have got well, they have got well so soon and so entirely, that I could never believe the disease to be pneumonia, although the symptoms seemed to indicate that it could be nothing else.

Last summer I went out of town to see a little boy, seven or eight years of age, whose life was very precious to his family. He was thought to be dying of inflammation of the lungs. I found him raised up in bed, supported by his nurse, and breathing with all his might. His skin was hot; his face flushed; and his chest heaved, and his nostrils quivered frightfully. There was no croupy sound. Whatever the disease was, it was all within the chest. I percussed the chest: it sounded well in every part: I listened: the air entered freely, and reached every cell and vesicle of the lungs; but there was not the least perception of the natural respiratory murmur: a shrill Sibilus had taken place of it altogether. Wherever you applied your ear to the chest, you might fancy you heard the piping and screaming of a nestful of unfledged birds.

But what was this disease? Surely it was inflammation largely diffused over the mucous surface throughout the bronchial ramifications, but inflammation as yet only in its *first* stage; for the air, as it passed through them, did not mingle with a particle of fluid anywhere, and the sound it produced was a dry Sibilus only.

But *how* inflammation yet only in its *first* stage? The boy had been already ill four days. Still it might be inflammation in its *first* stage. The boy continued ill two days longer, with the same kind and the same degree of suffering; and then, under the influence of tartar emetic, the fever began gradually to subside, and the dyspnœa to abate. The Sibilus gradually gave way to the healthy respiratory murmur, and he was well

again *without expectoration of any kind*. The inflammation began and ended with the *first* stage; and, although it continued with great severity for a week, it never got beyond the *first* stage.

This is an instance, which strikingly shows the value of Auscultation in detecting at once the state of things, about which you might go on conjecturing and conjecturing for ever what it *possibly* might be, and not gain the least assurance what it *actually* was.

In adults sometimes, but not so frequently as in children, I have met with the same evidences of acute inflammation widely diffused through the bronchial ramifications, and remaining in this its first stage for days and days together. In the meantime their mucous surface has still been dry throughout a great part of both lungs, and the ear has continued for days and days together to hear no other unnatural sound but a Sibilus. Convalescence has taken place without expectoration, and the Sibilus has given way, without the intervention of any *moist* sound, at once to the murmur of health.

But such inflammation, after lingering long in the first stage, will sometimes pass beyond it; and the whole mucous surface that was previously dry will pour forth an enormous secretion, and the widely diffused Sibilus will be changed into a widely diffused Crepitation. Still the lungs are unhurt beyond the lining membrane of the air passages, and the patient will get well, if he be not suffocated by the enormous expectoration.

I am speaking of a disease which must be distinguished from asthma, according to the usual acceptation—a disease not habitual to the individual, and of which, perhaps, he has never suffered a previous attack. I am speaking of acute inflammation extending throughout the bronchial ramifications, and reaching, perhaps, the vesicular structure of the lungs, putting on a peculiar form, and affecting a peculiar course; but still of acute inflammation, as further evidenced by the remedies necessary for its relief.

During the last summer I saw a gentleman, who had been, two days previously, seized rather suddenly with feverish symptoms, and with the most dreadful dyspnœa. His lips were

blue; he was labouring for breath, and coughing with hard and ineffectual efforts to rid himself of something which seemed to tease the larynx, but no expectoration followed.

Cupping on various parts of the chest (the state of vascular action required that blood should be drawn), and tartar emetic in frequent doses, were the remedies employed; but in the same state of agony he remained for a week, propped up in bed, striving with all his might to free himself from his oppression, coughing and endeavouring to expectorate, but ineffectually.

What was going on all this time? There was anguish enough for any disease of the most formidable name; for fluid in the pericardium; for extensive hydrothorax; for induration of a whole lung; for stricture at some orifice of the heart. A few years ago the most sagacious physician could only have guessed at the real state of disease, and probably would have guessed wrong. Such severe dyspnœa, so long continued, without expectoration, would probably have determined his diagnosis to hydrothorax.

But what was the disease? Every part of the chest sounded well to percussion. The heart beat regularly, and with a natural sound, only with too great frequency.

What could it be? There reached the ear from every part of the chest to which it was applied a loud Sibilus. The disease was an inflammation largely diffused through all, perhaps, of the bronchial passages, great and small; inflammation abiding long in its first stage, and limiting itself to one structure.

But in this case the inflammation ultimately passed beyond its first stage; for ultimately there arose an immense expectoration, and so the disease reached a favourable termination.

I have said that it is the peculiar praise of auscultation, not merely to discover disease in its ultimate results, but to analyse it in its several processes as it goes on; to mark its stay and continuance in one process, and its passage and transitus from one to another. Here we have a conspicuous instance of both these circumstances discovered to us by Auscultation.

It is, I suspect, among the characteristics of inflammation, that, in proportion as it is more widely diffused, it should be

less rapid in its progress; not necessarily less severe, as far as severity is measured by force of vascular action and by fever, but less rapid in accomplishing its whole course; dwelling longer in each particular stage before it passes to another, than the inflammation which begins at a point.

Of this we have an example in acute rheumatism, which is diffused over similar structures in many joints simultaneously. In acute rheumatism, action and suffering are carried to the utmost degree of severity. There are extreme heat, and extreme pain, and extreme vascular action, in the parts, and in the constitution at large; yet all are expended upon one stage of inflammation.

Rheumatism may exist for weeks and months together, with all its pain, and heat, and vascular action, unabated. Chronic in duration, but most acute in what respects action and suffering, it should seem that any length of time was permitted to it to do all that inflammation can effect within *a certain* limit, but that it was restrained by a strong, though not an invincible, law, from transgressing *that* limit, or doing harm beyond it.

After many weeks or months the inflammation will cease, and every joint be restored to the form, and feelings, and functions, of health.

Such inflammation may exist in internal parts. (I do not mean rheumatic inflammation. Do not let me perplex you with a name. I only refer to rheumatism as to something well known, for comparison's sake, or analogy.) Inflammation, I say, may exist in internal parts, which (like rheumatic inflammation of the joints) is of a diffusive character, and occupies a large extent of surface at once, travelling tardily from structure to structure, slow to disorganize, abiding long in each of its stages, and giving leisure for the application of remedies in all of them. Such an inflammation is especially incident to the lungs; and, of the pulmonary structures, especially to the mucous membrane which lines the air passages.

I have given instances of such inflammation of the bronchi lingering in *its first* stage, its stage of mere vascularity and defective secretion; and I have dwelt upon these instances for the sake of showing you the real value of Sibilus as a patho-

gnomonic sign. But for it, in the instances specified, I could not have made out the nature of the disease.

Of Sibilus I will venture to observe, that sufficient regard has not yet been bestowed upon what it is *in itself*. It is usually spoken of as conducting to Crepitations, and mixed with Crepitations, or moist sounds. But there wants some illustration of it in its separate import, as standing alone. To that illustration what I have said may, perhaps, contribute a little.

LECTURE IX.

ON THE DOCTRINE OF SYMPTOMS.

CREPITATIONS, OR MOIST SOUNDS, THAT ATTEND THE ACT OF BREATHING.—LARGE AND SMALL CREPITATIONS.—THE DISTINCTION OBVIOUS AND USEFUL IN ITS MAIN CHARACTERISTICS—UNCERTAIN AND USELESS IN ITS LESSER DEGREES.—CREPITATIONS THE MOST FREQUENT OF ALL AUSCULTATORY SIGNS.—WHAT THEY CAN, AND WHAT THEY CANNOT, TEACH.—HOW THEY NEED OTHER AND MORE GENERAL SYMPTOMS TO INTERPRET THEIR MEANING.—ILLUSTRATED BY ACUTE INFLAMMATION OF THE LARGER BRONCHI—OF THE SMALLER—BY THAT INFLAMMATION OF THE BRONCHI WHICH ACCOMPANIES DISEASES OF THE HEART—BY THAT WHICH SIMULATES PHTHISIS—BY THAT WHICH IS CALLED PERIPNEUMONIA NOTHA.—HOW MUCH IN EACH CAN BE INFERRED FROM THE KIND AND EXTENT OF THE CREPITATIONS.—THE CREPITATION CHARACTERISTIC OF PNEUMONIA.

THE sounds produced by the meeting and mingling of air with fluid in the bronchial tubes during the act of respiration, I have called Crepitations; and of Crepitations I have made one distinction only, large and small.

Between the largest of the large and the smallest of the small there are many intermediate degrees; and some of these may perhaps seem to deserve a name. But, for my own part, I always have had a great unwillingness to multiply names, especially where things are essentially the same, and differ only in being greater or less; therefore I cannot bring myself to invent several names to designate different degrees in the present instance.

But though the extremes are far apart, yet in the midway large and small Crepitation will so nearly meet, that there must often be a doubt which is which; and what one man calls large,

another will call small. Language, however, will afford no remedy where the difficulty is in the thing itself. You can only accurately distinguish things when they are some way apart, and not when they lie close upon the confines of each other. You can distinguish between light and darkness, but you cannot put a mark upon the boundary between day and night.

Large Crepitation arises from air meeting and mingling with fluid in the larger bronchi; small Crepitation from the same conditions in the smaller bronchi and the vesicles of the lungs. This is an important distinction, and I desire to make much of it: important, however, in its main and prominent characteristics, but useless if it be refined into many degrees.

Without wishing to enter upon a criticism of nomenclature, I would further remark that, in giving names to auscultatory signs, we should take especial care that the names themselves do not imply anything that is erroneous; and that they do not go beyond the truth, in pretending to designate that which they *certainly cannot* designate.

What I call "large Crepitation" is called by most French writers "râle muqueux," and by most English writers "mucous rattle." Call it râle, or rattle, or Crepitation, or what you will; but pray do not add "mucous" to it by way of specific difference; for this term must always seem to imply that the sound is produced by air passing through *mucus;* whereas it is produced equally by air passing through mucus, or pus, or blood, or any fluid whatever. Besides, it is beyond the truth to say that the *quality* of the fluid through which the air passes *can* be distinguished by the quality of the sound that results. The sound will indicate the *situation* and quantity of the fluid, and no more.

Therefore, by whatever name you choose to designate the moist sounds arising from the bronchial passages during respiration, be it Crepitation, râle, or rattle, you cannot distinguish it by any other epithets of more precise meaning than "large or small," without implying more than you intend, and that, too, something erroneous, or something beyond the truth.

Crepitation, or the sound which shows that the moisture of the bronchial passages is in excess, is the commonest of all auscultatory signs. Go round this hospital, and, out of the five

hundred and more patients which it contains, you will probably find Crepitation in forty or fifty. And in all these it arises immediately from one and the same condition; viz. from excess of fluid in the bronchial tubes or in the vesicles of the lungs.

But all these patients cannot, in any just sense, be said to have the same disease, because a single pathological condition is the same in all. Neither, on that account, will all be found to demand the same treatment.

I speak of disease and treatment in the largest and most comprehensive sense.

Now the forty or fifty patients who have all Crepitation of their lungs, are not suffering alike in *other respects*. And *these other* respects in which they differ include the great characteristics of their diseases, and the special indications of their treatment.

At present I am speaking only of *direct symptoms*, immediately referable to the part affected. When I come hereafter to speak of *indirect* symptoms, or those which, originating in one part, declare themselves through the medium of another; and of general or constitutional symptoms, the signs of the pulse, and of febrile and nervous affections in their various kinds and degrees; then I will endeavour to show that many diseases, apparently local, have a much larger range throughout the body, and that the treatment which is to compass their cure must be alike comprehensive in its influence.

Learn, however, all that is capable of being known concerning the *mere* part. Let the patient tell you how it feels amiss, and ascertain for yourselves how it acts amiss; and if it be a part within reach of the sight, the hearing, or the touch, make out what changes of structure it has undergone. Still its sensations, its functions, and its structure, will only half inform you what are the essentials of the disease it suffers.

Remember, there is a pathology of diseases *beyond* the part, as well as a pathology of diseases *within* the part; and that the things beyond it are really and practically the great interpreters of the things within it.

Remember, there is a great vascular system, and a great nervous system, and that these, according to the manner in which they are actuated, assign a character to the local disease, and determine its treatment accordingly. Concerning the

disease, they tell us what is its force of action, and what its rate of progress; and, concerning its treatment, they teach us to choose the remedies, and so to regulate their impression as to counteract this force of action, and to keep pace with this rate of progress.

Bearing these considerations in mind, you will be able to comprehend what I mean by saying, that the many patients in this hospital who have Crepitation as a common auscultatory sign, and redundant fluid in the bronchi or pulmonary vesicles as a common pathological condition, may nevertheless have different diseases. And, still bearing them in mind, you will now be able to follow me as I run over a few prominent distinctions.

Among the many who have this auscultatory sign, in some it has endured for weeks or for months, and in some it has sprung up since yesterday. In some it is accompanied by much fever and great vascular action, and life itself seems already in peril, although it has existed but for a few days.

In some it is accompanied by less fever and less vascular action, and there is yet no peril of life, although it has existed for many weeks.

This Crepitation is present in rheums and catarrhs, and chronic coughs, which cleave to old people, from the end of autumn to the beginning of spring, with little or no fever.

And it is seldom absent in chronic diseases of the heart; and here it is found sometimes with and sometimes without fever, or any signs of inflammation; as if the bronchial surface had the power of *simply* increasing its secretions for the relief of a burdened and baffled circulation through the lungs.

Crepitation also is a frequent accompaniment of pulmonary hæmoptysis, with or without fever.

Now, while the great constitutional symptoms are our paramount guides to the knowledge and treatment of the disease as a whole, it is to *this Crepitation* that we are to look, in each particular case, for the knowledge of what the disease is in the lungs; its exact seat, its extent, and the stage of its progress.

The case is one, perhaps, in which there is much fever and great vascular action; while cough and dyspnœa, and some

expectoration, denote the lungs to be the part upon which the inflammation has specially fallen.

We auscult, and discover Crepitation: and the Crepitation has one main and prominent characteristic; that it is large. It is *large*, and *large* exclusively; while through it, wherever it is heard, the respiratory murmur is also heard in every part of the lungs.

Such auscultatory Signs denote the mingling of air with the matter of morbid secretion in the larger bronchial tubes, and in them exclusively; the lesser tubes and the vesicular structure of the lungs, the seat of the respiratory murmur, being entirely free.

But the much fever, and the great vascular action, declare this condition of the bronchi to be the work of severe inflammation, and of inflammation that is still going on. Yet, while Auscultation continues to show that such is exclusively the seat and limit of the inflammation, severe as it is, we are warranted in expecting a favourable result; provided always that we pursue a just treatment. For even the acutest inflammation of the larger bronchi is unapt to extend itself to other textures, or to involve the structure of the lungs beyond those bronchi themselves.

But the case is one, perhaps, in which there are the same constitutional symptoms bespeaking the disease to be inflammation, and the same local symptoms, fixing that inflammation upon the lungs. But withal, the Crepitation heard during breathing is *small*, and *small* exclusively; and this small Crepitation, wherever it is found, has entirely obliterated the respiratory murmur.

Such auscultatory Signs denote the mingling of air with the matter of morbid secretion in the lesser bronchial tubes, or in the vesicles of the lungs.

In the former case, the disease was acute inflammation, and it is no more than acute inflammation in this. We have the same means of subduing inflammation in this which we had in that, and the same plain indications to direct our treatment; and the disease, *in its own nature*, is as amenable to remedies *here* as it was there. Yet are we *not* warranted in forming the same expectation of a favourable result. Because inflammation of the lesser bronchi, unlike that of the larger, is ever ready to

pass beyond them to other textures, and to involve the whole structure of the lungs.

Where you have small Crepitation one day, you may find that it has entirely ceased the next; and ceased, not to be replaced by the respiratory murmur, but by absolute dulness and total obliteration of sound.

And what is implied by this rapid change from small Crepitation to total obliteration of sound? Even this; that the bronchial ramifications and vesicular structure are so pressed upon, from within and from without, by the effused products of inflammation, by serum, or lymph, or pus, or blood, or a mixture of all, that air cannot enter, and the lung has become solid at that part, and may possibly have undergone irreparable disorganization.

Let it not, then, be esteemed a small thing, that, upon rational grounds, and by tokens which we can justify and explain, we are able to arrive at this diagnosis; that, in the severest inflammation of such an organ as the lungs, we can mark the cases which are within the probability of a perfect cure, and those which are beset with the most perilous hazards.

To distinguish between inflammation of the larger bronchi and inflammation of the smaller, or of the vesicular structure, is still important in all its degrees, and whether it be chronic or acute; and auscultation will alway enable us to do so.

Chronic inflammation of the larger bronchi, after months and years, is still reluctant to extend itself to other structures; whereas chronic inflammation of the lesser bronchi is always ready to spread beyond its original seat.

The most frequent instances that I meet with, of inflammation, slight in degree and chronic in duration, affecting the lesser bronchial tubes, and producing effusion into them, are those in which it accompanies chronic disease or disorganization of the heart. It is evidenced by small Crepitation proceeding from a considerable space at the lower part of one or both lungs, and often continuing to be heard for months and months together. Thus, even for months and months, the lung may remain quite pervious, but crepitating. Yet there is no security, in the meantime, that what is pervious and crepitating to-day, may not be absolutely impervious and dull to-morrow. In point of fact, I have known, by the test of auscultation,

nearly a whole lung become condensed and solid in the course of a single night, when there has been nothing to give warning of such a catastrophe.

Indeed, in the dissection of those who die of diseased hearts, we are accustomed to find the lungs generally loaded with serous and sanguineous effusions, while some portions of them are solid and impervious, and sink in water.

But chronic inflammation of the larger bronchi, after months or years, is still, I have said, reluctant to extend itself to other structures. After months, or years, Auscultation still finds it in its original seat; the air bubbling through a thickish, copious, puriform fluid, and producing the truest form of large Crepitation.

There is a form of Chronic Bronchitis, in which all the conceivable signs of Phthisis are present except the auscultatory; emaciation, hectic fever, cough, and a copious thick yellow globular expectoration. Yet the chest sounds well everywhere upon percussion, and the auscultatory sounds are *purely bronchial*, and nothing more, and proceeding from the bronchi in their first divisions, and not beyond them; large, not small Crepitation; but large Crepitation widely diffused, and permitting the respiratory murmur to be heard everywhere through it.

Here the *larger* bronchi alone are inflamed, and filled with the matter of morbid secretion, while the lesser bronchi and the vesicular structure of the lungs are free. There is, moreover, no cavity.

It would be beyond the power of the most sagacious physician upon earth, *without the help of Auscultation*, to distinguish this case from a case of phthisis; but by *such help* we not only determine this disease, which is so like phthisis, to be no phthisis at all, but we pronounce it curable; that is, curable in its own nature, although from circumstances not always cured.

Cases of this kind—cases of chronic bronchitis, in which all the conceivable signs of phthisis are present except those which indicate vomicæ—are not common: I see a few of them in the course of the year.

They are more frequent, to my experience, in the hospital, than in private practice. The disease usually begins in a catarrh, which, from neglect or unavoidable exposure, is

aggravated into a similitude of phthisis. Under this similitude it may endure even for a year or two; and it becomes difficult of cure in proportion to its continuance.

But, where there is Crepitation, it need not necessarily be of one kind only. In the same patient you may hear at one part of the lungs a Crepitation like the tracheal rattle of the dying; at another, like the bursting of large bubbles on the surface of water; at another, like the crackling of salt thrown upon hot embers.

There is a disease which was first called by Sydenham *Peripneumonia Notha*, and which is still known by that name. It is, in fact, a diffused inflammation of the bronchial tubes, chiefly incident to old people. One of its peculiarities consists in the enormous secretion that is poured forth from the mucous surface; and another which I have remarked is, that the inflammatory symptoms often still remain, nay, often continue to increase, after the secretion is freely established. This latter peculiarity (if auscultation informs me aright) is derived from the fact, that inflammation does not arise at once and simultaneously upon the whole surface which it is destined to pervade, but travels over it progressively; so that various portions of the same continuous surface are in different stages of inflammation at the same time. How possible this is, everyone knows, who has watched erysipelas travelling over the whole body.

Cases of Peripneumonia Notha I recommend to you as special studies for Auscultation: here you will often find, in the same patient, every modification of those sounds which are produced by the matter of morbid secretion mingling with air in the bronchial ramifications of every size, from the largest to the least.

The common opinion is, that old people die of Perineumonia Notha simply because they have not power to bring up the large accumulation of phlegm; implying, that the whole disease is limited to the first divisions of the bronchi, and that if they were freed from obstruction, there is nothing beyond them, nothing in the condition of the lungs elsewhere, capable of producing death. Doubtless, it is quite possible that accumulated secretion of any kind in this situation may be the whole and sole cause of death: it may suffocate by its very quantity. But this I know, that since I have had Auscultation to help my

inquiries, I have never seen anyone perish of Peripneumonia Notha, in whom there was not elsewhere within the lungs enough to claim a large share in producing his death.

But I find myself speaking of Auscultation too much in detail for the purpose I have in view. In these lectures I would rather wish to give you such a notion of the general bearings of this and that subject, as will help you to follow it up for yourselves in the wards of the hospital. *Here* I am not so much striving to teach, as I am encouraging you to learn.

There is a Crepitation which consists of the *smallest* conceivable crackling—a noise like the crackling of salt thrown upon burning coals. This is regarded as the characteristic Crepitation of Pneumonia, because, wherever it exists, it is always for a short period only; and it is quickly followed either by the return of the natural respiratory murmur, or by the absence of all sound whatever. In the one case the passage of air through the vesicular structure of the lung has again become free, and a resolution of the disease has taken place; in the other case the passage of air is altogether precluded, and the disease has passed into its next stage, and has condensed the lung.

There is a doubt in *what* manner this particular sound is produced, and *where*. Some conceive it to proceed from the structure of the lung exterior to the bronchi and vesicles, and to result from the tearing of parts asunder that have been united by effused lymph. It may be so; but the fact would be extremely difficult to prove. To my ear the sound is the same in kind with those which I have described by the generic term Crepitation. Of these it is the smallest in degree, and probably proceeds from the same continuous surface, from the extreme vesicles and air-cells of the lungs; and is probably formed in the same manner, by the mingling of air with the morbid secretion which it finds there. This little crackling sound, so well known to those whose experience has taught them to appreciate Auscultation in its practical use,—this little crackling sound, reaching the ear from a limited and circumscribed space within the chest, marks the commencement of Pneumonia. It is a *direct symptom*, having immediate reference to the structure of the part. And if we consider what the part is, and what the disease,—the part the lungs, and the disease inflammation,—

we cannot too highly value this single symptom (simple and mean as it may seem), which gives the earliest and surest intimation that such a disease has begun as tends to disorganization and the inevitable loss of life, unless quickly arrested by its counteracting remedy.

But Auscultation, having detected inflammation of the lungs at the point where it begins, still follows it as it spreads; and it follows not merely its progress, but its processes.

The Crepitus commencing at a small space, and gradually reaching further and further, gives notice of inflammation gradually passing from lobule to lobule, and effusing fluid into their vesicles as it spreads. Then the Crepitus becoming fainter and fainter, but not replaced by the respiratory murmur; and the spaces in which it was first heard becoming dull, and larger and larger spaces becoming dull in succession, until not a sound of respiration, either healthy or unhealthy, proceeds from, perhaps half the chest; these striking phenomena give notice how the vesicular structure of a whole lung is progressively obliterated by the effused lymph and morbid products of the still unarrested inflammation.

Thus far the intimations of the ear keep pace with the progress and processes of an acute attack of Pneumonia. Auscultation pronounces the permeable lung converted into a solid mass, and admitting no air, save what may just enter, and immediately return from, a bronchus or two which still remain pervious. Beyond this point Auscultation cannot go; but the disease may go further.

This is a painful period of suspense in every case of Pneumonia, when a whole lung, or a large part of it, has ceased to admit air, and the patient still survives.

The disease (I say) may go further than Auscultation can follow it. Auscultation only discovers that the lung does not admit air; that it has become solid from having been permeable. But its texture may be softened; its cohesion destroyed; and it may be reduced to a state of pulp and rottenness, which is irreparable.

But if its texture be *not* thus disorganized, it is yet capable of reparation; and then, the inflammation having ceased, Auscultation beautifully takes up its part again, and gives the first notice of reparation, as it gave the first notice of disease.

Crepitation again begins to be heard where there was no sound; at first in a small space, and then more extensively; and then some vesicular breathing is mixed with it; and the respiratory murmur and the Crepitation seem as if contending with each other for the mastery, until the respiratory murmur is predominant; and then all is well.

And what is going on all the while within the structure of the lungs? Even this. The lymph within and around the pulmonary vesicles is gradually absorbed, and the air gradually finds admission within them. At first, it is impeded by the extravasated fluid it meets with in its passage; but as the permeable texture of the lungs gets disentangled and set free, it glides through them unobstructed and alone, and with the genuine murmur of health.

LECTURE X.

ON THE DOCTRINE OF SYMPTOMS.

BRONCHIAL RESPIRATION AND BRONCHOPHONY.—DRY SOUNDS—WHERE AND HOW THEY ARE PRODUCED—INCIDENT TO SEVERAL DISEASES—PECULIAR TO NONE.—THEIR ANALOGY TO CERTAIN SOUNDS OF THE HEART IN THE MODE OF THEIR PRODUCTION.—ESTIMATE OF THEIR VALUE AS DIRECT SYMPTOMS.—THEIR VALUE RELATIVE, NOT ABSOLUTE—AND LITTLE OR GREAT, ACCORDING TO CIRCUMSTANCES—AS SEEN IN PHTHISIS, IN PNEUMONIA, IN PLEURISY.

Upon the subject of Auscultation, hitherto we have only considered certain sounds occurring during the act of respiration, and have endeavoured to estimate the amount of the information they furnish concerning diseases of the lungs, both by themselves and by their interference with the respiratory murmur. And we have found that arising, as they do, from certain pathological conditions of the mucous membrane which lines the air passages, they become *direct* symptoms of all those pulmonary diseases into which such conditions enter as ingredients.

The sounds which I have called Rhonchus and Sibilus, and large and small Crepitation, in the sense the terms bear at this hospital, and which others have called by other names bearing an equivalent sense, these sounds cannot carry us further in the diagnosis of pulmonary diseases than I have pointed out. Other sounds are required to detect pulmonary diseases consisting of other pathological conditions, or occupying other structures.

Of the other sounds, some still respect the respiration, some the voice, and some the act of coughing. There are what are called the Bronchial Respiration and the Bronchial Voice, or Bronchophony; and as Bronchial Respiration and Bronchophony will always be found to denote the same thing, I will consider them together.

When there is Bronchial Respiration you hear the breathing, and when there is Bronchophony you hear the voice, as you never hear them when all is healthy. In health, the respiration reaches the ear through the chest, in a clear, smooth, uniform murmur. In health, the voice does not reach the ear at all through the chest, except when it is applied just opposite the first divisions of the trachea—viz. on either side the second and the third dorsal vertebræ. But in certain conditions of disease you perceive at particular situations of the chest, while the patient breathes, audible *gusts* of air puffed in and puffed out of the lungs, instead of the smooth respiratory murmur; and at the same situations, while the patient speaks, a sort of humming or muttering, but no articulate word. Such is Bronchial Respiration, and such Bronchophony.

Now these sounds, accompanying the respiration and accompanying the voice, are called Bronchial, because they are formed in those first divisions of the air-tubes which are technically called Bronchi. But the Bronchi themselves are not in fault; they need not themselves have undergone any disease or change of structure whatever, in order to the production of these sounds.

Bronchial Respiration, or Bronchophony, arises when the lungs have undergone such changes of condition as are calculated to render them better conductors of sound than they are in their natural and healthy state.

Now there are so many diseases and so many morbid processes involved in, or contingent upon, those diseases, which have the common effect of rendering the lungs more solid, and thus augmenting their capacity of conducting sound, that Bronchial Respiration and Bronchophony cannot be diagnostic of any one in particular. The lungs are consolidated in phthisis by tubercles; in pneumonia, by effused lymph; in pulmonary apoplexy by effused blood; in hydrothorax and empyema, by compression, from without, of accumulated serum and of pus; —and in each of these affections, Bronchial Respiration and Bronchophony are apt to occur. But they are not properly diagnostic either of phthisis or pneumonia, of pulmonary apoplexy or pleurisy.

In going round the hospital, I have often taken occasion to point out to you cases of phthisis, in which the sounds of the

heart were audible over the entire chest, or a considerable part of it. With the sounds themselves, their kind, their rhythm, their succession, no fault was to be found; only they were heard beyond their natural limit, and were perhaps a little louder than natural. The impulse of the heart, in the meantime, has not been remarkably strong; perhaps it has been remarkably feeble, and the general state of the circulation has not indicated disease.

In such cases, where, during life, the sounds of the heart have been heard thus constantly and habitually beyond the præcordial region, I have found, upon examination after death, that the organ itself has not exceeded, and often that it has fallen short of, the natural and average dimension.

When the heart is perfectly healthy, it must depend upon conditions exterior to itself, if the sounds which accompany its contraction be heard not only in the præcordial region, but both there and beyond it. In the cases I have mentioned, it depended upon the condition of the lungs; which, being rendered more solid by the tubercular matter within them, and becoming better conductors of sound, conveyed the sounds of the heart extensively through the chest.

In particular instances of phthisis, where the patient has been long under my observation, I have sometimes remarked that the audible limit of the heart's sounds has varied from time to time, according to circumstances, which seemed to me not difficult to explain. During the first stages of the disease, and in proportion as the lungs have been more and more beset with *crude* tubercles, they have reached further and further beyond the præcordial region; during the later stages, and in proportion as the tubercles have been more and more changed into vomicæ, they have receded more and more within their proper bounds.

And not in Phthisis only, but in other and curable diseases within the chest, are examples found of the sounds, which accompany the heart's contractions, being conveyed beyond the præcordial region, while the lungs are rendered more solid by the various products of morbid action; and again receding within that region when reparation has taken place, and the lungs have again become pervious and free. Pneumonia, pulmonary hæmorrhage, and pleurisy, will often, during their progress, conduct the sounds of the heart over half the chest;

and the cure of pneumonia, pulmonary hæmorrhage, and pleurisy, will often bring them back again within their just limits.

In like manner, and for the same reasons, and under the same conditions of disease, do the *bronchi* acquire a resonance, or a voice, which in nowise belongs to them when the surrounding structures are perfectly healthy.

Such I believe to be the true account of Bronchial Respiration and Bronchophony. They are dry sounds, according to the explanation already given, being not produced by the mingling of air with fluid.

It now remains to determine the value that belongs to Bronchial Respiration and Bronchophony, as auscultatory Signs. If they be taken *absolutely* and *alone*, their value cannot be rated very high; for, inasmuch as they do not attach themselves to any single morbid process, but result from conditions that are common to many, they can never be *exclusively* trusted for the diagnosis of disease; yet they may be trusted very often for lending important aid towards it. In truth, their value consists rather in what they contribute as auxiliaries to other signs than as standing alone.

As auxiliaries, their value varies very much, according to the circumstances of the particular case, and as other auscultatory signs present are more or less precise, and stand less or more in need of that confirmation which these are capable of contributing.

Bronchial Respiration and Bronchophony are worth very little in Phthisis, when Gurgling Respiration, and Gurgling Cough, and Pectoriloquy, have already put their authentic stamp upon the disease.

They are worth very little in Pneumonia, when the respiratory murmur has been gradually overcome by small Crepitation, and small Crepitation been succeeded by dulness, and no sound is now elicited by percussion, or yielded to the listening of the ear.

They are worth very little in Pleurisy, when already, over one half the chest, the ear detects no respiratory murmur, and percussion can produce no Resonance; when the heart is pushed from its proper seat, and the patient is fixed on this side or that by a dread of suffocation if he move to the other.

Yet it may happen in these several diseases, that the Bronchial Respiration and the Bronchial Voice may throw just that light which is needed upon a number of doubtful symptoms, and give just that guidance which is required to a right diagnosis.

Phthisis may exist; but you cannot certainly pronounce that it exists, though the flesh may waste and the strength decline, and the pulse be habitually more frequent than natural, and the breathing be habitually a little hurried, and some cough be habitually present without expectoration; from all these circumstances you cannot pronounce the disease to be certainly phthisis; you still need the help of some auscultatory Sign to decide your diagnosis. But a little help is *now* enough. If the Bronchial Respiration or the Bronchial Voice be *now* added, and be always present, and always found in the same place, you may securely trust either one or the other as the sure index of phthisis.

Yet bronchial respiration and bronchophony are not *absolutely* diagnostic of tubercles or vomicæ, or of any morbid process essentially phthisical. But it is enough that in this case they are *circumstantially* diagnostic; for as such they are infallible.

Pneumonia may exist; but you can have no sure evidence of its existence, though fever be present, and Small Crepitation be more or less diffused through the lung. But if to this Crepitation Bronchial Respiration or Bronchophony be added, these, which are but indirect symptoms of condensation of the lung, are, *under the circumstances*, as certain tokens of such condensation having already begun, as if the chest at some part already yielded no sound to percussion; and none, either healthy or morbid, to the application of the ear. Small Crepitation is, indeed, the auscultatory sign of inflammation in the smaller bronchial ramifications; yet as long as this is the *only* auscultatory Sign, there is a hope that the inflammation may pass away without involving structures beyond them. But Bronchial Respiration and Bronchophony *are* enough to show that it has involved other structures, and that air is admitted less freely into the pulmonary vesicles.

In cases of Pneumonia, Bronchial Respiration and Bronchophony will sometimes precede, by no inconsiderable period,

those auscultatory Signs which more directly declare an impervious state of some portion of the lungs. And in cases of Pneumonia, Bronchial Respiration and Bronchophony will often exist at one part of the lungs, while Crepitation still exists at another: whereas dulness to percussion and to auscultation do not arise until the Crepitation has ceased, and then are found exactly in those situations where the Crepitation was before. Bronchial Respiration and Bronchophony seem to denote the growing incapacity of the general pulmonary vesicles to admit as much air as they ought: dulness to percussion and auscultation show the exact portions of the lungs that are absolutely impervious.

Pleurisy may exist. Fever, dyspnœa, and pain in the side, are enough to create a strong suspicion of it; a *suspicion*, however, and no more. But only let the least auscultatory sign be added, and the suspicion becomes a certainty. Bronchial Respiration or Bronchophony, is as much as you want. One or the other, found in any part of the affected side, and known not to have been there before, will at once fix the character of the disease. They show *absolutely* that the lung is beginning to suffer compression; and they show *circumstantially*, i.e. by their union with fever, dyspnœa, and pain in the side, that the compression in the present case is from fluid effused into the cavity of the pleura, which is the first effect of its inflammation.

But wait a little in this case; and if the inflammation be not arrested, and if the effusion within the pleura increase, the Bronchophony will be attended by a peculiar echo. And this echo (if echo it be) is the very pathognomonic sign of Hydrothorax. It has been well likened to the bleating of a goat, and therefore called Œgophony. But it is a sound that admits of varieties, and has been denoted with equal propriety by several similitudes.

Wait yet a little longer; and if the fluid still increase, the Bronchial Respiration, the Bronchophony, and the echo, and every the least perceptible sound, will cease.

Finally, concerning Bronchial Respiration and Bronchophony, if it be thought that they are things far too trivial to bear out such vast conclusions as, in one case, that the lungs are beset with tubercular matter, and the disease is Con-

sumption; in another, that inflammation, either in itself or in its products, has travelled beyond the bronchial ramifications, and the disease is Pneumonia; and in another, that inflammation has fallen upon the pleura, and Hydrothorax is already begun, I repeat, that this mere Resonance of the Breathing and the Voice within the larger bronchi, is diagnostic of these diseases, not in itself, but *circumstantially*, and in the relation it bears to other symptoms.

And thus we see, in the daily practice of medicine, that things in themselves mean and of no account, do often, by their place and by their relations, gain a just preponderance over things more prominent and striking, and have the largest share in guiding our decision upon the most important questions.

LECTURE XI.

ON THE DOCTRINE OF SYMPTOMS.

CAVERNOUS RESPIRATION AND PECTORILOQUY.—DRY SOUNDS—WHERE AND HOW PRODUCED.—CAVERNOUS RESPIRATION HAS MANY MODIFICATIONS.—WHENCE THESE ARE DERIVED.—THEY CANNOT AND NEED NOT BE CHARACTERIZED BY NAMES.—CONDITIONS MOST FAVOURABLE TO PECTORILOQUY.—GURGLING RESPIRATION AND GURGLING COUGH.—MOIST SOUNDS—HOW AND WHERE PRODUCED.—THESE FOUR AUSCULTATORY SIGNS, CAVERNOUS RESPIRATION AND PECTORILOQUY, GURGLING RESPIRATION AND GURGLING COUGH, ARE CHIEFLY CONCERNED IN THE DIAGNOSIS OF PHTHISIS.—ESTIMATE OF THEIR VALUE AS DIRECT SYMPTOMS IN EACH STAGE AND FORM OF THIS DISEASE.

But there are other auscultatory Signs which respect the respiration and the voice; those, namely, which are called Cavernous Respiration and Pectoriloquy. I will consider them together, as I did Bronchial Respiration and Bronchophony, because they too will be found to signify the same thing.

We call it Cavernous Respiration when, during the act of breathing, a hollow sound reaches the ear through the walls of the chest from some circumscribed space within. And we call it Pectoriloquy, when, during the act of speaking, the articulate words that are uttered reach the ear through the walls of the chest from a circumscribed space within.

Cavernous Respiration and Pectoriloquy both result from a cavity formed in the lungs, and communicating with the bronchi. They are, in the sense already explained, Dry Sounds—*i.e.* not requiring the presence of fluid for their production.

In Cavernous Respiration the hollow sound probably does not begin to be formed until the air enters the cavity, and results altogether from reverberation against its sides; whereas in

Pectoriloquy the articulate sound is first formed in the larynx, and then conducted down the trachea, and, entering the cavity, is merely augmented by reverberation against its sides.

I am convinced that the whole subject of Auscultation would have been better understood if a little less artifice had been used in the methods of teaching it—a little less industry to find a name for every sound that is heard. This very sound of Cavernous Respiration has been puzzled and perplexed to the student by the fancy of giving an express name to each of its modifications.

There may be, perhaps, now in the hospital, a dozen patients who have Cavernous Respiration; and in each one of them the sound, besides being cavernous, has some distinct peculiarity; it is large or it is small; it is a click, or a hum, or a squeak. It is like blowing into a bottle with a narrow neck, or into one with no neck at all; it is like the flapping of a valve; it is metallic; it is as if air was puffed *into* your ear, or as if air was drawn *from* it.

It would be easy enough to agree upon a dozen terms to designate the dozen different sorts of Cavernous Breathing in the patients now in the hospital; but then the next dozen patients would require as many new terms for as many new sorts of Cavernous Breathing, which would still be different.

The varieties of Cavernous Breathing are doubtless owing to different sizes and forms and situations of cavities, and to different conditions of the surrounding lung.

A cavity may be very large or very small. Several bronchi may open into it, or only one. It may be a simple cavity, or it may have many chambers. Its sides may be condensed and equal, or rough and ragged. The lung around it may be solid and indurated, or pervious and vesicular. It may be near the ribs, or far from them; adherent to, or separate from, the pleura.

It is quite obvious that these different circumstances are calculated to modify the sound, which will, nevertheless, be always such as indicates a cavity.

Would you know what Pectoriloquy is? Put the tube upon the larynx or trachea of a healthy man, and, when he speaks, his voice will seem to come through his throat, and pass directly up the instrument into your ear. Just in this

manner does the voice reach the ear through the chest, when the conditions within are favourable to Pectoriloquy.

The conditions *most* favourable to Pectoriloquy are, that the cavity should have dense walls, that it should be near the surface of the lung, and that, by mutual adhesion of the two pleuræ, the walls of the chest should themselves contribute to form it, and that the cavity itself should be empty. Thus a cavity that produces Cavernous Breathing may still not produce Pectoriloquy. It must not be too small, or it will not allow sufficient reverberation to the voice; it must not be too large, or the voice will be lost in an indefinite hum. Thus there may be a cavity without Pectoriloquy, but a cavity can scarcely exist without Cavernous Respiration.

There is yet another auscultatory Sign, which belongs to the respiration. It results from the mingling of air with fluid in the act of breathing; but it is heard in a circumscribed space, and evidently proceeds from a much larger quantity of fluid than is capable of being accumulated in the mere bronchi within that space.

The French call it Gargouillement; we call it Gurgling. It is essential to this sound that there be a cavity, and that the cavity contain fluid.

The Gurgling sound of the Respiration is exactly like that which a boy makes when he blows up soap suds with a tobacco-pipe.

Allied with the Gurgling Respiration, and requiring the same conditions to produce it, is the Gurgling Cough. I wish, however, I had another term for the sound which attends the act of coughing: for it is certainly different from the gargouillement which attends the act of breathing. If you place your ear upon the chest, immediately over a considerable cavity containing pus, or any fluid that admits air to pass through it, and tell the patient to breathe rather deeply and rapidly, you will hear the sound of a hundred great bubbles agitating the fluid and bursting in continual succession. But if you place your ear upon the chest, and tell him to cough, the sound is as if the whole contents of the cavity struck against your ear at once. The one sound is a *gurgling*, the other a *plash*. I wish Plash was not such an awkward word; for it expresses the very thing.

In the effort of coughing, more air penetrates the cavity. It goes in and out of it with a rush. Moreover, it dwells longer in it, and while it is there the whole chest suffers a succussion. Thus, by the act of coughing, not only are the air and the fluid mingled and agitated together within the cavity, but the cavity itself is shaken, like a bottle, against the ear.

This audible Plash of fluid is certainly produced during coughing, as well by the general succussion of the chest which then takes place, as by the forced impulse and agitation of air within the cavity itself. Indeed, succussion alone will produce it; and succussion occasioned by other means than by coughing. Very often, when the cavity has been close to the walls of the chest, and the patient has been much emaciated, after I have heard its contents Bubbling as he breathed, and Plashing as he coughed, I have desired him to hold his breath for a few seconds, and abstain from coughing if he could; and still I have heard distinctly a smaller sound of the same kind, a Plash synchronous with the pulse. The sound is doubtless owing to the motion derived to the same cavity from the impulse of the heart, or large blood-vessels.

And now for the practical application of the several auscultatory Signs; Cavernous Respiration and Pectoriloquy, Gurgling Respiration, and Gurgling Cough. To illustrate their diagnostic use I will take only one disease, but one which includes many and various morbid changes in the structure of the lungs, and see how far these signs, either by themselves or as auxiliaries to others, enable our knowledge to keep pace with such changes, as they arise. That disease shall be Pulmonary Consumption.

Let us consider Pulmonary Consumption in the stage of its first development, its most uncertain, but its most fearfully interesting stage. An individual is suspected to be phthisical: he has some fever, some acceleration of pulse, some emaciation, and some cough; all inconsiderable in degree, yet all abiding; but no expectoration.

In a patient thus suspected to be phthisical, Auscultation may discover no more than this; that beneath the clavicle and about the scapula the respiratory murmur is less clear on one side than on the other, and that, where the murmur is defective, there, too, the chest is less resonant to percussion.

Now if, after repeated examinations, Auscultation comes always to this result, no doubt can remain that Tubercles are already formed in the upper lobe of one lung.

But here is no unnatural sound, only the natural sound is in part defective; and this must arise from some impediment to the passage of air through that portion of the lungs. Now impediment may arise from the deposition of lymph, or any of the common products of inflammation, as well as from tubercular Matter. But inflammation is very unapt to take place, and its products to be effused into the apex of an upper lobe, while every other part of the lungs remains unaffected by it. It may ultimately reach this situation, but seldom, very seldom, begins in it. On the other hand, it belongs to phthisical disease to deposit Tubercles in the upper lobes first, and thence gradually to scatter them over the rest of the lungs.

Thus, by help of Auscultation, but still rather by what we do *not* hear, than by what we *do*, we arrive as surely at the persuasion that there are Tubercles in a certain case, as if there were sounds properly denoting their existence.

Always bear in mind that there are no auscultatory Signs which expressly bespeak Tubercles. You are left to get at the knowledge of their existence by that sort of evidence which has been called circumstantial; Auscultation, however, having an important share in the result. As thus, Auscultation finds the respiratory murmur defective at a certain part of the lungs; and hence we infer its obstruction by the deposition of some kind of matter or other. But the part is that which nature chooses, above all others, for the deposition of tubercular Matter; and hence we further infer that the Matter is tubercular in this particular instance. But, moreover, the constitutional symptoms are such as are wont to accompany phthisical disease; and hence we finally infer almost a certainty that Tubercles are deposited at the upper part of one lung. We conclude that the thing must be, because it *can* be nothing else. Circumstantial evidence, it is acknowledged, may be as infallible as the evidence which bears direct attestation to the simple fact.

I have been speaking of the auscultatory Signs that we possess of consumptive disease, when it has proceeded no further

than the deposition of Tubercles (crude Tubercles) at the apex of one lung.

In proportion as Tubercles are more largely deposited in one lung, or extend to both, the auscultatory signs are the same in kind, but more definite; and the portions of lung that are healthy are more strikingly contrasted with those that are diseased. The healthy part, being called upon to compensate by a more energetic respiration for the absence of respiration in the diseased, gives out a louder and more puerile murmur, while the diseased gives out no murmur, or almost none at all.

Recollect, I introduced the subject of Auscultation for the sake of illustrating to you the best means we have of knowing organic disease by direct symptoms. But, if there be any Signs, not auscultatory, which either give direct intimations of disease in the same organ, or tend to give more force and precision to the auscultatory, it is fit that I should introduce them as I go along.

At no period of Consumptive disease is hæmoptysis more apt to take place than at this which we are now considering, when the lungs are beset by crude Tubercles. And the hæmoptysis illustrates the auscultatory Signs, and the auscultatory Signs illustrate the hæmoptysis.

The cases are numerous in the course of a twelvemonth which I see in this hospital and elsewhere, of hæmoptysis occurring in every quantity, from a tea-spoonful to a pint, in individuals whose general health has been previously declining, but who have had no particular complaint except (what they have called) a little hacking cough.

The great majority of such cases, as far as the mere hæmoptysis is concerned, do well. In a few days the hæmoptysis entirely ceases, and very often the patients tell you that they are better since they spat blood than they were before. It is my habit, however, to keep such patients still in the hospital for a week or a fortnight, to satisfy myself respecting the actual condition of their lungs, as far as I can learn it by Auscultation.

And Auscultation gives the same results which have been described. Respiration is unequally performed in different portions of the lungs. There is less respiratory Murmur per-

ceptible about the scapula, or beneath the clavicle, on one or both sides, and less resonance upon Percussion, than elsewhere.

I would invite your attention to all such cases as these, whenever you meet with them in the hospital. I recommend you particularly to exercise yourselves, whether by Auscultation or otherwise, in detecting the signs of Tubercles deposited in the lungs at the earliest possible period: because it is *then* especially that a sound knowledge of the real state of things may enable you to do infinite good; by seasonable, and (I will add) practicable advice to postpone the progress of disease, and protract many a valuable life.

I do not wish you to fasten on small points, and swell them into importance, and by refining and sophisticating to make something out of nothing at all, and frighten families, and deceive yourselves into a belief that you have cured Consumption.

The cases I am pointing out are those in which there are well-defined constitutional symptoms—fever, wasting of the flesh, acceleration of pulse. These denote something going on wrong somewhere. The cough fixes suspicion upon the chest; and the chest being examined gives such results as I have mentioned.

Let us now consider Pulmonary Consumption in other stages. In all stages, subsequent to the tubercular, the auscultatory Signs are paramount and unerring, and you may implicitly trust to them for your entire knowledge of each and every other morbid change and process incident to the structure of the lungs from Pulmonary Consumption.

The earliest and the least, but still a very authentic sign of Vomica, derived from Auscultation, is a mere Click, or slight ringing sound, heard in breathing, at some point beneath the clavicle or about the scapula, in a patient in whom all the surrounding parts have been for some time dull. This Click, to remove all suspicion of its being owing to the accidental lodgement of a piece of tough phlegm in one of the bronchi, must always be found at the same point at several examinations of the chest. It is one modification of Cavernous Respiration. It results from a cavity or Vomica in its first formation, when the tubercular Matter is softening, and it is just beginning to admit air.

Those who have been accustomed to attend me for any length of time in my visits round the hospital, must know how often I have pointed out this particular sign, this little Click in the breathing, and desired them to listen to it day after day, until they gradually found it change into a sound of a more certain character.

Where there is a Cavity, in the progress of its enlargement and of the changes it undergoes, its auscultatory Signs are to be sought in Breathing and Coughing and Talking. Breathing will give them in one case, Coughing in a second, Talking in a third. Or often in the same case, whether the patient breathe, or cough, or talk, the evidence of a Cavity within the lungs is equally authentic.

Thus the Respiration, the Cough, and the Voice, may all give equal assurance that Vomicæ exist; one confirming the other, and all agreeing in the same result.

But in particular cases, from the situation of the Vomica, from its size, from the kind and quantity of its contents, or from the state of the surrounding lung, Auscultation gives sometimes less and sometimes more notice of the disease by the sound conveyed in one of these actions than another. One is needed to supply the symptom which another does not give.

Some time ago I was desired to pronounce upon the nature of the disease, in a gentleman who was affected in this manner. He had suffered a long and abiding hectic, and had reached a state of extreme emaciation, but had a very slight cough, and expectorated only one large globule of yellow heavy matter once a day, immediately after he woke in the morning. His little cough, his little expectoration, and his ability to inflate his lungs freely and deeply, encouraged a hope that he still might not have Consumption, his abiding hectic and his extreme emaciation notwithstanding.

I examined the chest, and found the respiratory Murmur clear and loud, and vesicular. In the act of breathing there was no unnatural sound, either Cavernous or Gurgling, anywhere.

Having learnt thus much, or, rather, having puzzled myself thus far, I was interrupted in my further examination by some accident, and I postponed it until the next day.

The next day I could get no more information from the mere Breathing, except that, upon the whole, the air entered more freely into one lung than the other; the other, however, not wanting the Vesicular Murmur in any part. Neither from the Voice could I get more information; it was neither Cavernous nor Pectoriloquous. Percussion elicited a somewhat different sound from the space between the clavicle and mamma on one side and the other. But the sound was dull on neither side.

What, however, neither the Respiration nor the Voice could declare by any authentic sign, was made clear and manifest by the act of Coughing; viz. that there was a large Cavity, full of fluid, occupying a space in one lung between the clavicle and the mamma. For when I desired the patient to make as deep an inspiration as he could, and then to cough with all the force he was able, instantly there came Plash after Plash against my ear from the whole of this space; a sound, which could only result from the agitation of fluid in a large Cavity.

But why was there a Vesicular Murmur at this space? Probably because the Cavity, large as it was, had a considerable stratum of healthy lung interposed between it and the walls of the chest. Why was there no Pectoriloquy? These same conditions, the size of the Cavity, and the intervention of healthy lung between it and the walls of the chest, were enough to prevent it. Besides, the Cavity was full, and thus was unfavourable to Pectoriloquy.

And why, above all, was there no Gargouillement, no Gurgling sound in the respiration, and little or no expectoration? The air during ordinary respiration might not have free access to the Cavity. The Cavity was there, but there might be no considerable bronchus entering it. Or, what is most probable, a considerable bronchus or bronchi entered it, but were obstructed by some obstacle, from within or from without, before they reached it. Either hypothesis will furnish the explanation, how a large Cavity full of pus can exist in the lungs, and yet not enough of air find its way *into* it, in ordinary breathing, to produce an audible agitation of its contents, and not enough of matter find its way *out* of it to furnish more than a scanty expectoration. In this case it

took the whole night, and the continual oozing of pus by some narrow passage from the cavity into the bronchi, to accumulate half an ounce ready to be expectorated in the morning.

So much for the present. But you must inquire further into the forms and stages of Phthisical Disease, if you would know the real value that belongs to the auscultatory Signs in question.

LECTURE XII.

ON THE DOCTRINE OF SYMPTOMS.

FORMS OF PHTHISIS COMPARED WITH KINDRED FORMS OF DISEASE IN EXTERNAL PARTS.—UNMIXED PHTHISIS.—MIXED PHTHISIS.—UNMIXED PHTHISIS COMMONLY OF LONG DURATION.—SOMETIMES LINGERING IN ONE STAGE, AND RELUCTANT TO PASS BEYOND IT.—SOMETIMES PASSING QUICKLY THROUGH ALL ITS STAGES, BUT OCCUPYING SMALL PORTIONS OF THE LUNGS IN SUCCESSION—CEASING IN ONE AND BEGINNING IN ANOTHER.—GENERAL AND AUSCULTATORY SIGNS OF UNMIXED PHTHISIS.—IS PHTHISIS CURABLE?

I WISH to consider some important distinctions of Pulmonary Consumption, grounded on pathology, and brought to our knowledge by Auscultation during the life of the patient.

You have all seen an absorbent gland of the neck become as hard and as large as a marble, but without pain, or heat, or discoloration of the integuments; and hard, and indolent, and marble-like, it has remained for weeks, or months, or years.

This is a mere deposition of tubercular Matter in the substance of the gland.

And you have all seen an absorbent gland of the neck hard and large, and without pain, or heat, or discoloration of the integuments, for a while; but presently pain, and heat, and redness, have arisen, and what was hard has become soft, and the integuments have become thin, and have ulcerated or burst; and pus has been discharged, and with it a hard nucleus of tubercular Matter; whereupon the swelling, heat, and pain, have subsided, and the parts have been restored without any remaining mark of injury, save a slight scar.

This is a deposition of tubercular Matter followed by inflammation in the substance of the gland. But the inflammation is restricted almost, if not altogether, to the gland

itself; and it has no sooner done its work of eliminating the tubercular Matter, than it ceases entirely.

In like manner you have seen many glands of the neck remain hard and indolent, or all or several of them go on to inflame and suppurate simultaneously, or in succession. But the inflammation and suppuration have not continued longer, nor extended further, than was needful for the purpose of eliminating the tubercular Matter.

There is (what is called) the *specific* limit of a disease. By this is meant the limit proper to its local morbid action, which, for any purpose it has to accomplish, it never need to transgress.

Thus, in the instances alluded to, the specific limit of the disease was strictly preserved; for if the tubercular Matter was to be evacuated, no less degree of inflammation could have succeeded in bringing it to the surface.

But in such tubercular Affection of the cervical glands, the disease may spread beyond its specific limit. It may give occasion to inflammation both more severe and more extensive than is needed for the mere elimination of the tubercular Matter; to inflammation pervading the whole neck widely and deeply, and accompanied by diffused redness, and swelling, and pain; the whole subcutaneous cellular structure, between the angle of the jaw and the clavicle, being loaded with effused serum and blood, and numerous apertures dripping with pus.

And all this inflammation, with its destructive processes, is engendered and spread abroad from a mere nucleus of tubercular Matter in a few absorbent glands. Yet in another case this same tubercular Matter lay indolent and harmless, neither the constitution nor the part feeling any apparent inconvenience from it. And in another case it created just inflammation enough (and no more) to produce a process of ulceration which might bring it to the surface.

Behold here, upon the surface of the body, that very Disease which in the Lungs constitutes Consumption! Behold *here* transacted before your eyes the same morbid changes and processes which (allowance being made for difference of structure) are *there* transacted within reach of the ear!

There are cases in which Pulmonary Tubercles abide long, and, perhaps, never suppurate, or at a very late period; and there are cases in which Pulmonary Tubercles excite around

themselves just enough of inflammation and suppuration to procure their own solution or evacuation, and no more; and again, there are cases in which Pulmonary Tubercles produce and spread abroad inflammation of every degree and every extent throughout the lungs, beyond what is necessary to produce their own solution or evacuation. And these cases are to be distinguished from one another by Auscultation. And their distinction is of vast practical importance.

Tubercles and Vomicæ are the specific part of Pulmonary Consumption. If you do not detect one or other of these, you cannot pronounce the patient to be consumptive. The auscultatory Signs which denote their existence have been already enumerated.

I shall take no notice of those cases in which a few Tubercles are deposited here and there in the lungs, without any auscultatory Sign of their existence. We find them after death, where they were never suspected during life, and where the entire lungs besides are perfectly healthy.

There are no auscultatory Signs which expressly denote Tubercles. Nevertheless, I have shown that Tubercles may be detected almost infallibly by circumstantial evidence, to which Auscultation essentially contributes. But, before they can be thus detected, Tubercles must be so largely deposited in some part of the lungs, as to impede perceptibly the passage of air through it.

Consumption is perpetually presenting itself to me in this form. An individual loses the complexion of health, and becomes thin; he coughs a little; but perhaps he has no notable fever, and no constant acceleration of pulse. I auscult his chest, and find a Dulness beneath one or both clavicles, or about one or both scapulæ, and a free respiratory Murmur through every other part of the lungs. Here there is no disease beyond Tubercles; and while they occupy the upper lobe, the whole lungs besides are without a vestige of disease.

This form of Consumption may endure for years and years, the auscultatory Signs continually denoting the same thing, and the patient getting neither a bit better nor a bit worse in the meantime. But he is a wretched invalid, and finds that there is something continually incapacitating him for the severer business of life.

To such a person it is a continual puzzle why he does not get well. He consults an infinite number of medical men; and it is remarkable that he gets no comfort or satisfaction from those who understand his disease the best, and the greatest comfort and satisfaction from those who understand nothing about it. Those, who know what it is, out of kindness do not tell him the truth, and they cannot asseverate a falsehood stoutly enough to carry any weight with it; whereas those who know nothing about it affirm boldly and unhesitatingly that *it is all stomach*, really believing that the whole and sole disorder is in the stomach, and that it is within the reach of an easy cure.

Surely Auscultation is so essential a help for arriving at the truth in such a case, that they who are skilled in the use of it always agree as to what the truth is: and, indeed, there is no wonder in *their* agreement: the wonder is, that they who do not arrive at the truth should so constantly agree in adopting the same fallacy. I have been somewhat curious in my inquiries concerning this matter, and the constancy with which I have found the whole malady imputed to the stomach has appeared to me very strange. There is, however, a circumstance in the history of these cases which gives a colour of truth to this opinion. The state of the bowels is very frequently such as to demand the continual use of purgative medicine; and the cough often comes on, and with it a kind of asthmatic breathing, soon after dinner; and both continue as long as the stomach is distended with food.

In this form of Chronic Consumption spittings of Blood are apt to take place occasionally; and, when they do, they must give fearful intimations of disease of the lungs to those who are not yet assured of it by Auscultation. But I have known *them* also imputed to the stomach.

In this form of Chronic Consumption abscesses are apt to occur by the side of the rectum, and to degenerate into fistulous sinuses.

But in this form of Consumption Vomicæ are not postponed *indefinitely*: they at length are formed, and from that time the patient sinks rapidly. Often, when a fistulous sinus has been cured by an operation, and the long abiding discharge from it abolished, an expectoration of pus will occur for the first time, and never afterwards cease.

From the first formation of Vomicæ the patient sinks rapidly. In Pulmonary Consumption, characterized by the length of its Tubercular stage (if I may so call it), and by a seeming reluctance to pass on to the formation of Vomicæ, when, after several years, Vomicæ do ultimately take place, it is often in great numbers simultaneously, or in very quick succession; so much so, that a lung which two or three weeks ago was, in a great part, dull to Percussion, and yielded no sound to the ear but Bronchial Breathing or Bronchophony, will *now* give the clearest auscultatory Signs that it is literally riddled with Cavities; and not only so, but, if the patient survive a little longer, that many Cavities have run together, and a multitude become one. The same simultaneous Gurgling when the patient breathes, and the same simultaneous Plash when he coughs, will reach the ear from half one side of the chest.

It is remarkable how to the very last the sounds are *often* properly and exclusively those of *Phthisical* Disease, or rather those which it belongs to the essential conditions of Phthisical Disease in the lungs alone to produce, and *those sounds only*. There are Cavernous Breathing, or Gurgling Breathing and Gurgling Cough, or Pectoriloquy; and in whatever parts of the lungs you have not these, if you have any sound at all, it is the vesicular Murmur of Health.

Nothing is more common, upon dissection, than to find the lungs most largely beset with Tubercles and Vomicæ; and at the same time every part of them, which a Tubercle or a Vomica does not absolutely occupy, altogether healthy.

Such is one form of Pulmonary Consumption; and it would seem to be, in many striking circumstances, distinguishable from others. I may fairly wish that I had a less accurate knowledge of it; for that knowledge first came to me from observing its symptoms in two of my most valued friends, and from watching in them, year after year, the sure but hesitating approaches of death.

But Consumption is perpetually presenting itself to me under a different character. The patient will live as long as he whose disease is slow to advance beyond the stage of mere Tubercles. His condition, however, is different; and that condition varies more from time to time: he will spit for a while considerable quantities of pus, and then cease from expec-

torating altogether. He will suffer hectic fever, and then throw it off, and then suffer it again; lose his flesh, and recover it, and then lose it again. Here, if you auscult the chest, you will find Cavernous Respiration or Pectoriloquy, a Gurgling Respiration or a Gurgling Cough at the apex of one or both lungs, and at every other part a clear vesicular Murmur.

These are the cases in which Pulmonary Tubercles excite around themselves just enough of inflammation and suppuration to procure their own solution or evacuation, and no more. The Phthisical Disease is carrying on its own specific processes within its own specific limits. It is depositing tubercular Matter, and then maturating, and softening, and evacuating it; and the result is the formation of a Vomica. But, except in the seat of the Vomica, the whole lung still remains healthy.

A very dear friend of my own was twelve years dying of Consumption; and another individual was twenty. They had expectoration, and hectic fever, coming and going during twelve and twenty years; but they died before the days of Auscultation, and, therefore, the exact condition of the lungs at different periods during the progress of their disease was not known. I know a man now living, who occasionally spits blood and pus, and who has occasionally spit blood and pus during the last twenty years. At various times during the last four years Auscultation has discovered a Vomica or Vomicæ at the apex of one lung, but, withal, a satisfactory Vesicular Murmur in other parts. This individual, in what regards eating and drinking, has lived a life of abstinence, but a life of great toil in what regards exertion of body and mind. Sometimes his friends are full of apprehension about him; his hectic fever, his emaciation, his cough and expectoration, seem preludes to the worst event; but again he rallies, and his mind and his body recover, or seem to recover, their wonted powers.

But in this form of Pulmonary Consumption, a time arrives at which there is no more resumption of the appearance or reality of health, no more pausing between (as it would seem) the formation of one Vomica and another. The hectic, the cough, the expectoration, continue; the emaciation increases; the strength declines; and Auscultation has no longer

to seek the Gargouillement, the Cavernous Breathing, or the Pectoriloquy, in one spot, but finds them at all times anywhere between the clavicle and the mamma, or anywhere about the scapula on one or both sides.

Here, too, however, it is remarkable, as in the other form of Consumption, that the Vesicular Murmur of health is often heard to the last in all parts of the lungs besides; and upon dissection, that all parts are often found healthy which a Tubercle or Vomica does not actually occupy.

The difference between the present form of Pulmonary Consumption and the former is this—that the former lingered long in the tubercular stage, tubercular Matter continuing to be deposited year after year, but no Vomica occurring, until, at a very advanced period, many were formed simultaneously, or in quick succession, and hurried on the patient to dissolution with great rapidity; whereas in the present, the Vomica, and Vomica only, is the object recognised by Auscultation. Tubercle must precede it. But the Tubercle is hardly deposited before the process of softening and evacuating it arises, and a Vomica is the result. Thus Tubercle is formed after Tubercle (as it should seem), with some interval of time between, and Vomica after Vomica; but the Vomica is the more abiding morbid condition.

These are genuine and unmixed forms of Pulmonary Consumption; and I have dwelt upon them because they are so, and because I am indebted for my knowledge of them, as distinguished from others, to Auscultation.

Of these two genuine and unmixed forms of Phthisis, the first is unquestionably the most hopeless. Where Tubercles are largely deposited, and continue still to increase, and do not pass on to Vomicæ, there is never the smallest attempt towards a restoration to health—not even of a temporary or apparent restoration.

But where Tubercles arise one by one, or a few together, and this one or these few pass rapidly into the state of Vomicæ, —and where a pause ensues between each successive formation of Tubercles and Vomicæ,—then, during that pause, there is an opportunity for the powers of reparation to come into action; and, in truth, there often does arise a manifest endeavour after health,—an endeavour which succeeds so far as to recover some of its conditions, and to suspend the disease: and then,

during that pause, there is always the hope (for where disease is suspended and health is partly recovered, we cannot help hoping) that reparation may be complete, and the disease abolished altogether.

But can this be? Does Consumption *ever* admit of cure? If ever, it must surely be when it occurs in that form which we are now considering. Let us, therefore, now give a few moments to this interesting question.

A vomica certainly admits of reparation so far as not to be a vomica any longer, but not so far as to leave behind it no trace within the lungs. It leaves behind it a scar—that is, the disease ceases in the part, but the part is not restored to the exact condition in which it was before the disease began.

In examining by dissection the bodies of those who die of Pulmonary Consumption, among many existing Vomicæ we occasionally find the traces of a Vomica healed. At the apex of the lung we find an indentation, and descending from it, for half an inch or an inch, a thick perpendicular line of tough ligamentous substance. Sometimes this substance, by being pulled asunder, is discovered to contain the remains of a cavity, and sometimes not.

But what imports this reparation of a single Vomica, if so many besides still exist? A reparation of a twentieth part of the existing disease cannot be called a cure.

But in those who have not died of any pulmonary symptoms, and who were never known during their lives to have had any symptoms apparently phthisical, the same evidences have been found after death of what once was a Vomica, but no existing Vomicæ together with it.

This is a cure, or tantamount to a cure. It is as much a cure as when a single scrofulous cervical gland goes on to suppuration and heals with a scar. A single Vomica, you may say, is as much of the essence of Consumption, as a hundred; and if the morbid structure (no matter how small) in which the disease essentially consists be repaired, the disease is cured—that is, the Consumption is cured. But it was a Consumption which nobody knew to exist.

Now all this may be very fine reasoning; but it does not meet the plain meaning of the inquiry whether Consumption be curable. It is not proof enough to common sense of its

being so, that a few isolated Vomicæ, which gave no sign of their existence, should have undergone reparation.

All the world is asking us whether Consumption be curable? Indeed all the world is interested in the question: for there is hardly a family into which Consumption, sooner or later, does not enter; and when a man makes the inquiry (as it were) speculatively or indifferently, he has most likely a real practical interest in it at home. He says, "Is Consumption a curable disease?" But he *would* say, "I have a wife or a child, a brother or a sister, who is decidedly consumptive; is there the least possible hope left me that they can recover?"

To the question proposed with *such intent*, it is a mockery to answer, "Consumption is a curable disease;" because, forsooth, its entire process from beginning to end—its formation, progress, cure—may be *secretly* transacted within the body without our knowing or suspecting anything about it.

If you ask me, as a Physician, whether I have ever had experience of a perfect and satisfactory recovery taking place, where there have been all the best known *popular* symptoms of Phthisis decidedly marked, symptoms which (*as far as they go*) no Physician could possibly say were not those of Phthisis? I answer, "Often."

But if you ask me whether I have ever had experience of the like perfect and satisfactory recovery where there were all these popular symptoms, and, withal, the conditions proper to Phthisis, ascertained by auscultatory Signs to exist beyond a doubt within the lungs? I answer, "Hitherto never."

What shall we say then? How shall we answer the popular question in the popular sense, and still answer it truly? We *cannot* say that Consumption is curable; but we *can* say (and truly) that there are cases of *imputed* consumption which put on such an aspect of the *real* disease that they are with difficulty distinguished from it, yet have not its essence. These are all within the possibility of cure.

We *can* say that there are cases essentially phthisical, in which the disease is so lingering in a particular stage, that many years are often required to bring it to its fatal termination. The decline is gradual, almost imperceptible, but sure. These fall within my first description.

And we can say that there are cases essentially phthisical

(and these fall under my second description) in which the disease accomplishes its course, as it were, by parts and parcels; many times apparently beginning, and many times apparently ending, but always (as far as I see) beginning again: a year or two of health, then a year or two of disease again. Yet, upon these terms, I have known those who have passed neither a short, nor a useless, nor an unhappy life. I have known those who have so gathered up the fragments of their broken health as to make them serve for high and useful purposes, and put to shame the fewer and smaller performances of stronger men.

LECTURE XIII.

ON THE DOCTRINE OF SYMPTOMS.

SUBJECT CONTINUED.—MIXED PHTHISIS.—THE MIXED CHARACTER OF ITS AUSCULTATORY SIGNS.

I HAVE described two forms of Pulmonary Consumption, distinct from each other in certain pathological particulars, and distinct also in their auscultatory Signs. Both were specimens of genuine and unmixed Phthisis;—genuine, because they consisted of Tubercles and Vomicæ, which are the essence of the disease; unmixed, because they consisted of Tubercles and Vomicæ *only* from first to last; these fulfilling their specific morbid processes within their specific limits, and leaving all other parts of the lungs which they do not actually occupy free from disease.

These two forms of Phthisis were represented as chronic; and chronic they are generally found to be: for disease which does not easily impart irritation to surrounding structures, is usually slow in its own actions.

Observe, I am not making distinctions of Phthisical disease for purposes of mere arrangement and nosology; but I am selecting such forms of it as I find suitable to the purpose I have in hand; that purpose being to show that there is such a disease as genuine and unmixed Phthisis, discoverable by Auscultation, and distinguishable by Auscultation from *mixed* Phthisis, which is presently to be considered.

There are forms, then, of Pulmonary Consumption, which Auscultation is able to distinguish from those already described mainly in this particular circumstance, viz. that the Tubercles and Vomicæ produce and spread abroad other disease within the lungs beyond the parts which they themselves occupy, and beyond the sphere of their own specific actions; and that other disease is Inflammation. These forms are characterized during life by a mixture of the auscultatory Sounds

belonging to Phthisis, and the auscultatory Sounds belonging to that other disease; by those which denote Tubercles and Vomicæ, and those which denote effusion, of whatever kind, into the bronchial and vesicular structure of the lungs; by Dulness beneath the clavicles or about the scapulæ, or by Cavernous Breathing or Bronchophony, Gurgling Breathing, Gurgling Cough, or Pectoriloquy; and, at any or every part, where these are not found, by Sibilus or large or small Crepitation.

The phthisical disease, however, must have reached a certain point of its development, before each order of auscultatory Sounds is clearly discernible.

Hence there are cases in which the disease indeed is mixed, or of two kinds concurrently, while the auscultatory Sounds are of one kind only. The specific Phthisical disease and the common Inflammatory disease exist together; but at the same time the auscultatory Sounds present only indicate the one and not the other: and, strange to say, the sounds that *are* present often belong, not to the *primary* Phthisical disease, but to the Inflammation which is secondary and incidental to it.

Mere Tubercles, at their first formation, are capable of imparting such irritation to the whole lung, as to produce inflammation of its entire bronchial and vesicular structure, and an early abiding effusion within it; and this effusion gives occasion to its own auscultatory Signs. But the Tubercles may be so scattered through the lungs, and so little accumulated in any one situation, as of themselves to cause no perceptible obstruction to the passage of air, and consequently to give no auscultatory Signs by which they can be suspected to exist.

In the autumn of the year 1833, a young man (Thomas King) was admitted into this hospital, and remained here three months, until he died. His case is so important pathologically, and so aptly illustrative of those practical points I am now considering, that I must be allowed to dwell upon it a little at large.

He had already been ill three months before his admission. His complaint began with Hæmoptysis, which occurred to the amount of four or five ounces when he was making some unusual exertion, and continued in smaller quantities for a few days, and then ceased altogether.

At his admission he was pale and emaciated; his pulse was 140 in a minute, and of extreme feebleness. He had profuse perspirations at night; he suffered an agonizing dyspnœa, and brought up a scanty glutinous expectoration, with great effort of coughing.

Auscultation discovered at this time a clear respiratory murmur in every part of the left lung, and in the right a clear respiratory murmur, confined to its *very upper* part, and then a widely-diffused small Crepitation, which gradually faded away into absolute dulness as you approached nearer the bottom. It was not long, however, before the condition of the left lung was the same to Auscultation as the right.

I know no distress greater than that which attends the collection of viscid mucus in the lesser bronchi and vesicular structure of the lungs. There is a constant dyspnœa and ceaseless provocation to cough, and an agony, and striving to tear up from the respiratory passages something that will not come. Such distress was unmitigated in this poor fellow for three months together.

This same condition of the lesser bronchi and vesicular structure, when it results from acute inflammation, seldom lasts long; yet even for a few days the agony of the dyspnœa and fatigue of the cough are hardly tolerable. But *here* they were abiding and unaltered for three months, kept up by inflammation (if you please), but inflammation of no action and no power, where the pulse was always of extreme frequency and extreme feebleness.

No remedies that were employed at all assuaged the distress or altered the conditions of the disease within the chest; and the auscultatory Sign to the last, the only Sign, was small Crepitation largely diffused through both lungs, which, at their lower part, were almost dull. He died exhausted.

Now, long before his death, I certainly did suspect that this effusion throughout all the bronchial passages was not kept up solely by the idiopathic disorder of their lining membrane, but that something would be found elsewhere within the lungs capable of producing and maintaining it; and I certainly did conjecture that we should find Tubercles, partly from the whole malady having commenced with Hæmoptysis, and partly from not knowing what else there could be.

Dissection discovered the pleura adherent on both sides,—on the left partially, on the right universally; and the whole membrane apparently converted into a thick cartilaginous substance; the entire lungs loaded with bloody serum, and their whole texture so softened as to break down easily under pressure of the fingers. These were all the results of common inflammation, which had reached to every tissue which composes the structure of the lungs.

But dissection discovered, moreover, myriads of Tubercles, distinct, and sprinkled universally throughout the lungs; grey, and as minute as millet-seeds, in the lower lobes; yellow and of a larger size in the upper.

This is not a common specimen of disease, but it is a very instructive one, and *therefore* I have quoted it. It is remarkable, in contrast with the forms of Pulmonary Consumption which I have before described, that *here* the minutest Tubercles, not one of which had passed on to the state of Vomica, should be capable of producing inflammation, and diffusing it so generally, and maintaining it so constantly; and that these mere Tubercles should continually supply such an amount of irritation that no remedy could in the least degree abate the inflammation (otherwise perhaps curable in itself) which they were always present and ready to renew. In this case the secondary and incidental morbid processes entirely outran those which were primary and specific; and the patient perished of inflammation of the entire lungs, the Phthisical disease having not passed beyond its earliest and tubercular stage.

But, trusting to my own experience, I should say, that it was a rare thing for Pulmonary Consumption thus to kill, by producing *chronic* inflammation and *chronic* effusion into the entire bronchial and vesicular structure of the lungs, itself still remaining in the tubercular Stage, and the Tubercles not yet occasioning any auscultatory impediment to the passage of air. It is more according to my observation that, *in this stage*, or under these conditions, it should produce *acute* inflammation, either proving fatal in a few days, or requiring active treatment to prevent it from becoming so. And it is still more according to my observation, that in this stage, or under these conditions, it should produce Hæmoptysis; which also may be called acute, being accompanied by fever and much vascular

action, and liable, too, to prove fatal in a few days, or requiring active treatment to prevent it from becoming so. In the fatal cases the unsuspected Tubercles are only discovered after death.

Yet attacks of acute Inflammation or acute Hæmoptysis from such a cause are not usually fatal: but, the cause still remaining, they are apt to recur again and again; and at length, when the Tubercles increase sufficiently to obstruct the passage of air through certain parts, and to occasion dulness here and there, and a more energetic and compensating respiration elsewhere, then the secret of the former inflammations or hæmorrhages are revealed. At this point the mixed character of the disease begins to declare itself by the mixed character of the auscultatory Signs.

Now, although Pulmonary Consumption (as we have seen) unquestionably can, and sometimes does, produce Inflammation or Hæmorrhage of the respiratory passages, long before it is so far developed as to give any direct tokens of its own existence, yet this is not the usual course: it most frequently happens, that neither Inflammation nor Hæmorrhage are added to it, until Tubercles have at least reached that degree of accumulation when they begin to give occasion to certain auscultatory Signs.

I think I have observed that, as long as the Pulmonary Consumption remains in its tubercular Stage, if an Inflammation or a Hæmorrhage be added to it, they are apt to occur in distinct attacks, occasionally and casually.

I formerly mentioned the frequent cases of Hæmoptysis admitted into this hospital, which were connected with Tubercles of the lungs. The attack is usually sudden; the quantity of blood lost in a short time considerable; the treatment required usually active; and the result, as far as the mere Hæmorrhage is concerned, usually successful. Moreover, the auscultatory Signs denote the mixed nature of the disease. While the spitting of blood continues, and perhaps for a short time after it has ceased, there is a large or small Crepitation commonly arising from a considerable space at the lower part of one or both lungs. This denotes the bronchial or vesicular effusion, as distinguished from the deposition of Tubercles. Then there is a diffused dulness both to Percussion and

Auscultation somewhere; perhaps between the clavicle and mamma on one side; and an exaggerated respiratory murmur somewhere else; perhaps between the clavicle and mamma on the other side. These denote the deposition of Tubercles, as distinguished from the bronchial or vesicular effusion.

To my experience bronchial or vesicular Hæmorrhage is more familiar as an accompaniment of Phthisis, than bronchial or vesicular Inflammation; the effusion of blood than the effusion of serum or mucus, while the disease is yet abiding in its tubercular Stage. But when Inflammation *does* occur, I have generally remarked in it the same circumstances and attendant conditions which belong to the Hæmorrhage; the same sudden and distinct mode of attack; and that degree of excitement of the blood-vessels which requires the same treatment; and the same successful result. Moreover, there have been the same auscultatory Signs; namely, crepitation at the lower part of the lungs, produced by the effusion of serum or mucus; and dulness at the upper part, produced by the deposition of tubercles. The only difference is, that in one case serum or mucus is expectorated, and in the other blood.

I here speak of Hæmorrhage and of Inflammation as of two things, not wishing to swerve from the customary language of medical men; but, if I might use my own language, I should speak of *vascular action terminating sometimes in effusion of blood*, and *sometimes of serum or mucus;* as of one thing tending to two results; for whether the result be the effusion of blood, or the effusion of serum or mucus, the vascular action requires the same treatment according to its degree, and is as much inflammatory in one case as the other. Moreover, it is this vascular action which is our only practical concern; we treat it, and *it only*, and have no special care of either blood, serum, or mucus, for which in themselves we can do nothing remedial.

But it is when Pulmonary Consumption has advanced beyond the tubercular Stage that we find the most frequent examples of its mixed character. Bronchial or vesicular effusion is almost the constant accompaniment of Vomicæ; and the expectoration is now often as much supplied by the mucus lining of the air passages as by the Cavities themselves.

You have only to go into the wards of the hospital, and

you may at once acquaint yourselves in a dozen instances with the mixed character of the auscultatory Sounds. Gurgling Cough, Gurgling and Cavernous Respiration, Pectoriloquy, one, or several, or all together, will show that this, that, and the other patient, have Vomicæ in their lungs; and Large and Small Crepitation, one or both concurrently, will show also that this, that, and the other patient, have fluid effused here, there, or everywhere, within the respiratory passages.

Now when Vomicæ have been long formed, and the expectoration long established, Hæmorrhage and Inflammation are less liable to occur in sudden and distinct attacks. The blood, or mucus, or serum, which are now separated from the surface of the air passages, result from a vascular action of less force, but of more permanency, and are themselves more abiding.

It should be remarked, however, that blood, which is more common in another stage of Pulmonary Consumption, is more rare in this; not that blood does not *now* sometimes appear, but it appears rather as a part of the expectorated matter, streaking or staining it, than as pure and sincere blood.

Assuredly, after the expectoration is established, sudden and profuse gushes of blood seldom occur. Probably the expectoration itself is the security against them, the circulation thus obtaining all the relief it stands in need of. Probably, too, the security becomes greater in proportion as the expectoration is more copious and more free, and proceeds from a larger extent of mucous surface.

All this is, in the nature of things, very probable, and it is confirmed to me by the striking fact which, in a few instances, I have known, of a copious muco-purulent expectoration suddenly ceasing, and a frightful Hæmoptysis at once bursting forth; as if the circulation, being suddenly baffled, had sought and found the nearest way to free itself. In these instances, when the Hæmorrhage ceased, the expectoration was re-established.

It should be mentioned, that in the destructive processes connected with the formation of many and large Vomicæ, the blood-vessels of the lungs do not always escape ulceration, or rupture, while they are yet pervious; and then a mortal Hæmorrhage is the consequence. But such Hæmorrhage is

purely accidental, and independent of any proper hæmorrhagic action (if I may so call it) in the vessels themselves.

Let me guard you against a vulgar error. Hæmoptysis and rupture of a blood-vessel are, in the popular sense, convertible terms; so much is one conceived to be the natural and necessary consequence of the other. But rupture of a blood-vessel, which has been esteemed the only cause of Hæmoptysis, is unquestionably the rarest cause of all; and this accident, which one might expect to find frequent in Pulmonary Consumption, nature has taken great pains to guard against; for no sooner does the destructive process of forming Vomicæ within the lungs begin, than she sedulously betakes herself to closing up the arteries which lead to them by clots of blood: and as to the veins, partly (I believe) by the same process, and by otherwise arresting the circulation through them, she reduces them to impervious shreds.

Now, in all cases of Pulmonary Consumption arrived at the stage of Vomicæ, I would recommend a constant regard to the extent of the disease beyond its specific limits. I would recommend that, besides attending to the sounds indicative of cavities, you should take especial note of *Crepitations*, and how they vary in the distance to which they spread themselves from time to time. The Gargouillement, and the Pectoriloquy, and the Vomicæ, from which they arise, are beyond our reach *remedially;* not so the Crepitations, and the vascular action which produces them. In my treatment of Pulmonary Consumption, I am accustomed to make these Crepitations serve me for practical indications, endeavouring by all means to lessen and circumscribe them, and thus seeking, under the guidance of Auscultation, to bring back the disease as much as possible within its specific limits.

The bronchial and vesicular effusion, which is the concomitant of Vomicæ, submits itself to the influence of medicine in various degrees. Very often, when there are Gurgling Cough and Gurgling and Cavernous Respiration and Pectoriloquy, at certain points, and, withal, Large and Small Crepitations diffused widely through the lungs, a seasonable remedy will entirely sweep away the latter sounds, and leave the former *alone*. A small cupping, a few leeches, a blister, a liniment, a mustard cataplasm—one or other of these, according to the degree of

vascular action, applied at the right time and in the right place, will produce immense relief, by bringing the disease back for a while within its specific limits.

It is thus, as perhaps you may have remarked, that almost every phthisical patient brought into the hospital experiences great relief for a short time after his admission. The poor, alas! are not only the chief victims of Phthisis, but they suffer the disease with all its occasional superadded evils, which their exposure, their hardships, and their needful toils, will not allow them to escape. With them, the superadded evils are often beyond all proportion to the disease itself. The Tubercles and Vomicæ may be few, and the bronchial and vesicular effusion immense; and this superadded effusion may be for the first time submitted to a remedy when they reach the hospital, and then it is often in a great part or altogether swept away. No wonder that, from the relief which follows, the patients should sometimes believe themselves cured at once and entirely!

But the effusion again and again returns, and requires again and again to be abated.

Thus I have given a sketch only of a vast subject, which is interesting, fearfully interesting, to all mankind. I have not crowded it with a multitude of instances, for I had not room for them; and I was afraid they might obscure the outline, which was all I designed to give.

In tracing this outline my own experience has guided me; and yours will soon enable you to fill it up, and to determine how far it is true and how much it is worth.

There are many strange things respecting Pulmonary Consumption—many striking discrepancies between case and case, and many contrarieties of opinion among the well-informed as to its proper mode of treatment,—which, heretofore, the best of us have been unable to reconcile or explain. In one case it is a slow decline; in another a galloping consumption. It is spoken of as incurable; and yet now and then an individual recovers, or seems to recover. Some are for treating it by bleeding. Some by bark and steel, and ammonia. Some restrict their patients to vegetables and asses' milk, and some give them animal food and wine; and all can boast of their success.

But these things have now ceased to be mysterious any longer; thanks to a more enlightened pathology and to that

method of diagnosis which not only marks the due course and progress of the specific disease, but does not allow even emergent and contingent events to escape its scrutiny.

This same method, still aided and guided by a just pathology, has enabled us to discern more clearly the avenues through which nature admits relief, and to direct our remedies with a steadier aim; albeit such remedies are of various and even opposite kinds.

Unquestionably, within the limits of *possible* success, Auscultation has contributed to render our treatment of Phthisis more successful.

It has been the means of discovering no new remedy. How should it? It has made nothing curable now, which was incurable before. But, while it has taught us to distinguish the unmixed from the mixed Phthisis, the disease from its accidents, what is reparable from what is not, *in the patient while yet alive*, it has enlarged the resources of practical medicine, by more clearly presenting to it the objects within its reach.

By keeping those objects, thus offered to you, steadily in view, you will be able to fulfil more satisfactorily every purpose that rational treatment can contemplate in a disease which, if it be not incurable, is rarely cured. You will know better how to remove its many superadded evils, to postpone or mitigate its many pains, perhaps to lengthen life, and to procure for it, while it lasts, some share of happiness.

I cannot finish this little sketch of Pulmonary Consumption, and of the uses which Auscultation serves in distinguishing its kinds, and in furnishing guidance for its treatment, without adding one further remark; it is *this*:—In Pulmonary Consumption death not unfrequently arrives at a much earlier period than seems consistent with the morbid processes going on within the chest. Patients die sometimes long before the disease of the lungs has reached the point at which it is necessarily fatal. I do not mean that phthisical patients may chance to suffer fever or cholera, or may chance to break a limb, and so die of other diseases or accidents. These cannot be anticipated or guarded against. But I mean something more worthy of your consideration.

In proportion as we are more intent upon investigating the local processes of diseases in a particular organ, scrutinizing

them pathologically, and nicely weighing their diagnostic signs, there is a danger that our minds may be withdrawn from those larger views which regard their constitutional origin, and their consequent liability to fall upon any or all organs of the body. Thus, while we are expecting a man to die of Pulmonary Consumption a twelvemonth hence, he may die in the meantime of (if I may so speak) Intestinal Consumption, Peritoneal Consumption, Mesenteric Consumption, or Vertebral Consumption. For, from the same strumous constitution, which engendered Tubercles and Vomicæ in the lungs, Tubercles and Ulcers are formed in the mucous lining of the bowels; hence comes an incontrollable diarrhœa or dysentery. From the same cause Tubercles are formed upon the peritoneum, and Tubercles in the mesenteric glands; hence come slow peritonitis, and ascites, and marasmus. From the same cause caries affects the bodies of the vertebræ; and hence comes lumbar abscess.

All these are liable to arise in the course of Pulmonary Consumption; and the fatal event may be greatly accelerated in consequence. Not that, from arising in the course of it, they are to be regarded as merely incidental to it; for both it and they are all of the same essence, the several products of the same morbid constitution; sometimes one and sometimes another taking the lead; Pulmonary Consumption as often following these, as these following it.

Finally, then, Pulmonary Consumption is no more than a *fragment* of a great constitutional malady, which it would be in vain to think of measuring by the stethoscope, and which it belongs to a higher discipline than any mere skill in Auscultation rightly to comprehend.

LECTURE XIV.

ON THE DOCTRINE OF SYMPTOMS.

POSSIBLE FALLACIES OF AUSCULTATION.—HOW IT MAY LEAD TO AN ERRONEOUS DIAGNOSIS—IN PNEUMONIA—IN DILATATIONS OF THE BRONCHI—IN EMPHYSEMA OF THE LUNGS.—PATHOLOGY OF DILATATIONS OF THE BRONCHI.—PATHOLOGY OF EMPHYSEMA OF THE LUNGS.—HOW THE INTIMATIONS OF AUSCULTATION AND PERCUSSION MAY APPARENTLY CONTRADICT, YET REALLY CONFIRM, EACH OTHER.

I HAVE endeavoured to speak of the auscultatory Signs that belong to the lungs as plainly as I could. First I took them singly, and tried to fix their separate value by a reference to the simpler forms of disease; and then I took them together, seeking their relative and combined value in forms of disease that are more complex. And thus I found that a single pathological condition might be denoted by a single auscultatory Sign; that in Bronchitis, when it had not passed the stage of mere vascularity, there was nothing but a *dry* Sibilus, and when it had reached the stage of effusion, nothing but a *moist* Crepitation. Moreover, I found that, as diseases were cumulative, so might their auscultatory Signs be cumulative also; and that a mixed case of Pulmonary Consumption often contained every one of the auscultatory Signs that have been mentioned.

These Signs, both singly and cumulatively, I have followed as far, perhaps, as they can be altogether trusted in the diagnosis of pulmonary diseases. Not that, beyond this point, they have no further aid to contribute; only they need more confirmation from concurrent circumstances, and require more care on our part, to avoid certain errors, to which, if too implicitly relied upon, they are apt to conduct.

Auscultation is not infallible. I have known it betray the most wary and experienced into downright error; as when a

certain sound, which in forty-nine cases indicates one thing, has in the fiftieth case indicated another. You ought to be aware how this may happen. As also, when the several auscultatory Signs of one and the same case seem to set themselves in opposition to each other, one indicating this thing, and another that; one confuting what another affirms. This apparent contradiction is at first very perplexing; but, being understood, turns out to be no contradiction at all; and the apparent contradiction and the real consistency furnish together a sure diagnosis of a particular form of pulmonary disease. You ought to be aware how this, too, may happen.

In what manner, then, an auscultatory Sign may give false intimations, and how several Signs may seem to contradict, and yet be perfectly consistent with, each other, I will now endeavour to explain. For this purpose I must touch a little upon some points of Pathology.

It has been by the light of certain facts in Pathology, considered as general truths, that Auscultation has reached some of its most important conclusions.

Of such facts these two deserve to be especially noticed; viz. that Tubercles and Tubercular Cavities have their origin in the upper lobes, and Inflammation its local origin in the lower lobes, of the lungs.

These two facts are so generally true, that they have been set up as signals (if I may say so) to steer our diagnosis by; and Auscultation has reached some of its safest conclusions entirely from faith in them.

But ordinary diseases will sometimes occur under extraordinary circumstances, or in unusual situations; and then we are as apt to be thrown out in our diagnosis, as the pilot is in his course upon any unexpected alteration of lights or signals on the coast. He makes false points, and so do we.

Thus in every instance of exception to the two general truths which have been specified, there is a perilous chance that Auscultation will lead us wrong.

In forty-nine cases out of fifty, Pectoriloquy is a direct symptom of a cavity formed in the lungs, the result of tubercular Disease, or the result of Inflammation. Then comes the fiftieth case, in which there is Pectoriloquy, arising not from a cavity either tubercular or inflammatory, but from

some other condition; and in this fiftieth case the Pectoriloquy, I suspect, almost always deceives us.

A young woman (Mary Taylor) was admitted into the hospital in September, 1833. For two or three years she had been liable to slight coughs, and in the last spring had suffered Influenza. The Influenza passed away, but a slight cough was still left behind.

Three days before her admission she had a rigor, inability to lie on the right side, and pain shooting from beneath the right clavicle through the chest to the scapula.

At her admission she was flushed and hot, her respiration hurried, her pulse 112 in a minute, and full and soft; and she complained of pain in the situation just mentioned.

Auscultation found a healthy respiratory murmur, unmixed with any unnatural sound, throughout the entire left lung, but in the right lung Cavernous Breathing, and a loud Pectoriloquy above the spine of the scapula, and small Crepitation all around it; also a space beneath the clavicle dull to the ear, and dull to Percussion, while the rest of the lung was healthy. Six leeches were the most active remedy which her strength would admit, and they were applied beneath the clavicle.

She was delirious through the night, and perspired greatly. The next day she was more flushed: her dyspnœa was aggravated; her pulse had gained in frequency, and had lost in power; she was altogether very much sunk. The Pectoriloquy was still clear and evident, and the *small* Crepitation still heard everywhere about the scapula, and moreover in front, about the mamma.

Our diagnosis in this case was, that one considerable Vomica at least existed in the apex of the right lung, and that *acute* Inflammation of the vesicular structure had arisen all around it.

She was treated by remedies as active as her feeble circulation would admit, chiefly by leeches to the surface opposite the parts where the Crepitation was heard. In four days the Crepitation began to abate, and in six it was gone; and, as it gradually went away, the respiratory murmur gradually returned, until it entirely took its place.

But what became of the Cavernous Respiration and Pectoriloquy? These surely remained unaltered; for the Vomica

could not be so soon cured, although the surrounding Inflammation might?

Not so. But the Cavernous Breathing and Pectoriloquy were first changed into Bronchial Respiration and Bronchophony: and these last soon ceased; when nothing was anywhere heard but the healthy respiratory murmur. The patient was well, and the whole work of reparation was accomplished in a week.

The diagnosis, I have said, was that she had one large Vomica at least at the apex of the right lung, and that acute Inflammation had arisen around it. I apprehended that she would die quickly, so fearfully rapid was her sinking in the first two days after she reached the hospital. But a week from that time she was well, and a fortnight from that time she was discharged from the hospital.

In this case I was entirely deceived in regard to one supposed ingredient of the disease, the Cavity. And I was deceived *by Auscultation*. Cavity there was none, which is the very thing we expect to find at the apex of the lung. But Inflammation there was, and of the acutest kind, and nothing but Inflammation, which is the very thing we *do not* expect to find there.

It is quite certain that the Cavernous Breathing and Pectoriloquy were formed in a bronchial tube passing through a portion of lung which had become partially condensed by Inflammation. As the lung began to admit more air, the Cavernous Breathing and Pectoriloquy lost their distinctness, and were changed into Bronchial Respiration and Bronchophony; and when the lung became entirely free, the healthy murmur was re-established alone and unmixed with any unnatural sound whatever.*

* Inflammation, independent of tubercular disease, is so rare an event in the upper lobe of the lungs, that my own experience does not enable me to speak confidently of its auscultatory Signs. What I have noted as a peculiarity, may be the natural and necessary result of Inflammation in a part where the Bronchi are large and near the surface, and the lung partially or altogether condensed around them. I am not prepared to say that the same pathological conditions, which in other parts of the lungs would produce Bronchial Respiration and Bronchophony, may not *always* produce Cavernous Respiration and Pectoriloquy in the upper lobes. The following case lately fell under my observation:—A girl twelve years of age had a

Let it, however, in justice be remarked, that although Auscultation misled me both in my diagnosis and prognosis, yet it betrayed me into no error of treatment. Nay, but for Auscultation, I might have treated the case less precisely and less effectively. It pointed out to me the very part where the disease was, and told me that Inflammation was at least an ingredient of it; things which could only have been conjectured without the aid of Auscultation.

There are certain conditions of the lungs now familiarly understood (or at least familiarly spoken of, as if they were understood) by medical men, which had been little investigated, and were little known, before Auscultation directed attention to them; and yet, both pathologically and practically, they are of the highest import. Laennec seems to have been first led to make accurate inquiry into the nature of Dilatations of the Bronchi, and Dilatations of the Vesicular Structure of the Lungs, for the sake of verifying certain auscultatory Signs. And all who concern themselves with Auscultation, and seek in like manner to verify its Signs by dissection, will soon feel their obligation to him for his elucidation of these subjects.

Dilatation of the Bronchi may take place in one or in several, in any or in almost all their branches, throughout the lung; it is, however, most frequently met with in the upper lobe, and nearer its anterior surface.

A Bronchus may dilate, and still preserve its natural cylindrical form. That, which would not naturally receive more than a knitting-needle, becomes large enough to admit a

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hot skin, a flushed countenance, a rapid pulse, difficult breathing, and small Crepitation widely diffused throughout the left lung. Every part of the lung was still pervious, but less air was manifestly admitted into the upper lobe than elsewhere. Moreover, at two or three spots about the scapula, Cavernous Respiration and Pectoriloquy were distinctly heard. No clear account could be obtained of the girl's illness before her admission into the hospital. But from present symptoms I had no doubt that the left lung was extensively inflamed, and that it contained several Vomicæ.

Nothing remained but to subdue the inflammation; and, indeed, the remedies succeeded beyond our hopes; for in curing the inflammation they (apparently) cured the Vomicæ also. In the course of ten days the healthy respiratory murmur had taken place of the Crepitation, and the Cavernous Breathing and Pectoriloquy were no longer to be heard.—February, 1836.

crow-quill, or a goose-quill, or even a little finger. It seldom happens that the common bronchial trunk is sensibly dilated, but the branches become larger than the trunk from which they are given off.

But a Bronchus may dilate and not preserve its natural form. It may dilate so as to take the form of a Cavity, having the same size and shape as a Vomica; it is distinguished, however, from a Vomica, by the structure of the Bronchus being traceable into it; its mucous membrane, its fibrous membrane, and sometimes vestiges of its cartilaginous rings.

Again, a Bronchus may so dilate as to form several Cavities; that is, it may dilate and then contract itself again to its ordinary calibre, and dilate and contract again and again at several spaces in its course. Thus, upon dissection, the lungs have sometimes appeared to be beset with Vomicæ or abscesses full of matter, which, upon examination, have turned out to be so many Cavities formed from the dilatation of several bronchial ramifications.

When the Bronchi are numerously and extensively dilated, they so compress the intermediate pulmonary structure as to preclude the admission of air into its vesicles; and thus it becomes squeezed together and flaccid, exactly resembling lung which has suffered from pleuritic effusions.

There seems good reason to believe that the Bronchi become dilated by the long-continued residence and accumulation of morbid secretion within them; and that the bronchial trunks are less frequently dilated than their branches, because this morbid secretion is accustomed to linger within them for a shorter time, being more easily dislodged by forced expiration, *i.e.* by Cough.

I know no instance of dilatation of the Bronchi where it has not followed or accompanied some disease especially characterized by abundant secretion; such as protracted Hooping-cough, chronic Bronchitis, or Catarrh.

Surely this little pathological sketch will at once show what the auscultatory Sounds must necessarily be which accompany dilated Bronchi. If they be enlarged uniformly through their whole course, they must give rise to Bronchial Respiration and Bronchophony, and much more so, if they pass through com-

pressed lung. If they be formed into Cavities, they must occasion Cavernous Breathing and Pectoriloquy; and when those Cavities contain fluid (as they generally do) they must produce Gurgling Respiration and Gurgling Cough.

But Bronchial Respiration and Bronchophony, Cavernous Respiration and Pectoriloquy, Gurgling Respiration and Gurgling Cough, have been dwelt upon and explained as the almost certain signs of condensed lung and pulmonary abscesses; of lung, condensed by the products of common inflammation or of specific disease; of abscesses formed by inflammation, or left after the evacuation of tubercular matter. And of such conditions they must still continue to be the *almost certain* signs. It is only when they are interpreted by the special circumstances of some particular case, that they can be construed into a different meaning, or serve to indicate Dilatation of the Bronchi.

But the circumstances are *seldom special enough* to turn aside the auscultatory Signs from their most common object, and to make them point to another which is of very rare occurrence. Hence I am persuaded that the most experienced and most skilful physicians generally fail to form a just diagnosis in cases of Dilatation of the Bronchi, and seldom fail to form a wrong one; the auscultatory Signs and every attendant circumstance conspiring to lead them into error.

Chronic Bronchitis is the most frequent cause and concomitant of Bronchial Dilatation.

Now chronic Bronchitis itself is often with great difficulty distinguished from Pulmonary Consumption. Its attendant emaciation, its copious puriform sputa, its abiding hectic, are all phthisical symptoms. And it is only after repeated examinations that we are able to exclude the idea of Phthisis when we find no auscultatory Signs of a cavity. There is large Crepitation extensively diffused through the whole lungs, and that only; showing that the copious puriform sputa come from the mucous lining of the Bronchi, and from it only.

But, suppose that in such a case there was Pectoriloquy withal and Pectoriloquy in several places, or Gurgling Respiration and Gurgling Cough in several places, no human penetration could distinguish the disease from Pulmonary Consumption;

and yet there might be neither Tubercle nor Vomica in any part of the lungs, but a dilatation of the Bronchi into the form of Cavities.

A man, 46 years of age, had been liable to Catarrhs for several years, and for one year had suffered a slight habitual oppression of the chest. He had once spit blood in December of the preceding year. In the following February, upon the occasion of his contracting a fresh cold, an expectoration came on, which was copious and puriform, and very fœtid. Finally, a week before his admission into the hospital, he suffered a severe pain in the left side of the chest, which came on for the first time after his being wet through, and ever afterwards he was constrained to keep his bed.

He entered the hospital at the end of March. He was in a state of orthopnœa, and his countenance expressed the greatest anxiety. He rejected, by an easy cough, a fœtid expectoration, consisting of yellow thick globules, mixed with a large quantity of serum, upon which they floated. The pain was so great over the whole of the *left* side of the chest, as not to allow the use of Percussion.

Auscultation found the respiratory murmur everywhere strong and clear on the right side, and much more feeble everywhere on the left; and on the left side, in the region of the mamma, and a little above the inferior angle of the scapula, there was manifest Pectoriloquy.

Nobody entertained the slightest doubt in this case that the disease was Pulmonary Consumption, and that a Vomica was formed. The patient remained in the hospital until his death, —nearly three months. In the meantime the pain of the left side, the fetid expectoration, and the Pectoriloquy, all remained, to which, at length, Diarrhœa was added. It was remarkable that every evening he suffered a chilliness, followed by burning heat, but without perspiration.

Upon dissection, the left lung hardly crepitated at all, although it floated in water. In its upper lobe was a cavity large enough to admit a middle-sized nut, which contained a fluid of the same kind with that which had been expectorated. A bronchial tube, as large as a goose-quill, entered into it; and dissection traced a continuation between the walls of the bronchus and the walls of the cavity: in both the same mucous

membrane, red and thickened, the same fibrous membrane, and some traces of cartilaginous rings.

Here was no Vomica, but a partial dilatation of a bronchial tube. In the same lung several other bronchial ramifications were dilated in the same way; suddenly they acquired three or four times their natural size, then contracted themselves again, and then enlarged again; thus in effect forming cavities. The pulmonary tissue between the dilated bronchi was compressed as by pleuritic effusion.*

This case did not occur at St. Bartholomew's, but at La Charité. I was not the person deceived, but Andral: the possibility of deception, therefore, you will the more easily conceive. I have abridged the case from the "Clinique Médicale."

I am very far from saying that a just diagnosis of bronchial Dilatation cannot be made during the life of the patient, or that Auscultation cannot contribute essential aid towards it; only I am persuaded that the physician must have very favourable opportunities of watching his patient, and that the case must be less complicated than such cases usually are, before he can arrive at it. The case, too, must be one in which the bronchi are enlarged, still preserving their natural form, and not dilated into cavities.

But Laennec was led also by Auscultation to make inquiry into the nature of Dilatation of the pulmonary vesicles; and this subject, as well as that of bronchial Dilatation, he has made his own by the accuracy of his research.

The surface of the lung will sometimes present to the naked eye the same appearance which is given to it by an ordinary magnifying glass. The pulmonary vesicles will appear of the size of millet-seeds, or hemp-seeds, or raisin-stones; and thus dilated, they sometimes preserve the level of the lungs, and sometimes transgress it a little.

This appearance results in part from the Dilatation of single vesicles, and in part from the union of several produced by the rupture of their intermediate partitions.

Sometimes a transparent vesicle, as large as a nut-kernel, will rise very much above the surface of the lung, and seem to spring from a pedicle or stalk. But this appearance is merely

* Andral, Clinique Médicale; Mal. de Poitrine, vol. i. p. 24.

owing to its simple constriction just at the point of its emerging above the level of the surface.

Where the air-cells appear thus dilated at the surface of the lungs, the same condition is found to exist within. In order to see this condition in the lungs to the best advantage, you should inflate them, and then let them dry. Afterwards, when you have divided them with a sharp knife, you will find, by examining the cut surface, that the air-cells are almost always more dilated within than they appear to be on the surface; and you will see, moreover, that of the air-cells some are simply dilated, and some are ruptured and united together. The smaller bronchi sometimes partake of the Dilatation of the air-cells to which they lead: but this event, which one would think likely to happen constantly, does in fact happen very seldom.

In a lung, of which the air-cells are dilated, there is something very peculiar to the touch. It has the feel of a downy cushion, not the crepitant feel of healthy lung. It is softer than healthy lung, and the same degree of pressure evidently displaces in it a larger quantity of air at once.

Now all this description belongs to conditions in which the air still remains within its proper vessels, those vessels being permanently and unnaturally distended; or distended and, moreover, ruptured into each other.

Hitherto I have abstained from using any name but what was necessary to the description. I have used none but *Dilatation of the air-cells or vesicles.* But this same Dilatation of the air-cells, in which the air *is still contained within its proper vessels,* is called by everybody Emphysema. And Emphysema let it be called; only take care that the misnomer does not convey an erroneous idea. I have no fancy for disputing about names; but this I would remark, that you might just as well call an Aneurism a Hæmorrhage, as a simple Dilatation of the air-cells, or the Rupture of the air-cells into each other, an Emphysema.

If an Aneurism bursts, then follows a Hæmorrhage; and if air-cells burst, otherwise than into each other, then follows an Emphysema. And, in fact, distended air-cells often do burst, not into each other, but so as to allow the air to escape into surrounding texture.

One form of this Emphysema proper (as it may be called) is, when an air-cell bursts near the surface of the lungs, and air is effused beneath the pleura pulmonalis. Thus a vesicle may be formed of any size, from the egg of a sparrow to the egg of an ostrich, or even larger still. The air, now extravasated beneath the pleura, is capable of being displaced by pressure.

Another form of Emphysema proper is described by Laennec, which I never saw. It is occasioned by rupture of distended air-cells; not on the surface, whence air escapes beneath the pleura, but in the interior of the lung, whence air escapes into the pulmonary tissue and lacerates it, and forms a cavity in it. The cavity thus formed is capable of receiving a moderate-sized nut. It is generally found about an inch deep beneath the surface of the lung. It is permanently blown up with air, and sometimes contains blood. The air-cells, which immediately form the walls of the cavity, are effaced, and do not retain their natural rounded form, either to the eye or the magnifier. But the air-cells at a little distance all around are still distended with air. The air, under these circumstances, is not necessarily effused into the interlobular structure; although it has escaped from its own vessels, it is still limited to this cavity.

There is one very curious circumstance attending this form of Emphysema. It is this: the pulmonary cavity, which is about an inch from the surface, being permanently blown up with air, exercises a great stress upon surrounding parts. Under this stress they yield the most readily in that direction where there is the least resistance—viz. in the direction of the surface; and *there* a bump is forced up, correspondent in size to the interior cavity, and just over the part where it is situated.

The Dilatation of the air-cells has been explained to depend upon the forcible incarceration of air within them; and a cause capable of producing that incarceration has been found in inflammation of the smaller bronchial ramifications which conduct to them.

There is an affection called by the French "*catarrhe sec.*" It is characterized by habitual cough, or cough going and coming for years, and accompanied by little or no expectoration. But what expectoration there is consists of small pieces of hard, tough, pearly phlegm.

Upon this chronic affection attacks of a more acute kind are liable to be engrafted from time to time, accompanied by fever, and producing an increase of the expectoration; and, when they subside, often leaving the habitual disorder worse.

In short, it is one form of (what is called) Asthma; and the essential morbid conditions in which it consists are a congestion and thickening of the mucous lining in the small ramifications of the bronchi, and a secretion by it of this little glutinous pearly phlegm.

Now the congestion and thickening of the mucous membrane in that situation, and the residence of tough phlegm upon it, may be obstacles sufficient to prevent the easy return of air *from* the vesicles, while they may *not* be sufficient to prevent the access of air *to* them. The force of inspiration is evidently far greater than the force of expiration; and the former is capable, moreover, of being augmented by an effort of the will, in a much greater degree than the latter.

It is not difficult to conceive how, more air being thus forced into the air-cells by each inspiration than each expiration can expel from them, there would result a constant imprisonment of air within them, and their consequent dilatation and their possible rupture.

I am, however, surprised that such congestion and thickening, and morbid secretion of the mucous membrane in this particular situation, should be insisted upon by Laennec as almost the sole cause of dilatation of the air-cells; for I suspect that all the ordinary mechanical impediments to breathing, whether within or without the lungs, have the common effect of raising hindrances and obstacles, *especially to the act of expiration*, and thus become capable of dilating the vesicles. It has occurred to me to witness their dilatation to the greatest extent, and their rupture and the extravasation of air beneath the pleura to the greatest degree, in cases of excessive deformity of the chest, arising from curved spine. I have found the same, of less degree and extent, in combination with Tubercles and Vomicæ; and I have found dilated vesicles, or ruptured vesicles, in clusters, at the edges of the lungs, where there has been no concomitant disease of the lungs to account for them.

And now for the ascultatory symptoms of Dilatation of the air-cells, or of their Dilatation and Rupture together, as far as

they have hitherto been described. These symptoms are of a very remarkable kind: they are derived both from Percussion and from Ascultation, but from neither singly. The positive intimations of the one are now in direct opposition to those of the other; yet do they point to the same thing, and illustrate it *especially by their contrast.* Percussion gives a sound which is loud and clear; while, to the ear, or the stethoscope applied to the same parts, either all is dull, and there is no respiratory murmur at all, or there is one rather suspected than distinctly audible. The chest does not give the same answer when you knock as when you listen.

Now it is not the clear sound upon Percussion that indicates Dilatation of the air-cells, or Emphysema; neither is it the little sound, or the no sound, to the ear and the stethoscope, that indicates it. Either of them taken alone would denote something else; but taken *simultaneously*, they denote Dilatation of the air-cells, or Emphysema.

There is nothing more interesting in the whole subject of Ascultation, than the various ways in which Percussion and Ascultation aid each other. They aid each other by their correspondent results, and they aid each other by their contrasted results; and each is thus made to go as far again by the help of the other, as it could possibly go alone.

Hitherto, in the course of our inquiry, I have spoken of Percussion occasionally only, introducing it to confirm the signs derived from Auscultation by its correspondent intimations. In inflammatory and tubercular diseases of the lungs, what Auscultation found pervious, Percussion found resonant; what Auscultation found condensed, Percussion found dull. But now I introduce Percussion, not to confirm by *its correspondent* intimations, the signs derived from Auscultation, but to rectify, by *its contradictory* evidences, what, if taken upon the sole attestation of Auscultation, would be false.

It is necessary for the sake of obtaining the full use of both these methods of appealing to the same sense in diseases of the chest, to understand them in their *disagreements:* to know *why* what one finds resonant, the other should find dull.

Percussion, by the resonance or non-resonance that attends it, *simply* intimates that air is or is not contained within the

chest beneath the part struck. It intimates so much, and no more, with certainty. It gives no notice respecting the condition in which the air exists, or respecting the exact situations which it does or does not occupy. Whether it be moving about or at rest, there is the same resonance upon Percussion; whether it be contained in the bronchi, or in the air-cells, or in the cavity of the pleura, there is still the same resonance; or so nearly the same, that no one would venture upon a diagnosis of its situation *merely* from a difference of sound elicited by striking the chest.

But Auscultation gives no intimations *absolutely* concerning the existence or non-existence of air within the chest. The air must be there under certain conditions for Auscultation to be able to detect it at all. For Auscultation to detect it, the air must be in *motion*. If it be at rest, Percussion can detect it, but Auscultation cannot. It must also be within the respiratory passages, or in situations with which they freely communicate; if it be beyond them, Percussion can detect it, but Auscultation cannot.

It is from air in these situations and under these conditions; from air in the respiratory passages, and from air put in motion by the act of breathing, that Auscultation conveys to us all the sounds diagnostic of so many varieties of pulmonary disease. For, though air be still in the respiratory passages, if it be imprisoned there, and no motion reach it from the act of breathing, as in the case of dilated air-cells,—or if it escape from the respiratory passages, as in the case of ruptured air-cells and emphysema, beneath the pleura,—then Auscultation can convey to the ear no sound, and is useless for the purposes of diagnosis.

But in this case Percussion still produces a resonance, and tells us (what is the fact) that there *is* air; yet Percussion does not tell under what circumstances or in what exact situation it is.

By comparison, however, and contrast of the results drawn from both methods, we arrive at conclusions in this case, to which neither could carry us separately. The chest is *resonant* to Percussion in every part, a sure evidence that in every part there is air;—but at the same time the chest is in *several* parts dull to Auscultation. Yet here is no contradiction to the fact

that air is still in these parts: but, upon comparison, it is a sure diagnostic sign that, being there, it is beyond the reach of the respiration to give it motion; in short, that it is either imprisoned within the air-cells, or extravasated beyond them; and that we have to deal with a case of Dilatation of the air-cells, or a case of Emphysema, or a case of Pneumothorax, of which last I shall speak hereafter.

These, then, being the auscultatory Signs of dilated air-cells and ruptured air-cells, and of air extravasated out of its proper vessels, or (in one word) these being the signs of Emphysema, it would seem to be of easy detection. And, existing *alone*, and unmixed with other morbid conditions, it unquestionably is so; but, in point of fact, it seldom does exist alone.

There are certain conditions of parts frequently met with, which can hardly with propriety be called morbid. Although they are departures from what is natural and healthy, they exhibit no progressive morbid action. The blood-vessels are laying nothing down, and the absorbents are taking nothing up. There is only a yielding of parts to accommodate themselves to some pressing necessity, and a consequent change of natural capacity and size. The common biliary duct, from the passage of a gall-stone, the ureter from the passage of a renal calculus, dilates, at the time, and remains dilated ever afterwards. So too the bowel, above the seat of a stricture, by gradually giving way to the pressure of its contents, will become permanently enlarged, and take the form of a pouch. These several states are all incidental to diseases, but are not diseases themselves.

The like conditions are exemplified, in the lungs, by dilatation of the bronchi, and dilatation and rupture of the air-cells, which grow out of preceding diseases, but hardly bear the character of diseases themselves.

But, although Emphysema may not itself come up to one's notion of what is understood by real disease, yet do real diseases constantly compass it round on every side. Diseases conduct to it, and diseases arise from it; and those that go before, and those that follow after, all remain and exist concurrently with it: so that I do not know any instances of more complicated thoracic affections than those of which Emphysema

may, and generally does, form a part. There may be distortion or disease of the dorsal vertebræ; chronic bronchial inflammation; tubercular depositions, or vomicæ: any or all of these together, may have been concerned in imprisoning air within the vesicles, and so causing their dilatation or rupture; and having caused it, they still remain to augment it. Then there is the Emphysema itself; and, superadded to these, and arising out of one or all of them, may be dilatation of the right cavities of the heart.

Imagine what must be the complexity of symptoms from such complexity of disease! To omit others, think what the auscultatory Signs must be! For the tubercular cavities, the bronchial inflammation, the dilated heart, and the Emphysema itself, each have their own. And I do not say that you cannot, from among all the rest, pick out the auscultatory Signs which denote the existence of Emphysema: but when you can, it is a great triumph of diagnosis.

There is (what is called) Interlobular Emphysema, in contradistinction to Pulmonary Emphysema, whose forms we have been considering.

Interlobular Emphysema is an extravasation of air into the cellular substance which intersects and separates the pulmonary lobules. This substance in its natural state is of so close a texture, that the infiltration of air into it could not have been thought possible. Yet the fact is certain. And then its real *cellular* texture becomes expanded and displayed. Thus lobule becomes separate from lobule, a space of half an inch or an inch being sometimes left between them, which is occupied by air that fills the intervening cellular tissue.

This tissue is more abundant and more cellular, and contains more air, as it is nearer the surface of the lung, and goes on diminishing and containing less air as it penetrates deeper. Thus it is a good deal like the natural segment of an orange, which contains more juice just beneath the rind, and less as it approaches the centre.

From the manner in which the interlobular partitions run parallel to each other, it must be obvious that, when several are infiltrated with air at once, there will result a separation of various pulmonary lobules entirely from each other, like little islets.

When this Interlobular Emphysema is near the root of the lungs, it soon reaches the mediastinum, whence air escapes into the cellular texture of the whole body.

Interlobular Emphysema, unlike the other forms described, has nothing to do with dilated air-cells: no dilated air-cells are found accompanying it, and where their rupture has taken place cannot be traced. The pulmonary lobules, which are (as it were) blown apart from each other, and have air infiltrated all around them, are themselves in a healthy state.

The Emphysema from Dilatation or Rupture of the air-cells is a chronic affection resulting from causes which are tardy in their operation; but the Interlobular Emphysema takes place in a moment, and is the result of accident. Any violent effort which holds or intercepts the breath may cause it; the striving of parturition, the straining to unload the bowels, or to lift a heavy weight. I have myself seen it produced by the convulsive struggle of hooping cough; at least I presume so: for I have seen the subcutaneous cellular tissue about the neck of a child become blown up with air after a fit of coughing; but this happened before I had Auscultation to help me in inquiring into the conditions of the lungs.

The auscultatory Signs of this Interlobular Emphysema are said to be such as cannot be mistaken, and strictly pathognomonic. They may be so, but I never had an opportunity of verifying them.

All this air effused must insure a clear resonance upon Percussion. Then there are, besides, the "Frottement Ascendant" on inspiration, and the "Frottement Descendant" on expiration; and the "Large dry Crepitation." I wish I could determine the degree of certainty which belongs to these auscultatory Signs: but I cannot, having had no opportunity of investigating the circumstances of such rare cases.

LECTURE XV.

ON THE DOCTRINE OF SYMPTOMS.

AUSCULTATORY SIGNS OF LESS FREQUENT OCCURRENCE.—HOW FAR WORTHY OF REGARD.—METALLIC SOUNDS.—WHERE AND HOW PRODUCED.

After you have been so long familiar with Auscultation, and have learned to appropriate its most important signs to the pathological conditions out of which they arise, it will still from time to time be presenting you with things that are new. Various sounds will reach your ear through the walls of the chest in breathing, talking, or coughing, which perhaps you never heard before.

Those unusual sounds, you will find, seldom occur alone, but are commonly superadded to others better understood, by which the essential character of the disease has been already ascertained.

How often, in the wards of the hospital, may you examine the chests of several patients, and find the same auscultatory Signs telling you that they have one and all the same disease; and not only so, but the same disease in the very same stage of its progress! The auscultatory Signs may be Cavernous Breathing, Gurgling Cough, and Pectoriloquy; the disease Consumption, and the stage that it has reached the stage of Vomicæ.

But these auscultatory Signs, which as to their leading characteristics, are the same in the several individuals, and bespeak the same disease, may include little minor differences. And, doubtless, these too are not without their causes. The various forms and sizes of pulmonary cavities, the firmness or flaccidity of their walls, the smoothness or roughness of their surface, the many or few, the large or the small bronchi that enter them, while they leave the auscultatory signs unaltered in their essen-

tial characteristics, serve to peculiarise them (if I may so speak) in individual cases.

I have not names for all the strange sounds which I hear every day proceeding from pulmonary cavities. And perhaps, it is well that I have not. Auscultation is a new thing; and therefore we are rather disposed to make too much of trifles connected with it; a fault which it is worth your while to guard against. If, by Auscultation, you already know the precise nature of the disease and its exact seat, the stage which it has reached, and the very processes which are now going on within the living body,—if you know the essentials of the case pathologically and practically,—it is a poor and profitless task to be guessing and speculating about mere matters of accident and uncertainty. It is the same thing as if a man, after he had read a book fairly through and mastered it, should think himself bound to count how many lines there were in each page, and how many words in each line.

Nevertheless, you are not at liberty to pass by all auscultatory Signs, except those which are the most constant, and which denote the most frequent forms of pulmonary disease. These, indeed, must always be practically the most important. Yet there are others less common but sufficiently definite, not belonging to the essential character of the disease, but to its accidental varieties, or to ulterior morbid changes derived from it, which require to be well understood. Some of these varieties and changes are of great account in our calculation of the result, and the signs which indicate them acquire a proportionate importance.

It is so especially with those auscultatory Signs which are called metallic sounds. These sounds accompany the acts of breathing, of speaking, and of coughing; of one or the other severally in different cases; or sometimes of one and sometimes the other, or sometimes of all equally in the same case.

These sounds have some variety, but they are all well expressed in general by the term "metallic;" for they are all either *ringing* or *tinkling* sounds, and like such as metal is concerned in producing. Often, too, they have in them something of the nature of an echo, and last for an instant, after the voice, the cough, or the breath that caused them has ceased.

Perhaps the best way of conveying the notion of what they

are, would be by stating what they have been thought to resemble. The voice of a person speaking into a well is, in kind, sometimes exactly like the metallic ringing that accompanies the voice and issues from the chest; and the sound produced by blowing sideways into the mouth of an empty bottle, is sometimes like that which is heard in breathing. Take an empty vessel of thin metal, or glass, or earthenware, and strike it lightly with your finger nail, or let a little dry sand fall into it, and you will produce the fainter sort of metallic tinkling. But the metallic tinkling is sometimes louder than the sounds thus produced. The small bells, which are borne by mules and pack-horses abroad, give a sound from a distance exactly resembling the metallic tinkling as I have heard it.

Now the metallic sounds may proceed either from a mere cavity within the lungs, or from some complex disease formed between the lungs and the pleura.

When the metallic sounds proceed from a mere pulmonary cavity, one condition always found to belong to that cavity is, that it is unusually large; and another, that it contains a small quantity of fluid in proportion to its size. And these two conditions are as clearly ascertainable by Auscultation during the life of the patient, as by dissection after his death.

Further, dissection discovers this large cavity always near the surface of the lungs, and adherent to the ribs so closely and with so little intervening substance, that they have seemed to form its external boundary.

I have myself had only one opportunity of examining after death a cavity of the lungs from which this metallic sound has proceeded during life; and this I owe to the kindness of a friend. At the Middlesex Hospital Dr. Watson met with a consumptive patient, in whom the metallic sound, which amounted to a ringing, was strongly marked, and was manifestly formed within a pulmonary cavity. Upon the patient's death he had a preparation made of the parts involved in the disease, and has deposited it in the Museum of King's College. This preparation he has permitted me to examine. It displays a cavity in the upper part of the left lung, large enough (I conceive) to hold more than a pint of fluid. It is of an irregular shape. Internally it exhibits elevations and

depressions, and pits and sinuses. Yet the surface, as it at present appears, is so smooth that it might be lined by a continuous membrane. At one part a firm rounded cord, which has the appearance of a blood-vessel, runs across it; and low down a single bronchial branch, of about the third division, is seen entering it. Externally it adheres by at least two-thirds of its circumference to the ribs; and in all this space not the least remnant of pulmonary structure intervenes. The medium of adhesion interposed between the cavity and the ribs is mere membrane, which the cut edges show to consist of several layers, each about as thick as ordinary writing paper.

Two cases have occurred within my own knowledge, in which the metallic sound proceeded from a mere pulmonary cavity. In them it was present both in breathing, talking, and coughing. It did not accompany the *ordinary* respiration, but a little more energetic breathing would always produce it.

With the metallic sound, and in the same situation there were always present some, but not all, of the common auscultatory Signs which denote a pulmonary cavity. These, however, which *were* present were so strikingly exaggerated, that any one listening at the chest for the first time could not help being convinced that there was a large hollow space beneath his ear.

In whatever way air was impelled into the cavity a dry hollow sound resulted. The breathing was cavernous; the voice cavernous; and the cough cavernous. But there was no Pectoriloquy; and no Gurgling in ordinary breathing, and none in ordinary coughing. But Gurgling was still capable of being produced by a hard and forcible cough.

In these two cases the metallic sound was heard over one large space on the right side the chest, circumscribed by the sternum and axilla laterally, and by the clavicle and the mamma above and below, while from every part of the same lung, except this space, the clearest respiratory murmur proceeded. In one of the cases there was a cavity in the corresponding part of the left lung, but occupying a more limited space. This gave out no metallic sound; but it furnished all those auscultatory Signs, which were defective in the other cavity from whence the metallic sound proceeded.

Here, upon a comparison of the auscultatory Signs derived

from both lungs, thus much at least may be learnt, that there are conditions belonging to pulmonary cavities which naturally produce one sound and naturally prohibit another; or, conversely, which prohibit one and produce another. The largeness of the cavity in the right lung, and the scanty fluid within it, gave occasion to the metallic sound, and at the same time prevented the Pectoriloquy and the Gurgling respiration from taking place; while the smallness of the cavity in the left lung, and the abundant fluid that it contained, forbade the metallic sound, and caused at one time Pectoriloquy, and at another Gurgling Respiration.

The metallic sound, arising from a mere pulmonary cavity, is a curious phenomenon. It would be interesting to know a little more than we do of its clinical history; especially, whether it belongs to any particular form of Phthisis to produce that sort of cavity which gives occasion to it.

If I could trust my own observation and that of others for proof of a general fact, of which the particular instances are few in my own and not many in any man's experience, I should say, that the pulmonary cavities, from which the metallic sound arises, chiefly belong to *unmixed* Phthisis.

It is where the disease has been of long duration, and where it has gone on enlarging itself within its own specific limits, and has imparted little or no irritation to the surrounding lung, that tubercle runs into tubercle, and vomica into vomica, and one large cavity is ultimately formed out of many.* This is the sort of cavity which gives occasion to the metallic sound. But Phthisis must meet with all the circumstances most favourable to its progress as a purely specific disease, in order that it may be able to form such a one. Above all, it must occur in lungs not over disposed to inflammation. Lungs naturally apt to inflame are impatient of the specific disease that is carrying on its own processes within them. They interfere with its progress from first to last, and do not allow it to reach the greatest increase of which it is capable in any stage, and least of all, in that which is the fullest of irritation to surrounding structures —the stage of vomica.

In the two cases to which I have alluded, within my own experience, the Phthisis had been suspected for years, and had

* Vide p. 155.

existed unequivocally for many months. In both I noted the gradual progress and enlargement of the vomica, before the metallic sound arose, and afterwards. And both before and afterwards there was a clear respiratory murmur in every other part of the same lung.

But it has been said that the metallic sound may result from a complex disease formed between the lungs and the pleura. Here also a cavity is equally concerned in producing it. But the cavity is not in the lungs, but in the pleura; or rather the pleura itself constitutes the cavity. Here, too, the cavity contains fluid, and it contains air. The fluid is supplied by the surface of the pleura itself, but the air is supplied by the lungs.

Now these pathological conditions arise after the following manner. Disease, first existing in the lungs, causes an ulceration or rupture of the pleura, and thus makes an aperture of communication between the bronchi and the pleural cavity. And this aperture once made continues ever afterwards, being kept open by the air that passes through it in the act of respiration. The fluid in the pleuritic cavity is either the mere serous effusion of hydrothorax, or the pus or puriform secretion of inflammation.

Such are the conditions of this complex disease, and such the manner in which they come to pass. But how is the metallic sound produced by them? In every act of breathing, talking, and coughing, the air which is forced through the aperture into the cavity of the pleura, puts in motion the air and the fluid already found there. Hence, a vibration results which is followed by the metallic sound.

But what is the element of the whole disease? Of what nature is the primary morbid process within the lungs which serves as the point of departure (so to speak) for the series of morbid processes that follow? Surely it may be any disease or morbid process that is capable of effecting a solution of continuity in the pleura investing the lungs; whether by ulceration, slough, or rupture. But, in point of fact, Phthisis Pulmonalis is found to do this more frequently than any other. A Vomica, being formed and already communicating with the bronchi on the one hand, reaches, penetrates, and communicates with the cavity of, the pleura on the other. In all cases within my own experience Phthisis has thus become the element of the

whole disease, except one. And there a gangrenous portion of lung opened the way of communication between the bronchi and the cavity of the pleura, and produced all the conditions necessary to the auscultatory Sign in question.

This, I believe, is a just explanation of the metallic sound when it is produced by (what is called) Pneumothorax; or that complex disease between the lungs and the pleura, of which the most striking result is a collection of air within the thoracic cavity.

For the sake of this explanation I have thus far represented the metallic sound as if it were a single auscultatory Sign standing alone, that so you might *understand* it the better. But you can only recognise it and see its importance and feel an interest about it, by having it represented to you with all its attendant circumstances, and especially with the other auscultatory Signs concurrent with it; these contributing to determine its value and it to determine theirs.

A patient may have all the general symptoms of Pulmonary Consumption, such as emaciation, hectic fever, and cough. A few days ago you may have examined his chest carefully, and found on one side (the right perhaps) gurgling respiration and gurgling cough, and cavernous breathing, and at times pectoriloquy, all in one circumscribed space beneath the clavicle; and, around this space and over the upper half of the right side, considerable dulness both when you knock and when you listen; but over the lower half a satisfactory resonance to percussion and a clear respiratory murmur. On the left side you may have found no unnatural sound whatever, but the respiratory murmur everywhere exaggerated, and the resonance to percussion everywhere louder even than at the lower part of the right side, where it was quite loud enough for health.

Here the diagnosis is plain enough. The upper part of the right lung is full of tubercles; and in the midst of them is one Vomica at least: while the lower part is either free from tubercles altogether, or contains so few that they furnish little or no impediment to the passage of air. The left lung, which is striving by a more energetic respiration to compensate for the obstruction of the right, may contain a few scattered tubercles, or none at all.

Such is the diagnosis at which you arrived a few days ago;

and such the obvious auscultatory Signs that led you to it. But *now* the gurglings, the cavernous sounds, and the pectoriloquy have ceased; the upper half of the right side, before, behind, and in the axilla, which was so dull, has become clear to *percussion*, and hollow and resonant as a drum; and the lower half, which was so clear, has become so dull, that neither ear, nor stethoscope, nor percussion, can ascertain or elicit from it any sound whatever. On the left side the auscultatory Signs remain just what they were.

But, on the right side, what sudden and extraordinary change has taken place in the parts within, correspondent with the sudden and extraordinary change in the auscultatory Signs? You will see, if you wait and look a little more inquisitively into all the circumstances. And I invite your most curious attention to this instance; for it assigns a sort of triumph to Auscultation, showing, I conceive, the utmost perfection of which the diagnosis of internal disease is capable.

Where it was dull, the chest is resonant; where it was resonant, it is dull.

It is resonant, loudly resonant to *percussion* at the upper part.* Then surely there is air within.—But the ear detects no respiratory murmur. The air, therefore, must be under some peculiar conditions. Is it imprisoned within the lung in dilated vesicles, or effused from ruptured vesicles beneath the pleura? There is, indeed, the same contrast between what percussion intimates and what Auscultation, as in the case of Emphysema. But there is no Emphysema, notwithstanding. The resonance is too loud, and too uniformly clear all round the chest; and the air, wherever it is, is in too free a space for Emphysema. For the same reasons the air cannot be contained in any mere pulmonary cavity, however large. Besides, the ear not only detects no respiratory murmur, but it has lost the gurglings, the cavernous sounds, the pectoriloquy which it once heard.

* I speak of "the upper part" of the chest *generally;* for it is probable that at its very summit the contrast will not be so marked. By the time the disease has reached the stage of vomicæ, the apex of the lung has often contracted firm adhesion to the ribs; and the air, finding its way into the pleura from an opening *below* this adhesion, cannot have the effect of rendering tympanitic that part of the chest which is *above* it.

But does the ear, by mere listening, catch no sound whatever, where the resonance to percussion is so loud? Yes! at each breath, each voice, or each cough, it catches a metallic sound, which lasts for an instant, like an echo, after the breath, the voice, or the cough has ended; and this sound is a ring or a tinkle.

But the chest is dull, absolutely dull, at the lower part. And the cause of this dulness is the effusion of fluid into the cavity. Do you doubt the fact? Then move the trunk of the body quickly to and fro; and listen the while with your ear to the thorax; and you will at each succussion hear the plashing of fluid within. Or make your patient alter his posture from the vertical to the horizontal, or *vice versâ*, and the resonance and the dulness will sometimes shift their situations.

Thus air and fluid share the cavity of the pleura between them. The air found its way thither from the Vomica, either by a direct perforation of its walls, or by a fistulous channel proceeding from it. The fluid is a secretion from the surface of the pleura itself. And the air and the fluid, thus sharing the cavity of the pleura between them, do, by the manner in which they are made to act upon each other, produce the ringing or the tinkling which accompanies the breathing, the voice, or the cough, and the plashing, which attends succussion.

Observe, I have here spoken of a single instance, but I have represented a class. And if you will view it as such, and will consider, in this instance, how wonderfully Auscultation enables your knowledge to keep pace with the actual procedure of the disease, revealing its exact conditions at each particular time, and its changes from one condition to another from time to time, you must allow that the ear may sometimes be trusted not less confidently than the eye itself for a sure diagnosis. But each sense has its proper sphere. Yet, in this instance, if you could look into the chest of the living man, what more could you know than you *do* know by listening at it?

Such is the metallic sound proceeding from Pneumothorax, with all its attendant circumstances, and concurrent auscultatory Signs. It has always been, when I have met with it, a *tinkling sound*; louder or fainter, more or less prolonged into an echo, like this thing or that, but always a tinkling sound.

This metallic tinkling does not occur at any one certain

period of phthisical disease. I have known it discovered for the first time a few days previous to the patient's death; and I have known it and the other accompaniments of Pneumothorax exist for six months; and in the meantime the patient's general health has improved, and he has even gained flesh. These varieties in the clinical history of different cases are just what might be expected. The conditions out of which the metallic tinkling springs, are, in a certain sense, accidental. A vomica may happen to be near the surface of the lung, and may ulcerate its way through the pleura, as well at one period in the progress of the disease as at another.

But I do not mean to limit the possibility of surviving, after Pneumothorax has taken place, to six months, or to any certain period. I recollect a patient or two, who, after they had been for some time objects of great interest, on account of the metallic tinkling and the audible plashing of fluid within the chest, suddenly left the hospital, and took care to leave us no means of tracing them. How long these men survived I cannot tell.

It is worthy of remark that Pneumothorax thus superadded to phthisis does not necessarily produce a great aggravation of the patient's distress. For he is often still able to move his body easily in bed, and even to walk about the room. In some so affected I have remarked the interest, and even the amusement, they have taken in the circumstances of their own complaint. The business of Auscultation, which to most patients, I fear, is a trouble and a distress, they submit to with alacrity twenty times a day. They hear, themselves, the plashing of fluid within the thorax. And this, which they think so strange, they are always ready to exhibit, and to shake their chests as often as they are desired, until they have satisfied the curiosity of every inquirer.

Probably the period to which the patient survives after the metallic tinkling and its concomitant symptoms have arisen, and the much or little augmentation of distress attending them, may depend upon the kind of fluid effused within the chest, and the kind of morbid process engaged in producing it.

In all the instances of Pneumothorax with the metallic tinkling, except two, with which I have been acquainted, the patient has lived several months, and the fluid found upon

dissection has been the mere serum of hydrothorax. In one of the accepted instances, the metallic tinkling was not present, or at least not discovered, until within three days of the patient's death; and from that time the agony was frightful. Moreover there was this peculiarity in the auscultatory Signs, that every part of the chest, in all positions of the body, returned the loudest resonance to percussion, and gave out the clearest metallic tinkling in breathing, in speaking, and in coughing. The patient was too ill to permit succussion. The absence of its direct auscultatory Signs made us doubt whether there was any fluid in the chest. Fluid, however, was found in very small quantity; but that fluid was pus.

In the other excepted instance, the metallic tinkling was heard for eighteen days before the patient's death. It was a case of pneumonia, in which, after the severity of the inflammation was subdued, Pneumothorax arose, with metallic tinkling and, during the last few days of existence, with fetid expectoration.

If, on the left side, a line had been drawn round the chest from the sternum to the spine, on a level with the mamma, you might have heard the metallic tinkling and the loudest resonance upon percussion, everywhere below it, and a clear respiratory murmur everywhere above it.

Upon examination after death, the left lung was found adherent to the ribs in a space corresponding with the imaginary line just indicated. By this adhesion the pleura was divided into two unequal cavities. In the upper and smaller cavity no fluid was effused, and the lung was healthy. In the lower and larger there were both air and fluid, and the lung was diseased. The fluid was dark and fetid, and the lung was partly in the state of grey and red hepatization and partly gangrenous. In the midst of a gangrenous portion was an orifice through which air found its way from the trachea into the pleural cavity.

AN

ACCOUNT

OF THE

DISEASE LATELY PREVALENT

AT THE

GENERAL PENITENTIARY.

BY

P. MERE LATHAM, M.D.

FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS, AND PHYSICIAN TO ST. BARTHOLOMEW'S HOSPITAL.

LONDON.

MDCCCXXV.

TO

JOHN LATHAM, M.D.,

PHYSICIAN EXTRAORDINARY TO THE KING,

&c., &c.,

THE FOLLOWING PAGES ARE DEDICATED

WITH EVERY FEELING OF

DUTY, GRATITUDE, AND AFFECTION,

BY

HIS SON.

PREFACE.

I FEEL it to be a duty which I owe to the Medical Profession to give them some account of the Disease lately prevalent at the General Penitentiary. I have abstained from doing so until the present time, in order that I might secure for it that attention which medical men never fail to bestow upon subjects essentially their own, when they are left entirely to themselves.

So long as such questions happen, from circumstances, to engage the interest of the public, or to admit the least mixture of party spirit, medical men can hardly feel confident of their own impartiality; and they would do well, perhaps, to suspend their inquiry, and, certainly, to withhold their decision, until that interest and that spirit have had time to subside.

While the Disease of the Penitentiary engrossed a large share of public attention; while the newspapers were discussing its nature and treatment; and a coroner's jury was giving judgment concerning its causes; while it furnished matter for debate in Parliament, and Committees of the House of Commons were examining evidence and reporting upon it, such a time would not have been well chosen for inviting formally the attention of medical men to the same subject; and had I then been prepared with this account, which I now submit to them, I should still have suppressed it, until the fairness and impartiality of their decision could be better ensured.

Dr. Roget and myself were employed in the service of the Penitentiary for fifteen months, from March, 1823, to May, 1824. During our attendance, we were accustomed to make

memoranda of all circumstances which appeared important at the time, respecting the prevalent disease; and we both of us find in our possession some short note of almost every case which occurred.

Besides the detail of symptoms, and of the effects of remedies in particular cases, I was in the habit of writing, from time to time, general descriptions of the Disease under its most striking forms, and of the modes in which, under each form, medical treatment seemed to operate its relief. All these details and descriptions, which I made in the course of my attendance, I preserved, and, when the period of my employment at the Penitentiary expired, I revised and put them in order, and found them capable of furnishing the materials for the following history of the Disease.

During the first five months of our attendance, Dr. Roget and myself felt our office one of great labour, and of peculiar difficulty. The medical management of the Prison alone, from the number of the sick, from the nature of the Disease, and the character of the patients, was by no means an inconsiderable charge. But in addition to this, we had formal Reports to make, questions to answer, and explanations to give, verbally and in writing, to the Managing Committee of the Prison, and to Government; occasionally coroners' juries to attend, and to undergo frequent examinations before a Select Committee of the House of Commons.

These duties, however, which were simply laborious in themselves, were only rendered painful by the extreme vigilance and circumspection which peculiar circumstances, unnecessary to mention in this place, obliged us to exercise in all our conduct. Nevertheless, our cordial co-operation, and our mutual friendship and confidence, enabled us to sustain the care

and responsibility inseparable from our office; and I only now allude to them for the sake of showing, that our daily labours were too pressing and anxious to allow us to expend much time in arranging the notes we had taken, or constantly to bear in mind the purpose of publication.

These notes, however, were faithfully taken at the time, and sufficiently copious; and the history which I have drawn from them, will be found (I trust) to represent the real character of the Disease, clearly and intelligibly, to the minds of medical men.

As the labour and responsibility of our charge were equally divided between Dr. Roget and myself, so the practical measures adopted were the result of our consultations. I am not aware that there ever arose between us the smallest disagreement either in opinion or practice; and I reckon it among the best fruits of my labours on this occasion, that they have procured for me in my colleague a faithful friend.

To Dr. Roget I am also indebted for suggestions, most useful to me in drawing up this account of the Disease, and I am allowed to appeal to him in confirmation of its truth.

Four months of the fifteen, during which we were employed at the General Penitentiary, that is, from the end of July until November, 1823, we had the able assistance of Drs. Hue, Macmichael, and Southey. Under what circumstances, in consequence of our own earnest solicitation, that assistance was granted to us, will appear in the course of my Narrative.

It is my duty, however, in this place, to state, that to these gentlemen, and to Dr. Macmichael especially, is to be attributed whatever success may be thought thenceforward to have attended our investigations into the origin of the Disease. Dr. Roget and myself had already notified to the Committee our belief,

that there was some cause in operation over and above those to which we had originally attributed it; and that there was a suspicion of contagion, and a suspicion, moreover, of an injurious influence peculiar to the place, but nothing ascertained concerning either.

Now, Dr. Macmichael, in perusing the evidence given by Mr. Pratt, the apothecary, before a committee of the House of Commons, found a statement, from which he took occasion to suggest to us a new train of inquiry. This inquiry ended in the production of certain documents, which established (we conceive) the prevalence of a disorder of the bowels, having the same general character with that which constituted a principal part of the late epidemic, within the Penitentiary, since its first foundation.

AN ACCOUNT

OF THE

DISEASE LATELY PREVALENT

AT THE

GENERAL PENITENTIARY.

CHAPTER I.

INTRODUCTION.

In giving a medical history of the disease lately prevalent at the General Penitentiary, I shall best succeed in rendering it intelligible to the reader, if I describe all the circumstances connected with it in the same order in which they originally presented themselves to my own observation.

The disease did not immediately discover its complete character. It put on a great variety of forms, while all its symptoms did not appear at one time and in each individual; and it is only, after continual observation of it during the space of more than twelve months, in which it has existed and ceased, and again and again recurred in many hundred individuals, that I can state, what I now venture to do, concerning its symptoms and its essential nature, its treatment and its probable causes.

There remain many circumstances respecting this disease, of which I profess myself still ignorant, and which the opportunity of watching those who have been the subjects of it for many years, could alone satisfactorily explain.

The character of the disease, as it first came under our observation, will be best collected from the Report, which Dr. Roget and myself delivered to the Committee on the 5th of April, 1823.

In consequence of the unusual degree of sickness which had recently been discovered in the Penitentiary, we were desired to visit it, and to take the sick under our care, to inquire into the origin of the prevalent disease, and to turn our attention particularly to the dietary.

This Report contains the sum of our observation and inquiries during a month, and the conclusions which seemed fairly to result from them.

REPORT OF THE PHYSICIANS ON THE STATE OF THE GENERAL PENITENTIARY AT MILBANK.

5th April, 1823.

TO THE COMMITTEE OF THE GENERAL PENITENTIARY AT MILBANK.

GENTLEMEN,

IN conformity with the instructions conveyed to us in your resolution of the 28th February last, we have visited the Penitentiary daily, since the 1st of March; we have carefully and repeatedly examined, at different times, the state of health of each individual prisoner; we have taken constant charge of the sick in the infirmaries; we have communicated continually with your medical officers, Mr. Hutchinson and Mr. Pratt, and frequently with the other officers of the establishment; we have made whatever inquiries seemed requisite to obtain correct information concerning the nature and extent, and the origin and progress of the disease lately prevalent in the Penitentiary, the causes which probably contributed to its production, and the means most expedient for its cure, and most likely to prevent its recurrence; and we have agreed upon the following Report:—

From the testimony of the officers of the establishment, and particularly of the matron, it appears, that during the last autumn the general health of the prisoners began visibly to decline. They became pale and languid, and thin and feeble. Those employed in tasks requiring much bodily exertion, were unequal to the same quantity of work as formerly. Those at the mill could grind less corn; those at the pump could raise less water. From time to time several of the laundry-women fainted under their work; and the business of the laundry could only be carried on by continually changing the hands engaged in it. Such was the general state of the prisoners throughout the winter.

Still, notwithstanding this remarkable depression of the general health, there appeared among them no manifest signs of any peculiar disease. The number of sick received into the infirmaries did not much exceed the proportion which, in the winters of former years, it had borne to the total

number of prisoners; and their disorders were those commonly incident to cold weather. It was not until the beginning of February, that any marks of scurvy were reported by Mr. Hutchinson, as having been noticed by him on a few individuals in the infirmaries. And here it may be observed, that these marks are, at their first appearance, peculiarly apt to escape discovery, unless the attention be particularly directed towards them; and that they often exist for a long time, entirely unnoticed by the patient himself. Between the 14th of February and the 1st of March, no less than forty-eight prisoners came into the infirmaries, affected chiefly with diarrhœa and dysentery. The diarrhœa and dysentery were of a peculiar kind, and were suspected to have a connexion with the scorbutic disease. At this time, also, all these various affections were found spreading extensively, but in different degrees of severity, throughout the prison.

On the 28th of February, our assistance was called for; and having learned the facts already detailed, we began our examination of the prison and the infirmaries on the 1st of March. We found the prevailing disease to be the same with that which is known by the name of *Sea Scurvy*, and which is characterized by livid spots, or blotches of the skin, especially on the lower extremities. Conjoined with the scurvy, in almost every case, there was diarrhœa or dysentery. There were indeed, a few instances of scurvy without disorder of the bowels; and moreover, numerous instances occurred of diarrhœa and dysentery, where no marks of scurvy had appeared. But still, whether the scurvy subsisted alone, or the diarrhœa or dysentery subsisted alone, or whether they were conjoined in the same individuals, there was found in all those who suffered from either, or from both, the same constitutional derangement, denoted by a sallow countenance, an impaired digestion, diminished muscular strength, a feeble circulation, various degrees of nervous affection, as tremors, cramps, or spasms, and various degrees of mental despondency.

These facts seemed to lead directly to the belief, that the diarrhœa and dysentery, and scurvy, had their origin in the same morbid state of the constitution. In this belief we were more and more confirmed by further observation; and we soon had the means of determining with certainty, that they, in reality, constituted one and the same disease. We examined by dissection the bodies of two prisoners who died dysenteric, and found in various parts of the intestines, the morbid appearances called, in medical language, *Ecchymoses;* that is, spots of the same kind as those which on the skin constitute scurvy. We found, in fact, an absolute scurvy of the bowels, of which the diarrhœa or dysentery was only a symptom and consequence.

With regard to the extent of this disease, we found more than one-half of the whole number of prisoners affected by it, in one or other, or in all its forms; but the proportion was not the same among the prisoners of different sexes, or belonging to different classes. The women were affected much more extensively than the men; and of both men and women, the second class, which is composed of those who have been longest in confinement, was affected in a much larger proportion than the

first class, which comprises those who have been more recently imprisoned. Of the women, about two-thirds were ill of the disease; of the men, rather less than one-half. Of the women in the first class, one-half were ill; of those in the second class, five-sevenths. Of the men in the first class, above one-third were ill; of those in the second class, rather more than one-half. The exact numbers are stated in the Table subjoined to this Report.

Some striking exemptions require to be noticed. Of the 24 prisoners employed in the kitchens (13 men and 11 women) belonging to the class which had suffered most extensively, all were free from the disease, excepting three, one woman and two men. These three had been promoted to the kitchen within four days. It is proper to add, that the officers and servants of the establishment, together with their families, residing within the walls of the prison, and amounting to 106 individuals, were universally exempt from the disease.

We took some pains to ascertain the period at which the disease in question might be considered as having commenced, and the gradations by which it had reached its present extent and aggravation. It appeared reasonable to assume, that whenever upon the feeble and drooping condition observed among the prisoners throughout the winter, diarrhœa or dysentery, or scurvy supervened, then the disease was fully constituted. With respect to the scurvy, it was scarcely possible to assign the exact time at which it commenced, on account of the insidious mode of its attack, and the facility with which it may elude observation on its first appearance. But we have fully satisfied ourselves, that there existed among the female prisoners, a few cases of decided scurvy, as early as the month of November. Among the men we cannot trace any instance of scurvy, back to a remoter period than two months. It is certain, however, that it was not until after Christmas that the scurvy had spread very extensively among either sex. About the middle of January, the instances had become numerous among the women; and among the men, about the middle of February; and it continued to increase progressively in both sexes, until the first week in March.

The diarrhœa and dysentery appear, in their origin and progress, to have kept pace with the scorbutic symptoms. Upon inquiry among the prisoners, we found that some of them had been occasionally suffering from diarrhœa before Christmas; but the instances being few, and the cases yielding readily to common remedies, they did not excite any alarm, and were naturally imputed to accidental causes. Under ordinary circumstances, such a conclusion might have been fairly admitted; but considering what the general health of the prisoners then was, and with our knowledge of what has since occurred, we cannot but suspect that in some of these instances, the diarrhœa belonged to the same disease, of which it has since been found to constitute the principal and most formidable symptom.

In the course of January, the instances of diarrhœa were too numerous to be attributable to common or accidental causes. But, even then, it had

not become matter of general complaint, for it was not attended with much pain, and in most of the sufferers it continued for a short period only, and then ceased; but it renewed its attacks from time to time on the same individual, gradually, though insensibly, impairing his strength. In this manner, through the month of January, many of the prisoners were sustaining a severe injury to their constitution, without being conscious of more than an accidental ailment, and without applying for relief.

Increasing daily in extent and severity, it at length became matter of complaint; and at the latter end of February, diarrhœa and dysentery constituted a large proportion of the cases in the infirmaries. Three deaths from this disease, occurred between the 14th of February and the 1st of March, the day on which we made our first examination of the prison and the infirmaries. In the prison, the disease had reached the extent already mentioned; and in the infirmaries there were 64 patients labouring under the disease in one or other of its forms.

In inquiring into the causes of the disease in question, we think it right to state our persuasion, that the situation of the prison has not contributed to its production. First, because, if this had been the case, it is reasonable to suppose that the same disease would have occurred in former years; whereas it has never appeared until the present winter. Secondly, had this been the case, the officers of the prison, being equally obnoxious with the prisoners to any injurious influence of situation, could not have been universally exempt, as it appears they have been, from the same disease. Thirdly, because, if the situation of the prison be injurious, it must be presumed to be so in consequence of marsh miasmata arising in its neighbourhood; yet, since its establishment, the prison has been altogether free from those diseases which marsh miasmata confessedly engender. Fourthly, because marsh miasmata always arise during the hot, and never during the cold seasons of the year; and the diseases which they engender belong to the same seasons. Lastly, because although scurvy and dysentery have undoubtedly been found prevalent in marshy districts, yet when marsh miasmata have produced them, they have been associated with intermittent fevers, and have occurred only at the hot seasons of the year. It may possibly be suspected that the simple dampness of the situation may have contributed something to the disease. But we can state with confidence, that every part of the prison is singularly dry; and that in no cell or passage, on no floor or ceiling, or wall of the prison, have we found the smallest stain or appearance of moisture.

Several circumstances respecting the disease in question, which have been already mentioned, seemed to limit the causes of its production to such as could have had their operation exclusively upon the prisoners, and especially at the present season, and now for the first time. One such cause is found, we conceive, in the diet of the prison. During the last eight months the diet was different from what it had been ever since its establishment. The change which took place in July last, reduced the animal part of the diet to almost nothing. In a soup made of pease or barley, ox heads were boiled, in the proportion of one ox head to 100

male, and one to 120 female, prisoners : and we found upon inquiry, that the meat of one ox head weighed, upon an average, eight pounds, which, being divided among a hundred, allows only an ounce and a quarter for each prisoner. This new diet had been continued until the present time ; and to it we mainly ascribe the production of the disease in question.

It does, nevertheless, appear to us, that the diet of the prison has not itself alone been productive of the disease, but that it required the concurrence of other causes, of which the severity of the winter was probably the chief. The origin of the disease has been traced to the commencement of the cold weather, and its progress and increase have kept pace with it. There are, moreover, two circumstances which confirm us in the belief, that diet and cold have been concurrent causes. The sufferers were most numerous in that class of prisoners which were most exposed to the influence of cold, from the lower temperature of the cells in which they pass the night : showing, that where both causes most conspicuously concurred, the disease was most extensively produced. Yet those individuals of that class, who, sleeping in the same cells and exposed to the same low temperature by night, were employed in the kitchen by day, and had access to richer diet, were universally exempt ; showing, that where one cause was withdrawn, the other was of itself inadequate to produce the disease.

Such being the character and extent of the disease in the Penitentiary, and such its most probable causes, we proceeded to adopt those measures for counteracting it, which its own nature, and the opinion we entertained of its origin seemed to suggest. We ordered an immediate change in the diet of the prison. In place of pease and barley soup for dinner, we substituted a daily allowance of four ounces of flesh meat, and eight ounces of rice daily for each prisoner, and white bread instead of brown : and, as the cheapest and best antiscorbutic article of diet which could be procured at this season of the year, we ordered three oranges for every prisoner daily, one at each meal.

It is unnecessary to detail the methods of medical treatment employed in the infirmaries.

On our examinations of the prison between the 12th and the 19th of March, we found the general aspect of the prisoners visibly improved. The taskmasters informed us that they were more cheerful, and did more work ; and particularly that those employed at the mill could grind one-third more flour. The scorbutic marks had, in almost every case, begun to decline, and in many of the slighter cases had absolutely disappeared.

On our general surveys of the prison between the 31st of March and the 4th of April, we could not find more than fifty individuals of both sexes, on whom any marks of scurvy remained ; and on the greater number of these, they were so slight as hardly to be detected.

The diarrhœa and dysentery have, upon the whole, kept pace in their decline with the gradual disappearance of the scorbutic spots. On each of our examinations of the prison, we found them relieved or cured nearly in the same proportion ; and, on our last examination, there were not

remaining so many as twenty cases of bowel complaints in the whole prison.

It is proper to remark that the diarrhœa and dysentery, being the most formidable part of the disease, was that for which medical treatment was especially required. Therefore, of the prisoners thus affected, we have constantly received as many into the infirmaries as there was room to accommodate, whether their cases were severe or slight. At the period when, as we have stated, the disease was upon the decline, that is, during the last weeks of March, it will be observed that there was a greater number of prisoners in the infirmaries than at the period when the disease in all its forms was at its greatest aggravation and extent, that is, during the first week in March. The truth is, that when we began our attendance, we found only the severer cases of bowel complaints in the infirmaries; but as soon as we had learned, by the dissection of two patients who died dysenteric, that the disease tended to produce irreparable organic mischief of the intestines, we thought it right to bring as many cases as possible under strict medical treatment: and moreover, as soon as we had learned in the course of our observations, the great liability of the diarrhœa and dysentery to return, we thought it right to use the greatest possible vigilance over particular cases, during the period of their convalescence. Hence, many in whom we most strongly suspected this proneness to relapse, were still kept in the infirmaries, after the actual symptoms of their disease had disappeared; and a convalescent ward, in addition to the ordinary accommodation of the infirmaries, was opened for their benefit. These are the circumstances that are to be borne in mind, in order to reconcile the apparent inconsistency of the number in the infirmaries being greater, at the very time when the disease in the prison was daily and rapidly declining.

From the 1st of March to the present day, 222 patients have been admitted into the infirmaries, making, with the 110 already there, a total of 332 patients. Of these, eleven have died, six of dysentery, and the remaining five of diseases unconnected with the present disease. At present, the total number of patients in the infirmaries is 101, namely, 64 women and 37 men. Of this number we consider that 36 are convalescent, and exhibit no symptom of disease; and they are retained in the infirmaries only by way of precaution against relapse: 19 only are still suffering the symptoms of the disease; and 46 are affected with other complaints.

It remains for us to fulfil the wishes of the Committee, by suggesting to them some considerations respecting diet. With regard to the diet of prisoners undergoing punishment for crimes, we presume the object to be, that they should have enough for nourishment and health, and nothing more. How much, and what quality of food will actually suffice for this purpose, can be deduced only from numerous and careful experiments. But no such experiments, as far as we know, have ever been made. There are certainly none upon record, to which we can refer for information. We beg, therefore, that the observations we venture to make, and the recommendations we offer, respecting diet, may be accepted as the result

of the best consideration we can give to the subject, in the absence of positive experiments.

Practically, the main question seems to be, Can animal food be safely excluded from prisons, and particularly from the Penitentiary? We are aware that a large portion of the labouring agricultural population of this country subsists altogether upon vegetable food, and is generally reputed vigorous and healthy; and we admit the justice of the inference, that an exclusively vegetable diet is *generally* wholesome; and we allow, moreover, that to submit those confined in prisons to such a diet, is a justifiable experiment. But still it is merely an experiment; and considering that every circumstance of the present condition and previous habits of those imprisoned for felonious crimes, is as different as possible from the simple condition and simple habits of an agricultural population, we should not be surprised to find that the experiment generally failed. At the Penitentiary there are, we conceive, peculiar obstacles to its success. These consist chiefly in the long periods of confinement, and the great number of prisoners.

To prisoners in a house of correction, whose period of confinement is limited to a few months, little hazard would result from an habitually scanty diet. People may be under-nourished for a short time, with impunity; but prisoners who are in the course of a confinement for five, or seven, or ten years (and none are condemned to less in the Penitentiary), cannot safely be subjected to the same system. Many injurious influences will arise in the course of years, which a few months would not produce. There will be changes and inclemencies of seasons to be provided against, and the heavy pressure of moral circumstances, for which, although they cannot be strictly appreciated, large allowances must be made. The great number of prisoners at the Penitentiary, independently of the contingencies to which they are exposed in the course of a long confinement, renders such an experiment peculiarly hazardous. Restriction to a vegetable diet, or to a diet that is considered just sufficient for nourishment and health, requires a constant vigilance over the health of each individual prisoner. Such a vigilance is the only security against the possible evils that may arise. In a prison containing 50 prisoners, a diet even of bread and water may be adopted without hazard; because there the requisite degree of vigilance can be obtained; and the medical superintendent of such a prison would become so familiar with the aspect of individuals, as to see at once the earliest indications of disease in any one of them. But in a prison containing 900 or 1000 prisoners, the requisite degree of vigilance would be impossible; and for the want of it, a great hazard would be incurred by adopting the same system of diet.

For these reasons, and especially because the diet of the last eight months, in which the animal matter was reduced almost to nothing, has mainly contributed, as we conceive, to produce the present extensive disease, we recommend that, in future, animal food should make a larger part of the diet at the Penitentiary.

Upon the subject of diet, we recommend :—

1st. That half a pound of flesh meat, without bone, be allowed to every prisoner, *once a week, on Sunday.*

2nd. That, in addition, half a pound of flesh meat be allowed to every prisoner once a fortnight, on any day that the Committee may think proper.

3rd. That white bread should always be given to the prisoners, that is, bread made of the best wheat flour, and free from all impurities.

4th. That the prisoners should have one meal each day entirely of solid food; that is, if they have gruel for breakfast, and gruel for supper, that their dinner should not be of soups or broth; but that, of whatever vegetable or animal substances it consist, they should be given in a solid form.

As to the kind of vegetables suitable for the principal meal of the prisoners, a certain latitude must be allowed in regard to those which are most easily procured. All the vegetables in common use are wholesome. Potatoes and rice can be procured at all times; and fortunately they are the most nutritious.

We recommend, that the present allowance of four ounces of flesh meat, with one orange, daily, be continued to every prisoner for a month: that afterwards four ounces of flesh meat be given on alternate days for a fortnight, and that then, if the general state of the prison be healthy, it be put upon the ordinary diet, that shall be determined by the Committee.

In closing our Report, we beg to express our firm conviction that there is now no obstacle to the entire re-establishment to the healthy state of the Penitentiary. We must, nevertheless, add, that for several weeks to come, occasional cases of bowel complaint will probably still be found to arise in the prison; we suggest, therefore, the necessity of great vigilance and frequent inspection, that none of such cases may pass undiscovered; and we recommend, that every case, as soon as it is noticed, be removed to the infirmary, and subjected to the strictest medical treatment. Security against relapse will best be obtained by whatever is calculated to strengthen the constitutions of those who have already suffered, and especially by still employing the means which have hitherto mainly contributed to their recovery. It is with this view that we have recommended the continuance of the present allowance of animal food for another month.

We have examined the accounts which have been transmitted to us from the Secretary of State's office, of the diet used in different prisons in England, contained in the answers to questions which were sent to the visiting magistrates, on this and other subjects connected with the health of prisoners. But on comparing the different plans of diet detailed in those answers, which have as yet reached us, with the objects and system of the General Penitentiary, we do not conceive that any of them will be at all suitable to that establishment. We have to observe, however, that answers to the above-mentioned questions, have been received only from seven of the prisons that have been written to for information on these subjects.

(Signed) P. M. Latham, M.D.
P. M. Roget, M.D.

5th April, 1823.

NUMBERS AFFECTED WITH THE DISEASE.

TABLE of the number of Prisoners of different denominations, who were labouring under one or other of the forms of the Scorbutic Disease, in the General Penitentiary, in the beginning of March, 1823.

MARCH, 1823.	Total Number of Prisoners.	Number ill of the Disease.	Equivalen Numbe per cent
MALES.			
First Class	309	110	35
Second Class	222	121	54
Confined ... under 1 year...	173*	40	23
„ between 1 & 2 years ...	156	74	47
„ between 2 & 3 years ...	165	91	55
„ between 3 & 4 years ...	28	19	68
„ above 4 years...	9	7	78
Total Males	531	231	44
FEMALES.			
First Class	94	52	55
Second Class	233	165	71
Confined ... under 1 year...	37	12	32
„ between 1 & 2 years ...	93	65	70
„ between 2 & 3 years ...	106	74	70
„ between 3 & 4 years ...	68	56	82
„ above 4 years	23	10	43
Total Females ...	327	217	66
OF BOTH SEXES.			
First Class	403	162	40
Second Class...	455	286	63
Total Prisoners ...	858	448	52

From this Report it is obvious, that we had no other opinion concerning the disorder, than that it consisted of a diarrhœa or dysentery, and a slight scurvy combined; that it had been produced by impoverished diet and a severe winter; that it was already nearly cured, and that, although occasional instances of

* Of these 85 had been received into the prison since the 1st of January, and therefore had been subject for a much shorter time to the influence of the presumed causes of the disease.

relapse might be expected, the health of the prisoners would probably be re-established at no distant period.

With respect to the origin of the disease, all the facts, which had come to our knowledge, seemed to conduct so obviously to one conclusion, that we could not hesitate to adopt it; and the facts themselves, being entirely of a nature to be comprehended by persons not medical, we thought ourselves called upon to state them fully in the Report, in order that the Committee might possess the means of judging how far the conclusion to which they had brought us, was correct.

There were certain other circumstances which further confirmed us in our opinion respecting the origin of the disease and the probability of its early disappearance. These, however, being more strictly medical, we did not think it necessary (considering to whom the Report was addressed) formally to specify. The rapid recovery of the sick, when the causes were removed from which their disease was suspected to spring, was a strong presumption, at least, that those causes were rightly ascertained.

In regard to such diseases, especially, as are engendered by defective nutrition, we knew it to be a matter of experience, that they are generally capable of being speedily and effectually cured by an improved diet; and this had been strikingly the case in the present instance.

Further, the medical expedients hitherto employed had been very simple and very successful; and we could not impute a very formidable character to a disease which chalk mixture and tincture of opium could cure.

These were the remedies which we found the medical officers prescribing, when we were first called to the Penitentiary; and seeing that they answered so well, the purpose for which they were intended, we abstained from instituting any new method of treatment.

Such were the views we had taken of this disease after a month's observation and inquiry; and unfortunately it was necessary that all we knew or believed should be published. As soon as our investigations had enabled us to form (what we thought) a satisfactory opinion, we offered our Report to the Committee. By the Committee it was presented to the House of Commons, and immediately ordered to be printed.

This Report, as a medical document, was unquestionably premature, yet I candidly confess we had no such belief at the time.

The conviction it expresses, that there is "now no obstacle to the entire re-establishment of the healthy state of the Penitentiary" was proved, by what speedily occurred, not to have been well-founded; and although our opinion respecting the sources from which the disease was originally derived, was confirmed by numerous medical men who were examined upon the subject, and was at the time entirely satisfactory to ourselves, and equally so to the Committee, facts, subsequently brought to light, have led us to doubt whether this latter opinion was entirely correct.

The Report had hardly been made public when the disease, so far as it was referable to the bowels, began to re-appear: by the middle of the month of May it had again pervaded the prison; and by the middle of the month of June, all the prisoners, without exception, who had formerly suffered; and all, with very few exceptions, who had been exposed to its presumed causes, yet had never suffered before; and all, with very few exceptions, who had been admitted into the Penitentiary since its presumed causes had been removed, were involved in the same calamity; and the remedies, which were formerly successful in controlling it, had not now the smallest beneficial influence.

It should be remarked, that that part of the disease, which consisted in scorbutic spots and blotches, never returned. The few fading vestiges of scurvy, which were still discernible in some, entirely disappeared even while the patients were suffering a relapse of the bowel complaint.

CHAPTER II.

THE SCURVY.

In the Report of the 5th of April, drawn up solely for the information of the Committee of the Penitentiary, all minute and formal descriptions of disease were purposely avoided. Scurvy, and dysentery, and diarrhœa have been hitherto mentioned only by name; but for the information of medical men it becomes necessary to show what it was, that under the name of scurvy, was once co-extensive throughout the prison, with the complaints of the bowels. And we must here make room for the description of it, before we enter upon the consideration of the bowel complaints themselves, which returned after the final disappearance of the scurvy, and of other forms of disease which were superadded.

There is a peculiar condition of the skin, which is vulgarly called goose-skin. It is formed by numerous little elevated points, which give a roughness to the surface. It occurs chiefly upon the extremities; and where it exists to a considerable degree, the surface is dry and unperspirable, and the cuticle falls off in minute scales, or in the form of a white powder. The act of stripping up the shirt sleeves, or pulling down the stockings, often shakes off a quantity of this white powder from the arms or legs. This state of the skin is produced by the common causes of constitutional weakness, and there can be no surer sign of the body being (if I may so say) out of condition. It is met with chiefly among the poor, who are ill-nourished and ill-clothed. Upon our first visit to the Penitentiary this peculiar condition of the skin at once attracted our notice, for it was found almost in every prisoner, and to a very remarkable degree.

In whatever part of the extremities this condition of the skin was most conspicuous, there were always found certain specks or spots of a blue or livid colour, formed by blood extravasated beneath the cuticle.

In some, these spots were no larger than a pin's head, and quite circular; when they showed themselves hardly anywhere, except just at the outside of the knee near the bend of the hams.

In some, intermixed with these spots of the smaller size, were others of the diameter of a pea, and quite circular; when they showed themselves over various parts of the extremities, but still especially about the hams.

In some, there were blue and livid spots of a much larger size, still preserving a sort of circular figure, and evidently formed by two or three of the smaller spots, with which they were intermixed, coalescing into one. These specks and spots of all sizes, intermingled and coalescing with each other, were distributed over every part of the extremities.

In others, together with these specks and spots which have been described, or without them, there were large blotches of an irregular shape, and of such an extent as to leave the legs, or arms, or thighs, or buttocks, almost uniformly livid, with hardly an appearance of their natural complexion.

In a few there was ecchymosis of the conjunctiva; and in a few, ecchymosis and swelling of the upper eyelid.

These appearances, occasioned by blood extravasated beneath the cuticle, from the clusters of little livid points about the hams to the large livid patches occupying the whole limb, must be regarded as indicating different degrees of the same disease, by whatever name it is called. Some concomitant conditions, which remain to be noticed, will enable us perhaps satisfactorily to determine what that name ought to be.

The gums were spongy, and soft, and livid, and disposed to bleed, in all those who had extensive discolorations of the skin; and in those who had mere specks and spots of ecchymosis, their condition, although it might not have been noticed under ordinary circumstances, was far from being healthy; they had a purplish hue, and were tender and sore, and often ragged, just where they come in application with the teeth. In a few, and especially in one man (Henry Peers) the mouth seemed in a state of absolute rottenness, the gums bleeding and broken down, the teeth loose, and their fangs half exposed, and the whole mucous membrane of lips and cheeks black and ragged, while a foul cadaverous smell was emitted with the breath.

In many who exhibited specks and spots only of ecchymosis, and in all whose limbs were covered with large blotches, the muscles of the legs were perfectly hard and rigid. In a few, the legs were œdematous, and one man (Henry Peers, already mentioned) was universally dropsical.

These forms of disease, which have been described, were justly denominated scurvy. The scorbutic character was equally unequivocal in all the degrees, from the least to the greatest, in which the disease was seen at the Penitentiary, and fortunately, in the great majority of cases, it was seen in its mildest form.

It has been already stated in the Report, what means of treatment were employed, and how far the scurvy had disappeared when those means had been in use a little more than a month. After the lapse of another month there might still remain several, in whom a very experienced eye could recognize certain little brown spots, as the last and almost worn out vestiges of scurvy; but not more than half a dozen individuals could have been shown, whom a common observer would have regarded as authentic examples of what the disease had been. These were the few who still exhibited the remnants of large blotches upon their limbs.

CHAPTER III.

THE BOWEL COMPLAINTS.

I WILL now proceed to describe the Disease under the various forms in which, after the decline and disappearance of the scurvy, it presented itself to our observation; and the methods of treatment, which, from experience, have been found most serviceable. It is necessary, however, in the first place, to premise, that although the flux of the bowels has throughout been its leading and most prominent symptom, the symptom, moreover, which has been most constantly present, and that from which it has derived its character and its name, yet has it constituted one part only of the Disease. The Disease was neither a diarrhœa nor a dysentery simply, nor did it belong exclusively to the bowels; but it belonged to the whole system, and was very extraordinary, and (as I believe) peculiar in its nature.

There was every degree and species of flux, that was ever seen or described. There were cases, which corresponded with the descriptions of the Indian cholera. The patients were seized with intolerable cramps at the pit of the stomach. They retched and vomited, and a thin turbid serum ran from their bowels, followed by severe tenesmus. The pulse became feeble and frequent; they were pale and chilly; and a sudden anguish pervaded the whole frame. Again, there were cases, which corresponded with the common autumnal cholera of this country. The patients had severe griping of the intestines generally, and cramps of the extremities, while pure and unmixed bile ran from the bowels, scalding them (as they expressed it) like melted lead in its passage.

Moreover, there was every kind and degree of dysentery; some purged pure blood in large quantities; others a fluid like the water in which raw flesh had been washed. In some, the evacuations, otherwise healthy, were just streaked with blood;

in some they contained (what seemed to be) lumps of flesh. In others they were mixed with mucus and slime, or they consisted of mucus and slime altogether.

Again, there were cases which differed very little from the diarrhœa of common casual occurrence, except that they were quite intractable by common remedies. The evacuations were loose and attended with griping, but feculent and without any morbid quality.

Lastly, there were cases which had no resemblance whatever, either to cholera, or dysentery, or diarrhœa, or to any disorder that has obtained a name. In the evacuations, there appeared nothing that had any sensible quality of fæces, of bile, or blood, or (of what is understood by) mucus and slime. But they consisted sometimes of a mass, like green or black grapes in a state of fermentation; sometimes of a matter like yeast; sometimes they were in colour and consistence like half-slaked lime, when it is beginning to crumble; and sometimes like a thin mixture of chalk and water, and always intolerably sour and offensive, and in enormous quantity.

Now the probable issue of particular cases, and their degree of danger could not be estimated by the kind of flux simply. Experience taught us that those who had the extreme symptoms of cholera or dysentery were as likely to recover as those who had simple diarrhœa: and those who had simple diarrhœa were as likely to die as those who had the extreme symptoms of cholera or dysentery. Therefore it became the more necessary to seek for indications among concomitant symptoms, which might conduct us to a better judgment of the disease. Accordingly we looked to the general condition of the abdomen, and the presence or absence of pain within it; to the condition of the tongue; to the pulse, and to the presence or absence of fever. For hence it is always thought, in cases of intestinal flux, that the actual state of disease is capable of being most safely inferred.

When the abdomen was examined, it was found in some partially distended, and chiefly about the epigastric region; in some universally distended and tympanitic. In others it was found collapsed and retracted towards the spine. The tympanitic state of the bowels was much more remarkable in the women than the men. After every symptom of their

disease had ceased, and they had gained flesh, and recovered the complexion of health, many of the women still continued, for months together, with the abdomen enormously prominent. In many, however, both women and men, during their disease and afterwards, the belly was soft and natural in every respect. In one instance, and in one only, was there any circumscribed tumour to be felt by pressure, within the abdomen, and there it arose from an enlarged liver.

Of pains referable to the bowels, there was great variety in the kind and degree which different patients experienced. Some experienced no pain at all, except just *before* each evacuation, when the common urgency to stool seemed aggravated into real suffering. Some experienced none at all, except just *after* each evacuation, when it consisted of a scalding of the passage or tenesmus.

The great majority, however, had some kind of perpetual uneasiness within the abdomen. There was a very general complaint of (what was called) *sinking at the pit of the stomach.* What this sinking is those only know who have suffered it. All patients speak of it by the same name, but do not describe it further. From observing and interrogating those who now complained of it, I suspected it to consist of a certain degree of actual pain, combined with a feeling which is akin to approaching syncope, and spreads from the stomach, as from a centre, over the whole frame. It is a painful and overpowering sensation, as if animal life itself was hurt and lessened.

Now this sinking was not only present with the bowel complaint, but many suffered it alone, long before their bowel complaint arose; and many still suffered it long after their bowel complaint was gone. In the one case, it gave notice that the disease was approaching, before its more characteristic symptoms arrived; in the other, it was an evidence that, although its more characteristic symptoms had subsided, the disease had not actually ceased. That this painful and depressing sensation, among many other severe sufferings, was often still the greatest of all, I infer from this consideration. Patients would continually endeavour to withdraw our attention from the more tangible symptoms of their disorder, for the sake of fixing it upon this. When we were interrogating them upon circumstances apparently more urgent, they would interrupt us, and

exclaim, "but this sinking, this sinking; pray do something for this sinking!"

Many suffered severe pains, which came and subsided, and came again, and were often aggravated into paroxysms of extreme torture. They occupied indifferently various parts of the abdomen, and parts contiguous to it; they were felt in the seat of the stomach, or of the bladder, or about the navel; they bordered upon the loins, or shot round to the back. In the same individuals, however, whatever part they once occupied, to that part they almost constantly returned after their remissions. These pains partook principally of the character of colic pains; while, in some respects, they were not strictly such. After having endured for a time, in great severity, they became tranquillized, but did not absolutely cease; and, although pressure, very carefully and equally applied over a large surface, would give relief when the paroxysm was present, it served to exasperate the pain which still remained after the paroxysm was gone. It seemed reasonable to infer that so much of the pain, as was constant, and abiding, and less severe, but capable of being exasperated by pressure, arose immediately from the diseased condition of the bowels, and that so much as was occasionally superadded and more severe, but capable of being relieved by pressure, arose from their temporary spasm.

Many suffered the less severe, and constant, and abiding pain, without any occasional aggravation of it into paroxysms of colic.

In the great majority of cases, whatever was the kind of flux, whether cholera, or dysentery, or diarrhœa, or any other of the disorders described, whether it was severe or mild, the tongue, during the whole course of the complaint, was quite clean and moist, and of its natural colour. All the physicians who visited the Penitentiary were struck with this anomaly. In a few cases, it might be a little more red than natural; in a few loaded with mucus; and in a few, even brown and dry; conditions which seemed determined by the presence of fever; but in no single case was there the red and glossy and smooth tongue peculiar to dysentery, and in no single case was there the slightest appearance of aphthæ.

With respect to the pulse, in a few cases it had frequency and strength enough to require bleeding, and in a few it had

frequency and strength enough to require some means of depletion short of bleeding. But in the great majority of cases it had no morbid character, which called for the employment of blood-letting or any other kind of depletion, and in many it had no morbid character whatever.

Of fever, as a part of the disease, I propose to speak more particularly hereafter, observing, in the meantime, that it was present in some cases, but that the majority were entirely exempt from it, and that, where it did occur, its symptoms only reached a very moderate degree of excitement.

Such was the remarkable diversity of symptoms which accompanied this strange and multiform disorder of the bowels.

From the species of flux (it has been already said) we could derive no criterion of the actual state of the disease, or its probable issue. Concomitant symptoms, which ordinarily afford an essential illustration of the nature of similar diseases, now rather contributed to perplex us. From the tongue nothing could be learnt; from the state of the abdomen very little; and, although pain, and fever, and an excited pulse, necessarily implied a more active disease, they did not bespeak one more hazardous in the event. The danger was not to be estimated, either by the degree of pain, or the degree of fever, or by the blood that was purged, or by the violence of the retching and vomiting, or by the frequency of the pulse. If any form of the disorder was more formidable than another, it was that which seemed to consist in mere diarrhœa.

Two cases especially, which occurred soon after our first employment at the Penitentiary, made (as they were well calculated to do) a striking impression upon our minds. They had arrived, by a slow, and certain, and uninterrupted progress, at their fatal termination. The patients had no other symptoms of disease but a simple diarrhœa.

They had no pain, no fever; their pulse was sixty, and no more. Never did I witness the process of dissolution so lingering. In the very act of dying, when the pulse could only just be felt, it had not exceeded sixty. Their purging was incessant and incontrollable; but there was no morbid quality in their evacuations, except that they were watery. One of these patients (a female) was examined after death, and the only traces of disease discovered, were three or four small spots

of ecchymosis at the upper portion of the large intestines, without any appearance of vascularity or inflammation in their neighbourhood.

In process of time cases of this kind became more and more numerous. They consisted of simple diarrhœa alone: such a diarrhœa as all physicians would have thought capable of being instantly arrested by the simplest remedies; yet this mere passive diarrhœa was the form of the disease which our own experience eventually taught us most to fear and deprecate.

Many poor wretches thus affected were given up by us as lost, and were just ready to perish at the time when we first resorted to the use of mercury. They lay in bed without fever; without pain; without excitement of the pulse; but with a turbid water continually running from their bowels. This was their only symptom; and this nothing could restrain. Their complaint (as long as they did complain) was of "that dreadful sinking." But now their complaining had ceased. Being roused, they looked up for a moment, but made no lamentation, and then laid their heads down again in despair. It was a dismal office to watch over their tardy dissolution, and witness the frustration of every expedient for their relief. These were the cases in which we first put the efficacy of mercury to successful proof; and I cannot help mentioning the relief my mind experienced from a sense of responsibility which had now become truly awful, as soon as the salutary influence of this remedy was apparent.

In those who died, dissection discovered various morbid conditions in the course of the intestinal canal. They were such as every one, at all conversant with morbid dissections, is well acquainted with; for there was nothing essentially new in their kind. But at the same time many of the forms under which they presented themselves were new, or at least not familiar to my observation.

They were principally of three kinds, ecchymosis, congestion of the small blood-vessels, and ulceration: the two first belonging exclusively to the mucous membrane; the last beginning in the mucous membrane, but subsequently extending to contiguous structures.

The *ecchymoses* were always of small extent, and few in number. I do not recollect an instance in which they occupied

a space much larger than the diameter of a pea, or exceeded the number of five or six throughout the whole tract of the intestines. They were found indifferently in all parts of the stomach and bowels.

Some died of long-continued and incontrollable diarrhœa, in whom no other morbid appearance was found after death, but a few of these small spots of ecchymosis.

With respect to congestion of the small blood-vessels, those who are accustomed to the examination of dead bodies well know, that nothing is more common than to find parts of the intestines changed from their natural colour, without any sensible alteration of their natural texture. These parts of the intestines assume every shade, from an indistinct blush to the most vivid scarlet and the deepest black. In its more intense degrees, the discoloration is seen as soon as you open the cavity of the abdomen, and appears at first to pervade the whole mass of the bowel in the situations which it occupies. Upon further examination, however, it is found to appertain especially to the mucous membrane, and that the essential condition out of which it arises, consists of an inordinate repletion of the minute blood-vessels.

These discolorations of the mucous membrane were frequently remarked in the intestines of those who died at the Penitentiary. They occupied indifferently all situations in the course of the intestinal canal, and often various situations remote from each other in the same individuals.

They were peculiar, in assuming the form of small patches, which were more distinctly circumscribed, and less shaded off into the surrounding parts than I had been accustomed to see them. There were perhaps, in the course of the intestines, half a dozen of these patches, each an inch or two in diameter, and no more, and of a deep and uniform red, while the membrane in its neighbourhood preserved its natural pale colour. Their deep and uniform red gave them the appearance of spots of extravasated blood. When, however, the portions of the intestine to which they belonged were held up to the light, it was manifest that the blood was still contained within the blood-vessels.

Some died of long-continued and incontrollable diarrhœa, in whom no morbid appearance whatever was found, but a few

of these small patches of vascular repletion. In some, these patches occurred together with spots of ecchymosis.

The ulcers, which were found in the bowels of several who died of this disease, did not occupy especially those situations in which ulcers are most commonly met with, namely, the termination of the small and the commencement of the large bowels, but all situations indifferently. They were generally few in number, and occurred at distant intervals throughout the whole tract of the intestines. Sometimes (most frequently) they did not exceed in diameter the size of a pea, and were of a circular shape. Sometimes they were large and irregular, occupying in diameter a space of one or two inches. They did not appear in the midst of an inflamed surface, but were circumscribed and distinct, while the mucous membrane in their neighbourhood was of a perfectly natural colour.

These ulcers had a certain correspondence with the ecchymosed spots and vascular patches already described. They were of the same size and shape, and, like them, they were found at intervals remote from each other, and without any marks of inflammatory action around them. These considerations led to the belief that, what was now an ulcer, had before been a spot of extravasated blood, or a patch of small blood-vessels in a state of congestion, and that one had terminated in the other. The ecchymosed spot, the vascular patch, and the ulcer, were found all three together in the same bodies; and the two first were sometimes found alone, and sometimes together. But the ulcer was in no instance observed without one or both of the other two. Indeed, the vascular patch was sometimes met with in (what appeared to be) its state of transition into an ulcer. In the space which it occupied, the mucous membrane was rough and unequal to the touch, and, upon close examination, appeared visibly corroded.

The ulcers were discovered at various stages of their progress in different bodies, and in the same body. Sometimes they had reached as far as the muscular coat of the intestines, and sometimes as far as the peritoneum, which alone remained to preserve the continuity of the bowel in these situations. In a few instances the perforation was complete; for they had penetrated the peritoneum, and reached the cavity of the abdomen.

Generally there was a mere absorption of parts in the space which the ulcer occupied, constituting the simplest kind of ulceration. But occasionally there was something more than this. A foul yellow matter appeared in the centre of the ulcer. This matter was either the sphacelated structure of the bowel itself in the process of separation, or it was a morbid secretion proceeding from the ulcerated surface.

Those who are conversant with morbid dissections, will certainly not discover anything new in these states of ulceration. There was, however, one appearance not unfrequently met with in our examinations, with which I was then unacquainted, and which (as far as I know) has never been particularly described. This was the appearance of ulcers in the course of their progress towards reparation. It is a question (I believe), whether ulcers of the bowels be capable of reparation at all. Here, however, there seemed to be sufficient evidence that they were so.

In several bodies, which we examined, one, or two, or three little spots were found, corresponding in shape and size with the smaller ulcers, which have been noticed, where there was no remaining character of ulceration, but the mucous membrane apparently drawn and puckered, and its continuous smoothness interrupted. At these spots, closer examination, by help of a lens, discovered a circular margin, which was slightly elevated, enclosing a space which was slightly depressed. This space had a reticulated appearance, formed by minute white filaments of lymph, crossing each other in various directions, among which small red blood-vessels were visible. There was no unusual vascularity of the mucous membrane in the neighbourhood, nor any alteration of its natural colour; so that these little spots would probably have escaped our notice, had we not been habitually minute in our examinations. In a few instances, however, we were led to them by observing a peculiar condition of the peritoneal coat, which seemed here and there gathered up and drawn to a point, appearing externally as if a small portion of the intestine had been taken up by the forceps and tied with a ligature on the inside. Wherever such was the condition of the peritoneum, upon examining the bowel within we found an ulcer in (what I presume to be) the course of reparation exactly opposite to it. It is probable that, where

such was the appearance of the bowel externally, the ulcer had originally extended to a considerable depth ; that it had reached perhaps beyond the muscular coat, and that its reparation was in some sort necessarily effected at the expense of the peritoneum. Granulations springing from the bottom of the ulcer, as they contracted and coalesced, would pucker and draw together that part of the peritoneum from which they grew.

Now it can hardly be doubted, that the conditions which have just been described really arose from ulcers in the course of reparation. They were found in the same bodies with ulcers in a progressive state, and with spots of ecchymosis, and patches of vascular repletion.

Upon a review of these several morbid appearances within the intestines, it struck me, that not the spots of ecchymosis only, but every morbid appearance which presented itself, had the character of certain partial and chronic eruptions upon the skin, when the whole surface is tolerably healthy, except the space which the eruption occupies.

Such eruptions upon the skin no remedy can immediately cure. They are curable only through the medium of the constitution, out of whose morbid state they arise, and after a long time, and with a choice of all the most favourable circumstances, it will hereafter appear that, in this respect also, a parallel subsists between them and the morbid conditions of the intestines, which we have been discussing.

In comparing the symptoms during life with the actual state of disease found upon dissection, there did not appear any very strict correspondence between them. The flux, according as it partook of the nature of cholera, or dysentery, or diarrhœa, or was of any other kind which has been described, could not be made out to arise from ecchymosis in one case, from congested blood-vessels in a second, or in a third from ulceration.

There was, however, a certain correspondence between them of a more general character.

Among the symptoms there was seldom found any vigorous excitement of the constitution at large, any strong pulsation of the arteries, or high inflammatory fever ; and, in the actual state of disease, there was seldom anything calculated to produce them.

Among the symptoms, there were those well-marked varieties which are conceived to denote that the disease, whatever its kind be, occupies especially certain portions of the intestinal canal. There was pain combined with great sinking and depression, which bespeaks the disease to be not far from the stomach. Pain combined with less and less of sinking and depression, which bespeaks it to be more and more distant from the stomach; and pain, which rather rouses and excites, and is augmented in severity until the bowels are relieved, showing the disease to be probably in the large bowels. Hence, before any bodies had yet been examined after death, it was anticipated, that disease would be found in various parts of the intestines, although its precise nature could not be foretold; and, as soon as dissections were made, such (we have seen) was found to be the fact.

Still, upon the whole, the disease, as traced out by dissection, was far from affording an entire explanation of the disease, as manifested by symptoms during life. In a few instances, indeed, there was enough to account for protracted illness of the kind we witnessed, and for death; as when ulcers were found which had destroyed, in various degrees, the texture of the intestines, or some one ulcer which had entirely penetrated them. But, in many instances, certainly in the majority, there was an apparent inadequacy of the cause to the production of such results; and we were surprised to find a few minute spots where blood was extravasated beneath the mucous membrane, or a few circumscribed patches where blood was detained in the minute blood-vessels, the only visible traces of disease in those who died, after long-continued and incontrollable diarrhœa or dysentery.

But the entire disease does not always consist in its visible marks upon particular organs. If injury be done to a healthy body, there, indeed, it may; and its anatomical character simply may become the best criterion, whether it be of easy or difficult reparation. But, where a visible change of structure arises, independent of injury from without, there must be something within the body that preceded, and conduced to it. This something, this inceptive movement, whether it be of the part or of the constitution, which foreruns the actual manifestation of visible disease, will not bear to be spoken of with precision.

We talk of cachexies, of constitutional taints, and morbid dispositions, not knowing how to define what we mean. This, however, we know, that the local diseases which follow the conditions we thus designate, upon whatever part of the body they fall, are much more difficult of cure than their mere anatomical character would imply.

Blood, extravasated in any moderate quantity from local injury, is easily absorbed; and small blood-vessels, which have become overloaded from the same cause, easily empty themselves. But blood extravasated, and vessels overloaded, when the cause is in the constitution, may long remain so, and reparation be tardy and difficult; while, in the meantime, there may arise various symptoms of disease, and various hazards of life, according to the organs which are engaged.

Now it is certain that, at the Penitentiary, long before the manifest signs of any particular disease had yet appeared, the general health of the prisoners had begun to decline. It was not until February, 1823, that scurvy, and diarrhœa, and dysentery, were found spreading extensively throughout the prison; but it was in the autumn of 1822 (as we collected from the officers of the establishment), that the prisoners "became pale, and languid, and thin, and feeble." This universal cachexy (for so it may be called) endured for many months; yet no man can tell what it was in its essential nature. But, indefinite as it was, it nevertheless was something real; and, in viewing the disease of the Penitentiary as a whole, this surely claims to be considered as a part of it, and perhaps its most important part; for, subsisting at first alone, before the more definite forms and symptoms appeared, it was probably this from which they derived their origin; and afterwards still subsisting, in many instances, it was probably this that mainly contributed to retard their cure.

These symptoms, as they declared themselves through the medium of the bowels, and the actual state of disease found within the bowels, upon dissection, I have already described. I shall now proceed to show how their cure was attempted.

At the beginning of the month of April, the flux, which had affected between four and five hundred individuals, had almost disappeared, no other remedy having been used but the chalk mixture and tincture of opium; and, under the influence

of an improved diet, the general health of the prisoners had visibly improved.

But, when the flux of the bowels returned, at the end of the month of April, it was no longer amenable to the same remedies. Not even in those cases, which bore the character of simple diarrhœa, did chalk mixture and tincture of opium procure the smallest relief. No effectual good was derived from practice directed to the simple purpose of restraining the flux, even where this was the only indication to follow.

Still we endeavoured to reach the disease, by addressing our remedies to the more conspicuous symptoms. Where there was pain, aggravated by pressure, and attended with fever, bleeding, or blistering, or fomentations were used, according to the urgency of particular cases. But little abatement, even of the pain, ensued, and none whatever of other symptoms, or of the flux. Where there was simple colic pain, all the methods of soothing were employed, by opium, fomentation, &c., &c. But the consequence was only a brief respite from suffering, while the flux continued.

When common remedies, directed to the fulfilment of plain and intelligible purposes, did not succeed, we were, in a manner, compelled to pursue other less certain indications, and to employ other expedients of less acknowledged efficacy; such as astringent bitters, aromatics, mucilaginous drinks, antimonials, and ipecacuan. Ipecacuan was given in small doses of a grain or two grains, three or four times a day; or it was given once or twice only in large doses, of fifteen or twenty grains, combined with a couple of grains of opium. Moreover, the root was infused in water, and thus it was employed as a lavement. But, in all modes of its administration, and in every quantity, the result was equally unfavourable to the efficacy of the remedy.

Glysters, composed of starch and opium, and of liq. plumbi acetatis dilut. and opium, were administered, with temporary benefit, in a few instances, but they did no permanent good.

Warm baths were used; the belly was swathed in flannel rollers; and stimulant liniments were rubbed upon the abdomen; but they did no apparent good.

It is proper to mention, that the most efficacious remedy (efficacious, I mean, as a palliative, and no more) was an

opiate plaster, or rather cataplasm, of which these were the ingredients:

Empl. picis comp. ℥ijss.
Empl. plumbi ℥ss.
Opii duri contriti
Olei menthæ
Camphoræ ā ā ʒij.
M.

These ingredients constituted a kind of poultice, which was spread upon folds of linen, and laid upon the abdomen. The effect was to procure sleep, or to maintain a state of drowsiness, or to excite and keep up delirium during forty or fifty hours. The delirium was manifested by continual talking and laughter.

As long as the nervous system was kept under this influence, the bowels seemed free from pain, and the frequency of the evacuations was greatly diminished; but, as soon as it threw off its torpor, or lost its unnatural excitement, the pain returned, and the evacuations became as numerous as before. Thus it was evident, that no progress whatever had been made towards the essential cure of the disease.

Still there was a remedy upon which all our reliance was eventually placed, and that remedy was mercury.

Owing to considerations which had an unavoidable influence upon our minds, some time elapsed before we resorted to its employment.

Upon the first appearance of the flux, mercury (as we understood) had been fairly tried in a few cases, and had failed. Upon its first appearance, too, the flux had been combined with scurvy, in a large proportion of all the cases which occurred, and spots of ecchymosis had been found in the bowels of several who died; and although there were now no longer any scorbutic marks upon the skin, the disorder of the bowels, upon its return, could not be certainly pronounced to have changed its original nature.

We abstained, therefore, from the use of mercury, because we suspected at least that we had still to deal with the same disease in which it had been already found prejudicial, and that it might possibly still partake of the nature of scurvy.

But, after the failure of every other medicine in the treatment of this disease, were we justified in abstaining even from

the cautious use of a remedy, which its present circumstances especially called for, merely from the recollection of certain symptoms which had been its concomitants a month ago.

With regard to mercury, as a remedy in scurvy, writers of authority had denounced it as injurious, and we could not help placing a certain reliance on what seemed to be the result of their observation. But still their theory about putridity and the crasis of the blood had evidently determined their notion of the essential nature of scurvy; and their theory about septic and antiseptic remedies, had evidently led them to assign to mercury a place among the former. Hence there was room to suspect, that these speculations might possibly have had a share in producing their unqualified condemnation of mercury in scorbutic complaints. So that, even if the scurvy had still been present, in the very slight degree (the slightest possible) in which it formerly existed, I cannot think, when every other remedy failed, that it ought to have been an absolute prohibition of the trial of mercury, after the cautious manner in which we proceeded to employ it. But, in fact, the scurvy had almost entirely disappeared.

While two hundred individuals were suffering a flux of the bowels at the same time, and many seemed gradually approaching to their dissolution; and while the numbers of the sick were every day increasing, and the forms of the disease were becoming more and more various and complex, and all the methods of treatment hitherto employed had served to palliate only, and not to cure, Dr. Roget and myself determined, after mature deliberation, to get rid of all restriction upon our practice, which had arisen from the consideration that the flux was originally combined with scurvy; and we agreed to employ mercury, in such forms and combinations as the exigencies of particular cases might seem to require.

We first made trial of this remedy in those cases which our experience had brought us to regard with the greatest apprehension, cases (if I may so say) of mere passive diarrhœa, where there was no excitement of the circulation, where there was little pain, and little of morbid quality in the evacuations, but where the evacuations were enormously frequent, and hitherto absolutely incontrollable. In these cases all medical expedients had failed, and we were now compelled to content

ourselves with such temporary relief, and such short intervals of ease, as opium, administered in draughts or clysters, or in cataplasms, was able to procure. In our trial of mercury for these cases, we proceeded thus:—Equal quantities of hydrargyr. c. cretâ and pulv. ipec. comp. were made into pills; each pill consisted of five grains, two grains and a half of each ingredient, and one of them was administered three times a day, to about twenty patients. Still there was no abatement of the diarrhœa. They were administered four times a day, and still the diarrhœa continued. They were given five times a day; when, upon our next visit to the Penitentiary, we found, among those who had taken mercury, one female in a profuse salivation, and the diarrhœa completely arrested in her, and in her alone. This poor creature had formerly had scorbutic spots upon the skin, at the same time that she suffered a flux of the bowels. The scorbutic spots had disappeared altogether; the flux had subsided, but returned; and that form of it, which has been described, had now brought her life into imminent hazard.

In this instance, the salutary effect of mercury was unquestionable; and the condition of its success seemed to be, that it had procured salivation. We proceeded, therefore, more boldly in the use of it, still giving it in the same form, and in combination with Dover's powder. We increased the dose to those who already took it; and, as they became salivated in succession, they were all freed from the symptoms of their disorder. We subjected more and more of the prisoners to the same treatment, watching them carefully in the meantime, for the sake of still more confidently ascertaining the precise condition, which was essential to the success of the remedy. This we uniformly found to be the production of salivation.

Be it remembered that these cases, upon which the salutary effect of mercury was first proved, were those which occasioned us the greatest apprehension. Several of the patients were so feeble and emaciated, so pale and faded in their aspects, that while, on the one hand, we were feeling our way with the mildest preparation of mercury for the purpose of curing their disease, we were, on the other hand, administering wine and cordials for the purpose of upholding their existence.

The success of mercury, under these unpromising circum-

stances, led first to the more general, and, finally, to the universal, employment of it. We resorted to it in every case of flux, where the remedies hitherto used had not satisfied our expectation. In short, we resorted to it in every case without exception.

But, as it became more and more obvious, that salivation was the condition of its success, there was no reason for restricting its use to one preparation only. Yet, at the same time, it was not enough that salivation should be procured in any way, gradually or at once, quickly or slowly.

Experience taught us, that its curative effect depended, in some degree, upon the manner in which salivation was brought about. Hence a choice and a discretion were to be exercised upon the kind of preparation, the quantity and frequency of the dose, and its combination with other remedies.

Where the flux was attended with severe tormina, or colic, or cramps at the stomach; or where the attack was sudden and recent, and accompanied with fever, it was doing nothing to prescribe small doses of hydrarg. c. cretâ and Dover's powder, which would produce their effect some days hence. It was expedient that the impression of the remedy should be in proportion to the force of the disease, and the rate of its progress. Accordingly, large doses of calomel and opium were given, to make the mouth sore immediately, or as soon as it could be done with safety.

In several cases, in which the agony from tormina and tenesmus was extreme, and the evacuations were enormously frequent, and consisted altogether of morbid secretions, or blood, fifteen grains of calomel and two grains of opium were given at a dose.

The patients, to whom so large a dose of calomel was given, were most attentively watched. Especial care was taken, that nothing should divert it from its influence upon the constitution, and that every accidental inconvenience that might accompany its operation should be rendered as tolerable as possible. If the griping increased, they had peppermint water to have recourse to; if it still increased, they were to be largely and frequently fomented with flannels wrung out of warm water; and if it still increased, they were to be supplied with small doses of laudanum at short intervals.

It will hardly be expected, that this single dose of calomel

and opium could be effectual to the complete cure of the disease. The degree of relief, which the patient experienced the next day, and the changes which his condition had undergone in the meantime, determined the manner of proceeding in the further treatment of the case.

The dose of fifteen grains of calomel and two grains of opium administered under such emergencies as have been described, had almost always the effect of calming the symptoms; but the degree of relief it procured was various.

On the next day, we sometimes found, that the patient had passed an easier night, that the evacuations had been somewhat less frequent, and the tormina and tenesmus had been somewhat moderated, but that since the morning, the symptoms had become worse again, the pains were as severe as ever, and the evacuations as frequent, and quite unaltered in their appearance. Under these circumstances, fifteen grains of calomel and two of opium were given a second time.

Sometimes, the day after the first large dose of calomel and opium, we found the relief, which had been procured through the night, still maintained, and the appearance of the evacuations changed in some obvious respect for the better. Perhaps they were now free from all admixture of blood, which they contained the day before. Under these circumstances, half the former dose of calomel and opium was given.

Sometimes, the day after the first large dose of calomel and opium, we found the patient exulting that he had been cured as by a charm; that he had slept all night, and his pains were gone; and that he had had several evacuations, of which the two or three last were almost natural. With this sudden improvement, salivation had either already arisen, or it was at hand. Under these circumstances, the use of mercury was either suspended altogether, or small doses of calomel and opium were given until ptyalism appeared, which was generally obvious at our next visit.

Our ultimate object, in all cases, was to produce salivation. But, in these cases of severer suffering, we found a salutary impression capable of being immediately produced by a few large doses, or even by one large dose of calomel and opium. This it was expedient to make the most of. Nevertheless, this immediate salutary impression was soon lost, unless the same

practice was followed up to salivation; for which purpose mercury was afterwards sparingly or largely exhibited, according to the circumstances, which have been set forth.

It was remarkable, that the constitutions of many were most slow and reluctant in admitting the peculiar influence of mercury, and that the disease, in the meantime, was still unyielding. In such constitutions, after calomel and opium, given internally, had failed of their accustomed effect, and now served to irritate rather than to soothe the bowels, we resorted to inunction; and salivation, thus procured, was productive of the same benefits.

As an object of pathological interest, it was instructive to observe the different modes of curative action, by which, under the influence of mercury, different constitutions set themselves free from their disease. In many the process of cure was so gradual that there was hardly any perceptible *action* engaged in it. The constitution seemed rather to lose the disease, one symptom after another, than to surmount it by an effort of its own. The pains became gradually less and less, until no pains whatever remained; and the healthy secretions gradually predominated over the morbid, until all were healthy.

In many the process of cure was by a sudden, vigorous, and painful effort, when the constitution threw off its disease by a sort of critical paroxysm. Sometimes the critical effort commenced as soon as the mercurial fœtor was perceptible in the mouth. Sometimes salivation would exist twenty-four hours before the crisis began; and sometimes the crisis preceded the salivation twenty-four hours. But it never took place but where there was salivation at the time, or immediately before, or immediately after.

The critical effort was of this kind. After a calm, procured by one or two large doses of calomel and opium, or after the employment of inunction for two or three days, the constitution would become suddenly roused, and a very severe griping would arise, and then a sensation would follow, as if the bowels were filling and distending themselves with something, and, afterwards, an incontrollable urgency to stool. With the evacuation came the relief of all the preceding misery. The stools were entirely changed. A few hours before, they consisted, perhaps, of slime or blood, or some colourless turbid fluid.

Now they were a colluvies of the foulest, blackest matter, and of every kind; heavy ropy mucus and bile formed a considerable part of them. After one or two such evacuations the patient felt himself entirely restored and well. It generally happened, however, that the same sort of paroxysm returned, and was terminated by the same kind of relief. Thus, after a whole night spent in a succession of these critical paroxysms, the patients were found, the next day, bathed in a warm perspiration, and fast asleep; and, from this time, the evacuations from the bowels became natural and healthy.

Such is the history of our employment of mercury; and such was its success in the various forms of bowel complaint which have been described. Beset with all those doubts concerning the propriety of using it, which the previous history of the disease of the Penitentiary was calculated to suggest, we were, at first, driven to make trial of it by the failure of every other remedy. We began, therefore, and proceeded with the greatest caution, venturing no further than observation did (as it were) lead us by the hand. Thus, gradually making good our ground, we succeeded, at last, in exploring experimentally a very large field; and learnt how, by varying its preparations and its dose, and varying also its combinations, to adapt this same medicine safely and successfully to the exigencies of the disease under many different forms.

CHAPTER IV.

THE DISORDERS OF THE BRAIN AND NERVOUS SYSTEM.

MUCH still remains to be added to the description already given of the disease prevalent at the Penitentiary, in order to fill up its entire character. Hitherto the description has been chiefly conversant with certain symptoms apparently contingent upon it as a disease of the bowels, and with the treatment which had respect to it as such; and thus the history of the disease has (as I proposed) strictly kept pace with my own observation and knowledge. For as it was gradually only that it discovered its entire character, so it was gradually only that we learnt to regard it, neither as a dysentery, nor a diarrhœa simply, nor belonging exclusively to the bowels, but pervading other organs and systems of organs, and, in fact, belonging to the whole constitution.

No part of the disease was more striking and characteristic, none more formidable and difficult to treat, than that which declared itself through the medium of the brain and nervous system.

In our Report of the 5th of April, 1823, we remarked that, in all who had suffered either scurvy or dysentery, "there was found the same constitutional derangement, denoted by a sallow countenance, an impaired digestion, diminished muscular strength, a feeble circulation, *various degrees of nervous affection, as tremors, cramps, or spasms*, and *various degrees of mental despondency*." Now these various nervous affections are here mentioned, only as symptoms of constitutional weakness; for in that light they were certainly regarded by us at the time. It is true, two prisoners had already died suddenly, after symptoms immediately referable to the head, and some instances had already occurred of very frightful convulsions. But hitherto we saw no reason to conclude that these cases had any essential connexion with the predominant disease. In process of time,

however, disorders of the brain and nervous system became more and more frequent, and of various kinds, head-ach, vertigo, cramps, and twitching of the limbs, delirium, convulsions, and apoplexy. But since these disorders did not immediately discover themselves in all their variety and magnitude, it was not until after much observation, that we were enabled to tell their genius and character, and to know that they constituted one form of the predominant disease; that they were not contingent upon the flux, nor the flux contingent upon them; that either might exist separately, although they were generally found in combination; and that both arose from a morbid condition, essentially the same, but falling upon different parts.

I shall best convey a notion of what these nervous affections were, by following the order of my own experience concerning them, and describing them as they fell under my own observation.

Upon our visiting the Penitentiary, out of seventy men, whom we found in the infirmary, there were five who had no other complaint but severe head-ach, three who had no other complaint but cramp in the limbs, and one who had cramp in combination with diarrhœa; and, out of ninety-three women, whom we found in the infirmary, thirteen had simple head-ach, three had no other complaint but cramp, and five had head-ach combined with diarrhœa. We did not, however, from these cases, at all suspect that the predominant disease involved the brain and nervous system.

We had not, however, been in attendance more than a week, when a man (Robert Dyer), thirty-one years of age, in the infirmary, and suffering cramp and diarrhœa, died suddenly, apoplectic. Upon dissection, we found the vessels of the brain slightly turgid with blood, and a few spots of ecchymosis in the intestines, although there had been no scorbutic appearance upon the skin.

About ten days afterwards, a woman (Harriet Church), twenty-six years of age, whom we had found in the infirmary, suffering head-ach, began to exhibit a strange perverseness, which was rapidly aggravated to mania, and she died. Upon dissection, no morbid appearance was found beyond a very slight congestion of the blood-vessels of the brain; so slight, that its existence might be doubted.

Soon afterwards, as we were one day going round the infirmaries, our attention was called to a young woman (Louisa Cornforth) who was suffering the most agonizing spasms. Her legs and arms were as rigidly tense as in tetanus. Suddenly she gave a loud shriek, and her eyes were fixed, and she became as pale as death. No pulse could be felt, and her breathing was only just perceptible. By æther and ammonia, and all the means of stimulating within our reach, we succeeded in rescuing her. In two or three minutes perhaps (but it is not easy to reckon time on such occasions), we could feel the pulse beginning to undulate, and see the countenance beginning to redden, so that it was evident that the blood was in motion again. Then her eyes began to pass from object to object, and it was plain her consciousness had returned. She could not yet speak; but, by inarticulate sounds, and by the motion of her hands around the heart and stomach, she made us understand, that it was there the sudden agony had seized upon her. This young woman survived six weeks; and, in the meantime, all her dreadful symptoms frequently recurred, and her existence was upheld, from hour to hour, by the most potent stimulants. If she was left more than a certain period, without a small quantity of brandy, her pulse became imperceptible; but it was felt again, as soon as the stimulus was again applied. And, in this manner, even for as long a time as six weeks, existence was still maintained, while it was continually tottering upon the verge of dissolution. Before her death, diarrhœa was added to her other complaints. Upon dissection, nothing was found at the origin of the nerves, to account for the dreadful symptoms referable to them. At intervals, throughout the intestines, there were small circumscribed patches of red, occasioned by blood accumulated in the small blood-vessels; but there was no ecchymosis.

These three cases occurred in the month of March; and, occurring (as they did) among various other nervous affections, they may be thought to have excited in us a suspicion, at least, that the disease of the Penitentiary did not belong exclusively to the bowels. Still we wanted further experience, to assure us of the fact.

Early in the month of April, the bowel complaints and the scurvy, in which the peculiar disease of the Penitentiary (as

we then thought) exclusively consisted, almost entirely disappeared. The scurvy, indeed, never returned; but the bowel complaints returned before the end of April, and spread throughout the prison more extensively than before. With them there appeared nervous affections of every kind, and of that kind especially, which betrayed itself in cramps of the muscles. The character and the frequency of these cases were very remarkable. They occurred both to those who had, and those who had not, bowel complaints, to men as well as women; but it was in the women that, from their severity and the frightful circumstances accompanying them, they occasioned us the greatest alarm.

Many women were affected nearly in the same manner as Cornforth had been, and our apprehension was that they would all come to the same miserable end. They had cramps in the limbs and in the trunk. A few had that indescribable agony at the heart, and the pit of the stomach, bringing with it those frightful circumstances which seemed to threaten instant dissolution. Their life was again and again brought into jeopardy; but one only (Sarah Farley) eventually died.

Upon our first visiting the Penitentiary, we found this young woman (her age was only twenty-three) slightly affected with scurvy, and complaining of diarrhœa, which she had suffered occasionally for many weeks. As she became convalescent from these disorders, there arose a succession of complaints in every part of her body, and with them that frightful agony at the pit of the stomach, which occurred in the case of Cornforth. It was not a sinking merely, but an evident spasm, attended with severe pain. She lingered, too, in the same manner as Cornforth, and her existence was upheld by brandy and laudanum, but not for so long a time. For several days previous to her death, a remarkable lividity appeared upon the extreme parts of her body. Upon dissection, small vascular patches were found in the mucous membrane of the intestines and nothing more.

Now, in all such cases, where there had already been cramps of the voluntary muscles, and sudden pains within the chest, or at the pit of the stomach, followed by a failure or suspension of the circulation, it must necessarily be believed,

that an impression was communicated through the nerves expressly to the heart.

One instance occurred where, without previous spasms of the extremities, or pains within the chest, and without any forewarning symptoms, the functions of the heart were suddenly suspended, and the patient died.

Charles Thomson (aged twenty-nine) had suffered a degree of scurvy, which was comparatively severe. He was one of the few in whom the extravasated blood had formed large blotches upon the legs; whereas, in the generality, it had appeared only in spots or specks. Of the scurvy, however, no traces now remained. He had also suffered diarrhœa, from a period long before we first visited the prison in the month of March, with short respites, down to the month of June. His bowel complaint still continued in a mitigated degree. He was much emaciated, and his feebleness confined him almost constantly to bed. He was in a very precarious state, but he did not yet bear the marks which denote the near approach of death.

On the 24th of June, I had been speaking with this poor fellow about his complaints, and he had distinctly told me he was better; when, feeling his pulse, I remarked extraordinary intervals between the beats. I called to Dr. Roget, who was at the bedside of another patient, to come and feel it; and while I was in the act of passing his hand from my own to Dr. Roget's, his head fell back, and, *to all appearance*, he was dead.

We believed he was actually so. We applied warm fomentations over the whole surface of the body. Still there were no signs of life. Several persons were employed together in rubbing and irritating the extremities. It seemed as if several minutes had passed, and still he was dead; but a tinge of red was now seen running along his lips, which was the first sign of life; and then he breathed, and then his pulse became perceptible. With life, sense and consciousness were not immediately restored; and, as soon as the circulation was re-established, there arose a violent convulsion of the whole body. When we left him, on this day, he was yet insensible. The next day we found him sensible, but learnt that he had had several convulsions. On the next day he died. Upon dissection, little patches of vascular congestion were found in various parts of the mucous membrane of the intestines, mixed with small circum-

scribed ulcers; also, increased vascularity of the brain, and fluid effused between its membranes, and into the ventricles. The heart was free from disease.

Among other cases of nervous affection, there were several of phrenitis. To a sudden and acute pain in the head was added, first, vertigo, then bewilderment of the intellect, then twitching of the tendons, then strabismus, and dilated pupils, and lastly, distortion of the mouth and hemiplegia. These symptoms sufficiently indicated what the disease was. But, unfortunately, we looked in vain for other symptoms which, in ordinary cases of phrenitis, are accustomed to indicate what the remedy should be. In almost every case the pulse was most feeble. There was the disease, without the force of the circulation, which is deemed essential to maintain it. There was the disease, and, at the same time, a prohibition of the remedy, which is deemed essential to its cure. Happily, as these cases became more frequent, we learnt an effectual method of treating them. In the meantime, we proceeded, in great perplexity, adapting (as we could) common methods of treatment to the exigencies of these extraordinary cases. In a few, we ventured to try the effect of bleeding from the arm, sitting by the patient while the operation was performed; and thus, perhaps, when four or five ounces of blood had flowed, the pulse would falter, and we were compelled to stop; or we were compelled to stop when hardly a single ounce had been lost. I am sure there was no good derived from this practice, but, with all our cautions, I am not sure there was no evil. We put leeches upon the forehead, and found them of very uncertain effect. It was to extensive blisterings upon the head, and in its neighbourhood, that, for the present, we were obliged mainly to trust; and these remedies would check the symptoms, and postpone the progress of the disease, if they were fortunately employed so as to take effect at the time of its earliest formation. From this insidious inflammation of the brain, we lost two females, both of whom would, in all human probability, have been saved, if they had been submitted to the treatment which we eventually found successful. Upon their examination after death, we found an universal vascularity of the encephalon, and fluid effused between the membranes of the brain, and into the ventricles.

In process of time, disorders referable to the brain and nervous system prevailed, almost as extensively throughout the prison, as bowel complaints. In the majority of cases, indeed, they did not reach the formidable character which has been just described. They consisted, for the most part, of pain, and strange sensations in the head, and cramps of the limbs. But to this mere head-ach was superadded, so often and so suddenly, vertigo, or delirium; and to these mere cramps of the limbs, was superadded, so often and so suddenly, a more terrible spasm of some internal organ, that we could not help seeing, in the minor affections, a tendency to some fatal result.

Sometimes, in going round a division of the prison, where the patients had all been in a state of improvement the day before, we found a dozen in bed, and were told, that they had severe pain in their heads, and that some were so giddy as to be unable to stand. The next day, perhaps, three or four of them had become delirious, and had twitching of the muscles, and were rapidly passing into that formidable state which has been described.

There were instances of individuals falling down suddenly, as if they were shot (thus the seizure was described by the by-standers), who, being brought into the infirmary, gradually rallied, and referred to the head as the seat of severe uneasiness. In these cases, no symptoms of a more formidable kind ensued, in respect of the brain or nervous system. A permanent head-ach, or vertigo, was the common consequence.

The description and the instances, which have been given, plainly imply disease of the brain and nervous system, disturbing the functions of the different organs to which their influence extends.

If that part of the disease which belonged to the bowels, from its great extent, from the strangeness and diversity of its forms, and its intractability, under every form, by common remedies, was formidable, this part, which belonged to the brain and the nervous system, was surely not less so; for it, too, had put on many unusual shapes. It was daily increasing in extent, and was already found unmanageable by ordinary methods of treatment.

Moreover, the frequent suddenness of the attack threw a peculiar terror around many such cases, which was communi-

cated to their fellow-prisoners, who were looking on, and expecting themselves to be the next victims. The frightful sufferings of Louisa Cornforth and Sarah Farley, and the sudden death of Charles Thomson, struck such dismay into the sick prisoners, occupying the same wards, as was not easily appeased. It was expressed in their looks, and by their conduct, and especially by an aggravation of the form of disorder (whatever it was) that each was suffering at the time. This circumstance is peculiarly worthy of remark, since it raised new and unexpected obstacles to the medical management of the sick.

Thus far the flux of the bowels, and the disorders of the brain and nervous system have been spoken of separately, as if they were essentially distinct, and their occasional occurrence in the same individuals was merely accidental. In process of time, however, a belief arose of some natural alliance subsisting between them. As soon as the disorder of the brain and nervous system became co-extensive with that of the bowels, it had an equal claim to be regarded as *the disorder* of the Penitentiary; and, as soon as both were not only co-extensive throughout the prison, but co-existent in the same individuals, almost constantly and inseparably, insomuch, that hardly an instance occurred of one being present without the other; and, moreover, as soon as the sole remedy of the one was found to be the sole remedy of the other, there seemed to be enough to constitute the requisite proof, that they were, in a certain sense (and that the most important sense) one and the same disorder.

Disorders may be different in the manner of declaring themselves, and may go by different names, according to the organs which they involve; and yet they may have in common one morbid condition of the system at large, from which they are derived, rendering them the same in their origin, and requiring them to be treated by the same remedy.

Thus does Nature often bring together what nosologies and artificial arrangements have put farthest asunder; and thus does it become possible, that convulsions and dysentery, although in their symptoms they are absolutely unlike, may be essentially the same, and curable by one and the same remedy.

It remains to speak of the various disorders of the brain and nervous system, and of the various kinds of flux, as they were found in combination. For the manner of their alliance

in the same individuals, and certain conditions which they possessed in common, seemed especially to lead to the belief that they were, in the sense which has been intimated, the same disease.

One condition, common to both, was the frequent suddenness of their accession; and there was an alarm connected with it which rendered it the more observable. Those who, to-day, were quite free from pain and disorder of the bowels, and appeared with the aspect, and with the perfect consciousness of health, to-morrow we found purging blood, or pure bile, or a turbid, watery fluid, and completely beat down and mastered by their disease; and, in like manner, those who, to-day, had the same appearance and consciousness of health, and were alert and cheerful, and working at their trades, to-morrow we found so giddy, that they could hardly stand, or confined to bed with racking pains in the head, with twitching of the tendons, or cramps of the muscles, or even with delirium.

Here was an instant transition from the midst of health to the midst of disease in both.

But where the accession of the disease, either as it belonged to the brain and nervous system, or to the bowels, was not thus instantaneous, still there was often a condition common to both in the character of their premonitory symptoms.

There was a pale, and faded, and melancholy aspect, which, in process of time, had become so familiar to us, as the harbinger or attendant of the disease in all its forms, that we were accustomed to select from among the prisoners those in whom it was most conspicuous, and send them into the infirmaries, for the sake of having them more constantly under our observation. Wherever this well-known aspect appeared, it was certain that the symptoms of real disease would soon arise; it might be from the bowels, or it might be from the brain and nervous system; and, in fact, it was as often from one as the other.

As to the alliance of these diseases in the same individuals, no form of nervous complaint was more common than head-ach and vertigo, and they were often combined with the bowel complaint in the following manner:

The attack was introduced by simple head-ach or vertigo, the bowels yet remaining in a perfectly natural state. Some of the patients thus affected, when the head-ach and vertigo

were not urgent, were simply brought into the infirmary and watched; all medical treatment being postponed for the present. Others, in whom they were more severe, were bled with leeches, or blistered, or had such *common* remedies administered to them as they seemed to require. But every patient, almost to a man, some the next day, some (the greater number) in two or three days, and some at a more distant period, were overtaken by some species of flux. The flux followed, whether the head was relieved or not, and when the flux was established, the head-ach or vertigo continued or ceased indifferently.

Another, and a frequent combination of nervous disorder with bowel complaint, was the following:

Certain sudden seizures have been mentioned, arising from the brain, in which the patients were laid prostrate, at once, with some circumstances of alarm. These seizures were by no means of rare occurrence; and all who were thus attacked, if they were not suffering a bowel complaint at the time, were, to a man, inevitably overtaken by it in a day or two.

But this head-ach, or vertigo, or this sudden seizure, did not, in all cases, necessarily lead the way; often they came on in the midst of the bowel complaint, and at any stage or period of it.

The disorders referable to the head were not combined especially with any particular kind of flux. They were not more frequently found in cases which partook of the nature of cholera and dysentery, or in cases where there was fever and excitement of the circulation, than in those of slow passive diarrhœa, where the pulse was in its natural state, and the tongue perfectly clean.

Further, they did not seem to have their duration or their degree of severity at all determined by the character of the flux. Where the bowel complaint was of the mildest kind, and the shortest duration, the disorder of the head was often the most severe and the most abiding, and *vice versâ*.

What has been said of the head-ach, and vertigo, and the sudden seizures referable to the brain, and the manner of their alliance with the flux of the bowels, may be said, with equal truth, of every other nervous affection which has been described.

The cramps of the extremities, and the internal spasms, and the order of symptoms constituting a low phrenitis, were

found to proceed, or to attend upon, any period or stage of the flux indifferently, and to belong indifferently to any kind of it.

The cramps of the extremities were not exclusively, and scarcely in a more remarkable degree, attendant upon the cases of cholera, than upon other cases of bowel complaint.

With respect to the morbid appearances found in the bodies of those who died of such forms of nervous disease as have been described, I regret not to be able to speak with the precision I could wish.

They who are accustomed to dissections, will agree with me, where the question is concerning slight watery effusions in any part of the body, that it is difficult to form an accurate judgment of their nature, if a considerable period has elapsed since the death of the patient. At the General Penitentiary, as in all prisons, a coroner's inquest was held upon the bodies of those who died, and no dissection was permitted until their verdict was returned. Hence three or four days elapsed before we could make our examinations.

The appearances upon dissection have been already anticipated. What we found, when we found anything, was some degree of vascular fulness of the brain and its membranes, and some watery effusion between the membranes, and into the ventricles. Both were generally inconsiderable.

There were instances in which, during life, the symptoms were expressly referable to the brain, and in which death had taken place, by an immediate interruption of its functions, where, nevertheless, upon dissection, we found nothing in the brain or its membranes apparently different from natural and healthy structure.

No examination was made of the spinal canal. The requisite means of doing it successfully were not at hand as in an hospital. I regret the omission; but the circumstances already mentioned, as calculated to perplex our judgment concerning what was found in the brain, would also have stood in the way of any satisfactory inference drawn from appearances of the same kind in the spinal marrow.

As, during life, the symptoms referable to the brain, and to the intestinal canal, were found to co-exist, for the most part, in the same individuals; so, after death, changes of structure were found, for the most part, in both organs. Hence another

confirmation is added to the belief, that the diseases of both had a natural alliance, and in their origin were essentially the same.

Experience convinced us that common methods of treatment were not to be trusted, for any effectual good they could do to these various nervous disorders. They either failed entirely, or they fell short of the salutary impression they are accustomed to produce upon the same symptoms, occurring under ordinary circumstances.

It has been already intimated, both what the means were that proved unsuccessful, and what the principal remedy was upon which our reliance eventually rested. There is no need, therefore, to dwell long upon the details of treatment. It will be enough to state the general results of the practice adopted, and to make some few reflections upon the character of the disorders in question, to which the extraordinary failure of all the most probable means of cure, and the success of this particular remedy, seem naturally to conduct.

There are ailments concerning which physicians hardly allow themselves to feel the smallest anxiety, holding (as they conceive) in their own hands the almost certain means of their relief. When, however, such ailments unexpectedly resist the remedies which are accustomed to cure them, they cannot help suspecting that there is more to contend with than the mere symptoms seem to imply. Thus simple pain in the head they are apt to think lightly of, until it is found incapable of relief by common remedies. Of such intractable cases the number (it has been seen) was very large at the Penitentiary.

Of pain in the head, *accompanied by vertigo*, they are apt to think a little more seriously, but still not very seriously, until simple remedies bring no relief; and intractable cases of this kind, also (it has been seen) were very frequent at the Penitentiary.

Again, there are many, even acute diseases of vital organs, which physicians always regard with great apprehension, but which, with the advantage of seeing and treating them early, they nevertheless have a good expectation of bringing to a favourable termination. When, however, even with this advantage, they absolutely fail in every case that presents itself, they cannot help experiencing some perplexity, and suspecting

that there may be more in these diseases than they have been able to discover. Thus, when to pain in the head or vertigo, there are superadded cramps of the muscles, or twitching of the tendons, or delirium, or strabismus, they look upon the cases constituted of these symptoms with the greatest apprehension of the result; and there were (it has been seen) a few such cases at the Penitentiary. But even in the cases constituted of these symptoms, they do not look upon death as inevitable, until the remedies, upon which they are accustomed to rely, fail to make their ordinary impression. And thus it happened with the few cases of this kind at the Penitentiary; they passed, uninterruptedly, to their fatal termination, their symptoms hardly receiving the smallest check or abatement from the remedies employed.

Now, before we resorted to the use of mercury for the various forms of the disease prevalent at the Penitentiary, the state of the prison, in regard to that form of it which involved the brain and nervous system, was this:—Seven had already perished under our own observation; of whom one died apoplectic, one maniacal, two with the symptoms of phrenitis, two from cramps referable to the region of the stomach and the heart, and one from symptoms belonging in part to the heart and in part to the brain; and there were not less than two hundred now labouring under various degrees of disorder belonging to the same organs. Of them a few only were dangerously ill in respect of the magnitude of their symptoms. These few were suffering that insidious form of phrenitis already described, while we were checking the symptoms and postponing the progress of their disease, with little hope of eventually saving their lives; and unquestionably, they were in great present peril. But all the rest, though not in imminent danger from the magnitude of their symptoms, gave just cause for anxiety, from the consideration that they had not been cured by any means hitherto employed, and moreover that they were under the same conditions of disease through which the seven had passed, before they reached their fatal consummation.

Such was the state of the prison in respect to the disorders in question; and it may well be conceived that, under great apprehension for the event of all these cases, and under great

present alarm for a few, we sought most anxiously for the means of their more successful treatment.

At this time many patients, in whom mercury was first successfully employed for the cure of diarrhœa, were likewise freed from certain obscure nervous complaints; some from head-achs, and some from vertiginous sensations. This occurrence, while it served to strengthen the belief that the flux of the bowels, and the nervous affections, had a natural alliance, and were, in some sort, the same disease, determined us to give the same remedy a fair trial in its application to both.

And first, we most eagerly resorted to its use in those cases which occasioned us the greatest present alarm, viz. in three cases of insidious phrenitis. The patients already suffered subsultus of the tendons, and delirium, and one had strabismus. If life was to be rescued, it could only be by giving the remedy in such a manner as to bring the constitution as speedily as possible under its influence. Accordingly, as much calomel was prescribed, in repeated doses, and in combination with opium, as procured salivation in thirty hours; whereupon the most formidable symptoms were at once dissipated, and the patients were left in a condition favourable to recovery; and they eventually did recover.

Next we resorted to its use in certain cases which occasioned us peculiar perplexity, and some apprehension of distant consequences, but no present alarm. There were many individuals in whom an affection of the head had been originally combined with bowel complaint. The bowel complaint had been very slight, and of short duration, and had ceased altogether during many weeks. But the affection of the head had been very severe; and although it had obtained a few respites from common remedies, it still remained unmitigated; and at length all our medical expedients had lost the little temporary influence which they once possessed. There was every motive for trying the effect of mercury in these cases, and it was tried, and succeeded.

One of these cases was peculiarly striking, and is well worth relating as an example. Upon our first visit to the Penitentiary, on the 1st of March, we found, among the patients in the infirmaries, a young man of the name of Robson. He was twenty-one years of age; and told us that he had suffered a

head-ach of the most excruciating kind during several months. His sight was dim, and he had a constant twinkling of the eye-lids, and great agony was depicted in his countenance. He had moreover that faded, pale, and melancholy aspect already alluded to. Yet the functions of his bowels were performed naturally. His tongue was clean, and his pulse was of the natural frequency and strength. At this time the alliance between the disorders of the nervous system and the bowels was hardly ascertained; therefore no inquiry was made whether he had suffered diarrhœa. Subsequently, however, we learnt that, during the preceding winter, his bowels had been twice slightly disordered for a few days. This poor fellow was under our constant observation and treatment, from the beginning of March to the end of June. He gained no respite from his agony, but by means of leeches applied to the forehead, day after day, for a week together, or by blisters kept open, or applied in quick succession behind the ears, or on the nape of the neck; and the respite thus obtained was of short duration: it might be for ten days; it was never longer than a fortnight; and then the same agony returned, and the same cruel treatment was to be resumed. In this case we eagerly resorted to the use of mercury; salivation was procured and maintained, to a certain degree, during several weeks. Whereupon the patient was released from his misery, and ever afterwards, during eleven months that I had the opportunity of watching him, he continued free from complaint of every kind.

The beneficial influence of mercury upon that part of the disorder of the Penitentiary, which belonged to the brain and nervous system, soon became as unquestionable as upon that which belonged to the bowels. It was proved upon the various forms, both of one and the other, and most conspicuously upon that form of each which occasioned us the greatest present alarm.

Before the use of mercury, it was impossible to contemplate the state of the prison, and not consider an extensive mortality as inevitable. Experience of its effects during a fortnight entirely changed our anticipations of the result, and encouraged a hope that, with great care and vigilance in its administration, the mortality would still be kept within narrow bounds. Thus, for every species of nervous complaint, as well as for every

species of flux, whether they were combined or separate (for either might occur alone, although they were generally found together), we were led by our own experience to the employment of mercury.

Seeing that the head-ach and vertigo were often the first symptoms, and that they often subsisted a considerable time alone before the accession of others, we thought it expedient to begin the treatment of the disease, as soon as it showed itself under this form, by our most efficient remedy. Our experience hitherto had been, that these affections of the head, when they were the first to declare themselves, were very seldom controlled, or in any way relieved by common remedies, and that, whether they were relieved or no, the flux almost inevitably followed. Treated with mercury, however, in the great majority of instances, they ceased; and, where they did cease under such treatment, in the great majority of cases, no flux followed.

Thus did this remedy effect the relief of both disorders, when they appeared in combination, and of each, when either occurred alone; and, in the latter case, under such circumstances occasionally, that it seemed to prevent the accession of the other.

In our administration of mercury for the relief of these complaints, we observed that, in the great majority of cases, no striking abatement of symptoms took place, until salivation was procured. Whether the very salivation itself was essential to the cure, cannot be determined; but being the only sensible effect of the remedy, with which the cure could be connected, it served us for guidance in the administration of it. Hence it always was our purpose to obtain salivation.

But we did not observe, upon the whole, that the abatement of symptoms was connected with any *certain degree* of salivation. Therefore we were, in every case, content with any, the least degree of salivation, under which they would disappear.

As in regard to the various bowel complaints, so in regard to the various nervous disorders, the condition most essential to the success of the remedy was unquestionably this, that the force and rate of its impression should be in proportion to the force and rate of the disease. And the chief object of our care was to preserve that proportion.

Thus, where the disease was less severe, and was slow in its

progress, salivation (without reference to its degree) was to be procured gradually; where the disease was more severe and rapid in its progress, salivation (without reference to its degree) was to be procured at once. Head-ach and vertigo which had come on tardily, and had abided many weeks, without any perceptible excitement of the circulation, were to be made to yield under the slow and alterative influence of mercury, which the constitution could bear without injury. Head-ach and vertigo which had been sudden in their accession, were accompanied with excitement of the circulation, and already seemed to threaten something beyond themselves, as convulsion, or delirium, or phrensy, were to be at once mastered by such a sudden and powerful impression of the remedy as the constitution would severely feel. Hence the quantity of the remedy was continually varied, according to the exigencies of particular cases. For some we prescribed one grain or two grains of calomel, with a small quantity of opium, once or twice in twenty-four hours, and thus succeeded in procuring relief after the lapse of a week or ten days; doing no harm, in the meantime, to the general health and sensations of the patient. For others, we prescribed five, or ten, or even twenty, grains of calomel, with proportionate quantities of opium, once, or even twice in twenty-four hours; and thus succeeded in dissipating the symptoms at once, and in rescuing life at the expense of some present injury to the constitution.

CHAPTER V.

THE FEVER.

When I was describing that part of the disease of the prison, which consisted of intestinal flux, I inquired how far it was attended by the symptoms which usually accompany such complaints, and serve to illustrate their nature. And with respect to fever I stated, "that it was present in a few cases, but that the majority were entirely free from it, and that, where it did occur, its symptoms only reached a very moderate degree of excitement." Now wherever, in cases of flux, and (I may add) in cases of disorders of the brain or nervous system, fever was present, the manner of their alliance was such as to raise a doubt, whether the fever was derived from the local disease, or the local disease from the fever.

For my own part, I believe that a fever arose at this time in the prison, which was sui generis and idiopathic, however its character might be obscured by an association with those forms of disease which have been mentioned. When this fever occurred alone (as it sometimes did, even at the time when the bowel complaint, and the disorders of the brain and nervous system were most prevalent), its type was manifestly peculiar. It was a fever of very moderate excitement, and generally went off in three or four days by perspiration. Or, if it failed of such relief, either spontaneously or by the help of medicine, it was apt to be protracted in the form of hectic during several weeks. When this fever occurred (as it generally did) in combination with some form of bowel complaint, or some affection of the brain or nervous system, its own peculiar type was still visible, notwithstanding certain differences which it exhibited correspondent with the disorder of a particular organ.

Further, since the fever and the local affection did not bear the same relation to each other in the different cases where they were found combined, it might hence be suspected that they

had no necessary alliance. Thus, in some the fever would first arise, and the flux would follow, perhaps immediately, perhaps not for several days, or perhaps not until the fever had been protracted in the form of hectic for several weeks. In some the flux would arise first, and then the fever would follow, perhaps immediately, or perhaps not until the flux had become a chronic disease.

But still, while the flux of the bowels and the disorders of the brain and nervous system prevailed to their largest extent, the cases of fever were rare. It was not until these complaints began to subside, that the fever showed itself, in a sufficient number of cases at once, to make us accurately acquainted with its type. At no time did it pervade the prison to an equal extent with the other two forms of disease. But its extent, nevertheless, was such, that it had a just claim to be considered as a part of *the disease* of the Penitentiary; and the manner in which it was mixed up with the disorders of the bowels, and the brain and nervous system, led to the belief that they had all a natural relation to each other, and that they all sprang from one and the same morbid condition of the constitution at large, and were all, in some sort, the same disease. It remains for me to give a more precise description of this fever, both alone, and in its combinations.

In many cases, a slight shivering, followed by heat and langour, and want of appetite, and a pulse ranging between ninety and a hundred, constituted the whole disease, and the use of common salines constituted its whole treatment. The patients being put to bed, began to perspire, and in three or four days they were well, with little loss of strength. Thus far it was the mildest and the most manageable fever I had ever seen.

In many cases, to the common symptoms of fever, were added pain at the pit of the stomach and head-ach; which together constituted the whole disease. In these cases, an emetic, or a brisk purgative, followed by saline medicines, operated a speedy and effectual relief. Thus, when the stomach and bowels were cleared, and perspiration promoted, the patients were well in three or four days, with little loss of strength.

Now, with two or three exceptions, in which the lungs were affected, and hæmoptysis took place, the stress of the disease fell

(if upon any particular organ) always either upon the stomach or bowels, or upon the brain, or upon both together; and in those who died, the brain, or the stomach and bowels, presented traces of recent disease. For the symptoms referable to these parts, mild as they generally were, yet were aggravated to an alarming degree in several instances, and were rapidly fatal in a few. A relation of some cases, in which these symptoms reached their greatest pitch of severity, will best serve to show the formidable character under which the disease occasionally presented itself.

A young woman (Mary Chapman), aged twenty-six, was attacked, like the rest, with the common symptoms of fever, except that she had a shivering fit, which was remarkably severe. After which, there arose a sudden and excruciating pain in every part of the abdomen, a vomiting of bilious matter, and a profuse purging of matter like tar. The patient sank at once into a state, from which it was evident that she could never rally. Her countenance was pale and full of terror; and, if she was moved, she was ready to faint away. After the lapse of twelve hours her pulse was imperceptible, and she was thought to be dying. Nevertheless, she survived four days; and, in the meantime, nothing could be attempted but to uphold life by such small quantities of wine or brandy as she could take. On the third day, her constitution in a manner re-acted, and her countenance was a little flushed. Soon afterwards her respiration became stertorous, and her mouth was a little distorted, and then she died.

After the first gush of matter from the bowels, no further evacuations took place spontaneously, or could be procured by medicine. There was no tension of the abdomen, but, on the contrary, it was flat and soft to the last. Upon opening the cavity of the abdomen we found, here and there, throughout the whole course of the intestines, from the stomach to the rectum, large and extensive vascular patches, all of a very dark colour, and some absolutely black. The great end of the stomach, and the whole of the duodenum, were intensely black. The small intestines were, in several places, puckered up and contracted, for the space of an inch; and wherever these contractions were found, the bowel was of a deep black colour. Within the cavity of the abdomen, a small quantity of fluid

blood (about two ounces) was found. It seemed to have exuded from the surface of the duodenum, just where its last turn commences, and especially from that part of it which is uncovered by peritoneum. It was just in this situation that the blood was found, and this part of the intestine was soaked in it. There was no effusion of lymph, or of any other fluid within the peritoneum, but the blood above-mentioned.

The stomach, being opened, was found empty; and, at its great end, the mucous membrane seemed in one uniform state of ecchymosis. But this was not really the case; for when the stomach was held up to the light for inspection, it was evident that no extravasation had taken place, and that the apparent ecchymosis was occasioned by every vessel, great and small, in this part of the organ, being filled and gorged with blood. There was also this same appearance of ecchymosis in the mucous membrane of the duodenum, occasioned by a condition of the blood-vessels essentially the same. Upon the valve of the pylorus, however, there were three small spots, where blood was actually extravasated. At the various spaces of the small intestines, which were black from without and contracted, there was the same apparent ecchymosis of the mucous membrane, which was, in fact, a remora and accumulation of blood within the blood-vessels. The whole tract of the intestines was filled with a matter resembling tar.

In the whole course of the bowels there was only one small ulcer. This was in a part of the small intestines, most free from vascularity, and seemed to be undergoing a process of reparation.

The vessels of the brain and its membranes were loaded with blood. But there was no effusion of fluid anywhere within the cranium.

This case occurred among many other cases of fever, in which symptoms were present expressly referable either to the stomach and bowels, or to the head, or to both. It could not but be regarded, therefore, as essentially the same, although it was distinguished from the rest by the extraordinary severity of those symptoms, and the rapidity with which it passed to its fatal termination.

In the following case the fatal symptoms were more exclusively referable to the brain:—

It was our custom to visit occasionally the various apartments of the Penitentiary, to examine the prisoners who were *professedly* well, and to select from among them, for the purpose of placing under medical observation, any whose looks seemed to threaten the approach of illness. In one of our visits to the laundry, we found a young woman huddled up among some dirty linen, and lying upon the boiler, fast asleep. The fire of the boiler was put out, but some warm water still remained in it. Being roused, she looked strange and bewildered, but made no specific complaint. She said, "I only feel cold at times, and this is a good way of warming myself." The other women in the laundry considered that not much was the matter with her. As to lying upon the boiler, they told us, it was a fancy of her's, and she often did it.

Nevertheless, from this very feeling of cold which she described, and from her general appearance, we thought fit to remove her into the infirmary. Here we soon found that this sense of cold was the beginning of a fever. The next day her skin was hot and her tongue dry. The following day the stress of the disease had fallen upon the brain, and her head was in continual agony. As long as she retained her senses, she described the head-ach as most distracting. Then delirium arose, and the pulse lost all its power. And before a week had elapsed, she died with stertorous breathing, and with symptoms much akin to apoplexy.

The name of this woman was Mary Venables; she was in her twentieth year.*

But there were a few cases of fever nearly resembling those two fatal ones just described, and even approaching to them in the severity of the symptoms referable to the head and the abdomen, which nevertheless did well.

In many cases, after the common harbingers of fever, and at the time when the symptoms of re-action were expected, there arose a pain, like a fixed and unshifting cramp, at the pit

* I am not able to find the note taken of the morbid appearances, upon examination, of this patient. As far as I can trust myself to speak from recollection, the brain and its membranes were loaded with blood; fluid was effused into the ventricles, and between the membranes; and there were vascular streaks and patches in the mucous membrane of various parts of the bowels, but no ulceration.

of the stomach, and with it a most distracting head-ach; while the region of the scrobiculus cordis was most impatient of pressure. Whereupon the pulse became very frequent and feeble, and the countenance pale and full of terror. Clammy perspirations broke out, and brought no relief; and extreme sense of anguish seized upon the patient, and if he was moved he was ready to faint away. In this condition there was sometimes a spontaneous discharge of dark-coloured morbid colluvies, upwards and downwards, which put an end to the extremity of distress. Sometimes there was a discharge of turbid water only, by vomiting and stool, which was ineffectual for any purpose of relief. Sometimes there was a most painful retching, a striving in vain (as it should seem) without vomiting, and consequently no relief.

The further description of these cases shall be accompanied by a description of their medical treatment, since it will serve to illustrate their nature.

It has been said, that there was sometimes a spontaneous discharge of dark-coloured morbid colluvies, upwards and downwards, which put an end to the extremity of distress. Hence it seems probable that, in the case of Sarah Chapman (described above) "the vomiting of bilious matter, and the profuse purging of matter like tar," constituted in themselves a curative effort. But the magnitude of the disease (as it appeared upon dissection) was insurmountable; and Nature, in the struggle for relief, went beyond the purpose which she intended, and thus occasioned a complete exhaustion of her own powers, from which she could never afterwards rally. An effect of the same kind, and produced in the same manner, is occasionally seen in certain critical hæmorrhages.

But as to the cases now in question, as soon as there was a spontaneous gush of morbid secretion, upwards and downwards, then the cramp at the pit of the stomach, and the distracting head-ach ceased, and the pulse rose, and an universal warm perspiration followed.

Seeing then through what channels Nature succeeded in operating her own relief, we endeavoured, where there was need for the interposition of medicine, always to direct the remedies towards the same channels. Thus, when there was a discharge of turbid water only, by vomiting and stool, or an

ineffectual retching, and nothing rejected, and the distressing head-ach still remained, and the cramp was still fixed at the pit of the stomach, we sought by medicine, to procure the evacuation, from the stomach and bowels, of matter of a different kind, without which (as we conceived) there would be no relief; and this was effected by purgatives, but not by purgatives simply. If purgative medicine alone was given, the evacuations were increased in quantity, but their quality was still the same. But if the means, calculated to soothe and to abate pain, were first successfully employed, together with the remedies calculated to dispose the bowels to remit their morbid action, then the administration of purgatives compassed the end we desired. Thus, first by leeches applied to the pit of the stomach, and by fomentations of the whole abdomen, with flannel wrung out of warm water, and by two or three grains of calomel, given every hour, or every other hour, for three or four successive times, and lastly (after all these several remedies had been daily administered), by a drachm of sulphate of magnesia, given hour after hour, until the bowels were moved, we seldom failed to bring away copious evacuations of a foul and dark-coloured colluvies, and thus to dissipate the severe epigastric pain, and the distracting head-ach, and with them all the danger of the disease.*

* These cases were so peculiar in themselves, and the mode of relief, whether spontaneous or by the help of medicine, was so striking, that I may be permitted (in a note, at least) to make a few observations upon their probable pathology.

Formerly much stress was laid upon "turgid matter" and "morbid colluvies" in the first passages, as a cause of fevers; and certain distinctions of symptoms were thought to indicate that this colluvies was "pituitous" in one case, and "bilious" in another. At present, medical men are content to speak in more general terms of an accumulation of "morbid secretions," still regarding them as the cause of disease, and directing remedies for their removal. Emetics are given, and foul matter is rejected by vomiting. Purgatives are given, and the same is passed by stool. In consequence of which, the previous symptoms cease, or are greatly mitigated; and thus the theory is confirmed.

Nevertheless it may still be doubted whether the popular notion be correct respecting the actual condition of the stomach, at the time the symptoms referred to it are most intense; and still more, whether the matter rejected be really the "materies morbi," the accumulation of which produced the symptoms, which cease upon its evacuation.

It consists better with sound pathology to believe that, at the period of

Now, upon these cases I have been induced to dwell more at large, on account of the express and paramount indications of treatment contained in certain peculiar symptoms which belonged to them. These cases, however, were not essentially of a different type from the fever that prevailed in the prison at the same time. In them, indeed, its type was obscured or obliterated by the accession of the symptoms in question,

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the severest local pain and severest constitutional disturbance, there is hitherto no accumulation of morbid matter within the stomach, but that its blood-vessels are engaged in a morbid process, which, if its termination is favourable, will be finally resolved by a gush of foul secretion from their extremities.

That it is not anything extraneous to the blood-vessels which produces the symptoms, but the blood-vessels themselves by their own morbid action, is rendered probable by the relief which leeches often procure, when they are applied to the skin immediately over the seat of the pain; also by the relief more effectually obtained by remedies which have an express influence upon the blood-vessels, being employed together with purgatives, than by purgatives alone; for instance, by several doses of calomel, given in succession, at short intervals, and followed by senna or jalap, than by senna or jalap alone.

Further, a spontaneous vomiting will sometimes bring away from the stomach nothing but a turbid water, and a spontaneous purging will sometimes bring from the bowels nothing but a fluid which is thin, pale, and inodorous. Hence no relief follows. This striving of Nature is premature and ineffectual.

Moreover, emetics and purgatives will procure sometimes mere watery evacuations, upwards and downwards, and thus fail altogether of their curative effect.

The fact seems to be, that the vessels must first pour out the morbid colluvies into the stomach, before it can be rejected out of the body. This its separation from the blood-vessels is the first and principal curative effort; its expulsion by vomiting or purging is secondary and consequential, and curative only in a less degree; but still necessary. Emetics and purgatives, so administered, as simply to procure its rejection out of the body, do good, in assisting the last process of the cure; but emetics and purgatives, so administered, as first to aid its separation from the blood-vessels, and then its rejection from the body, conspire with Nature in every purpose she endeavours to effect, from the first to the last.

It appears, therefore, most probable, both from the course of the symptoms themselves, and from the efforts of Nature for her own relief, from the remedies, and from the conditions of their successful operation, that the disorder of the stomach, described as incident to the early stage of fever, is caused and maintained by a morbid action of a peculiar kind, in which its blood-vessels are engaged at the time.

derived from the stomach and the brain. The sudden exhaustion, the clammy sweats, the rapid, feeble pulse, and pale countenance, were derived from both; while the stomach betrayed its disorder more immediately by severe pain and vomiting, or fruitless efforts to vomit; and the brain, by pain and delirium, or hurried and oppressed breathing, or diminished sensibility. But when the severe distress, belonging immediately to the stomach and the brain, and the overwhelming symptoms derived to the constitution at large, through the medium of these organs were dissipated; if the fever still remained, it put on the same character, and pursued the same course with the fever of other cases, in which such symptoms had never appeared.

Now, the fever of the Penitentiary, both when it was a simple fever from the beginning, and when it was attended, from the beginning, by symptoms of the milder sort referable to the epigastrium and the brain, did generally disappear altogether within a week. But in some cases it was protracted beyond that period. Moreover, this fever, when it was attended, from the beginning, by the severer symptoms referable to the epigastrium and the brain, unless it proved fatal (as it did in a few cases) within a week, was generally brought to a favourable termination within the same period. But, in some cases, it still continued its course.

Here, then, it must be observed, as a thing most remarkable, respecting this fever, that whatever had been its character and progress hitherto, if it was continued into a second week, the type which now belonged to it was the same in all cases, viz. the type of hectic. It was constituted of heat and perspiration, going and coming with little intermission, and a frequent pulse.

When the fever arrived at this stage, we were at first disposed to leave it henceforward entirely to itself, administering no medicine, either with a view of abating the symptoms, or of pushing the patient into health; for the symptoms seemed such as would spontaneously wear themselves out. Experience, however, soon taught us the necessity of a more active interference; for the fever, being left to itself, contrary to our expectation, showed no disposition to cease spontaneously. We were compelled, therefore, to resort to tonic remedies, and found them more beneficial the earlier the period at which they were

administered. The salutary effect of such remedies was most strikingly manifest in those cases, where the accession was marked by symptoms of peculiar severity referable to the head, or the epigastric region. After these symptoms were dissipated, by the means already pointed out, and simple fever remained, bark and acid were required to prevent that fever from becoming a slow hectic of many weeks' duration. And, if the hectic had already begun, and continued for a time, bark and acid were the medicines which sustained the patient under it, and brought him safely through it.

I mention bark and acid more expressly, because from positive experiment it is certain that these remedies did good, while wine and (what are called) diffusible stimulants did harm. The employment of bark, to a good purpose, on the fourth or fifth day of fever, may, perhaps, startle modern apprehension; but the necessity of it, and its success in these cases, were unquestionably proved.

In the cases in question, the whole secret of conducting the fever to a successful termination consisted—first, in not being deterred from using plentiful evacuations in the earliest stages, on account of the sudden pallor, and faintness, and feeble pulse, and apparent exhaustion (symptoms derived from the present oppression of particular organs); and next, in not withholding bark and acid (remedies which sustained without heating) as soon as the evacuations had relieved the internal oppression, and nothing but the fever and its symptomatic sweats remained.

Thus I have described the complaints prevalent at the General Penitentiary, as they fell under my own observation. These were a scurvy, a flux of the bowels, a disorder of the brain and nervous system, and a fever. All four, from their extent and frequency, had a claim to be considered *the disorders* of the prison; and the manner in which they were often combined in the same individuals, and the way in which they were taken up and succeeded by each other, induced the strongest belief that they all sprang from the same cause, and were, in a certain sense, the same disease. This belief was further confirmed, in respect to two of them, the flux of the bowels, and the disorder of the brain and the nervous system, by the consideration that they were both amenable to one and the same

remedy, and (as far as our experience went) intractable by any other.

What then (it may be asked) was the essence of the disease, considered as a whole? Of the inceptive error, or primary morbid action, from which it arose, I am entirely ignorant; whether it belonged to the blood-vessels, or the nerves, or to what particular organ, or system of organs. I only know that its first cognizable effects were seated in the blood-vessels, and that they consisted both in the admission of blood into their capillaries, beyond the natural sphere of the circulation, and in its transmission through their capillaries out of the sphere of the circulation altogether; that the consequences were ecchymosis of the skin, and ecchymosis and vascular patches of the mucous membrane of the stomach and intestines, and determinations of blood to the brain and its membranes, and (probably) to the spinal marrow; and that, out of these, arose diseases which obtained different names, according to the manner of their occurrence and the parts upon which they fell, such as the scurvy, and the various species of flux, and the various species of nervous disorder, and some forms of fever, all of which were nevertheless still the same disease in the conditions of their production.

I am well aware, that the essence of the disease must have consisted in something prior to these effects, and productive of them. But the present state of our knowledge does not enable us to ascertain what it was; and, indeed, the search after the essence of any disease, beyond the point at which it begins to fall within the reach of the senses, has seldom brought the pathologist to any more certain conclusion than this, viz. that it consists in "a morbid disposition or action, which is sui generis."

CHAPTER VI.

INTERCURRENT DISEASES.

THERE were certain other complaints in the prison at the same time, which were remarkable for their frequency, and some which were peculiar in their character. But whether they had any natural alliance with the reigning disorders, or were merely accidental, I cannot tell. Cases of erysipelas were constantly arising, and upon the whole were numerous. They were all slight cases, yet they lingered in their cure, although they all did well. An affection, not unfrequent among the females, was an inflammation and swelling of the labia pudendi. It was accompanied by a smart febrile attack, which lasted two or three days, and uniformly terminated in the formation of abscess, without further inconvenience. Accidental sores and bruises were, with difficulty, brought to heal; and one young woman, whose hand had been injured by pearl-ash, suffered a sloughing of the integuments of the fore-arm, by the spreading of the sore. In several individuals, the application of leeches to the surface produced extensive extravasations of blood beneath the surrounding cuticle. This effect was most remarkable in a boy (John Shaw, aged seventeen), who, during a painful and protracted illness, required their application to different parts of the body, at various times; and in all these parts a blotch of ecchymosis spread from the leech-bite, as a centre, to a considerable distance. Nearly the whole thigh became black, in consequence of half-a-dozen leech-bites. The ailment, for which the poor boy required this treatment, was itself peculiar, and deserves a brief notice.

Early in May, he suffered an inflammation of the conjunctiva, which was cured by leeches. In the middle of the month, he had severe pain in the head, which was relieved by leeches. Next he had inflammation near the knee-joint and shoulder-joint, and an inflammation and swelling opposite to the sacrum, which required the same treatment, and eventually

matter formed in all these situations; lastly, the whole thigh became swelled, and painful, and hot, requiring the same treatment, until a large quantity of pus, mixed with blood, was discharged. These abscesses continued to form, in succession, during six weeks, accompanied by fever, of great intensity and excitement, and wherever a discharge took place, it continued ever afterwards. The poor boy died exhausted, on the 7th of July.

Another prisoner (George Prior, aged nineteen) suffered the same kind of disease with Shaw, but in a more chronic form. In the middle of the month of March, he had a soft tumour beneath the scalp, opposite to the left parietal bone. Soon afterwards, he had another soft tumour opposite one of the ribs. They were both about the size of a hen's egg. He became feverish, and his strength declined. The tumours were punctured, and pus was discharged; and for several months the punctured orifices still continued open, and matter still came away. Afterwards, another soft tumour was formed opposite the sacrum, and became an abscess. He eventually died consumptive, on board one of the hulks, at Woolwich.

At the Penitentiary, there were some cases of diseased joints, where the complaint appeared a mere rheumatism in its commencement, but soon put on a more formidable character. In one instance especially, under the most careful treatment, with the advantage of rest and change of air, the knee-joint was rendered useless, and active disease, which would probably render the boy a cripple for life, was still going on, after the lapse of several months.

Now, I do not wish to lay an undue stress upon these cases. Unquestionably, occasional cases such as these are met with elsewhere, and under ordinary circumstances, when it is quite in vain to inquire their cause. But when, in the midst of an epidemic, certain other diseases, occurring at the same time and under the same circumstances, yet not obviously connected with it, have an unusual course and termination, we cannot help suspecting that the same cause which engendered the epidemic, may have impressed upon them their peculiar character; especially if that character is such as to bring them into a similitude with the epidemic, as far as in their own nature they are capable of being thus approximated.

It was the custom of Sydenham (whom it is safe to regard both as an authority and an example), after he had described the epidemics of particular seasons, to dwell shortly upon certain other complaints, which had happened at the same time, and which he called intercurrents. In so doing, he would show how, by the force of the predominant influence, their character was sometimes altered into a kind of conformity with the reigning disease; and thus he considered them as furnishing as strong a proof of the existence of such an influence as the epidemic itself.

These observations contain all the commentary I wish to make upon the preceding cases, and, at the same time, explain the reasons why I thought it necessary to take some notice of them.

To them, however, I must add one case more. It created great interest at the time, and some alarm lest this kind of disease should not be limited to a single case.

S. W. (middle aged), a nurse in one of the infirmaries, a large, corpulent woman, with a most florid countenance, and an habitual drinker of spirits, became suddenly ill. She had buboes in the left groin, and, at the same time, a bunion on the great toe, from the irritation of which the buboes were thought to proceed; and so, indeed, they might, for there was a streak of red running up the leg and thigh. The skin about the glands in the groin began rapidly to inflame. Fever arose, and with the fever spots of ecchymosis were scattered over the inside of the left thigh. The spots became more and more numerous, spread round the limb, and coalesced into one large patch, which became completely black. In the meantime spots of the same kind appeared on other parts of the body, on the other leg and thigh, on the hands and arms, and loins and back. They coalesced here and there into large patches, and were universally black. Then there was purging of blood; not a copious discharge of unmixed blood, but of blood tinging other matter evacuated from the bowels. The pulse sank so as to be scarcely perceptible. The buboes in the groin inflamed and suppurated and sloughed, and emitted a horrible fœtor. The cuticle over the ecchymosed patches was, in several parts of the body, raised into vesicles. The hands and feet became cold. Still the intellect was perfect; the stomach hitherto had not failed, and she was continually upheld with wine and brandy.

Five or six days were occupied by the disease passing through these stages, and arriving at this condition, which seemed hopeless. From this condition, however, the constitution made a vigorous effort to rally. The pulse first became perceptible, and then a little warmth returned to the skin. The discharge from the buboes changed its quality, the fœtor abated, the slough was thrown off, and the edges of the wound became firm and healthy. The spots and patches of extravasated blood changed from black to brown, and their margins were florid. The vesicated cuticle peeled off, leaving no sore, and a new cuticle was formed beneath it.

Under these favourable changes, it was still necessary to uphold the circulation by brandy. It was thought one day that the brandy might be dispensed with. It was so. But the next morning her pulse had become so much more feeble, and the circulation was so obviously leaving the extremities, and the patient was altogether so sunk, that the brandy was again eagerly resorted to, and with effect; for in a few hours the extremities became warm again, and the pulse rose.

There were still various changes favourable and unfavourable for a week. The favourable, were the improved complexion of the wound, and the partial fading of the ecchymosed spots. The unfavourable, were an occasional coldness of the extremities, and the impossibility of sustaining the circulation but by the continual administration of brandy. This was a state of things that could not last long. By continual watching, however, she was still kept alive, until, on the 20th day from the commencement of her disorder, she died.

Upon examination, after death, the intestines were of a dusky brown colour externally, while they exhibited, at irregular distances, spots and patches which were absolutely black. This appearance was derived from the condition of the mucous membrane, which was generally loaded with blood, but in a very few places only in a state of ecchymosis. Some portions of the bowel, that were of the deepest black, being held up to the light for examination, showed distinctly that the blood was contained within its vessels. The number of ecchymosed spots, throughout the whole tract of the intestines, did not exceed six, and the size was not greater than the diameter of a pea. They all appertained to the mucous membrane. In the ilium, just at

its termination with the cæcum, there was a very superficial ulceration of the mucous membrane, about two inches in circumference. The valve of the cæcum was especially loaded with blood.

The liver was pale; the spleen was exceedingly soft. In the chest, numerous spots of ecchymosis were found upon the pleura covering the ribs of both sides. Their size varied from a mere speck to a patch two inches in diameter. On the pericardium, both on its loose and reflected portions, the same appearances of ecchymosis were very strikingly exhibited.

This case appeared to me of sufficient importance to require that all its particulars should be recorded as they were noted down at the time. I know not with what form of the predominant disease it ought to be classed, and therefore I have placed it alone. There is, indeed, an extreme possibility that it was merely an accidental case, and that the patient might have suffered in the same manner wherever she had been. But taking the most prominent symptoms, and the morbid appearances found upon dissection together, we must consider them as furnishing an almost certain proof that the disease was essentially the same with the reigning disease of the prison, and had derived itself from the same source.

CHAPTER VII.

REVIEW OF THE EXTENT OF THE DISEASE AT DIFFERENT PERIODS—UNCERTAINTY RESPECTING ITS CAUSES.

THE following Tables will serve to show the extent of the disease, from the numbers under medical treatment, at different periods during the month of May, June, and July :—

Prisoners under Medical Treatment, on the 15th of May, 1823:

		MEN.	WOMEN.	TOTAL.
Diarrhœa	Ill	46	44	90
	Better	48	87	135
	Well	49	20	69
Other complaints		21	30	51
Total		164	181	345

On the 23rd of May:

		MEN.	WOMEN.	TOTAL.
Diarrhœa	Ill	63	46	109
	Better	51	56	107
	Well	64	47	111
Other complaints		24	36	59
Total		202	185	386

On the 11th of June:

		MEN.	WOMEN.	TOTAL.
Diarrhœa	Ill	73	35	108
	Better	86	38	124
	Well	82	88	170
Other complaints		24	28	52
Total		265	189	454

On the 3rd of July:

		MEN.	WOMEN.	TOTAL.
Diarrhœa	Ill	17	22	39
	Better	70	37	107
	Well	179	83	262
Other complaints		8	22	30
Total		274	164	438

In these Tables we designated as "Ill," those whose disease was either progressive or stationary; as "Better," those who had already lost some symptoms of their disease, and made some advance towards health; and as "Well," those who were well, so far as they were free from the symptoms of disease, yet not *strictly well*, inasmuch as they were judged still prone to relapse, and were still the subjects of medical observation and care.

It is necessary to observe, respecting these Tables, that they are the same which were presented to the committee. In them diarrhœa alone is mentioned, being put for every species of flux which has been described, and no separate enumeration is made of nervous complaints and of fevers, which have been considered as *the disease* of the prison, equally with the flux of the bowels. The fact is, it was late before we ourselves arrived at the conclusion that they were really so. Hence, when they occurred alone (and this was rarely the case), they were placed among the "Other Complaints," or accidental disorders of the prison, until they were certainly known to be peculiar to it; and when, as it happened in the great majority of cases, they were combined with bowel complaints, it was thought enough to specify that which was the more notorious disease.

Thirty prisoners died at the Penitentiary while Dr. Roget and myself were in attendance; of whom thirteen were men, and seventeen women. Twenty-two fell victims to *the disease* in some of its forms, and the remaining eight to complaints which had only a suspected connexion with it, or none at all.

Of the thirteen men—

7 died of the various species of bowel complaints.
2 „ of disorders of the brain and nervous system.
2 „ of fever.
1 „ of abscesses, which had a suspected connexion with the reigning disease.
1 „ of struma, which had no connexion with it. This man died by accidental suffocation. A mass of diseased absorbent glands occupied the whole space between the ears and the clavicles on both sides, and met in front of the trachæa. His respiration was habitually oppressed; he was under medical treatment in the infirmary, and one morning he was found dead, with his countenance swollen and livid.

Of the seventeen women—

4 died of the various species of bowel complaints.
5 „ of disorders of the brain and nervous system.
2 „ of fever.
1 „ of hepatitis, which had a suspected connexion with the reigning disease.
1 „ of diseased spine, which had no connexion with it.
4 „ of phthisis, which had no connexion with it.

When the last of the Tables, given above, was presented to the committee, it was accompanied by the following observations.

GENTLEMEN,

In presenting our Report of the present state of the Penitentiary, we must be permitted to accompany it with a few observations.

When we were first engaged at the Penitentiary, it appeared to us important to determine the period at which the diseases we had to treat began to prevail; and for this information we resorted to the testimony of the prisoners themselves. Owing, however, to certain suggestions made to us by others, we were led to distrust the statement of prisoners respecting their own complaints, unless it was confirmed by other circumstances; and thus we were able to trace only a few cases of either diarrhœa or scurvy back to a remoter period than Christmas.

But our greater experience of their conduct and character has led us to give further credit to the prisoner's own statements; and from the uniformity and consistency of their testimony, we have no doubt that both diarrhœa and scurvy prevailed most extensively throughout the prison at an early period of the autumn. Not longer than a fortnight after the diet was changed, in July, the disorder of the stomach and bowels began first to show itself. Even in the very month of July a few of the prisoners suffered vomiting and diarrhœa. These (as we judge from the manner in which they occurred) were unquestionably owing to the disagreement of food. They ceased and recurred at intervals. The prisoners themselves thought lightly of them, and did not make them a subject of complaint either then or at a much later period. Hence (as we have observed in a former Report) no blame is imputable to the medical officers, that they went for a long time undiscovered.

Nevertheless the fact is certain; and it is no wonder, therefore, that the diarrhœa especially, which had been so long unrestrained by any effectual treatment, should at length have proved an intractable disease, and been protracted by a succession of relapses nearly to the present time. For this complaint (independently of other circumstances connected with it) is, according to the length of its duration, always suspected of being maintained by a disorganization of (what is called) the mucous structure of the bowels.

Such disorganization, in its least and lowest degree, is reparable tardily only, and with difficulty; and in its greater degree, it is altogether irreparable.

In our Report to the Committee of the 5th of April, we ascribed the forms of disease then prevalent in the Penitentiary mainly to the influence of diet and cold, and our opinion has been confirmed by that of other physicians who have been consulted upon this point. But many prisoners admitted since the diet thought to be injurious, has been changed, and since the weather has become milder, have become the subjects of dysentery; and several of the officers of the establishment most employed about the sick have suffered the same disease.

Unquestionably, then, we do believe, that some injurious influence has been in operation, over and above the causes to which the epidemic was originally imputed. This injurious influence may have been present from the first, or it may have been subsequently superadded. Whatever it be, it has hitherto eluded our detection; and, whether it is, or is not in operation at present we cannot tell.

If it consist of contagion (and such possibly may be the case) dysentery will still probably linger in the prison, as long as any remain there who have not suffered it; and then it will entirely disappear. If it consist of something peculiar to the place, or to the season, or to the moral and physical condition of people so confined, it may be still capable of renewing the same disease, or of creating another form of epidemic.

We are aware that the public mind is impatient to be satisfied as to the causes which, from first to last, have produced and maintained the various forms of complaint in the Penitentiary. Upon this subject we wish to dwell for a moment, in order to show that the satisfaction which is sought cannot be obtained.

The causes (the exciting causes, as they are called) of diseases are involved in much uncertainty; and when the question is concerning an extensive epidemic, this uncertainty is felt, and confessed to be most painful: still the inquiry into these causes can never be hastily dismissed; for to discover what they are, and to remove them, if they are within our reach, may be essential to the effectual cure of the disease.

With respect to the Penitentiary, then, while we confess that there has been, and still may be, a cause of disease in operation, which we are ignorant of, we are most anxious for the committee to feel convinced, that the fault does not entirely rest with us that it remains undiscovered, but that the difficulty of detecting it is inherent in the subject itself.

We beg to state, that opinions respecting the causes of diseases are formed, not from the mere observation of one or two things immediately obvious to the senses, but from a cautious investigation of a great variety of circumstances, and from a series of reasoning upon them.

It is obvious that opinions necessarily so formed, are very liable to error; and hence it happens, that no prudent physician ever arrived at conclusions about the causes of a disease, with so certain a conviction that he was right, as not to confess that he might possibly be wrong.

Numerous cases in the Penitentiary, to which we have already alluded, have seemed to us quite inexplicable, except upon the presumption of contagion. The fact may be otherwise; and authorities (we are aware) preponderate against the contagious nature of dysentery; nevertheless, we have not thought ourselves justified in neglecting the practical measures which the facts before us appeared to suggest, until medical opinion is settled upon this point.

We beg to add a few remarks upon the Table just delivered in, which represents the number of prisoners at present under medical treatment.

In this Table the committee will see with satisfaction, how small a proportion those who are now suffering the severer symptoms of the disease, bear to those who are convalescent, and to those who are well.

Nevertheless, it may appear strange to the committee, that so many should be still kept under medical observation and treatment, who as far as health is attested by the absence of all symptoms of disease, are perfectly well: in them, indeed, the disease may be effectually cured, and we trust it is; but the instances of relapse, after many weeks of apparent health, have been too numerous to allow us to dismiss all apprehension concerning them.

How to obtain for those who have already suffered the disease, and are now apparently well, an effectual security against relapse, has become our chief care and anxiety. This security (as it seems to us) can be procured only by guarding them from the influence of all things that are obviously injurious, and by still keeping them, in a moderate degree, under the influence of the same remedies which have thus succeeded in restoring them to health.

For ourselves we must be allowed to observe, that we have never felt the responsibility of our charge so sensibly as at the present moment. While we were engaged in actively ministering to above 400 sick, we were not at leisure for useless anxieties; but now that we have time to reflect upon our situation, and consider that the public still holds us responsible for the employment of all possible means for protecting this vast establishment against the recurrence of the same terrible visitation, and the invasion of any new epidemic, we cannot refrain from begging of the committee, that they would be pleased to grant us the benefit of consulting with some physicians of eminence and authority, who may either confirm us in the means we are using, or suggest others more effectual.

We are not disposed to look with despondency upon the state of the Penitentiary. There is one event, however, which, under the most fortunate circumstances, will unquestionably come to pass, and it is proper that the committee should be informed of it.

If we were sure that, from the present time, all causes would cease to operate for the renewal of the same disease, and for creating any new form of epidemic, and if we were sure, that all who have suffered the disease were henceforward secure against relapse, still we must entertain the confident belief, that among nearly 500 persons who have suffered so long and so severely complaints peculiarly apt to debilitate the general frame, many

will be found, whose restoration to complete health will be tardy, difficult, and precarious, and who, in the meantime, will be ready to fall into any diseases to which the constitutions of each may have an original proclivity. Hence, we foresee, that independently of any general or epidemic complaint, there will arise from time to time in the Penitentiary, occasional cases of complicated and anomalous disease: of these cases some may be trivial, but others will be formidable, and a few fatal; and they will be as various in their characters as the constitutions of individuals are various.

We cannot close this Report without thanking the committee for the uniform kindness and confidence with which they have regarded our labours. We have (we can assure them) most anxiously and scrupulously reviewed all the practical measures we have employed during the prevalence of this most formidable and extensive epidemic. These measures were adapted to emergencies as they arose, with as much care and deliberation as circumstances would allow; yet, upon reflection, we conscientiously affirm, that we discover no error which we desire to palliate or amend, and see no suitable remedy or medical expedient which we have omitted to use; and that, should we again be called upon to treat the same malady, we should only study to retrace our own footsteps, and should employ the same remedies and medical expedients with an increased confidence in their success.

(Signed) P. M. LATHAM, M.D.
P. M. ROGET, M.D.

July 4th, 1823.

Our purpose in this Report was to intimate to the committee, that the disorder *as a flux*, had been of longer standing in the prison, than we had at first been led to believe; that its origin could not be *exclusively* owing to those causes to which it had been imputed; that there had been, and perhaps still was, some cause in operation, the nature of which had not been discovered, and might not be discoverable; that there was a suspicion of contagion, and a suspicion of local injurious influence, but nothing ascertained about either; that the extent of the disease was at present greatly diminished, but that all the prisoners were in a state of health, which (to say the least) was dangerously valetudinary; that disease was still to be expected in some form, either in the shape of relapse, or of some new epidemic, or of those various complaints which are the consequence of debility.

The date of this Report marks the time at which the disease of the Penitentiary had come to a kind of pause. Nevertheless, the many apprehensions and doubts which we felt, as to what might happen, and a very painful sense of responsibility, led us, for our own satisfaction, and the satisfaction of the public,

to ask for further medical assistance. Our request was made known to the Secretary of State, who directed the College of Physicians to appoint whom they thought fit. The College appointed four physicians of hospitals—Dr. Hue, Dr. Macmichael, Dr. Chambers, and Dr. Southey. Three only of the four undertook the charge. Dr. Chambers's professional engagements obliged him to decline it.

CHAPTER VIII.

REMOVAL OF THE PRISONERS FROM THE PENITENTIARY, AND ITS INFLUENCE UPON THEIR HEALTH.

WITHOUT calling in question, for the present, the salubrity of the situation of the Penitentiary, it seemed to Dr. Roget and myself, that the best security against the return of the old, and the appearance of any new, forms of disease, would be obtained by change of air and change of place. To individuals in private life, after long illness and frequent relapses, physicians are accustomed to recommend change of situation as indispensable to their recovery. But such a recommendation, when the question was concerning several hundred convicts, would (we thought) be nugatory, from the impossibility of carrying it into effect. Nevertheless, by the zeal of the committee, and the special activity of the visitor, Mr. Holford, and the aid furnished by Government, through the earnest interference of Mr. Secretary Peel, it was entirely accomplished; and all the prisoners, both male and female, were eventually removed from Millbank to situations deemed more eligible for the recovery of their health.

The Ophthalmic Hospital, in the Regent's Park, being vacant, was made fit for the reception of female prisoners, and 120 were removed thither from Millbank, between the end of July and the beginning of August. The Ethalion hulk, at Woolwich, was prepared for male convicts, and 200 were sent on board it, between the middle and the end of August.

The prisoners, both male and female, selected for removal, were those who had suffered the most severe and the most frequent attacks of the disease in all its forms; and the effect which change of air might have upon them was to determine how those were to be disposed of who still remained at Millbank.

The care of the prisoners at Millbank, and at the Regent's

Park, was now divided between Drs. Roget, Hue, Macmichael, Southey, and myself; while we all paid frequent visits to those on board the hulks, who were under the immediate care of Mr. Bayles.

The benefit of change of air and situation was immediately apparent, both upon the women at the Regent's Park, and the men at Woolwich. In a fortnight after their removal, there was much less complaint of illness among them, and their general aspect bore the appearance of returning health.

In the meantime, the prisoners who remained at Millbank, amounting to about 100 women, and nearly 300 men,* were stationary as to their general health, while they were still harassed by visitations of their former disease. It exhibited itself, however, under none of those frightful forms which have been before described. With the exception of two or three cases of fever, which were formidable, rather on account of their protracted duration than their severity, there were none which occasioned us any apprehension. The present form of disease was diarrhœa almost universally; and it nearly resembled that which we had witnessed upon our first visiting the Penitentiary, in the month of March. It was then controlled by chalk mixture and opium, and the same remedies, or remedies of which opium was the main ingredient, had now a beneficial influence. But if the disease, even now, went beyond a certain degree, it was in vain to attempt its abatement by any medicine but mercury; a fact, to which the physicians lately called in, bore equal testimony with Dr. Roget and myself.

Upon a comparison, during the month of September, of the prisoners, male and female, still remaining at Millbank, with those at Woolwich, and at the Regent's Park, the balance of health was greatly in favour of the latter; yet their condition, five or six weeks ago, was so much the worse, as to obtain for them the preference of being the first selected for removal. The benefit, therefore, of change of air, and change of place, was unquestionable.

On the 20th of September, we were able to report formally to the committee, that "the habit of all the prisoners, both at

* I cannot state the exact number, since it varied from time to time, on account of the pardons that were obtained.

Woolwich and the Regent's Park, is strikingly improved, and the majority have recovered the appearance of robust health. In this number many are included, whose lives had been brought into hazard by successive attacks of the disease in its several forms, and who, at the time of their removal, were in a state of great debility. The disease itself, we have the satisfaction to state, has assumed a much milder character, but even yet it is extensively prevalent; and it is remarkable, that many whose general health seems *entirely* re-established, still experience, from time to time, the recurrence of their former disorder in a mitigated form."

Another month elapsed, and the prisoners at Millbank, although they were suffering no formidable disease, still experienced some insurmountable impediment to the recovery of their health. Their condition at this time, and the condition of the men on board the Ethalion, and of the women at the Ophthalmic hospital, respectively, may be learnt from the following questions and answers:—

LETTER from G. R. Dawson, Esq., containing certain questions to be answered by the Physicians, by direction of the Secretary of State.

WHITEHALL, 21ST OCTOBER, 1823.

SIR,

In reference to your letter of the 18th October, I am directed by Mr. Secretary Peel to request, that you will call upon the physicians in attendance upon the sick belonging to the Penitentiary, for a detailed report upon the several points following:—

1st. The state of the disease at present existing in the Penitentiary, as compared, in point of malignity and extent, with its state at former periods.

2nd. What has been the general result of the experiment made in August, and at subsequent periods, of transferring a portion of the male prisoners from the Penitentiary to the Ethalion hulk.

3rd. The same question, with respect to the transfer of females to the Ophthalmic hospital in the Regent's Park.

4th. Is there reason to believe that the transfer of the male prisoners now in the Penitentiary to another hulk to be prepared for their reception, would be an advantageous measure to the prisoners themselves, as conducive to the recovery of the sick, and preventing the risk of disease in the case of those who have hitherto suffered.

5th. The same question as to the removal of female prisoners now in the Penitentiary, to some place of reception like that in the Regent's Park.

6th. Could the whole of the female prisoners, now in the Penitentiary,

be safely removed to a hulk at this period of the year; or could any portion of them, those for instance who have not suffered, or very slightly suffered from the disorder.

I am, Sir, your most obedient servant,

(Signed) GEO. R. DAWSON.

The Chairman of the Superintending Committee
of the General Penitentiary.

ANSWERS by the Physicians to the questions contained in Mr. Dawson's Letter of the 21st October, 1823.

OCTOBER 23rd, 1823.

1st. At former periods there was in the Penitentiary every gradation of complaint from the severest dysentery to the mildest diarrhœa; and the most formidable, or the dysenteric cases, were greatly predominant. At present, with a few exceptions, there is no disease beyond a diarrhœa, and the milder cases predominate. The malignity of the disease is therefore greatly abated. With respect to the extent of the complaint, if it be estimated from the number ill at the present moment, it is greatly diminished; but all in the Penitentiary, who have once had the complaint, are continually suffering short relapses.

2nd. The result of this experiment has been highly satisfactory. The prisoners transferred to the Ethalion hulk have gained a greater degree of health, and have suffered fewer relapses than those in any other situation.

3rd. The female prisoners now in the hospital in the Regent's Park, and who had been originally selected as being those who were suffering the most from the prevailing disease and its effects, have since their removal, notwithstanding they have experienced frequent relapses, on the whole, gained ground considerably. Comparing the general improvement that has taken place in their health, with the progress made by the females who have remained in the Penitentiary, we think ourselves warranted in concluding that they have derived considerable benefit from their having been transferred to the Ophthalmic hospital. It is certain, however, that they have benefited much less than the men on board the hulk.

4th. We think such a transfer of the male prisoners now in the Penitentiary to another hulk, would be a measure advantageous for the purposes contemplated.

5th. We think that such a removal would unquestionably be desirable.

6th. Although the period of the year be unfavourable, still we should consider that the removal of all the female prisoners to a hulk, if it could be soon effected, would, upon the whole, be an advisable measure.

(Signed) P. M. LATHAM.
P. M. ROGET.
CLEM. HUE.
H. H. SOUTHEY.*

* Dr. Macmichael's absence from London at this period prevented him from joining in these answers.

Upon this representation, it was determined to transfer all the prisoners, male and female, remaining at Millbank, to hulks fitted for their reception at Woolwich. Accordingly, on the 14th of November, the women, whose number was now reduced by pardons to eighty, were removed on board the Narcissus, and between the 8th and 10th of December, the men, reduced to 281, were put on board the Dromedary.

At length the Penitentiary at Millbank was empty, and all the prisoners were divided between the hulks at Woolwich, and the Ophthalmic hospital in the Regent's Park.

Now, considering what had been the condition of the prisoners at Woolwich, and of those at the Regent's Park, since their removal from Millbank, and comparing the present state of both, we had much more reason to be satisfied with the result of the experiment in regard to the former than the latter. Immediately after their removal, both alike seemed to throw off the remaining symptoms of their disease, and to put on the appearance of returning health; yet both afterwards continued alike to suffer an occasional recurrence of their disease, chiefly in the form of diarrhœa. There was, however, this manifest difference between the two, that while those on board the hulks at Woolwich, continued to recover their general health, in spite of frequent occasional attacks of diarrhœa, the females at the Regent's Park did not, in respect of their general health, go on to improve during more than the few first weeks after their removal. Moreover, among the prisoners on board the hulks, during a period of between four and five months, there had not occurred a single case of formidable disease; whereas, among those at the Regent's Park, during the same period, there had been several. At the end of November, several women at the Regent's Park, were seized nearly at the same time with that insidious form of phrenitis which, five months before, had been a great cause of alarm at the Penitentiary, and had, in some instances, proved fatal. Dr. Roget, to whose care the hospital in the Regent's Park was especially assigned, found himself compelled to resort to the same remedy by which these symptoms were formerly brought under control, and with the same success.

Under these circumstances, and moreover because the females recently removed to Woolwich from the Penitentiary, had

experienced a striking improvement, it was thought desirable that all who now remained at the Regent's Park, and were reduced from 120 to 91, should (if possible) be transferred to Woolwich; and accordingly, on the 21st and 23rd of January, 1824, they were put on board the Heroine, which had been prepared for them.

All the prisoners belonging to the General Penitentiary were now on board hulks at Woolwich. They were 635 in number, namely, 468 males, on board the Ethalion and Dromedary, and 167 females, on board the Narcissus and the Heroine.

When we first visited the Penitentiary, in the month of March, the total number of prisoners was 858, of whom 531 were males, and 327 were females. Of the 531 males 50 had been set at liberty, by the expiration of their terms of confinement, and by pardons, and 13 had died; and of the 327 females, 143 had been set at liberty, and 17 had died.

The immediate superintendence and treatment of the prisoners, after their removal to Woolwich, were consigned to medical men resident on the spot. Mr. Bayles had the care of the men, and Mr. Pratt, the apothecary of the Penitentiary, had apartments on board the Narcissus, and took charge of the women. The physicians, however, continued still to visit all the Penitentiary hulks at intervals; and some circumstances, which thus fell under my observation, require to be mentioned.

One remarkable circumstance was, that the prisoners in the several hulks, although transferred at various times, and at different seasons of the year, all experienced a striking change for the better, almost immediately upon their arrival at Woolwich, and that their disorder ceased for a time. This happened equally to the men put on board the Ethalion in August, and the Dromedary in December, and to the women put on board the Narcissus in November. When they had been on board ten days, we found scarcely any complaint among them.

This speedy amendment, which uniformly followed change of air and change of place, held out at first a most encouraging prospect. But in every instance it was fallacious; for, after a temporary pause, the disorder returned in the form of diarrhœa. It was mild in its symptoms, but still it was evidently the same which had prevailed at Millbank.

Another remarkable circumstance was, that neither the

total absence of the disorder, during a considerable period, nor the complete re-establishment, in the meantime, of the general health, furnished any security against its recurrence. The men on board the Ethalion were under our observation during three quarters of a year, after their removal, and among them there were many, who, having been free from all symptoms of complaint during two, three, four, and even five months, and having in the meantime recovered robust health, again suffered diarrhœa.

This liability of all the prisoners to suffer the recurrence of their disorder, in spite of the re-establishment of their general health, and of their complete immunity from it for a season, rendered their condition very fluctuating. It was impossible to anticipate what would be their condition from one week to another. At our visits to Woolwich, we were accustomed to see each individual prisoner, and to question him respecting his health; and thus, when at one visit we had found, among 200 men, not more than five and twenty with complaints of the bowels, at the next visit we found half of the whole number affected with diarrhœa.

These sudden transitions we were at first ready to impute to sensible changes in the state of the atmosphere, but soon we could discover nothing to which they could be reasonably ascribed. It seemed as if, in all who had once suffered, there still remained some morbid condition of the bowels, the relics of former disease, which required time, and the choice of all the most favourable circumstances, to effect its reparation. This condition, however, appeared compatible with the re-establishment of the general health, and compatible even with the natural functions of the bowels themselves, holding them nevertheless in a perpetual proneness to disorder, from occasional causes, until reparation was complete.

In the description of the disease, as it prevailed when all the prisoners were at Millbank, some account was given of the morbid appearances found in the bodies of those who died. From this it may be collected, what was the actual condition of the parts upon which the stress of the disease fell, when it declared itself in symptoms referable especially to the bowels, or to the brain and nervous system, or when it appeared under the more general character of fever.

And now, having described the same disease recurring in one of its forms, that of diarrhœa, among the prisoners after their removal, and still continuing to recur during many months, and under every variety of circumstances, and even after the complete re-establishment of their general health, I shall be expected, perhaps, to state what was the actual condition of the intestines in those who were the subjects of such attacks. I possess, however, no certain information upon this point; for the only means of obtaining it would have been by dissection at the time; but these, fortunately, were wanting, since the attacks did not prove fatal in a single instance. Several dissections, however, performed at other times, and with different views, seemed to me to throw some light collaterally upon the question.

In those who died at Millbank, after symptoms immediately referable to the head, having formerly suffered disorder of the bowels, but having been a long time free from it, we found ulcers of the intestines. The intestines were otherwise exempt from disease. The ulcers were few in number, not more than three or four throughout the whole tract of the bowels. They were of very small extent, and in progress towards reparation.

A young man who had originally suffered the bowel complaint very severely, at Millbank, and whose life had been rescued by the treatment adopted there, and who subsequently at Woolwich, in spite of the re-establishment of his general health, had frequent returns of diarrhœa, was ultimately pardoned and set at liberty. A short time afterwards he presented himself to me among the patients applying for admission into the Middlesex Hospital. He was a miserable object, worn down and emaciated by impoverished diet, and the hardships he had suffered since his liberation. His present complaint was diarrhœa, with severe pain in the head. The diarrhœa was soon arrested; but the disorder of the head assumed the form of that insidious phrenitis already described, and he died. Upon dissection, there was found inflammation of the membranes of the brain, and fluid effused between them and into the ventricles. In the intestines, after careful examination, nothing was found but three minute spots, exhibiting the appearance already described, of ulcers in the process of reparation.

A young woman who had suffered the bowel complaint, at Millbank, with several slight relapses, died in the course of the last summer, at the Westminster Hospital, of pulmonary consumption. Upon dissection, besides the disease of the lungs, healing ulcers were found in the intestines. They were very circumscribed in their situation, and entirely free from surrounding inflammation.

It is not unreasonable to conjecture, that the same condition of the bowels, which was found in these cases, existed also in those who suffered the frequent recurrence of diarrhœa at the hulks. A few minute ulcers, somewhere in the tract of the intestines, tardily undergoing the process of reparation, and without any surrounding inflammation, constitute just that sort of morbid condition which is calculated to produce such a disorder. Being very circumscribed in extent, and free from inflammation, they would be capable of existing without constant injury to the general health, and even without constant impediment to the functions of the parts to which they belonged; yet, from the natural irritability of those parts, they would still be liable to produce temporary disorders, under the influence of occasional causes; and such disorders would continue apt to recur, until the process of their tardy reparation was complete.

But however reasonable, and however true this conjecture might be, we were not yet acquainted with all the facts just mentioned, upon which it is grounded. So that, during the actual prevalence of the disorder at the hulks, our method of treating it was determined by the best view we were enabled to take of its general character, in the absence of information derived from dissections. And that view, which has been already stated, led us to suggest the propriety of employing such remedies only as were found to have the effect of checking the symptoms as they arose, and the propriety of abstaining from the use of mercury, and of being content to leave that to time which time alone could repair. Opiates and aromatics had the palliative effect which was desired, and they were accordingly employed.

Where, however, more disorder of the bowels arose (as it did in a few instances) than could be supposed to proceed merely from the relics of former disease, and these remedies

had no influence in checking it, then others were resorted to from necessity, and all others were still in vain except mercury.

Such a disorder arose on board the Narcissus; and the symptoms seemed to bespeak a morbid action of an acute kind, set up afresh, or ingrafted upon the old. The women on board the Narcissus had, since their arrival at Woolwich, in November, rapidly recovered their general health, and had been strikingly exempt from their former complaints, when, in the month of March, there were found among them numerous instances of disorder, expressly referable to the stomach. It was of a severe and formidable kind, and did not strictly resemble any of those forms of disorder which have been described. There was a sense of sinking, with extreme pain over the whole epigastric region, great impatience of pressure, and enormous distention of the abdomen, violent retching, with the rejection, in several cases, of pure florid blood. The blood was generally small in quantity, but, in a few instances, it amounted to more than a pint. The pulse was feeble. Neither food nor medicine would remain upon the stomach; both were rejected, with extreme aggravation of all the symptoms.

The disorder had already existed during several days, and various methods of treatment had been employed, when Dr. Roget and myself were sent for to Woolwich. We found ten women at least in a state of peril. The remedies used had produced no abatement of the symptoms, which were daily becoming worse in every case. Mercury had not been of the number, the apothecary conceiving that he acted in conformity with our wishes, when he scrupulously withheld it.

Now, although the order of symptoms which we now witnessed, was not precisely identical with any we had formerly seen at the Penitentiary, yet it was impossible not to regard them as partaking of the same nature; and surely, upon the failure of every other remedy, we were not justified in withholding that, which had already rescued life under various circumstances of peril belonging to kindred diseases. Accordingly, we resorted to calomel and opium. Five grains of calomel and one grain of opium were first administered to several patients; but there followed, to our disappointment, an aggravation of all the symptoms. The retching, and vomiting, and spasmodic

pain were prolonged through the night. Yet it was not the remedy itself, but the largeness of the dose, that produced the disagreement; for when one grain, or two grains of calomel, and a fourth, or half of a grain of opium, were given every second, or every third hour, they were retained, and the stomach was tranquillized, and there followed a gradual abatement of the symptoms. Still all the symptoms, and especially the pain, were not finally removed, until the gums were perceptibly sore.

This affection of the stomach was different from that already described as the concomitant of fever. It did not tend to a critical termination, and its relief, although it was procured by the same remedy, was brought about in a different way; not by its producing a discharge of morbid secretions, but (as far as we could judge) by its specific influence upon the constitution.

But what was the precise condition of the parts to which the symptoms were expressly referred in these cases? Fortunately, we had not the opportunity afforded us of determining, by dissection, what it was. But whatever it might be, undoubtedly it was such as constituted an acute disease, tending to the destruction of the individual, if its course had not been arrested. From the symptoms, and from the method of treatment which procured their relief, and from our general experience of what morbid anatomy has disclosed, under circumstances nearly the same, perhaps we should not be far wrong in concluding, that the seat of the disease was the blood-vessels of the stomach, or of the upper part of the intestinal canal, and that their sudden and undue repletion was the condition which constituted the disease itself.

In the course of ten days, this form of disease, which has been described, attacked nearly half the women on board the Narcissus in succession, and there was a continual apprehension that it would prove fatal to some. In the meantime, almost all on board the same vessel, who were not afflicted after this manner, suffered either some form of bowel complaint, or some affection of the brain or nervous system, such as formerly prevailed at Millbank. Thus there was hardly an individual who escaped an attack of severe illness, but none died.

It was melancholy to remark, when their severer sufferings had terminated, the sad contrast which the condition of these poor women now bore to what it had been a month ago. It

was not a long time since the Narcissus was so conspicuously healthy, that it furnished an argument for the removal of the women from the Regent's Park to Woolwich. But now, from the relapse of former symptoms, and the accession of new ones; from the relics of old disease, and from disease, either set up afresh or ingrafted upon the old, a state of apparent health had, in the space of a month, given place to emaciation and weakness, and to that peculiar look of distress and exhaustion which belongs to those who have just, with difficulty, surmounted an acute disease. There was no one peculiar cause to which these late attacks could be confidently ascribed. Yet there were many causes inseparable from their condition as prisoners, from which they might possibly come. Therefore, as long as they remained in that condition, there was no security against the return of the same or similar forms of disease. And in such an event, if it soon occurred, there was every probability that many would perish; for the same individuals had not strength enough remaining to support it.

At the same time, the women brought from the Regent's Park, and put on board the Heroine, at the end of January, had not found that immediate benefit which all the other prisoners experienced upon their removal. Moreover, many severe cases of relapsed bowel complaints had occurred among them since their arrival at Woolwich.

Deplorable, indeed, was the condition of all these wretched women, both on board the Narcissus and the Heroine in respect of their bodily sufferings, but more deplorable still (if possible) in respect of their mental misery. It was painful to witness the manner in which they all abandoned themselves to despair, and those especially who had still before them the prospect of a long captivity. Every day brought some fresh proof how great was the influence of mental distress in augmenting bodily pain and sickness. Whatever circumstances were calculated to make a strong impression upon the spirits, threw them back at once from a state of convalescence, into absolute disease. I will mention one such circumstance, as an example.

Upon the recommendation of the committee, individuals were pardoned, from time to time, for good conduct. Recently, claims for such recommendation had been more easily admitted, and consequently pardons had become very numerous. Hence

the prisoners themselves naturally began to think that the claims of all were nearly equal, and everyone pleased herself with believing that she should be the next who would be set at liberty. Whenever, therefore, an individual was pardoned, all the rest were thrown into an agony of the bitterest disappointment, and were, at the same time, overtaken by disease. It was not a mere nervous or hysterical ailment, but some actual form of real disease, such as they had before suffered, and requiring the strictest medical treatment for its relief.

Now it was evident that every measure had been resorted to for their benefit which was compatible with the condition of prisoners. But there was reason to believe that causes inseparable from that condition still prevented their recovery. As to those who had been set at liberty, it can hardly be expected that much accurate information could be obtained concerning them after they left the prison. Several, however, had presented themselves at the Penitentiary since their enlargement, and it appeared certain that they had enjoyed more uninterrupted health and a longer exemption from disease than those who had remained in confinement. Several, also, who had been pardoned upon the representation of the physicians, that their enlargement afforded the only probable means of saving their lives, had, after the lapse of a few weeks, appeared in perfect health.

With such facts before us, it became a question whether the same representation which had at different times been made in favour of a few ought not now to be extended to all. After witnessing the nature of the late severe attacks, the numbers they involved, and the consequences they left behind, it did appear to us that the only measure which remained was to set at liberty all the female prisoners without exception, for the sake of preserving their lives. The course of their sufferings during more than twelve months might be thought justly equivalent to many years' imprisonment; at least, the humanity of Mr. Secretary Peel led him to adopt this opinion, and to admit the one in compensation for the other, when he recommended them all to the mercy of the Crown for pardon.

Some delay, however, was requisite before this act of grace could be carried into execution. It was easy to set these poor women at liberty, but care was to be taken that, being set at

liberty, they were not left destitute. As soon, therefore, as Mr. Peel made known his intention, the committee undertook the laborious duty of corresponding with the friends of the prisoners in every part of the kingdom. Thus each was pardoned, as it was ascertained that they had friends willing to receive them, until, finally, they were all pardoned. The last left the Penitentiary hulks on the 18th of June, 1824; and the committee had the satisfaction of knowing that there was not one who had not a place of protection to go to immediately upon her dismissal.

Of the men, two died consumptive, one in October, the other in January; otherwise, no formidable disease arose among them after their removal to Woolwich.

They were still liable to occasional attacks of diarrhœa, but it was probable that every individual who thus continued to suffer only required to be put under circumstances a little more favourable to health to secure his complete recovery. Although they were on board hulks, they were still Penitentiary prisoners, and, unfortunately, not amenable to hulk discipline. They could not, therefore, be taken on shore to work, but were necessarily allowed to remain in perfect idleness on ship-board—a state which had already led to alarming acts of tumult and violence. What, then, was to be done? There were serious objections to letting 400 men, who had been guilty of felonious acts, at once loose upon the public. Yet it was impossible they should continue where they were. It was determined, therefore, to transfer all the male prisoners to the hulk establishment; and, accordingly, an Act of Parliament was obtained on the 12th of April, 1824, to give Government the power of doing so. We hoped that, by being distributed among the various hulks, and subjected to active occupations, under their new discipline they would be placed in a condition more favourable to the recovery of their health. Some were sent to Sheerness, and some to Chatham, and some remained at Woolwich, being transferred to the hulk establishment there.

The gentlemen who have the medical superintendence of the hulks, in these several situations, have favoured me with answers to inquiries, which I lately took the liberty of addressing to them, respecting the health of the Penitentiary prisoners under their charge. These are all of the most satisfactory

kind. Mr. Bayles, of Woolwich, writes, on the 1st of November last, "that from the time the Penitentiary prisoners were incorporated with the other convicts, they have worked on shore at whatever kind of labour they were ordered to, and have continued in good health." Mr. Hope, of Chatham, on the 19th November, writes, "that the convicts from the Penitentiary were mingled and employed with the other labourers, and that he has not had complaints made to him by them more frequently than by those received from other places." Mr. Robertson, of Sheerness, on the 19th of November, informs me, that only three or four boys came from the Penitentiary to the hulk under his superintendence, none of whom had ever been under his care for illness." And Mr. Cullen, of Sheerness, on the 30th of November, says, "that the general state of health of the prisoners, lately received from the Penitentiary, did not appear to be different from that of the others, who all enjoy a tolerable share of good health, considering all circumstances."

In this manner were the prisoners of the Penitentiary all finally disposed of. It would be interesting to know the present state of many, and especially of the women, who were set at liberty from a conviction on our part, that their condition as prisoners presented some insurmountable obstacle to their perfect recovery. But such information could not now be easily obtained. A few, whom Dr. Roget and myself have casually met, had the appearance of health, but were not exempt from occasional attacks of diarrhœa.

CHAPTER IX.

ORIGIN OF THE DISEASE.

THE same course which I followed in my description of the disease prevalent at the General Penitentiary, I shall still pursue in my inquiry concerning its probable origin, stating the circumstances connected with the question, according to the order in which they presented themselves to my own observation.

Dr. Roget and myself, having observed the disease of the Penitentiary during a month, and having witnessed its apparent cure by the simplest remedies, and being then unable to trace it back to a period more remote than the preceding autumn, did, in our Report of the 5th of April, 1823, ascribe its origin to the conjoint influence of an impoverished diet and a severe and protracted winter. We considered, moreover, that there was sufficient evidence of these being its *exclusive* causes; and we had the satisfaction to find, that the opinion of many eminent medical men coincided with our own.

The reasons assigned by us, and adopted by others, as conclusive at the time, are stated at length in the Report.

But the disease returned; and our further observation of its nature, and our subsequent acquaintance with new facts connected with it, convinced us that our first opinion respecting its origin was not tenable in its fullest extent. As to the nature of the disease itself, when we looked back upon its progress during many months, and considered its various forms, its duration, and its intractability by ordinary remedies, we could not help acknowledging it to be contrary to experience, that such a disease should derive itself *exclusively* from the causes to which it had been imputed.

Cold, and scanty nourishment, are among the causes of disease with which medical men are best acquainted. Among the poor of large towns, they are known to be productive of complaints at all times, and in seasons of scarcity, and during winters of long and severe frost, being then applied in their

most intense degree, they are even capable of engendering epidemic diseases. But at the Penitentiary, they were certainly not applied in their most intense degree. The winter, indeed, was cold, but the prison afforded a protection against its inclemency. The diet was defective in animal matter, but it was wholesome in its kind. Still the winter being such as it was, and the diet not a nutritious diet, might well be supposed capable of producing some disease, but not a disease which should reign like a pestilence within the prison, and require all the prisoners to be removed for the sake of preserving their lives. They might, in short, be thought capable of producing such a disease as that of the Penitentiary seemed to be in the month of March, but not such as our subsequent experience found it to be.

In the month of March, not only was the disease such as the alleged causes seemed capable of producing, but its history and all the circumstances connected with it (all, at least, which had then come to our knowledge), apparently excluded every other cause, and pointed directly to these, and to these alone. In its subsequent progress, however, the nature of the disease itself made us first dissatisfied with our original opinion respecting its exclusive causes, and the gradual disclosure of new facts afterwards convinced us that others were actually engaged, both in its production and continuance.

In our Report of the 3rd of July, 1823, we intimated to the committee, that there was some cause in operation, over and above those to which we had originally imputed the disease; and that there was a suspicion of contagion, and a suspicion, moreover, of an injurious influence peculiar to the place.

I will state all the facts and circumstances within my knowledge, which seem to bear upon the two questions, in order that medical men may be able to form their own opinion concerning them, keeping one question distinct from the other.

I will speak first respecting contagion. At the date of our first Report, April 5, 1823, the disease had been found exclusively among the prisoners. Subsequently, however, several officers of the establishment were affected with it, and those especially who were in frequent intercourse with the sick. Twelve male and six female officers suffered the same forms of disease with the prisoners themselves. The chaplain, also, and

various individuals of his family, residing within the walls of the Penitentiary, had the disease in the form of bowel complaint, himself (as he believed) in consequence of his attendance upon the sick prisoners, and his family (as he believed) in consequence of a female, selected from among the prisoners to become his servant, being received into his house, before she had entirely recovered from the disease, which she had suffered in the form of bowel complaint, and a slight scurvy.

Further, prisoners recently admitted into the Penitentiary, and not obnoxious to the influence of the causes to which the disease was originally imputed, were nevertheless not exempt from its attacks. The diet was changed on the 1st of March, and fresh convicts had been received into the Penitentiary during the latter part of February. These certainly were not subjected to the diet then in use for a period sufficiently long to have derived any possible injury from it. The winter, too, was now past, and fresh convicts were still sent to the Penitentiary until the month of June. From the 16th of February to June, 132 prisoners were admitted, namely, 127 males, and five females. Of these 103 suffered the disease, namely, ninety-eight males and five females. There was no difference in the symptoms of their disease to distinguish it from that of those who had been longer in confinement; and, as far as the testimony of one dissection goes, there was no difference in the internal morbid conditions.

John Lampard, aged 29, was admitted into the Penitentiary on the 25th of March. He had not been there more than three weeks when he was seized with violent pains in the head. To these fever succeeded. The fever declined into a slow hectic, in the course of which dysentery arose, with evacuations of slime and blood. The dysentery was mitigated, but the fever continued, with delirium. Lastly, abscesses began to form in the neck about the angles of the jaws, and a purulent discharge took place from the ears. Thus he lingered until the 15th of June, when he died.

Upon dissection, there were found vascular patches in various parts of the intestines, ulcers of the large bowel, and here and there spots of ecchymosis. The brain and its membranes appeared free from disease.

These are the facts which led Dr. Roget and myself, in the

month of July, to express our suspicion to the committee, that the disease had become contagious, and to recommend the practical measures which such a suspicion would naturally suggest. These facts were regarded in the same light by the other physicians who were afterwards associated with us; for when, on the 27th of September, this question was formally proposed to us by the committee, "Are the physicians of opinion that the disease is contagious?" we returned the following answer conjointly:—"There are grounds for believing that the disease has become contagious; the evidence may not be of a nature positively to establish the fact; but, practically, we feel it imperative upon us to act upon the presumption that it is contagious."

After the removal of the prisoners to Woolwich and the Regent's Park, certain circumstances occurred which seemed still further to establish the prevalence of contagion; and the physicians (I fear) were thought rather perverse in not considering them quite so conclusive of the question as they were regarded by others.

First. Three persons employed at the Ophthalmic Hospital, who had not been at Millbank, suffered disorder of the bowels, under one of the forms in which it had prevailed among the prisoners, that of cholera morbus. But it was at a time of the year when cholera morbus was everywhere a frequent complaint. These three cases, therefore, could not be allowed to contribute much weight towards the proof of contagion.

Secondly. The female prisoners on board the Narcissus had been convalescent during several weeks; when the females from the Regent's Park, of whom several had recently suffered renewed attacks of their disorder, were removed to Woolwich, and put on board the Heroine. Both vessels were moored close to each other, and some women from the Heroine, for the sake of better accommodation, were transferred to the Narcissus. Not many days afterwards, there was a general complaint of illness on board the latter vessel, which terminated in that form of disease which has been described, occurring in the month of March; and thence it was concluded that the disease was communicated by the prisoners of the Heroine to the prisoners of the Narcissus, and that another proof was obtained of its contagious nature.

Now, if this were the fact, the disease was not only contagious, but contagious in a manner peculiar to itself. For it has never been shown (as far as I am informed) that the convalescents from any disease are capable of re-infection by those who are the more recent subjects of it.

I am not disposed, therefore, to believe that the prisoners of the Narcissus, in this instance of their sudden disorder, were re-infected by those of the Heroine, because the fact itself would be contrary to general experience. And, further, I am not disposed to believe it, because transitions from a state of convalescence to a state of disorder had been just as sudden among the men on board the Dromedary and the Ethalion, when there was no possibility that it could have been derived from contagion, unless you suppose the same people capable of receiving and communicating the same complaint, from and to each other, indefinitely.

Thirdly. When the women were put on board the Narcissus, a waterman was kept in constant employment communicating between the vessel and the shore. This man had a severe attack of dysentery.

If there had been many instances of this kind; if it had been notorious that, of the many individuals who in fact went on board the Penitentiary hulks, such as friends, workmen, &c., several had suffered the same symptoms with the prisoners themselves, then there would have been evidence enough that the disease was contagious after the removal from Millbank. But this was not the case, and the single instance of the waterman surely cannot justify this conclusion. Upon the whole, then, no other circumstances can be fairly admitted in proof of the contagious nature of the disease but those which occurred within the Penitentiary itself. Why the evidence which they furnish "was not of a nature positively to establish the fact," will plainly appear, when we have considered the question of an injurious influence peculiar to the place.

I proceed next to state the facts and circumstances within my own knowledge which bear upon the question of a noxious influence peculiar to the place. The following Report, presented to the committee of the General Penitentiary on the 11th of October, 1823, contains some of the most important. But before it is given, I think it necessary to state, concerning

the day-books which are there mentioned, and to which frequent allusion will be made in the discussion of the present question, that we had no knowledge whatever of their existence until they were voluntarily produced by the apothecary himself a few weeks before the date of the Report. The apothecary either forgot that he had them in his possession, or he did not think them capable of furnishing so much important information, and therefore did not mention their existence.

TO THE COMMITTEE OF THE GENERAL PENITENTIARY.

11*th October*, 1823.

GENTLEMEN,

THE severity of the disease prevalent at the Penitentiary having greatly abated within the last few weeks, we have had more time to turn our attention from the immediate care of the sick, and to make further inquiry into the origin and progress of the epidemic. We beg to lay before the committee all the details of our investigations.

We have perused all the written communications of the medical officers to the committee, from June, 1820, when such communications were first regularly made, down to the present time.

These communications consist of Reports made quarterly by the medical superintendent and the apothecary; of Reports made monthly by the apothecary; and of numerous special Reports made by one or other, or by both of them, at various times and at uncertain intervals.

The quarterly and monthly Reports contain observations upon the state of health of all the prisoners in the Penitentiary; the condition of those in the prison at large, ascertained at their general inspection, as well as the condition of the sick in the infirmaries, who were under constant observation and care.

The special Reports consist of returns made by the medical officers, of all deaths that occurred, of notices of the diseases that proved fatal, also of answers to inquiries respecting the health of individual prisoners; and they contain, moreover, various requests, suggestions, and observations, relating to circumstances connected with their department.

In these communications we find the healthy state of the Penitentiary announced after a general inspection in June, 1820, and constantly and uniformly confirmed after every quarterly and monthly general inspection, down to the commencement of the present year; we find its exemption from disease again and again insisted upon, as something striking and peculiar; and we also find comparisons drawn between the health of those in common life and the health of the prisoners in the Penitentiary, and conclusions deduced to the advantage of the latter.

Nevertheless, diseases did occur from time to time in the Penitentiary; but we discover in these communications no opinion expressed by the medical officers, that any one disease was predominant, or any one disease of a peculiar character.

Throughout these communications, the only notices of disease resembling that which has lately prevailed in the Penitentiary, are the following:—

In a Report after a general inspection, dated October 2nd, 1820, among forty-seven cases of various diseases then in the infirmary, five cases of diarrhœa are mentioned without further comment. No trace of this disease is afterwards found, until a Report, after a general inspection, dated November 2nd, 1821, a single case of diarrhœa is mentioned, which proved fatal, but without comment. Again, no trace of this disease is met with, until in a Report, after a general inspection, dated January 6th, 1822, one dangerous case of dysentery is mentioned; and again, in a Report, after a general inspection, dated February 2nd, 1822, one fatal case of diarrhœa. Here the dangerous case of dysentery in one month, and the fatal case of diarrhœa succeeding, refer to the same individual.

No allusion to the disease is afterwards made, until in a Report, after a general inspection, dated June 4th, 1822, two fatal cases of diarrhœa are mentioned; and among the dangerous cases then in the infirmary, one of diarrhœa is noticed, which, in a special Report made the next day, is said to have proved fatal.

Afterwards, in a Report after a general inspection, dated July 1st, 1822, one dangerous case of diarrhœa is mentioned, which terminated fatally, as we learn from a special Report on the following day. From July, 1822, to January, 1823, there is no allusion made to a single case of diarrhœa, when in a Report, dated the 10th of the latter month, "a few more cases of diarrhœa" are spoken of; in the following month, viz. February, scurvy and flux are said to be gaining ground, and in the beginning of March, these two diseases pervaded the whole prison.

Thus, in these authentic records of the health of the Penitentiary, regularly drawn up by the medical officers, and regularly presented to the committee for their information, we can only find, during the period of two years and eight months which immediately preceded the declared existence of the epidemic, eleven cases of any diseases at all similar to that epidemic, in its character and symptoms. Of these eleven cases, six proved fatal.

From these fatal cases, however, occurring as they did at periods remote from each other, or indeed from all of them, mentioned as they were at the time of their occurrence, without any special comment by the medical men who observed and treated them, we should not now be justified in drawing any inference. As far, then, as any information can be obtained from these documents, we should still attribute this disease which has prevailed in the Penitentiary, entirely and exclusively to the influence of diet, and to a severe and protracted winter.

But we have thought it our duty to have recourse to other documents, and we have extracted from them facts of unquestionable importance, which were hitherto unsuspected by the medical men who have watched the course of all the diseases that have occurred in the Penitentiary since its first establishment.

We have examined the apothecary's day-book, and every paper upon which any record has been preserved, of medicines ordered for the sick,

from the year 1816 to the present time, including every prescription for the severer ailments of those in the infirmary, and for the complaints of those who were still well enough to pursue their ordinary occupations in the prison. We have also examined the bills of charges for different kinds of medicines that have been furnished to the Penitentiary within the same period.

By the help of these documents, and inferring, as far as we could with safety, the nature of disease from the medicines that have been procured, and the kind of remedies prescribed for individual cases, we have endeavoured to form the most reasonable conjecture, whether any, and what disease has been predominant in the Penitentiary before the last twelvemonth ; and if any, whether it has been at all similar to the epidemic that has recently prevailed there.

The following Table gives the number of prisoners in the Penitentiary every year since the year 1816, with the number of cases treated as diarrhœa in every year :—

	*1816.	1817.	1818.	1819.	1820.	1821.	1822.
Prisoners.....	72	212	246	351	609	798	866
Cases of diarrhœa	23	104	106	82	85	87	88

We are aware that inferences concerning the nature of a disease deduced from the remedies employed for its cure would, in general, be hasty and inconclusive. But as in the present case, the nature of the disease is unequivocally indicated by the particular remedies used, we may, with certainty, conclude that diarrhœa has existed in the Penitentiary from its first establishment, and that it has prevailed in various degrees of extent at different periods ; that, proportionably to the number of prisoners, it prevailed to the greatest degree during the year immediately after its establishment, and that it has prevailed in a less and less degree each succeeding year, down to the period when the present epidemic was discovered.

From the same documents we discover certain peculiarities belonging to this disorder, peculiarities which become more and more remarkable in each succeeding year (even although the numbers decrease), evidently distinguishing it from the disorder of the same name, which proceeds from common and accidental causes.

Common diarrhœa is easily curable, and by the simplest means ; and in constitutions otherwise healthy, we are not aware that it is at all liable to recur habitually.

But this disorder (we find) did not readily yield to the methods of

* The numbers in this year refer to a period of six months only.

treatment employed. These records show how pertinacious and intractable it was in many whom it attacked. The same prisoners were again and again brought under medical treatment for it in the same year. Many of the patients of one year are found to have been the patients of the preceding year, and as the period becomes more and more remote from the first establishment of the Penitentiary, we find prisoners still suffering diarrhœa, who had already endured it one, two, three, or four years.

The following Table gives the gross number of cases treated as diarrhœa in every year, and also the number of cases continued from preceding years to the succeeding, the latter being included in the gross amount. It furnishes also the number of new cases in every year, by subtracting from the gross amount of each year the cases continued from preceding years:—

	1817.	1818.	1819.	1820.	1821.	1822.
Gross number of cases of diarrhœa in each year ...	104	106	82	85	87	88
Number continued from preceding year	11	32	31	23	20	17
Number of new cases in each year	93	74	51	62	67	71

The following Tables show how far back each case of diarrhœa that was continued from one year to another can be traced:—

In 1817 : of 104 cases, 11 are traced back to — to 1816

In 1818 : of 106 „ 32 „
- 26 to 1817
- 6 to 1817 & 1816

In 1819 : of 82 „ 31 „
- 18 to 1818
- 13 to 1818 & 1817

In 1820 : of 85 „ 23 „
- 12 to 1819
- 6 to 1819 & 1818
- 4 to 1819 & 1818 & 1817
- 1 to 1817 & 1816

In 1821 : of 87 „ 20 „
- 7 to 1820
- 4 to 1820 & 1819
- 5 to 1820 & 1819 & 1818
- 1 to 1819 & 1818
- 1 to 1818
- 1 to 1818 & 1817
- 1 to 1817 & 1816

In 1822 : of 88 cases, 17 are traced back to 8 to 1821
4 to 1821 & 1820
2 to 1821 & 1820 & 1819
2 to 1820
1 to 1821 & 1820 & 1819 & 1818.

Upon the whole, we think that the facts adduced, warrant us in concluding:—

That a disorder of the bowels, of a peculiar nature, at all times difficult of cure, and of the same general character with that which has constituted the late epidemic, has prevailed in the Penitentiary ever since its establishment ; but that until the commencement of the present year it became gradually more and more limited in its extent, and that although it has been always difficult of cure, it has not upon the whole been attended with much hazard to life, until the breaking out of the late epidemic.

Such are the details of our investigations, the facts they have disclosed, and such the conclusions to which they have conducted us ; but as the existence of any local influence productive of disease can only be presumed from certain effects, it is also only from these same effects that the degree and sphere of its activity can be estimated. Whatever noxious influence peculiar to the Penitentiary may be suspected to exist, this influence must have abated of its activity in proportion as the disease became more limited ; that is, as the period was more remote from the first establishment of the prison. It was, therefore (as everything seems to testify), when this disorder was reduced within narrower limits than at any former period, that suddenly the same disorder became much more extensively prevalent than it had ever been, that it assumed the form of an epidemic, and went far beyond its former character, in the severity of its symptoms, and in its fatal consequences.

Our belief is, that but for the change of diet and the severe and protracted winter, the disease never would have assumed the form of an epidemic. The universal debility produced by these causes, rendered the prisoners more obnoxious to an influence which, as far as we can judge, had become less powerful in itself for the production of disease.

(Signed) P. M. Latham, M.D.
P. M. Roget, M.D.
Clem. Hue, M.D.
Wm. Macmichael, M.D.
H. H. Southey, M.D.

This Report, being intended solely for the information of the committee, was restricted to a plain statement of such facts as were deemed capable of being understood, without explanation, by unprofessional persons, and a plain statement of the inference to which they seemed unavoidably to lead. It re-

presents that, from the first establishment of the Penitentiary to the present time, there had been a very large prescription of such remedies as could only have been given for a disorder of the same general character with that which had lately prevailed there; that a disorder of this character must always have existed *there* to require that remedy, and that some cause must always have existed *there* to produce this disorder.

The remedies were designated "mist. cretæ," and "pulv. cretæ." This "mist. cretæ," we understood to be a compound medicine, consisting of chalk mixture, aromatic confection, tincture of calumba, and laudanum. The "pulv. cretæ," we understood was the pulv. cretæ comp. c. opio.

Now medical men will acknowledge, that there is no species of medicine from which the character of any disease can be so certainly inferred, as a flux of the bowels from chalk mixture or chalk powder with opium. From a large prescription of bark, we could not have presumed the existence of ague, nor from the extensive employment of sulphur or of mercury, could we have safely inferred the prevalence of itch, or of syphilis. Bark, and sulphur, and mercury, are continually administered for other complaints besides those, for which they are the reputed specifics. But chalk mixture, with the other ingredients which have been mentioned, could only have been employed for a bowel complaint which had flux for its prominent symptom. If there could be any doubt of the fact in the present instance, it was removed by the testimony of Mr. Pratt himself, who prescribed the remedy, in whose examinations before a committee of the House of Commons, on the 4th of March last, are found this question and answer:—

> Question—Do you give the chalk mixture for any disorder but diarrhœa?
> Answer—No!!

We were in possession of other circumstances tending to establish the same conclusions, which might have been introduced into the Report, but which were purposely withheld, because they were of a nature which medical men alone could justly appreciate. At least they could not have been made intelligible to others without certain explanations, which would have been inconsistent with a simple statement.

These circumstances, however, must now be added, that

medical men (whom I consider myself now addressing) may give them the weight to which they are entitled.

Long before the books were brought to light, which furnished the facts upon which the Report is grounded, we were morally convinced (as all physicians must have been) that complaints, which had flux of the bowels for their prominent symptom, had been frequent in the Penitentiary during former years; and this conviction was founded upon certain recorded statements of the apothecary. In the minutes of evidence before a committee of the House of Commons in the session of 1823, we found that Mr. Pratt, in his examination, had referred to a particular letter of his written in March, 1822, when a change of diet was contemplated at the Penitentiary. The letter contained a prediction that, in the event of the change contemplated being carried into effect, those disorders, which actually did take place, would be the consequence. This warning was not forgotten when the evil became manifest. Mr. Pratt was acknowledged by all to have predicted truly, and allowed considerable credit. He was himself accustomed to refer us to this prophecy and its verification, not without some exultation; and indeed, well he might; for if, without any help or suggestion from what had already occurred, he really foresaw, not that some disease or other, but expressly the very disease would follow the change of diet, which actually did follow, it is one of the most splendid instances of medical anticipation upon record. Nevertheless we were slow to allow him more than the credit due to a sensible man, rightly conjecturing what would take place hereafter, from what had heretofore fallen under his own observation. In short, we were quite certain, that the prophecy could have had no other foundation than in the *bonâ fide* experience of the prophet.

But convinced (as we were) of the fact, from the course in which things are accustomed to happen in our own profession, we could not insist upon it until some proof could be obtained, which would be convincing also to others. We continued, therefore, in search of this proof, suppressing still our own persuasion, until we had discovered it.

At length the books in question came to light; and in them it appeared, by the testimony of Mr. Pratt's own handwriting, that at the very time he was prophesying to the committee,

that certain diseases would take place upon their projected change of diet, he had already been prescribing most largely for those very diseases ever since the foundation of the prison, for six years in succession.

Knowing that bowel complaints always had been prevalent in the Penitentiary, he came to the obvious and just conclusion, that they would become still more so, if the diet was rendered less nutritive than it was. Whatever would debilitate must render the prisoners more obnoxious to disease generally, and especially to those complaints to which they had hitherto always been liable.

With us, then, the facts stated in the Report only came in confirmation of a belief which circumstances had already led us to entertain, and which, as physicians, we must still have entertained, whether those facts had been brought to light or not. These circumstances, taken alone, were not calculated (we knew) to conduct others to the same belief. But being added, as they now are, to the facts of the Report, it will be at once seen how much they strengthen its conclusion.

Another circumstance deserves to be mentioned, which gave additional weight to the same conclusion. Before the books in question were produced, we had no difficulty in seeing how the apothecary came to foretell the disease of the Penitentiary just as it came to pass. But we had the greatest difficulty in conceiving how he alone should happen to foretell it, and the medical superintendent have no such anticipation. His books, however, soon cleared up this perplexity. For in perusing them we found, that the apothecary alone was *fully* acquainted with the facts upon which the anticipation was grounded, and therefore that he alone could confidently entertain it. The prisoners for whom chalk mixture, &c., was so largely employed were, for the most part, under Mr. Pratt's exclusive care. More than half the number had medicine given to them while they were following their ordinary occupations in the prison. The functions of the medical superintendent and the apothecary were so far divided, that while the daily business of the former was only with the prisoners in the infirmaries, in prescribing for their severer maladies, that of the latter was moreover with the prisoners in the Penitentiary at large, in prescribing for various ailments which did not require their

removal into the infirmaries. Further, the medical superintendent had nothing to do with the prison, except to make a general inspection of it, accompanied by the apothecary, once a month, when it is probable that the numbers taking chalk mixture were not so regularly reported to him, as to make him sensible how constant and extensive the prevalence of diarrhœa had been within its walls.

There remains yet another circumstance to be insisted upon, which bears upon the question of a noxious influence peculiar to the place, and strongly confirms the conclusion of the Report.

From the description of the disease lately prevalent at the Penitentiary, it has been already seen that an affection of the brain and nervous system formed as much a part of that disease as did the bowel complaints themselves; that it was co-extensive with the bowel complaints, and consisted, for the most part, of head-ach and vertiginous sensations, which, in many instances, were aggravated into tremors, convulsions, and phrenzy.

By help of the apothecary's day-books, we were enabled to show that one part of the disease (the bowel complaints) had prevailed extensively in the Penitentiary since its first establishment. We could, moreover, have shown from the same books that another part of the disease (the affection of the brain and nervous system) had prevailed there almost co-extensively, and during the same period. This latter fact (I say) was also derived from the day-books; not, however, from the books alone, but from them coupled with the explanatory testimony of the apothecary himself.

We needed nothing but the record before us of the medicines prescribed to ascertain the prevalence of bowel complaints in the Penitentiary since its foundation; but we were indebted to the *voluntary* suggestion of Mr. Pratt for enabling us to trace out retrospectively head-ach and vertigo through every page of his own books. So confident was he, from a knowledge of his own methods of prescribing, that certain remedies there recorded were given by him for head-ach and vertigo, that he undertook to draw up, and actually did draw up, a list of the numbers afflicted with these disorders from the establishment of the Penitentiary to the present time. These numbers were almost as great as of those who suffered the bowel complaints

in every year. They were arranged in a tabular form, and were to have made part of the Report presented to the committee. But, upon deliberation, it was thought better not to offer anything to the committee which was not self-evident, or which stood in need of explanation beyond the mere statement of the fact.

Chalk mixture and tincture of opium could only have been prescribed for a flux of the bowels, and therefore unequivocally denoted the disease. But emetics might have been prescribed for various other complaints besides head-ach and vertigo, and therefore could not be shown to denote their existence in the present instance without further explanation, and without the express testimony of the prescriber himself.

Now, concerning the circumstance last stated, it is too little to say, that it merely confirms the inference of the Report—it does more. If you admit as evidence that only which the books, upon the very face of them, unquestionably prove, you must conclude that a disorder *of the same general character* with the late epidemic has prevailed in the prison since its establishment; and we went no further in our Report. But if, moreover, you admit the explanatory testimony of the apothecary, you must go near to allow, that the disorder which so prevailed there was *identical with* the late epidemic.

I wish to add a few remarks concerning the books in question, as bearing testimony to the existence of *flux alone* within the Penitentiary, and concerning the use we have made of them for that purpose. It was at first our intention to have taken the books themselves to the committee, and to have turned over the leaves in their presence, pointing out, in one page after another, and frequently in the same page, the abbreviated marks which signified the compound chalk mixture and the compound chalk powder; to have stated that such medicine could only have been given for disorders whose prominent symptom was flux of the bowels, and then to have left the committee to draw their own inference. But it was thought more respectful to present a formal Report, and it was foreseen that such a Report would be required for the information of Government. Accordingly, in endeavouring so to frame it, as to convey the most accurate notion of the extent to which flux of the bowels had prevailed in the Penitentiary, we could

conceive no better method than that of the numerical Tables which have been given. Nevertheless, these mere figures do not convey so much as the books themselves. The *form* of certain entries which appeared there gave perhaps a more certain assurance that diarrhœa was the predominant complaint of the prison than any that could have been derived merely from their numbers.

Among frequent entries of medicine delivered to individuals by name, there were occasional entries of chalk mixture, sent by quarts and half gallons, to companies of prisoners working together at their various employments. Thus we find, without any specification of the individuals who were to take the medicine—

In 1816,	an entry of	1	quart bottle of mixture for	the kitchen women.
„	„	1	„	Mrs. Clarke's women.
In 1817,	„	2	„	Mrs. Clarke's women.
„	„	1	„	Mrs. Evan's women.
„	„	2	„	the laundry.
„	„	1	„	the carpenter's cell.
In 1818,	„	1	„	Mrs. Croome's women.
„	„	1	„	Mrs. Gould's women.
„	„	1	„	Laban's men.
„	„	3	„	Brett's men.
In 1819,	„	1	„	Mrs. Clarke's women.

Here it is quite evident, that among these several companies there was a predominant disorder requiring to be treated by one and the same remedy, and that, from the nature of the remedy, the disorder was diarrhœa. It is evident, also, that, in each company, the cases of diarrhœa were so numerous that it became needless or impossible to specify the individuals who should take the medicine. The medicine, therefore, was delivered out in large quantities, with general directions (we may presume), that it should be taken by all, or by as many as required it.

These are all the facts and circumstances within my knowledge which bear upon the two questions of contagion and of a noxious influence peculiar to the place, as causes engaged in the production and continuance of the disease prevalent at the Penitentiary.

Now, to revert to the question of contagion, if its existence

and influence be allowed upon the proof of those facts and circumstances which have been mentioned as occurring within the Penitentiary at the time specified, our first opinion concerning the original and exclusive causes of the disease suffers, solely on this account, no necessary contradiction. Those facts and circumstances were of too late a date to allow a belief that contagion had any share in its original production. Impoverished diet with cold, might still be the causes which first engendered the disease, while contagion might be a property superadded to it in the course of its progress, and serve to explain whatever has subsequently appeared in its nature and extent, which these causes alone seem inadequate to account for.

This might be deemed a complete theory of the origin and progress of the disease. But the disclosure of new facts demands the admission of another cause, by which the theory is destroyed.

To revert, then, to the question of a noxious influence peculiar to the place, which is the cause I mean, if it be granted upon the proofs adduced, its existence renders a contagious property in the disease extremely doubtful, and, at the same time, manifestly contradicts our opinion concerning its *original and exclusive* causes. For all the imputed effects of contagion admit equally of explanation, upon the presumption of a local injurious influence. And whatever share an impoverished diet and cold might *ultimately* have in promoting the disease, they could have nothing to do with its origin, since it is traced back to the first establishment of the prison, and found to have prevailed during more than six years prior to that severe winter and that reduction of animal food to which it has been ascribed.

Finally, then, it may be asked, did an impoverished diet with cold, did contagion, and did a noxious influence peculiar to the place, all bear the relation of causes to the disease of the Penitentiary? And, if so, what was the share which belonged to each in its production and continuance?

That an impoverished diet with cold, and that a local noxious influence really bore this relation to the disease, is established (I conceive) by a train of facts and circumstances, which constitute a moral proof as strong as the nature of the subject will admit. And as to the share which each had in its

production and continuance, it is pointed out (I conceive), with great probability of truth, by the physicians, in their Report of the 11th of October. But with respect to contagion, nothing can be stated with confidence. For we cannot pretend to assign to it its proper share in the production and continuance of the disease, while it yet remains very uncertain whether it had any share at all.

In what has been stated concerning the origin of the Penitentiary disease, my purpose has been rather to furnish medical men with the materials of forming an opinion for themselves, than to assert my own. My own opinion, indeed, I might have entirely suppressed, had it not been the same to which four other physicians were led by a contemplation of the same facts. On this account, certainly, I have myself adopted it with more confidence of its truth.

At the same time I am fully aware of all the difficulties which naturally oppose themselves to the satisfactory solution of such questions as the present; and these difficulties are of a kind which it may not be uninstructive briefly to point out.

Where it is admitted that a disease can be derived from one cause only, its origin is easily assigned whenever it occurs. Where there is small-pox, it must come from contagion. But there is the greatest difficulty, where a disease is capable of being produced by several causes, to point out any single cause, or any two or three causes in combination, which have actually produced it at a particular time.

The disease of the Penitentiary is of this latter kind. It has no one proper and specific cause, but may arise from various causes; from heat or cold, from moist or dry, from almost any kind of injurious influence operating immediately upon the body, or upon the body through the mind; from bad diet, from the influence of situation, and perhaps from contagion; and the difficulty of assigning such a disease expressly to any one or two causes consists in this, that you cannot feel confident in their particular operation until you have the best possible reason for the exclusion of all others which are capable of producing it.

Amidst all these difficulties, then, I may well be asked, whether I really feel assured that the disease of the Penitentiary was (as the Report suggests) derived originally and

essentially from something noxious in the situation, and was rendered epidemic by an impoverished diet and a severe winter? and I answer, that I do not pretend to impute it to these causes as punctually and confidently as medical men ascribe small-pox to contagion; for the nature of the thing forbids such absolute certainty; but that, from all the circumstances within my knowledge, I cannot help coming to this as the most probable belief.

I wish to add a few words upon the question of contagion. Whatever may have been the original source of this disease, may not one part of it at least be entirely ascribed to contagion?

The prisoners admitted into the Penitentiary, after the improved diet was adopted and mild weather had returned, were affected with the disease. Of 132 so admitted there were 103 who suffered. In them was it not entirely derived from contagion?

Assuredly it was in contemplation of these cases that, when the question concerning its contagious nature was put to us by the committee, we answered, that it was practically imperative upon us to act as if it were contagious. By which we meant to say, that the facts already known went to such a suspicion of contagion, as to require us in prudence to act upon it, as if it were an ascertained truth; but that, nevertheless, the facts known did not come up to the best proof that could be had of contagion, and, therefore, we, as physicians, could not be so satisfied of this truth as absolutely to pronounce it contagious.

Contagion is a very obscure thing; and so, too, is the noxious influence of situation. They are not only obscure in themselves, but perpetually obscure the operation of each other in the production of disease. We can never be sure of the operation of contagion, except under circumstances which exclude the operation of local influence. Thus it may take ages to settle the question of contagion respecting a particular disease, because it may not be found under circumstances in which local injurious influence is unquestionably excluded. It is a subject of much controversy, at the present day, whether the yellow fever be contagious or not—and for the reasons to which I allude.

Thus with regard to the disease of the Penitentiary, my

colleagues and myself have held the belief of contagion less confidently in proportion as facts have arisen which go nearer to impute an injurious influence to situation.

If I am asked, what would go to the full proof of contagion in the disease of the Penitentiary? I should say nothing less than this; namely, that various prisoners, under the actual symptoms of the disorder, having been set at liberty, various people, with whom they had intercourse in the several situations to which they resorted, had been seized with symptoms precisely the same.

On the 7th of February, 1824, a paper was laid by Mr. Holford before the managing committee of the Penitentiary, entitled "Observations on the Medical Report made by the Physicians, dated 11th October, 1823."

It is, in fact, a formal answer to our Report; is written with great deliberation, and contains (I presume) every objection which can be brought against our conclusions.

Now, I feel myself under great difficulty how to deal with this document. If I make no allusion to it, I may be justly blamed for suppressing whatever tends to recommend an opinion different from my own; while, in noticing it (as I am bound to do), I unavoidably incur the hazard of diminishing the value that might otherwise belong to a simple statement, by the introduction of controversial matter.

The document in question was communicated to the physicians at the time it was laid before the managing committee of the Penitentiary, and we ought (it may be thought) immediately to have presented an answer to it, if any was capable of being given. But we knew (as all medical men must know) how difficult it is to argue upon the subjects of our own profession with unprofessional persons. This was one reason of our silence. Another, and a stronger reason, was our determination never to become a party in any of the controversies around us, and even to avoid, as much as possible, being drawn into disputes with others about our own opinions and practice; and although the respectable quarter from which the "Observations" proceeded might seem to demand some notice, yet we did not think that either our credit, or our usefulness, would be increased by entering into a contest with a member of the

managing committee. Besides, we were aware, that a committee of the House of Commons was about to sit upon the affairs of the Penitentiary, and that we should be individually subjected to examination upon all the points to which the "Observations" relate.*

After recapitulating the conclusion of our Report, the "Observations" continue thus :—

"Before I proceed to inquire how far these Tables prove the prevalence of diarrhœa of any kind, I beg to ask, how they can show that the diarrhœa, for which the medicine alluded to is supposed to have been given, was of a peculiar nature, or had any resemblance to the present disease? Can the exhibition of the chalk mixture or powder prove that this disease was preceded or accompanied by petechial spots or blotches? that it had a dysenteric character? that it was attended with tenesmus? or with the inflation of the lower regions of the abdomen, or the very sudden and violent pains, which were observable in the present disorder? All these symptoms will be negatived by the medical man who gave the medicine; nor was the treatment of the two disorders the same; but we are called upon to infer the existence of a diarrhœa similar to the present one, merely from the supposed use of a medicine, by which the present disorder has not been cured, the physicians having, on the contrary, found themselves obliged to have recourse, for its cure, to the use of mercury, pushed in general to such an extent as to produce salivation."

I presume that the several interrogations which here follow each other in succession are intended to have the force of so many distinct objections, imputing to us that we really did insist, in our Report, that the diarrhœa of former years was of a peculiar nature, and had a resemblance to the present disease; and that, extending our notion of such resemblance to the

* I have been under some embarrassment what method to adopt in replying to this document of Mr. Holford's. One method would have been, without printing the document at length, to state the objections which it contains, and then to give answers to them. But this I could not follow, in consequence of the objections not being put in distinct propositions. Another method would have been, still without printing the document itself, to extract what seemed to me to bear the force of objections, and to put them into distinct propositions in my own language. But this would have had too much the appearance of making out a case for myself to reply to, and might have exposed me to a suspicion of misrepresentation. It only remained, therefore, for me to take the "Observations," paragraph by paragraph, that the original might be at hand to determine whether I have interpreted each objection in its true sense, and have fairly answered them.

minutest particulars, we really did insist, in our Report, that the exhibition of chalk mixture or powder proved the diarrhœa of former years to have been preceded, or accompanied, by petechial spots or blotches; to have had a dysenteric character, and been attended with tenesmus, and inflation and violent pain, like the present disorder.

Whoever will take the trouble of referring to the Report itself, will find that the disorder formerly prevalent in the Penitentiary is there spoken of in the most general terms, and that the name diarrhœa is used in the largest sense, and as synonymous with flux. He will find, moreover, that we strictly abstain from ascribing to it any particular symptoms or accompaniments, and that, in mentioning its peculiarities, we expressly state them to consist, not in its symptoms or accompaniments, but in its difficulty of cure, and liability to recur in the same individuals. Lastly, he will find that all the correspondence we pretend to have discovered between the disorder of the bowels which has recently, and that which has always, prevailed in the Penitentiary, is that they were both "of the same general character." This was a flux, and that was a flux; and as such they were as like each other as one fever is like another, or as diseases commonly are which come under the same generic denomination. In fact, the symptoms brought forward to mark the essential difference of the two diseases might or might not exist without changing their nature, or altering any opinion which might otherwise be formed concerning their origin. They apply to degrees of severity and malignancy, not to the essence of the disease.

It is implied by the argument (otherwise the argument fails altogether) that the signs enumerated were attendant upon the whole course of the late flux, and the petechiæ, the dysenteric character, tenesmus, inflation, and violent and sudden pains were present in every case. Now, the petechiæ, if by them are meant scorbutic spots upon the skin, were, indeed, very general, as long as they lasted, but they lasted only six weeks; and if by them are meant ecchymosed spots in the intestines, they could only be known to exist where they were found, namely, in several who died and were examined after death, although they might, and probably did, exist in many others. With respect to the tenesmus, and the blood (or whatever is meant

by the dysenteric character), and violent pains, they did not occur in more than half the cases. So that, should these symptoms be held to constitute essential distinctions of disease (which is impossible), the late flux was not only different from the former diarrhœa, but was itself a different disease in one-half of the prisoners from what it was in the other half.

The argument from the treatment of the two disorders not being the same, proceeds upon the supposition that diseases of the same general character and denomination are always treated by one and the same remedy; and, further, that their being amenable or not to one and the same remedy, is the test whether they are or are not entitled to such or such a character, and to such or such a name. The supposition is not unreasonable in itself, and very likely to occur to any unprofessional person. But physicians (I fear) must admit that the present state of their knowledge will hardly enable them to arrive at an axiom which presumes so precise an insight into the essence of diseases, and the operation of medicines. Although fevers, and influenzas, and erysipelas require to be treated at different times by different, or even opposite, methods, in the same place and in the same individuals, yet physicians still talk of fevers, and influenzas, and erysipelas, and still discern a certain conformity of character in each, whenever it occurs, by which they are entitled to the same names under all circumstances. Granting, therefore, that the bowel complaints of the Penitentiary were, during six years and a-half, treated with chalk mixture, and subsequently were treated with mercury, we did not (I presume to think) greatly err, either in conceiving them always to have borne the same general character, or in calling them by the same generic name, *the chalk mixture and the mercury notwithstanding.*

Medical men will hardly pardon me for dwelling so long upon these observations; but I have been led to do so from recollecting the impression they made upon those to whom they were originally addressed.

Mr. Holford next proceeds thus. "I deny, however, that the liability of the prisoners to diarrhœa of any kind can be estimated by the quantity of medicine administered for that disorder in the prison.

"The first thing which it occurs to a prisoner in any prison to say, when he wishes to feign illness, is, that he has a pain in his bowels; and when-

ever such a complaint has been mentioned in the Penitentiary, the party has had the chalk mixture or powder, of course, without any previous investigation into the reality of the disease, as the surgeon will testify. Every prisoner, therefore, who has pretended to have a pain in his bowels as an excuse for not doing work enough, or from a wish to have a few days' enjoyment of better food, or of ease and idleness in the infirmary, or from a desire to miss chapel, or plague his turnkey to let him out of his cell to the privy in his ward oftener than was convenient, or from any other cause, even though he may have been discovered afterwards to have been shamming, is included in these Tables as a case of diarrhœa, his name appearing on the surgeon's books among those who had taken chalk; and I believe the medical gentlemen who drew the Report may safely be appealed to as witnesses for this fact, viz. that the class of cases of feigned illness is not a small one in the Penitentiary; and can speak from their own experience of the curious tricks and devices practised in support of imposture, and of the difficulty of detecting it; they can also probably remember instances, in which prisoners have purposely brought back their illness by drinking cold water and other expedients; and I can tell them, that when the prisoners were employed in flax-beating some time ago, several of them were detected in chewing the flax to produce a temporary derangement in their bowels, all of whom now stand, of course, in these Tables as cases of genuine diarrhœa."

The sum of the objection contained in this paragraph is, that diarrhœa was the disorder which the prisoners were accustomed to feign, for the sake of procuring indulgences and avoiding labour; yet that all cases, real and feigned, are included in our Tables.

We unquestionably did not pretend to distinguish between real and feigned cases, where all were treated alike. The fact of feigned disease could only have been ascertained at the time. If it was not ascertained at the time, it cannot now be assumed; and, if it was ascertained, it is rather extraordinary that the counterfeits should still have been treated as actual diseases, and indulgences still granted as to real invalids, and fresh motives held out to counterfeits.

If there is anybody who, from memory, can speak to the fact of a considerable number of these cases being counterfeits, it must be the apothecary. Now, the apothecary was employed by us, day after day, in our investigation, and when it was completed, he was perfectly aware of the conclusion to which it led. He was ready and unreserved in his communications; yet I do not recollect, neither do any of my colleagues recollect, that he mentioned a single word about counterfeit cases, or

made any objection whatever to our mode of proceeding, or stated any circumstance within his knowledge which could invalidate our conclusion. Our impression was, that Mr. Pratt regarded his own books as authentic records of medicines prescribed for real diseases. He brought these books to us unsolicited, and he produced them with this memorable observation, "How lucky it is I have kept them."*

But, when Mr. Holford concludes, that cases feigned, for a great variety of motives, which he specifies, and cases purposely produced in a great variety of ways, all "stand in our Tables as cases of genuine diarrhœa," he, to our surprise, quotes this very Mr. Pratt as the source of the information from which he proceeds to his conclusion; for he means no other person, when he says, "as the surgeon will testify." Yet, after all, what does he quote him as testifying? That prisoners, making certain complaints of illness, "had chalk mixture or chalk powder, *of course, without any previous investigation into the reality of their disease.*" Surely this is not an announcement on the part of Mr. Pratt of the truth, as it was actually ascertained by himself, that all the prisoners who so complained were "shamming," but an acknowledgment of his own omission to ascertain whether they were so or not.

I will venture to make this general observation, for the truth of which I appeal to all physicians of public institutions, namely, that when people have an interest in seeming to be ill, they always counterfeit disorders of *sensation, and sensation merely*, and thus they often succeed, owing to the extreme difficulty of detecting the deceit. If a person affirms that he has pain, how can you be sure he has not? But never was an instance known of feigned diarrhœa, because no one was ever silly enough to believe that the pretence could go undiscovered for a moment. Surely the prisoners of the Penitentiary are

* These words are in the recollection of all the physicians; for they were the subject of frequent, very frequent remark at the time, seeming (as we thought) to intimate the apothecary's own opinion, that his day-books contained something useful towards the object of our inquiry. Now, the object of our inquiry at the time, was some definite proof of the existence of diarrhœa in the prison prior to the autumn of 1822, a fact which we already more than suspected. (See page 308.)

the last one should be inclined to suspect as the authors of a stratagem which would necessarily detect itself.

But let it be admitted, for an instant, that the prisoners of the Penitentiary did pretend a flux of the bowels for six years and a half in succession, yet is it not incredible, that any medical man could be so deceived as to go on prescribing, during six years and a half in succession, for any disease whatever, as if it had affected from one-half to one-tenth of a certain community; while, in point of fact, the disease had not existed at all during the whole of that period? And is it not still more incredible, that any medical man should so prescribe for a disease, the characteristic symptom of which, if it had been real, must have continually forced itself upon his senses?

But let it be admitted, not only that flux of the bowels was the feigned disease of the Penitentiary, during six years and a half, but also, that feigned as it was, Mr. Pratt went on prescribing chalk mixture and chalk powder for it, as if it had been real during the whole of this period: what follows? A coincidence of the most extraordinary kind, namely, that this same disease which had been the *unreal* and *fictitious* disease of the Penitentiary during so many years, became all at once so unquestionably *real*, that the lives of half the prisoners were in jeopardy from it, and many actually died; and, moreover, that the very remedy which had been prescribed during so many years, *for no purpose whatever*, was the same which, at length, was found *most necessary and indispensable*.*

Of the motives assigned by Mr. Holford for counterfeiting a disease, which never can be counterfeited with success, I have little to say. Of the minor motives, such as missing chapel, and plaguing the turnkey, I know nothing, and can say nothing. But concerning the principal motive, which includes every other that can be imagined, that of obtaining admission into the infirmary, where the prisoners enjoyed a better diet, idleness, ease, society, &c., I wish to make a few observations.

It appeared to me, when this subject was investigated last year, that those gentlemen (not professional) who dissented from the conclusion of the physicians, respecting the existence

* *Vide* page 217, where it will be seen what remedies we found the medical officers prescribing, with apparent success, when we were first called to the Penitentiary.

of flux within the Penitentiary since its foundation, did so upon the presumption, that all persons for whom the medicine, considered by us unequivocally to denote the disease, was prescribed, were, during the time of taking it, treated in every other respect as invalids; that they were released from their usual labours, and *brought into the infirmary*, and allowed all its indulgences and comforts. Consequently, as there seemed to exist such strong motives for prisoners to pretend a trivial disease, they could not help believing that they did so. The fact, however, was not as it was presumed. Of those who took the chalk mixture and powder, some were received into the infirmary, and some were not; and the latter, upon the whole, were the majority. These had the medicine sent to them in their cells; they were allowed no indulgence, and no exemption from their ordinary labours, and could have no imaginable motive for pretending disease.

My friend Dr. Macmichael, who took a peculiar interest in this question, and to whose acuteness the discovery of a predominant disease always existing in the Penitentiary is principally to be ascribed, has furnished me with an important document, showing how many of those who were treated for a flux of the bowels, were received into the infirmary in each year, and how many were not:—

	1816.	1817.	1818.	1819.	1820.	1821.	1822.
Number of Prisoners ...	72	212	246	351	609	798	866
Number of those who were treated for a Flux of the Bowels	23	104	106	82	85	87	88
Number of those so treated who were admitted into the Infirmary	14	4	36	37	60	73	54
Number of those so treated who were not admitted into the Infirmary	9	100	70	45	25	14	34

It is remarkable that, as the disease became more limited in extent, the numbers admitted into the infirmary were propor-

tionably greater; and that, in the three last years, the cases treated as flux in the infirmary, far exceeded those so treated in the prison. My belief is, that as the extent of the disease became less, its severity was greater, and that, from a smaller number of cases, there were more that required to be carefully treated. For, in the year 1822,* between January and the 2nd of July, and before the less nutritive diet was adopted, five deaths are reported from diarrhœa, or dysentery; while, during the whole year, not more than eighty-eight suffered those diseases, as far as we can judge from the remedies employed; a mortality proportionably greater than that which subsequently occurred from diseases of the same character, when they constituted a part of the epidemic.

I had almost forgot to advert to the means which prisoners are said to have used for purposely procuring bowel complaints. Drinking cold water, and masticating flax, might, and probably did, reproduce diarrhœa in a few, who were hardly convalescent from recent attacks. But the question is not concerning what has happened lately. It is allowed by all, that a disorder of the bowels has lately prevailed, which was capable of being reproduced by anything that had the least power of irritating. We are inquiring what could have produced a flux of the bowels *de novo* in a community otherwise healthy several years ago, and continued to produce it for several years in succession. The means specified certainly could not. He who, being in perfect health, takes pure water for his purgative, or sucks out the little juice that lingers in a bit of dried flax, will surely not suffer such a commotion of the bowels, as will be mistaken for disease.

"I come now to cases of sickness, neither feigned nor purposely produced. I believe that a great majority of the cases, for which medicine may have been properly given in the Penitentiary, would never have been brought under the observation of a physician or apothecary, if they had occurred out of the prison, or have been known to any but the parties affected, who would (to use a common phrase) have allowed the disorder to carry itself off, or perhaps have varied their food. If a medical man were to go round certain streets inhabited by poor families in a part of the town esteemed the most healthy, prepared to dole out his medicine to any individual who chose to apply for it, and this for nothing; and if he were

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* *Vide* page 304.

besides to enter upon a regular examination of every inhabitant in those streets once a month as to the state of his health, I suspect he would find at the end of the year, that he had expended more medicine than had been sold in any other district of the same size from the apothecaries' shops in the neighbourhood; but he certainly would not be warranted in drawing any unfavourable comparison between the streets under his care and the neighbouring districts. Now the wards of the Penitentiary are just like these streets."

Here the matter of fact, as it regards the Penitentiary, that the cases of real disease within it were such, for the most part, as required no medical treatment, and the matter of fact, as it regards certain districts of the town, that the inhabitants have a natural love of physic, are both mere assumptions.

It will not, therefore, be thought disrespectful if I decline answering them, since they can have naturally no weight in determining the matter in question. I will only take the liberty of observing, concerning the latter assumption in this paragraph, that the parallel which is imagined should have been carried further; for, as it stands, it would not, if true, lead to the inference which is intended. It is not enough for the argument, that people in certain districts should have an inherent longing for physic *generally*, and pretend *any disease*, for the sake of obtaining it, they must have an express longing for chalk mixture, and the disease which they pretend must be diarrhœa.

"Disorders of the bowels are, I am told, not uncommonly found in prisons, or among any large bodies of men who are all fed alike, and have not the opportunity of varying their food, until there shall be an actual appearance of some derangement of the system; and it is not improbable that diarrhœa may have been prevalent in a prison where very coarse brown bread has been the basis of the dietary; but I deny that this fact can be inferred in opposition to other evidence, from the mere examination of the quantity of medicine sent into the prison, even if the surgeon had stood by to see it taken, which he undoubtedly was not in the habit of doing."

When Dr. Roget and myself were first employed at the General Penitentiary, questions were drawn up by us, and addressed by the Secretary of State to various gaols in England, respecting their schemes of diet, and their ordinary diseases; and, from the answers returned, it did not appear, that bowel complaints, of the same general character with that of the

Penitentiary, had been prevalent in any of them. Thus much I think it proper to state, as a matter of fact. Further I am not concerned to reply to what is admitted to be hearsay.

"If it be true, that the number of cases in which the chalk mixture has been given, has been gradually diminishing during the period of six years and a half, alluded to in the physician's Report, so that comprehending one-third of the whole number of the prisoners in the first half year ending on the 31st of December, 1816, they amounted only to one-ninth or one-tenth of the number in the prison during the whole of the year 1822 (as is stated in the Tables in the Report), I certainly cannot infer from that fact, any change in the climate of the Penitentiary, or any gradual improvement in the local circumstances connected with the prison, but should rather look for the causes of the decrease in the number of patients or quantity of medicine, to the surgeon's having discovered that he had been too lavish of his physic, or to his having become more skilful in detecting the attempts of prisoners to impose upon him, or to the greater care taken by himself or the officers to see the medicines taken, or to such changes in the diet or discipline of the infirmary, as may have diminished the desire of the prisoners to be removed thither," &c. &c. &c.

This mode of arguing, that the diminution in the cases of diarrhœa, year after year, was not real, but in consequence of the apothecary and officers having become more skilful in detecting the tricks of the prisoners, proceeds upon an implied assumption of the whole question at issue. It is first taken for granted, that a flux never existed in the prison, and then a theory is set up to explain some deceptive circumstances which have led "credulous" people into erroneous notions upon this subject.

"There are, moreover, other classes of persons residing within the prison, who seem to have been strangely overlooked upon this occasion. We have a considerable number of inferior officers, male and female, within the walls of the Penitentiary, and it is well known, that bowel complaints have not been prevalent among them before the month of April last, when the disorder was evidently infectious, and several of the officers employed among the prisoners were attacked by it. We have also had, from the first establishment of the prison, families of superior officers residing in the very centre of the building, which have been so healthy, that no individual belonging to any of them has died since the prison was opened."

Here is the induction of a particular fact with nothing raised upon it; and it would be hardly fair for me to pre-

sume what was the inference intended, and thus to make an argument for myself to reply to. Surely the health of the resident officers cannot be intended either to negative the fact that the prisoners were ill, or to intimate that, of two classes of people, differing from each other in all the circumstances of their lives, although living in the same place, one could not possibly derive disease from a source to which the other might be exposed with impunity.

"To come now to the Tables exhibiting the number of patients affected by diarrhœa in each year, and of those in whom that disease is traced to successive years. Assuming, for the sake of argument, that every dose of medicine was given for a real disorder, I must still doubt, how far the principles upon which these Tables, though drawn up with great labour, have been constructed, are correct, for the purpose of showing the prevalence of diarrhœa. The first defect in them appears to me to be, that they make no distinction between cases, in which the bowels of the patient have been relaxed for a single day, and cases which have been obstinate and protracted."

We did not make the distinction here required, because it was not warrantable from the data before us.

"If the greater number of convicts within the prison should appear to have had a looseness for one day in the course of the year, I think it can hardly be stated, from any number of such cases, that diarrhœa has been a prevalent disorder in the Penitentiary ; nor if many prisoners have had relaxed bowels once in the course of each year, for several successive years, can such persons be considered as having had a disorder "difficult of cure," such repeated instances of relaxed bowels in a succession of years being nothing more than is experienced by a large proportion of the inhabitants of this country. Now there is in these Tables no distinction between the cases of patients, to whom one single delivery of medicine has taken place, and of those who may have been under a long continuance of medicine, either in the same year, or in successive years. I have been furnished by Mr. Pratt, from whose papers these Tables have been formed, with a list of the prisoners who have taken the medicine alluded to, from the first opening of the prison, and with the daily quantity of medicine delivered out to each. I cannot make the numbers amount to those mentioned in the Report ; but there are a great many cases, in which one single delivery of "a mixture," or of "chalk powder," appears to have taken place during the whole period of the prisoners' confinement. There are also a great many cases of prisoners who appear to have had medicine delivered to them on two days only in the course of two or more successive years ; and there is one case of a woman who had medicine only on the 31st of December in one year, and on the 1st of January only in the next, who, I learn from

Mr. Pratt, stands in these Tables as a patient for diarrhœa in the two successive years."

From "a looseness for one day in the course of the year," appertaining to any number of prisoners, unquestionably it could not be inferred that diarrhœa had been the prevalent disorder of the Penitentiary; and from many instances of "relaxed bowels once in the course of each year for several successive years," unquestionably it could not be inferred that the disorder had been difficult of cure. But how were these facts concerning "looseness for one day in the course of the year," and "relaxed bowels once in the course of each year, for several successive years," to be ascertained?

Mr. Holford, relying on certain lists which were furnished him, seems to intimate (if I rightly understand him), that these facts might have been ascertained from the quantities of medicine therein stated to have been supplied to different prisoners; and that a single delivery of medicine might be considered to indicate a diarrhœa of a single day, and the gross number of single deliveries to stand for the gross number of diarrhœas of one day in each year, for several successive years.

But this calculation and its results are contrary to my constant observation, which assures me that nine people out of ten, in every condition of life, and especially among the poor, would rather run their chance with a common diarrhœa, than take medicine for its relief; and that nine people out of ten never do apply for medicine until it is gone beyond (what they conceive to be) a common diarrhœa. By no other rule can I pretend to judge concerning the disorder of the Penitentiary, and the medicines prescribed for it, than that of my own experience; and thus so far am I from believing a single delivery of chalk mixture or chalk powder to have been always given for a single day's diarrhœa, that I conceive nine prisoners out of ten never took even a single dose, until the disorder had already been troublesome to them during several days.

But the question is not concerning a *dose* of the medicine, but concerning a *delivery*. I do not know what quantity of the chalk powder went to one *delivery;* but one *delivery* of chalk mixture amounted to eight ounces, or five full doses. To half an ounce of tincture of calumba, and twenty-five drops of lauda-

num, and two drachms of aromatic confection, was added as much chalk mixture as would complete the eight ounces.

Now, from what obtains, in ordinary practice, and especially in the practice of public institutions, I should infer that the prisoners for whom this mixture of eight ounces was prescribed, had, in the opinion of the prescriber, something more than a common diarrhœa, or a diarrhœa of a single day. Since for such a disorder, in a person otherwise healthy, one dose, and one dose only, would be thought enough, and repeated doses, to the number of five, would be deemed inexpedient, and not without the hazard of some inconvenience.

Upon the whole, then, I must continue to believe, that the physicians acted a prudent part in not admitting any distinctions of the kind intimated into their Report; and that, although they were quite aware of numerous cases, for which the medicines were prescribed more and less frequently, and might suspect that such cases were more and less severe, they were still right in inferring no more than the general prevalence of a certain disorder from the general use of certain remedies. Thus much they thought they could do with safety. But, it is said, that they cannot do even this; while, at the same time, it is complained, that they have not done more, namely, that they have not made a distinction of cases, grounded upon the greater and less frequency with which the medicines were prescribed.

There is one circumstance especially pointed out by Mr. Holford in disparagement of the method of proceeding adopted by the physicians, upon which I must make a short remark. It is, that "a woman, who had medicine only on the 31st of December in one year, and on the 1st of January only in the next, stands in these Tables as a patient for diarrhœa in two successive years."

Now, it was the purpose of the physicians to show, by their Tables, the extent of the disease at different periods of time since the foundation of the Penitentiary; and it was natural, with this view, to fix upon the division of years. Thus they reckoned all who were treated for a flux of the bowels in each year, taking care not to count the same individuals more than once, how frequently soever any might have been under treatment between January and December; for they considered that the disease had not extended its sphere within a certain

period, so long as the same individuals were attacked by it. Moreover, it was the purpose of the physicians to show, by their Tables, how far the disease was maintained in the Penitentiary by new cases, arising at different periods, and how far by the same cases continued from one period to another; and with this view, also, it was natural to fix upon the division of years. Thus, beginning each year as a fresh period, they reckoned in the same manner as before, all who took chalk mixture or powder in the course of it, including, however, both those who had, and those who had not, been enumerated in any former year; yet finally distinguishing them, and specifying the numbers capable of being traced back from one year to another. Hence an individual case, being upon the confines of two periods would be reckoned twice; while occurring at both extremes of the same period, and many times in the course of it, it would be reckoned only once; and thus it happened, that the case of the female who was treated for flux on the 31st of December in one year, and on the 1st of January in the next, was included in two periods.

I have entered upon this explanation, because the instance, so expressly pointed out, seemed to impute a sort of stratagem to the physicians in their mode of reckoning; nevertheless, I am much surprised that it was not at once seen how this single fault (if it be a fault), unavoidably arose from the structure of the Tables. And indeed, all such tables must, from their very nature, be obnoxious to faults of the same kind in single instances. For no form of generalizing was ever known, which could give a satisfactory view of a subject upon the whole, and at the same time do exact justice to every particular included in it.

The words "difficult of cure," are quoted from the Report of the physicians, as falsely characterizing a disease, which in many instances seemed to require little medical treatment. Nevertheless, however mild it might have been in particular instances, yet, since it was the predominant disease of the place during many years, and since the patients of one year were traced back, in the proportion of a third, a fourth, or a fifth, as the patients of preceding years, and since it, or a disease of the same general character, finally involved all at once, both those who had, and those who had not suffered it before, namely,

almost the whole population of the prison, I do not think the physicians were far wrong in stating *summarily* that it was "difficult of cure."

"From these Tables, moreover, if framed with a view to the discovery of the extent in which diarrhœa can have been produced by any local influence in the prison, should be excluded all cases in which the looseness of the bowels has arisen in the latter stage of other disorders, from the debility occasioned by consumption, &c. &c., and cases, where the patients are known to have had the digestive organs materially injured by drinking, or other vicious courses, before they came into confinement, &c. When all these shall be withdrawn, the numbers will, I suspect, be very materially diminished; at all events, so long as they stand on the file undistinguished, the Tables cannot be considered as containing the result of an investigation into the effects of local influence, even admitting, what I believe no person will be credulous enough to believe, that all the cases, for which medicine has been given, have been cases of real sickness for which physic would be taken in ordinary life."

When the Tables were drawn up by the physicians, they had the constant assistance of the apothecary, and some cases were excluded for reasons which he suggested, and which were deemed satisfactory at the time. I can therefore hardly conceive it possible that many cases are still to be found in them, which ought not to have been admitted.

Mr. Holford has added a postscript to the "Observations," which I proceed to notice with great reluctance. Most willingly should it pass without a single remark from me, but that my total silence might seem to admit the censure as just, which it is its express object to cast upon my colleagues and myself. I say its express object, because the writer in the meantime loses sight of every other, and even so entirely forgets the conclusion, which he has hitherto been labouring to establish, as to bring forward facts in support of his censure, which furnish stronger grounds for the opinion of the physicians than those which were adduced by the physicians themselves.

The Postscript is as follows—

"Since the foregoing Observations were written, I have looked more narrowly than I had done before into part of the papers given to me by Mr. Pratt, which he assures me are faithful extracts from his books of all the entries respecting the delivery of chalk mixture, or chalk powder, since the opening of the Penitentiary (these entries being the ground of the

Physicians' Report) ; and if these extracts have any pretensions to correctness, the Tables of the physicians must be abandoned as entirely useless with reference to the matter in question, or indeed as to any matter. I have taken up that portion of the Tables which professes to give the whole number of the patients who took chalk in 1822, and the numbers traced back as having taken it in former years, and have examined these numbers with the entries in Mr. Pratt's books as vouched and explained by the extracts given to me. I chose the last year in the Tables (1822), because I thought the year in which the prison began to be affected with the prevailing epidemic, was that from which, if any similar disorder could be traced back to former years, it was most important to trace it, and I have no reason to suppose, that the Tables are more or less accurate in respect to the patients of that year than they are concerning those of any other year.

The Tables make the whole number of patients for diarrhœa in that year 88 ; I make them 90 ; but I find that more than the half of that number, are cases in which medicine has been delivered out only once. The physicians make the number traced back 17 ; I make them 24 ; but in a very large proportion of these, the patient has only had the medicine once given to him in most of the years into which he is traced. If the physicians mean, that the number given comprises all who had taken chalk in the preceding years, they have omitted several ; but if they mean, that there are 17 cases in which persons who were afflicted with diarrhœa in 1822, had been under the influence of chalk medicine in preceding years for any considerable length of time, that is certainly not the case according to these papers."

First, for the justice of the censure. The apothecary furnishes Mr. Holford with certain papers, assuring him that they are faithful extracts from the day-books of all entries respecting chalk mixture and chalk powder ; and Mr. Holford, comparing the numbers given in our Tables for one year, 1822, with the entries of the day-books as vouched and explained by these extracts made by the apothecary, finds they do not entirely accord ; hereupon he lays the foundation of his somewhat sweeping censure, and adds, " if these extracts have any pretensions to correctness," (putting the case hypothetically, but arguing upon it as a fact,) " the Tables of the physicians must be abandoned as entirely useless with reference to the matter in question, or *indeed as to any matter.*"

The few last words contain something more of contempt than the physicians (I am persuaded) will be thought to deserve, certainly something more, than any reasons which are apparent will be thought to justify.

Several times in the course of the "Observations" has

Mr. Holford raised his argument upon the sole authority of statements furnished him by Mr. Pratt; and in so doing, when those statements related to professional points, upon which Mr Pratt had peculiar means of information, he did what was right; but in so doing, when those statements related to points which were capable of being ascertained, and verified by himself, he did, what perhaps is hardly allowable in any inquiry like the present. But, however this may be, when he finally takes upon himself to dismiss the physicians with a sentence of very strong censure and contempt, I may be pardoned for thinking, that then especially he ought to have verified for himself the facts which are the grounds of his harsh opinion, or that at least he should not have allowed it to appear, that he had taken them altogether upon the credit of another. The facts were entirely within his own reach, and Mr. Holford could, and (I presume to repeat) ought to have examined for himself the original entries in the day-books, and compared them with the Tables of the physicians, before he ventured to hold up their labours to the contempt of the managing committee, and characterized them "as entirely useless with reference to the matter in question, or *indeed as to any matter.*"

Nevertheless the physicians are, upon the whole, under some obligation to Mr. Holford for adding strength to their conclusion by the very facts, which he has chosen to accept from Mr. Pratt in support of his censure. For grant that, in respect to the gross number of cases in the year 1822, and the number of cases traced back from that year to preceding years, we are wrong, and that he is right. We are wrong in understating that which he is right in putting at a higher amount. We make the number of cases in the year, 88; he makes them 90. We make the cases traced back 17; he makes them 24. Whence it will follow arithmetically, that he goes so much further than ourselves in imputing disease to the Penitentiary, as 90 are more than 88, and 24 are more than 17.

But after all it must be admitted that extracts of entries from journals of the kind in question are very liable to error. The question is to which side in the present instance the error most probably belongs, whether to that of the physicians or of the apothecary. If several persons should be employed *separately* upon a journal, containing entries of various kinds ex-

tended over many years, in extracting from it those which related to a particular subject, it is probable that the numbers as calculated by each would be different, and that in every instance the numbers would be incorrect. But, if several persons should be employed *together* upon such a journal, for the same purpose, and so distribute their labours, that each should be a check upon the other, it is probable that the numbers, thus calculated by all, would be correct. In a long catalogue a single entry is very apt to escape the eye, and to go unreckoned.

Feeling this liability to error, the physicians and Mr. Pratt were *conjointly* occupied upon the day-books in question, which contained prescriptions of various kinds, for various complaints, during a period of six years and a half, for the purpose of extracting from them the entries of chalk mixture and chalk powder. And it is no disparagement of the accuracy of any of us, to believe that the numbers thus calculated by us altogether bear a greater probability of truth, than any numbers which each might have calculated singly. Is it too much to suppose that they are really more accurate than those so calculated by Mr. Pratt?

Subjoined to the Postscript are two Tables, upon which I desire to make a few observations. The first is entitled

ABSTRACT OF THE NUMBER OF PRISONERS

To whom any delivery of Chalk Mixture, or Chalk Powders, has taken place in the Penitentiary, during the latter part of the year 1816, and during the years 1817, 1818, 1819, 1820, 1821, and 1822; distinguishing the Number of deliveries to any one Prisoner during each year.

Number of deliveries.	1816.	1817.	1818.	1819.	1820.	1821.	1822.
One	9	40	49	53	62	71	72
Two	7	16	13	19	18	28	21
Three	...	7	4	7	4	7	6
Four	3	2	3	4	2	8	3
Five	...	...	2	4	3	1	1
Six	..	...	1	1	...	2	...
More than six ...	2	3	5	4	...	5	10
Total	21	68	77	92	89	122	113
Average number of prisoners in the Penitentiary	64	151	224	273	427	631	745

The purpose of this Table is to show that of those, for whom chalk mixture or chalk powder was prescribed in each year, the majority took so much only as was contained in one delivery. But it has been already proved, that this fact, being admitted, does not go to negative the existence of diarrhœa as the predominant disorder of the prison.*

The following Table, which is the second subjoined to the postscript of the "Observations," I have already taken the liberty of using in another place,† because it seemed to me to contain, in the most succinct shape, as strong an argument as could be imagined of a predominant disease existing in the Penitentiary, and of that disease being diarrhœa. In speaking of the day-books, I observed that the form of certain entries in them went especially to prove the fact, those, namely, of chalk mixture, sent wholesale to prisoners working in companies, without any specification of the individuals who were to take it. But I had mislaid my note of the number of such entries, and had I not accidentally turned to Mr. Holford's "Observations," and found this Table subjoined to them, I should have been at a loss how to put the argument in its most convincing form. I have yet another use to make of the same Table, and therefore I now give it again in its proper place.

In 1816, an entry of	1	qrt.	bottle of mixture for	the kitchen women.	Is not noticed in the above Table.
„	„	1	„	Mrs. Clarke's women.	
In 1817,	„	2	„	Mrs. Clarke's women.	
„	„	1	„	Mrs. Evan's women.	
„	„	2	„	the laundry.	
„	„	1	„	the carpenter's cell.	
In 1818,	„	1	„	Mrs. Croome's women.	
„	„	1	„	Mrs. Gould's women.	
„	„	1	„	Laban's men.	
„	„	3	„	Brett's men.	
In 1819,	„	1	„	Mrs. Clarke's women.	

It is here well worthy of remark, that Mr. Holford himself, who, in one part of his "Observations," insists so strongly upon the motives for "shamming" being a sufficient proof of the fact, does in this very Table give the most glaring prominence to a circumstance which destroys the supposition altogether. For, by it we find the complaint, for which chalk

* *Vide* page 328. † *Vide* page 312.

mixture was prescribed, was so far from furnishing a claim of indulgence, that not merely those who were employed at their trades, but those who sustained the hard labour and household drudgery of the prison, were not exempt from that labour and drudgery in consequence of taking medicine.

Thus much I have thought it my duty to say in reply to the "Observations" of Mr. Holford. They were considered (I know) by those to whom they were addressed, to be a complete refutation of all which the physicians had advanced concerning flux, as the predominant disorder of the Penitentiary since its foundation. The physicians themselves, however, presumed to think otherwise, and even to believe that much was contained in the "Observations," which tended rather to confirm their own opinions. The question is an important one, and it is now left for the decision of medical men.

In closing my review of the "Observations," I shall refrain from passing, in my turn, any summary opinion upon them. For I feel much too strongly what is due to a man, who, during many years, and under circumstances of peculiar difficulty, has bestowed his best exertions, zealously and profitably, upon the great objects of the Penitentiary, to characterize any part of his labours as "entirely useless with reference to the matter in question, *or indeed as to any matter.*"

APPENDIX.

Respecting diarrhœa as the prevalent disorder of the prison since its foundation, I have one more remark to make, which, having omitted in its proper place, I think of sufficient importance to subjoin in an Appendix.

The Tables of the Physicians are (I must own) imperfect in one respect, and the day-books are not now within my reach to enable me to supply the defect. They give the proportion which those ill of diarrhœa in every year bore to the whole convict population of the Prison, both sick and well; whereas they ought to have given the proportion of those ill of diarrhœa to those ill of other complaints, or rather, to have given this last proportion in addition to the former.

The Tables, indeed, as they stand, show enough: nevertheless, parallel statements are of great use in such cases; and, if the Physicians had admitted into their Tables, first the total number of prisoners, then the number of all who were ill, whatever was their complaint, and lastly the number of those ill of diarrhœa, they might have obtained, with respect to the two last, a parallel statement from any Hospital or Dispensary, which would have shown at once, whether the proportion of diarrhœa to other disorders in the Penitentiary, was greater or less than elsewhere.

The following Tables are formed from Dr. Bateman's Book "on the Diseases of London," in which an account is given of the kind of Diseases and the number of Cases treated at the Public Dispensary, Carey Street, during twelve years. They show the proportion which the disorders, whose prominent symptom is flux of the bowels, including dysentery, diarrhœa, and cholera, bore to all others whatever in each year, during the whole of that period, and enable us to form a comparison between the Penitentiary, and one particular District of London, in respect to the prevalence of those disorders.

Year.	1805.	1806.	1807.	1808.	1809.	1810.
Gross Number of Cases	1821	2049	2063	1998	1957	2118
Cases of Flux	91	87	134	112	95	119

Year.	1811.	1812.	1813.	1814.	1815.	1816.
Gross Number of Cases	2224	2305	2504	2656	2610	2462
Cases of Flux	140	123	144	109	121	130

It appears that, during the twelve years, the cases of flux, in the proportion they bore to the gross number of cases treated at the Public Dispensary, varied between a fifteenth and a twenty-fourth.

Now, if at the Penitentiary the proportion of those ill of diarrhœa to the whole of the prisoners both sick and well, was so considerable as one-tenth in the last and most favourable year which our Tables embrace, and so enormous as one-half in the first year, what must it have been to those only who were sick?

But, if all the prisoners in the Penitentiary had been sick the proportion of those suffering diarrhœa in the most favourable year, far exceeds that which is found in any Hospital or Dispensary, where I have made inquiry.

ON THE

USE OF OPIUM IN FEVERS.

By P. M. LATHAM, M.D.,

Physician to St. Bartholomew's Hospital.

[*Read before the College of Physicians, March* 17, 1832.]

There are forms of fever which are very simple and very easy to treat; and the simplest form is that in which all the symptoms observe a certain proportion to the state of the vascular system, so that, however various they may be, and how many soever the organs they involve, they become greater or less, and rise or fall, according to the degree of excitement manifest in the heart and arteries. Here the treatment is easy, because it hangs upon a single indication. Reduce vascular action, and the fever begins to decline; and with it also begin to decline whatever symptoms appertain to particular organs—to the sensorium, to the organs of the chest, or the organs of the abdomen, or to any other part.

But there are forms of fever which are very complex and very difficult to manage; where the symptoms, taken altogether, do not conform themselves to the existing state of the vascular system, and where a single indication thence derived does not guide us in the treatment of the entire disease. Here, whatever the condition of the blood-vessels may require to be done, some special treatment is moreover demanded for the relief of particular organs; so that how far depletion should be carried, is often in practice a very subordinate question; while the life of the patient may depend upon a just determination in giving or withholding purgative medicine, or in making use of calomel

largely or sparingly, according to present conditions of disorder in the abdominal viscera.

But my present purpose is with certain disorders of the brain which arise in the course of fevers, and with opium as their appropriate remedy.

It hardly ever happens that a fever passes through its entire course without some symptoms declaring themselves which are especially referable to the sensorium; and in the majority of cases where such symptoms appear, the morbid affection of the sensorium is unquestionably derived from the blood-vessels. Still the cases are not few in which it is derived from some other source.

Now, how important the brain is as an object of the physician's attention in fevers, must appear, not only from its extreme proneness to participate in the disease, but chiefly from the consideration that it is through the medium of this organ that death very frequently takes place.

The morbid affections of the brain in fevers are never without an evil tendency, come from what source they may. If from the blood-vessels, the danger is upon the whole less, because the method of cure is more evident; if from other sources, the danger is augmented by the difficulty of obtaining clear indications of treatment.

My observation has taught me that, connected with fever, there are affections of the brain which are essentially inherent in the organ itself; and capable of relief only by remedies which exercise an immediate impression upon that organ. But of such remedies, it is opium only which I have learnt to employ to any salutary purpose.

With respect to the diagnosis of these affections, they are rather denoted by concomitant circumstances than by any stamp of peculiarity they bear on themselves. So much so, that I could not mention *any series* of symptoms flowing from the brain, and cured by opium, which I have not also seen cured by bleeding. Indeed, I only know a *single symptom* referable to the brain which would suggest the use of opium as the suitable remedy; and that symptom is a state of wakefulness.

There may be much, or little, or no delirium—much, or little, or no tremor, or subsultus; in short, cases successfully

treated by opium have, in respect of other symptoms belonging to the sensorium, differed as much as possible from each other, while they have agreed in this one of perpetual wakefulness.

Yet the state of wakefulness does not alone justify the remedy; if it did, nothing more would remain to be said in defining its use. But the state of wakefulness may be present, and still opium not be the appropriate remedy. That *it is* or *is not*, must be determined by circumstances to be sought for, not in the sensorium itself, but elsewhere. To these concomitant circumstances, therefore, both as best discriminating the disease and best indicating the remedy, our attention must be mainly directed.

These circumstances are often such as to exhibit a sort of contrast with the existing affection of the brain. The proper febrile symptoms are just enough to characterise the disease and to constitute it *a fever:* some chilliness and some heat, alternating with each other; the countenance now a little pale, and now a little flushed; and the pulse more frequent, but not more forcible than natural; and the tongue merely clammy, and the stomach and bowels not notably deranged. Yet, with these moderate symptoms, there will arise at an early period, or at the very commencement of the fever, a disorder of the sensorium which is strangely disproportionate. Day after day the disproportion will be more striking, until at no distant period (for such cases pass on with great rapidity to their fatal termination) with a pulse almost too feeble to be felt and too frequent to be counted, will be combined the wildest delirium and the most violent exertion of muscular force. If, under these circumstances, a state of continual wakefulness form (as it generally does) a part of the sensorial affection, opium is the appropriate remedy; and there is no other remedy (as far as I know) that offers the slightest chance of saving life.

In the cases described, the symptoms referable to the sensorium outrun, from first to last, every other symptom, whether belonging to particular organs or to the constitution at large. The symptoms, in their aggregate, constitute a fever; but the sensorium has its own special disorder, requiring a special remedy, and which is independent of the blood-vessels. When the patient dies, it is this disorder which kills him; for he dies of exhausted nervous energy, and upon dissection, you find no

visible vestige of disease either in the brain or in any other part.

But there are other cases of fever in which there is no evident contrast between other symptoms and those belonging to the brain, *at first*. The disorder of the sensorium keeps pace with that of other parts, and with the state of the circulation; and thus the disease proceeds until it is somewhat advanced, when the harmony between its symptoms is disturbed. Those of the brain outrun the rest. New indications of treatment arise, and if the patient can be saved at all, it must be by opium.

Thus the heart and arteries may be full of activity, and every symptom in proportion to it. Remedies are accordingly addressed to the vascular system, and succeed for every purpose they are intended to fulfil *except one*. The general febrile symptoms are abated by venesection. By venesection, or by topical bleeding, each organ loses its peculiar distress. The respiration is easier; the abdomen bears pressure, and even the head ceases to ache and throb. But, withal, *the delirium continues*. As other symptoms are relieved, the delirium is even aggravated. The patient mutters, or sings, or talks nonsense; he is absolutely sleepless day and night, and is jumping up and endeavouring to get out of bed. Such is the condition of the sensorium with an improved state of the circulation and of other organs—and even of the tongue.

To this delirium, low, muttering, or wild, as it may happen (for it may be all of these in the same individual in the course of a few hours), subsultus is liable to be added, and the unrestrained passage of the different evacuations.

In such a case as this, I have seen a single dose of opium, dextrously administered, change the whole complexion of the disorder in a single night, and place the patient at once in a state of safety.

Again, I have seen the sensorial affections incident to fever, which require opium for their cure, manifest themselves in another form. There has been high vascular action from the first; and large depletion has been required to subdue it and to guard particular organs, and especially the brain, from injury. Under such treatment, all has gone on successfully, and the patient has reached a point of convalescence; with a soft pulse,

a cleaning tongue, no pain, and refreshing sleep for two or three days; when suddenly (the tongue, the pulse, and all other circumstances continuing the same) some strangeness of manner has arisen, and then the wildest delirium, and then the unrestrained passage of the evacuations. I have known the transition from such a state of convalescence to such a state of peril, take place in a few hours; and I have known the patient again brought back to a state of convalescence in twenty-four hours by a moderate dose of opium. This is a rare form of disease, but one in which, when it does occur, opium is eminently indicated.

Now, when the affections of the brain put on a character of great energy and violence, they are apt to suggest the notion of inflammatory action. Indeed the mere sensorial symptoms alone cannot be always distinguished from those of phrenitis; but there may be no phrenitis nevertheless. Those who are experienced in the medical treatment of the insane, have ably discriminated a class of cases characterized by great excitement, which they know to be curable by no remedies which deplete, but specially and exclusively by such as have a direct influence upon the nervous system: and although, in fevers, the very nature of the disease would always lead us to watch narrowly the state of the circulation, and suspect its possible agency in the production of any sensorial affections that occur, yet it need not surprise us to find that even in fevers there should be some such affections which are independent of the blood-vessels, both in their production and their cure.

Still there are cases of fever in which the symptoms belonging to the brain have no such energy or violence, and the symptoms belonging to the vascular system present no such contrast with them as we have described, and yet opium is essential to the cure.

There may be simple wakefulness, and literally no other symptom whatever referable to the brain; and, as to symptoms belonging to other parts, they may be just enough to constitute a fever, and no more.

This is a form of fever which often ends ill, to the utter amazement of those who witness it, and who cannot tell how it has happened thus. The patient has hardly any fever; therefore there is no thought of danger. There is no change in his

symptoms during several days; until suddenly all his strength is gone, and he cannot raise himself in bed. His tongue is become dry, and it trembles; some muttering delirium is added to his wakefulness; he passes his evacuations involuntarily, and soon dies.

In this manner does death sometimes take place, because one symptom, small and unobtrusive indeed, but of inestimable importance as a guide to practice, has been unfortunately overlooked from the beginning. The patient, perhaps, never mentioned it, and the physician did not inquire after it. Yet such a case as this, if this single symptom of wakefulness be duly estimated, and opium administered in season, will generally terminate well, and at an early period.

These affections of the brain, incident to fever so peculiar and so perilous, and requiring opium for their cure, deserve to be illustrated by any circumstances that can be brought to bear upon them beyond the mere symptoms of the particular case; and, indeed, they are capable of much illustration by the habits and state of health of individuals before they became the subjects of fever.

And here I would make one general remark—that, by knowing *what* a man is, and *how* he lives habitually, the physician often arrives at a much better judgment and a better treatment of his diseases. It is trying a man's diseases by his health; and a most valuable test it is, and of great practical utility.

In healthy and vigorous bodies there is a certain balance and regularity of function which, even when disease befalls them, is seldom lost, but their morbid action is still harmonious and proportional. In them diseases are often severe, but they are generally simple: they often require the most active remedies, but they are generally easy of cure. On the other hand, the weak and valetudinary, who at the best are full of jars and incongruities, are obnoxious to the strangest forms of disease, hard to understand and hard to treat.

Now fever, when it happens to a perfectly vigorous and healthy man, is never characterized by any such peculiar affections of the sensorium as have been mentioned. These are incident, according to my observation, to those only whose habits and mode of living have been calculated to do an abiding

injury to the nervous system, and who have been long actually suffering from such injury.

Every class of society has furnished me with instances of this form of fever, and every instance has confirmed the truth of the remark.

Among the higher and educated classes there is, in this age and country, a wonderful striving for all the objects of wealth, and honour, and power. We need only think upon the strife of politics, the hazards of mercantile gambling, and the wear and tear of hard professional toil, to see how many there must be who, from the common business of life, have derived both to their minds and bodies new feelings and impulses, and new susceptibilities of disease. These susceptibilities belong chiefly to the brain and nervous system, and they are apt to come forth into frightful activity when such men become the subjects of fever. The trouble of the brain gets the mastery (as it were) of the disorder of every other part.

The poor and mean among mankind have the mind overwrought, and the nervous system exhausted, by real calamity, just as the high and the educated by their more refined cares; and thus they often claim an unenviable approximation to them in the character of their diseases.

Into the hospitals of London a miserable class of patients is often admitted—the wretched outcasts of the streets. They have crawled about asking alms all day, and at night have lain down in the open air and slept. They are sometimes picked up in a state of half-consciousness, and brought to a hospital. Of these some only require to be washed and fed, kept warm, and allowed to sleep, and then they recover without manifesting any real disease. Others (and the greater number) after they have lain awhile, and their blood-vessels begin to react, put forth the symptoms of some serious malady; it may be an inflammation of some particular organ, or it may be a fever; and if a fever, it is almost always of that kind in which the derangement of the brain outruns that of the vascular system, and of every other part.

It appears probable, then, that the sensorial affections alluded to are ultimately determined by something peculiar to the constitutions of individuals before they become the subjects of fever. One may call it an unhealthy susceptibility at least,

if not an actually morbid condition, belonging at all times to the sensorium and its functions. It is one of those things which cannot be spoken of with precision, and of which more may be learnt by attending to the *moral* causes (for such they are) out of which it seems to arise than to the thing itself. To these moral causes, in further illustration of it, I will add one *physical* cause, which is of most extensive influence—the habitual indulgence in spirituous liquors. Individuals, who have done a permanent harm to the functions of the nervous system by the abuse of spirits, do never, when they become the subjects of fever, suffer a delirium of the ordinary kind, in which the brain is excited nearly in the same proportion with the blood-vessels, and which, by remedies addressed to the blood-vessels, is uniformly controlled; but they suffer a delirium in which the brain is actuated disproportionately to and (perhaps) independently of, the blood-vessels, and, if curable, to be cured by opium. This I venture to state almost absolutely, and without exception.

Now, when we contemplate these sensorial affections, said to be incident to fever, in their kind, in their causes, and in their cure, we cannot help seeing how much they possess in common with what is called "delirium tremens." In fact, they are the same thing. Yet I will not call them the "delirium tremens of fever," for fear of misleading by a name. Besides, the name itself is ill-chosen, inasmuch as there are affections of the brain, which, from their causes, their method of cure, and their own essential nature, must be supposed to belong to the same category, and yet have no tremor whatever among their symptoms.

There is a wide range of sensorial affections all *pathologically* the same. But the extreme instances, being the most striking, they have been picked out, and called by a particular name—"delirium tremens," as if they constituted a class by themselves, whereas they are only individuals of a much larger class. To this class also belong the sensorial affections in question, which are incident to fever.

Having then settled the nature of the disease, and its remedy, we must now determine the mode in which that remedy is to be applied, and its quantity.

When we desire to abate pain by opium, the degree of pain

is the measure of the dose required. So, too, if we would subdue nervous irritation by opium, the degree of excitement informs us how much we ought to give. Simple wakefulness may be gently lulled to sleep by a few drops of laudanum, but wild delirium requires to be mastered, and (as it were) forced into repose by a much larger dose.

Thus the sensorial affections incident to fever, which are curable by opium, require various quantities of the remedy according to their degree. But in no case, even if the excitement reaches to convulsive action of the muscles, and the wildest delirium, is the quantity required *absolutely very large.* And here my experience has led me to a conclusion which no reasoning *à priori* could have reached, viz. that a much larger quantity of opium is necessary to remedy certain sensorial disorders when they exist alone, than when they are combined with fever.

In extreme cases of delirium tremens you fling away the resources of your art unless you venture to administer opium in doses which would run the hazard of poisoning a healthy man. The very same symptoms, carried to the greatest extremity, *and combined with fever*, are still to be subdued by opium; but twenty minims of the tincture are now quite sufficient for the purpose. I never gave a larger dose at once, and I have seldom found it necessary to repeat it. If the dose be too little it is easily increased, but there is great peril if it be too much—*the peril of coma.*

The success of the remedy turns entirely upon the condition of its procuring sleep; and it is more or less complete in proportion as the sleep procured is, within certain limits, of longer duration.

When, therefore, in a case of fever, after long wakefulness, accompanied by wild delirium and a violent exertion of muscular force, with such a state of pulse as absolutely forbids the use of further depletion; when, in this extreme case, we administer the extreme dose (for such it is) of twenty minims of tincture of opium, we must be content to wait patiently the result; for the use of every other remedy is now sacrificed to this single one; indeed, while it is in the course of operation, the effective employment of any other is necessarily precluded.

We should wait patiently four hours at least; and in the

meantime let everything be made to favour the success of the remedy: let the room be kept silent and dark, and one individual only remain with the patient, charged not to utter a single word.

The four hours having elapsed, we are to determine from the state of the patient whether still more opium be needed or no, for the accomplishment of our purpose. We may find, that soon after taking the laudanum, the patient fell fast asleep, and has continued sleeping ever since. Thus the remedy has done all that we desire. Or we may find that he fell asleep, and soon woke again, and so has slept and woke, and slept and woke again ten times in the course of an hour, and in the intervals of waking his delirium has returned. Under these circumstances, should more laudanum be given? I think not. After a considerable dose has been administered, if any real sleep, however short, has succeeded, I would not give more until after a long interval, because I would not hazard the peril of an overdose. Besides, though the remedy has not entirely fulfilled my wishes, it has not entirely disappointed them. These little snatches of sleep, after long-protracted wakefulness, are often productive of great benefit. The sleep, thus fitfully obtained in the course of one night, has entirely changed the complexion of a doubtful case, and placed the patient in security.

But we may find that the patient has not slept at all, or that it is doubtful whether he has or has not; yet, though the wakefulness remain, there may be an evident abatement of other symptoms which flow directly from the sensorium, of the delirium, of the muscular twitching; that the pulse is a little diminished in frequency and increased in power; in short, that the patient is altogether more composed, although he has not slept. Under these circumstances, it is safe and expedient to give more opium to the amount of half the original dose.

In cases where the delirium and excitement, accompanied by wakefulness, are less in degree, a smaller dose of opium may be relied upon for effects equally beneficial. In such cases I have been accustomed to give five minims of the tincture every hour, or every other hour, until the patient begin to dose.

There are cases where the indications for the employment of opium are doubtful. Wild delirium, and long wakefulness,

and a circulation weak and fluttering, seem to call for a considerable dose of opium. Yet, withal, there is *a certain jerk in* the pulse, so that we cannot help suspecting that the blood-vessels have something to do with the sensorial excitement. Under such circumstances, I have certainly seen twenty minims of laudanum produce tranquil sleep, from which the patient has awoke quite a new man. But I have also seen the same quantity produce a fatal coma, from which he has never been roused.

Now, since it is a fearful thing to strike a heavy blow in the dark, where the alternative is of such magnitude, it is the safest and the best method to administer a small dose at intervals of an hour or two, so as to stop short of actual mischief at the first glimpse of its approach, or be led by a plain earnest of benefit to push the remedy to its full and consummate effect. Many doses may be required for this purpose, but we shall see, after the first or second, whether to go on or to desist.

There are cases where all the curative effects of opium are obtained by very small doses given at very distant intervals.

I have mentioned a form of fever in which simple wakefulness exists from the beginning, while the proper febrile symptoms are very moderate, and delirium is only at length added from the very exhaustion of the nervous system. Here *the whole treatment* consists in the dextrous use of opium, and it may be given at any, even the earliest period of the disorder. Sleep, sleep is the want of nature. Five minims of laudanum given every night will be enough to procure it while there is mere wakefulness, but when a little delirium is added, five minims more should be given twice in the twenty-four hours.

Do not let this be thought an insignificant practice. It is enough to save life, which will inevitably be lost without it.

GENERAL REMARKS

ON THE

PRACTICE OF MEDICINE.

A SERIES OF ARTICLES

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GENERAL REMARKS ON THE

PRACTICE OF MEDICINE.

I.

THE PRACTICE OF MEDICINE PRIOR TO THE KNOWLEDGE OF DISEASE.—WHAT IT WAS.—KNOWLEDGE OF DISEASE IMPROVED IT, WHILE IT PRESERVED ITS ORIGINAL OUTLINE.—WHAT PRACTICE OWES TO PATHOLOGY NOT EXACTLY CALCULABLE.—DISTINCTION BETWEEN CURING THE DISEASE AND TREATING THE PATIENT.

THERE was a practice of medicine long before there was any knowledge of disease. The griefs, pains, and necessities of man's body and mind did not call the less loudly for relief because they were not understood. The practice of medicine was always a daily need, and always brought with it a certain measure of benefit. It seems providentially ordered, that what is for our good should not altogether wait attendance upon our knowledge.

But this practice of medicine, which was prior to the knowledge of disease, and so far uninstructed, was not always without reason and method. It had two ways of proceeding. 1. It dealt largely in *specifics* (special remedies). It did cures by remedies, which had (as was thought) the direct power of curing, and which seemed to reach the disease in its essence, and abolish it without any intermediate or cognisable operation whatever. Thus it had a reputed remedy for almost every disease which had a name. 2. This uninstructed practice, when it had no name to give the disease, and had no remedy for curing it, was not without resource, if, notwithstanding, a great illness were obviously suffered. It took note of heat and cold, and hunger and thirst and pain, and of conscious weakness and incapacity, and of some more obvious varieties of the pulse. And to these, and to whatever else went plainly and discernibly

wrong in those feelings and functions and movements, by which the body makes chief display of its vitality, it tried to minister as it could; and it often ministered well and successfully. For thus it often hit unconsciously upon right indications of treatment, and thus procured recovery from a disease, of which it knew neither the nature, the seat, nor the existence.

Here we have a sort of primitive plan of medical practice; and the same remains to this day. Knowledge of disease and a more cultivated experience have added much and corrected much. But it has preserved its original outline.

If any man, a little accustomed to self-questioning, will call to mind what he was at first as a physician, and what by increase of knowledge and experience he afterwards became, he will find not unfaithfully reflected in his own example the beginning and progress of the art itself.

As the knowledge of disease increased, the practice of medicine improved; but neither proportionably nor with equal steps. Many diseases which we knew the best, we did not therefore manage the better; often, indeed, not better than in the time of our ignorance; because a fatal part of our knowledge was simply this, that the diseases, in their own nature, were beyond the possible reach of any remedy.

Nevertheless, to affirm that the more we have known of diseases, the better we have been able to manage them, would be quite true in the general sense; but to give an exact account of the debt due from practice to pathology would be hardly possible. We could not take our knowledge of diseases as it is at this day, and assort it, and weigh it, and put a value upon it according to the help it affords us in practice. In some diseases, its help is very great; in some, it is very little; and in some, it absolutely amounts to nothing at all. In some, it is plain, appreciable, and at hand; in some, uncertain, equivocal, and remote. Here we want more knowledge; there we have already more knowledge than we can use. Here we are in the dark; there we have plenty of light, but we cannot contrive to throw the light we have upon the objects which require it. The misfortune is, that practical medicine has from time to time been darkened, as it were, by cross lights, let in from strange quarters. They have set off the subject; they have made a

show of it; but they have given it lustre, rather than illustration.

If any one, who had a turn for the thing, would spend half his life in carefully surveying the various sorts of knowledge deemed needful to make a good physician, and take, as it were, their natural bearings, and at last reckon how far, in fact, they had or had not fulfilled their aim, he would produce (supposing him to have acquitted himself of his task tolerably well) an interesting, a very curious, and an utterly useless exercitation. To speak of practical medicine and the things concerning it profitably, a man must draw either from what is his own entirely, or from what he has made his own by experiment and trial. Whether he handle particulars merely, or embrace general facts and principles, he must make himself the representative of the art he practises.

When first I found myself in the midst of between four and five hundred patients in St. Bartholomew's Hospital, the scene bewildered me, and I learnt nothing for months. It was something, however, to become reconciled to the objects around me, and to look with complacency on what was going on. All I saw was "a great multitude of impotent folk," and the physicians busy among them with the expedients of their art. And some were recovering, and some dying; some getting better, and some worse, and some remaining unalterably the same. The physicians and their art confronted the patients and their diseases, and exercised, I plainly perceived, a great power upon the whole. But I could not yet discern their points of contact. It was as if some mysterious scroll were being daily unrolled before my eyes; and all its inscriptions were unintelligible, for want of the key for deciphering them.

At length this small ray of truth found me out in the dark; viz. that some medicines were remedial simply by bringing diseases to an end without any intermediate operation being apparent, or intended, or thought of; and that some were remedial by bringing diseases indeed to an end, but not without intermediate operations, both apparent and designed, and looked for as conditional to the result. This little light gave me heart and encouragement, and a new interest. Not that by help of it I was able to penetrate the *modus operandi* of medicines in the least degree, but I could just read by it that they

had, or seemed to have, at least two several ways of working out their ends, and that physicians had two distinct purposes in prescribing them. To me, as yet an uninformed looker-on, the practice of medicine first presented itself taking this outline; it was its natural outline; and it has remained distinct and permanent and the same in my mind ever since. Whatever I have since learnt has taken its shape from it, and its place within it; and so will what I have now to say.

It would save some trouble, and not, I trust, show me bent upon handling the subject too artificially, and so spoiling it, if of these two modes of dealing with disease remedially, I called the one their *cure*, and the other their *treatment*. According to popular notion, cure is the aim and end of all treatment, and the result and complement of successful treatment. But let cure and treatment now be taken to denote different things, so far as the aims and objects of the physician are concerned in his management of disease. Let cure be looked upon as concerned with the disease as such, and having little or no regard to the individual patient whom it befalls. Treatment is concerned with the individual patient, and leaves his disease to take care of itself.

There are eminent and familiar instances of cure in this sense: the cure of ague by bark and arsenic; of scabies by sulphur; of syphilis by mercury; of scurvy by lemon-juice; and of certain periosteal diseases by iodide of potassium. And small-pox, measles, scarlatina, the typhus and typhoid maladies, are eminent instances of diseases which have no cure, and yet issue in recovery and health by means of treatment.

It is an instructive fact that, as the knowledge of disease has increased, the practice of medicine has been less and less conversant with cures and more and more conversant with treatment. The knowledge of disease is not naturally suggestive of special remedies, which are always hit upon by chance; it rather goes to reducing the number of the old ones. From its habit of inquiry, it is ever trying the claims of certain remedies to the credit of *curing* certain diseases; and it is ever finding good reason to disallow such claims and reject such remedies largely. Hence, practice has betaken itself of necessity to manage many diseases by *treatment*, which were once deemed within easy reach of cure. And thus the present state of our

knowledge has come to warrant the conclusion that the number of diseases is very small which are capable of cure by a proper remedy of their own, and which exclude the need of other remedies addressed to conditions belonging to the individual patients; whereas the number in which the converse obtains would embrace the vast majority of human maladies.

But this cure of diseases by single special remedies is a thing so complete and off-hand, so saving of thought and trouble, and so accordant with the popular notion how diseases are, or ought to be, dealt with, that one cannot help some regret for the number in this happy predicament being small. Did it include all or almost all diseases, it would go near to produce unanimity among physicians, for there would be nothing for them to disagree about; and at the same time to banish all thought from their practice, for there would be nothing for them to think about. Nevertheless, we should greatly rise in the opinion of the world, which, even with things as they are, is ready to magnify none so highly as those who, whether deceiving or self-deceived, have a cure for any or for all diseases.

Cure by special remedies addressed to the disease, and treatment by common remedies addressed to present indications in the man, divide the domain of practical medicine between them; unequally indeed, but still they divide it. Moreover, they mix themselves a good deal together; cure and treatment running into each other, and special and common remedies co-operating for good.

Let it, however, be remarked, that practical medicine takes this shape from its own necessity, as things are. Our knowledge is incomplete. But such as it is, we must use it; and the first condition of using it safely or profitably is to know that it is incomplete. An imperfect instrument is in our hands, and we cannot trust it simply and entirely. It needs some art and management in the handling; but these must not be too much, lest they hurt the fair play of our instrument, imperfect as it is.

II.—Cure.

ON SPECIAL MEDICINES, AND ESPECIALLY CINCHONA.—SPECIFICS AND CURE CONTRASTED WITH NON-SPECIFICS AND TREATMENT. THE DIFFERENCE BETWEEN CURING AND TREATING A FEVER.

Remedies given to cure diseases in some occult way of their own we have called "special" remedies. Would not "specifics" have been a better name, as being of more popular use? Why, no. Medical men, indeed, have never been remarkably exact about the meaning of the terms they employ. Nevertheless, as long as terms convey no error, it is well to abide by them and let them pass. They cannot, however, be always made to suit our purpose without some little explanation. Thus the common notion of a "specific" is, that it deals with the disease after its own way; and, moreover, that it deals with it *successfully*. The term "specific" always includes the idea of success; and for this reason it would be too restrictive for our purpose. "Specifics," in the popular sense, would still remain the choicest and best, but would comprise a few only, of the remedies which have place in the department of medicine which we call "cure"; whereas "special" remedies would comprise them all, whether they cure single-handed and by themselves, or conjointly with others and as auxiliaries; with constant or occasional, with complete or partial, or with whatever degrees of real or imputed success.

"Specifics," then, represent the perfection of special remedies. But perhaps they may not be always found the most interesting of the class to which they belong. Special remedies, in proportion as they approach nearer to specifics, work with a concealment which is impenetrable, and invite inquiry with little hope of reward. But, in proportion as they fall short of specifics, they are less absolutely covert in their operation, and leave, as it were, chinks and crevices through which, as they work, we may catch sometimes a glimpse of practical and pathological truths.

The most universally allowed specific is cinchona; and its specific virtue is most eminently declared when it cures an

ague. It may then be taken as a standard by which to try the claim of any other remedy pretended to be a specific; and a standard, moreover, by which to try itself when it is used as a specific for other diseases. In the cure of ague by cinchona, the peculiarity of the whole affair is this, that the cure follows the use of the remedy, without the apparent intervention of any prior operation whatever.

Now cinchona has been found to do the work of cure in certain other diseases as summarily, and as exclusively of any intermediate operation, as it does in ague; and so far it equally deserves the character of a specific. But it does not do this work of cure so constantly in them as in ague; and therefore in them it is not at all times to be fully trusted as a specific.

Cinchona often cures the severest headache; the more surely if it take the form of hemicrania, and have an intermittent character; and the more surely still if the patient be, or have been, where agues prevail. And so cinchona often cures severe neuralgia in all parts of the body, the intermittent character and the probable malarious origin still giving a better promise of its success.

It is remarkable the number of diseases (if diseases they can be called of which nosology takes no notice)—the number, I mean, of undescribed and indescribable ailments having their origin, growth, and increase among agues and in agueish districts—which are cured by quinine.

But a disease need not always claim known kindred with ague, either in its intermittent character or in the place where it is found, to be sometimes curable by quinine. Erysipelas has often found in it a summary remedy. By its use, foul and ill-conditioned sores have changed their complexion completely and at once, and become healthy; and thus the expectation of gradual sinking and death has in twenty-four hours given place to a confident hope of recovery and life. Such effects, so rapid and so springing up under our very eye, can hardly be brought about by any mere *tonic* power belonging to cinchona in common with many other substances. Surely none of the class of bitters could be trusted for the same result. Neither can they be well conceived to come from cinchona operating as a mere stimulant. It does not merely do the work which wine or brandy could do just as well. One cannot help believing it

does the work which only itself can do, and nothing else; and it does it acting by a specific power of its own, as much as when it cures an ague.

The specific effects of cinchona which are best known are really so full of wonder, that new and less familiar displays of its curative power occurring from time to time do not surprise us. Indeed, we can hardly make up our minds to be incredulous when report is made of the marvels wrought by it, which go far beyond our own experience. What, however, has been of late promulged is startling. It should almost seem that, as long as there is quinine in the world, no fever need go uncured. Intermittent, remittent, and continuous fevers, typhus and typhoid fevers, simple and complex, and in all climates and in all stages of their progress, are to be cured by quinine. So it is positively stated. The fault of physicians, it should seem, has been that they have never given it often enough or in quantity enough, otherwise they would have acknowledged it for an universal antifebrile specific long ago.

Now, for the present, I would hint no suspicion that the accidents of time, place, or circumstance might have largely influenced the results, giving a new element to the disease, and allowing an extraordinary virtue to the remedy. I will take it for granted that the fevers mentioned by name, each and several, and the classes of fevers indicated, are in their own nature curable, and were actually cured by quinine; for I wish to make the important difference between *cure* and *treatment* as intelligible as possible. And this I can best do by displaying them in contrast; and the contrast can never be more striking than when each is seen in its separate dealing with the same disease.

Let us then consider fever in its largest sense, and see first what it is to *cure* it, and then what it is to *treat* it. This difference will display the difference between *cure* and *treatment*, as applied to the management of all other diseases whatever.

Take cold and heat, and perspiration, occurring interchangeably, whether regularly or irregularly, and in any manner or degree; and a frequent pulse, whether strong or weak, and pain in the head with wakefulness and delirium, or drowsiness and torpor; and the tongue dry and red and clean, or sordid and black; the breathing hurried, and perhaps the

bronchi wheezing, the abdomen tympanitic and painful on pressure, and perhaps a sanious discharge running from the bowels; take these symptoms on any day early or late in the course of the disease; take them all in a heap, and regard them as one, and make them serve collectively for the single aim of the single remedy, and that remedy quinine. This is pre-eminently the cure of fever; and its success, which comes suddenly when it comes at all, must be a sort of triumph.

Again, take all these same phenomena which tell of disorder and disease in the vascular system and the nervous system, and in any or every organ of the body; take them, not as they appear on any one day, but as they ebb and flow and fluctuate every day and night for more than twenty days or nights in succession; and watch each, and try to interpret the meaning of each, and judge which is most perilous in itself, and which bears most perilously upon the whole disease; and which is most within reach of a remedy, and which is most urgent for relief; then choose and apply, not *the* one remedy, but remedies many and various, or few and simple, according to the aims proposed; and take care the right remedy of to-day be not the wrong remedy of to-morrow: pure air to breathe, and pure water to drink; affusions or ablutions of the whole body, cold, warm or tepid; lotions or fomentations of this part or that; remedies for pain according to its nature and seat; expedients for preventing sores, and expedients for healing them; aperients, and astringents, and mercurial alteratives, according to their several needs; also opium, that great saver or destroyer of life as it is rightly or wrongly given in fever, in its various uses. These are the sort of implements which the physician has to work with when he has to *treat* a fever. And being thus vigilant and analytical about the bedside emergencies of fever, and dextrous in ministering to them, he saves many lives.

And this is pre-eminently the *treatment* of fever; but it may be, and often is, one part of it only. For, after all has been done that need, or can, be done, in the way of thus noting particulars and ministering to them, lives cannot be saved without engaging observation and treatment in larger aims. More in one case and less in another, more in one stage of the same case and less in another; but in some stage of almost every case, we have to take the sum of the symptoms, and

calculate whether, upon the whole, they mean strength or weakness, vital reaction or vital yielding; and to make choice accordingly, whether it be right to restrain, to lower, to deplete, or to support, to raise, to stimulate; whether to give calomel and antimonials, or to use some kind of bleeding, or to give wine, or brandy, or ethers.

This is the *treatment* of fever in another of its parts. Both together, that which is analytical and that which is summary, comprise the *whole treatment*. If it succeed, its success is after the lapse of many days, and the trial of many hopes and fears. And when success comes at last, it produces nothing like exultation and triumph, but rather quiet satisfaction and thankfulness.

Such is the difference between cure and treatment, when we come to follow them out in their operations, and to think what the one is and what the other, practically and in truth. The difference is so great, that if this vast class of diseases, called fevers, belonging to the whole world, and always existing and raging somewhere, could be taken at once out of the domain of *treatment* and transferred to the domain of *cure* now and for ever; and if, instead of needing remedies, as many and various as are the times and places, and men and circumstances wherein they arise, they were found capable of being safely and successfully consigned to one and the same remedy always, and everywhere, and in all persons, the practice of medicine would suffer the nature of a revolution. The thoughts, studies, habits, and feelings of medical men would be utterly changed.

So great a revolution is not likely to happen. But a man need not have grown old in the practice of medicine to bear witness to its having undergone considerable changes; and the diseases which have been the objects of them are especially those we have been speaking of; and the changes themselves have still respected *treatment* and *cure*. They have not, however, amounted to anything like a substitution of one for the other; but have only involved measures of more or less, in which treatment and cure have been mixed together, and that not always and everywhere, but partially and from time to time, yet for long times together, and in this place or that; yet it may be over many countries. Thus, febrile diseases which have a contagious or malarious origin, or which arise from

causes so occult that they seem spontaneously engendered; also, typhus and typhoid fevers, scarlatina, erysipelas, spontaneous febrile hæmorrhage, and purpura, will for a time, even for a series of years, and over a great portion of the world, be capable of successful management by remedies addressed to the circumstances and conditions of individual cases—that is, simply by treatment. Again, some or all of these diseases in the same place, during another series of years, will yield less satisfactorily to the same remedial method. The indications will be as well chosen as before, and the medicines used will fulfil their immediate purpose, but the whole disease will not decline as easily or come to an end as soon or as completely as before. And then physicians will not be able to make up for the failure of their tried resources by seeking and finding new indications and new remedies rationally suited to them, but by using a special remedy which acts and cures nobody knows how or why, even the great specific cinchona. Then cure has become the needful auxiliary of treatment; and a special remedy given on the faith of a large experience has completed the imperfect work of remedies addressed to rational indications.

Now there are subjects upon which the most sober and practical minds cannot help speculating a little beyond what they know. Sure and great results—yet familiar and common, and procured at will and by certain means, but in an unaccountable manner—naturally set us thinking and forming notions how they come to pass; and then it is safest and best to fill up the gaps of our knowledge from analogy. When we are treating a disease, our aim may be to raise the pulse or to lower it, to cool the skin or to warm it, to purge the bowels or restrain them, to augment the secretion of this or that gland, or to diminish it or alter its quality; and as our remedies do their appointed work, we witness the whole disease brought to an end. But, when we give cinchona for an ague, we have no single aims; we give it for the whole disease, and the cure follows. Nevertheless, it is according to analogy to believe that it hits a mark of its own, which is more particular than any within the scope of our discernment. Thus cinchona, and all so-called specifics, may work after the same manner with other remedies, which are better understood. They may all equally produce their great results by intermediate operations;

only in the one case the intermediate operation is seen, and unseen in the other; aimed at in the one, and unaimed at in the other.

From this view of the matter, it would turn out that our distinction between *cure* and *treatment* is not in the nature of things, but in the imperfection of our knowledge.

Further, from the same analogy, we may fairly believe that there is a certain morbid element, of which cinchona is the specific antidote; that it exists most pure and unmixed in ague, but that it may add itself as an accident to other elements whose property is to produce other forms of disease; and that wherever it exists, and in whatever combinations, its power is still predominant, and cinchona is still needed to bring the disease to an end.

These things may be as analogy would suggest. But in practice there is a mischief in conceiting ourselves wiser than we are. Granted, that of the many morbid actions and sufferings which make up the disease, one only may be reached and swayed, or annulled, or curatively modified, by the one special remedy; granted, that thus the complex disease may cease by removal of some one main element, and by some intermediate operation of the remedy; but inasmuch as we know no such element of the disease, and see no such intermediate operation of the remedy, they must be to us as if they did not exist. We cannot construct aims and indications of practice out of hidden things, but must be content for the present to regard the whole disease as a single comprehensive indication, and so prescribe for it the single remedy and expect the cure. We shall perhaps know better some time; but no good will come from our pretending to know better now. Patience under imperfect knowledge is no proof of an unwise mind.

III.—Cure.

CINCHONA: THE ACCEPTANCE IT FIRST MET WITH FROM SYDENHAM AND HIS CONTEMPORARIES.—CURE OF AGUE BY IT ABOLISHED ERROR, BUT TAUGHT NO TRUTH RESPECTING PROCESSES OF RECOVERY.—OBLIGATION TO USE SPECIAL REMEDIES IN OUR PRESENT STATE OF IMPERFECT KNOWLEDGE.

I have somewhere heard or read of the doctrine that every disease has its antidote; and that the proper specific remedy capable of curing it exists somewhere in the world, if we could but find it out; and that the Divine wisdom and mercy are pledged for such being the nature of things. This being admitted would at once render the search after new specifics our supreme duty. But I cannot follow the doctrine either with my head or with my heart. On the contrary, I conceive it hardly possible for a physician to employ his time worse than in quest of new specifics. His common sense would be about equal to that of the man who should trust his hopes of growing rich to the chance of finding a bag of money. But a specific medicine is an excellent thing, and so is a bag of money; and, *being found*, it is worth the study of a life to turn them both to all the good purposes of which they are capable.

Now it strikes one that we should be able to make a better use of specifics or special remedies if we knew more about them; if we could raise the curtain a little, and catch a glimpse of them at work in some positive manner; if we could discern them doing something conducive to the ultimate end for which we give them. I do not mean their *modus operandi*, but quite a different thing, and more important and more practical. I prescribe all my best non-specific remedies for the sake of certain immediate effects, promising myself through these effects to bring the disease to a favourable termination. But, in the meantime, I know nothing of their *modus operandi* in producing even the immediate effects for which I give them; and yet I continue to give them with as much confidence as if

I knew all about it. How mercury makes the bile to flow, or how opium brings quiet and sleep, I do not know; but my experience trusts them for these effects; and these effects are in my hands preparatory to the successful management of many forms of disease.

To ask, then, about specifics, What is their *modus operandi?* is to put a question which has not yet been answered about the commonest and most useful remedies which we employ, being non-specifics.*

Cinchona cures one disease at all times, in all places, and in all men; and that disease is ague. As a specific for ague, it seems tied to no conditions. But, ever since cinchona has been the known specific for ague, all diseases that have borne the name of fever, all that have had fever in any way annexed to them, have at *some* times, in *some* places, and in *some* men, been deemed curable and cured by it. And in these diseases it often does its work as cleanly and summarily as when it cures an ague. It seems now the same noble specific as then; but now it must surely be tied to conditions, if we could but make out what those conditions are.

There is a good deal about Peruvian bark in Sydenham, and the acceptance it met with at the hands of physicians a quarter of a century after its virtues were first heard of in English; and it is curious to note what manner of men Sydenham and his contemporary physicians were, as he represents them and himself sitting, as it were, in judgment upon the new remedy. And some of them were wise, and some were foolish, as physicians are now; and the most foolish were those who were the wisest in their own conceit, as they are now. And then, as now, a really wise man would occasionally make shipwreck of part of his wisdom by venturing out of his depth and pretending to know more than possibly could be known.

Men died largely of agues before bark was given to cure them. For want of it, they displayed symptoms many and shocking enough the longer they lasted and went uncured. For these, Sydenham intimates, some physicians were on the watch; and no sooner was the bark given than they were ready

* Quid scammoneæ radix ad purgandum quid aristolochia ad morsus serpentum, possit, video, quod satis est; cur possit, nescio. (Cicero, *De Divinat,* Lib. I.)

to ascribe them to it forthwith, though the patient might have taken but a single dose of it.* Here was downright prejudice or wilful dishonesty. Others, and the most part of physicians, seeing the bark got rid of the disease *by a secret virtue*, and *not by any sensible evacuation*, would have it that the *materies morbi* was not got rid of at all, but that it was pent up and confined by the astringent power of the remedy, and thus only lay covert, like an enemy within walls, ready to begin new commotions.† This was to take measure of the truth by the standard of their own preconceptions, and believe Nature more likely to err doing her own work in her own fashion than doing it in theirs.

Elsewhere Sydenham alludes to the extravagance of theory and practice which had long prevailed touching intermittent fevers, and which in his time seems to have gone so far that it could not well go farther. "For how many ages," he remarks, "had the observant men been exercising their wits in search of the causes of these fevers; and so each adopted the practice best suited to the theory of his own excogitating. But how little these exploits of medicine served to prove the truth of their speculations was pretty evident from what was of recent memory, when they placed the various species of intermittents in various kinds of redundant humours, and then aimed at curing them by alteration and evacuation of the same. How unfortunate was the whole business, both theory and practice, their own failure showed; but above all the more successful use of the Peruvian bark, by means of which, he adds, we now-a-days, turning topsy-turvy all notion of humours and regimen and diet, and everything of the kind, and, only looking to the right way of exhibiting the powder, aim directly at the mark, and hit it."‡

But let not this brief allusion to the prejudices of past times touching the Peruvian bark be deemed altogether useless.

* His autem potissimum de causis (nisi fallor) cortex malè audit: primo, quia plurima illa horrendaquæ symptomata quæ febres intermittentes jam diutius ægrum cruciantes comitantur, etiam ubi ne minimum quidem corticis degustaverit, cortici vel semel assumpto imputantur. (*Epist.* I. *Responsoria*, § 15.)

† Secundo, quia cùm occultâ vi et non per sensibilem aliquam evacuationem morbum averruncet, statuunt plerique materiam morbum committentem quæ propelli debuerat, tanquam hostem intra mœnia vi corticis astringenti inclusam latere, novos daturam tumultus. (*Ibid.*)

‡ Per quot jam sæcula homines quique solertissimi ingenia exercuerant

Bark may be taken as the type of all specific remedies; and what though it and they are only to be simply given, and the disease simply ceases? What though it and they threw no *direct* light upon the nature of the diseases they cure, or upon their own way of curing them? Yet the indirect light which they contribute to all rational study of pathology and practice is considerable; and of this Sydenham was well aware, and ascribed to them a peculiar value on this account.

Sydenham believed (a curious fact) that there would some day be found out a specific for gout. Long thought upon the subject had, he tells us, brought him to that belief; and he adds, "should it ever be realised, it will rebuke the dogmatists for their ignorance, and show their extraordinary hallucinations in pretending to discriminate the essences of diseases, and in choosing remedies to compass their cure. Of this we have an instance clear enough in the discovery of that supreme specific for intermittent fever, the Peruvian bark."*

The next best thing to seeing, knowing, and getting well hold of the truth, is to clear away impediments that block up the road which leads to it. This the Peruvian bark did for us two centuries ago, when first it was found a specific for intermittent fevers. In curing them, it disclosed nothing of their essence, and nothing of its own way of dealing with

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in investigandis harum febrium causis, quibus praxin quilibet aptavit theoriæ a se excogitatæ optime respondentem! Quomodo vero hæ medicationes theoremata valebant defendere, ex iis constat quorum adhuc recens est memoria, in quibus varias intermittentium species in variis humoribus in corpore redundantibus collocantes, curationem ad humorum istorum alterationem atque avacuationem dirigere solebant. At quam inauspicato id egerunt, eorum in his conaminum frustratio, præcipuè vero hujus corticis usus felicior, palam faciunt: cujus ope nos nunc temporis tum humores hujusmodi omnes, tum etiam diætam et regimen quodcunque, susque deque habentes, ac nihil nisi methodum pulveri exhibendo debitam observantes, scopum recta attingimus. (*Tractatus de Podagrâ*, 71.)

* Non his majora promitto; quamvis a longâ cogitationum serie, quas huic rei impendere tantum non sum coactus, inducar credere ejusmodi remedium quandoque inventum iri. Quod si unquam acciderit, inscitiam suam dogmaticis exprobabit, atque exinde liquebit quàm insigniter, tam in dignoscendis morborum essentiis, quam in medicamentis quibus eos aggrediuntur, hallucinentur. Cujus rei exemplum satis luculentum habemus in repertione summi illius ad febres intermittentes specifici, Corticis Peruviani. (*Tractatus de Podagrâ*, 71.)

them. It simply swept them away; but, in so doing it swept clean out of mind and remembrance many far-fetched notions of them, their nature, and their cure, which by possibility could not be true. It removed notions which had usurped the place of efficient causes, but it did not substitute truths in their stead.

And now, after the lapse of two hundred years, what do we in our day think of this same Peruvian bark? Next to opium, we value it above all remedies in the world. But, after two hundred years, what more do we *know* of it? Of the thing itself we, indeed, know more, much more—even all, perhaps, that can be known. Peruvian bark was the first or nearly the first of vegetable substances used in medicine upon which chemistry essayed its power of analysis, and seized and separated the simple element containing all its curative power. And not of the thing itself only, but also of its curative power, do we know more, infinitely more. And the one knowledge has led the way to the other; for mere grains of this simple element, quinine, were found curatively equivalent to scruples and drachms of the bark. And hence came the readier use of the remedy, and then its larger use; and its larger use has rapidly multiplied our experience of its curative power. But neither scientific experiment upon the thing itself, nor trial of its remedial effects during two hundred years, has enabled us to catch a glimpse of it actually at work. It increases nothing, and diminishes nothing. It neither evacuates nor restrains. It has to do neither with bile, or with urine or saliva; nor with any special secretion. It neither directly soothes nor directly stimulates. It neither puts to sleep nor keeps awake. It works the cure; but it does nothing, as far as we see, prior, preparatory, and intermediate to the cure it works.

There are things in our living bodies which have never yet been reached or touched by human knowledge. Much of health and disease, and life and death resides in these things; and the means and instruments of health and disease, and of life and death, are largely concerned with them. What strengthens and weakens, what heals and injures, what preserves and destroys, even all food and all medicine, whether for good or harm, have their operation more or less in this region of things inscrutable. Our present business is with medicine; and even of the remedies which we understand the best, and choose

according to rational indications, and are able to guide, stage by stage in the work we wish them to do, there is not one which in the way to its accomplished purpose does not do something beyond or beside what we comprehend. We lose sight for awhile of it and its operations in this region of things inscrutable; we cannot follow it with a complete intelligence of what it does from first to last. But the remedies which fulfil their one and complete work of cure without show of doing anything whatever prior or conducive to it, even specifics or special remedies, afford the surest conviction of a region of vital operations beyond the range and reach of our knowledge.

Yet we must not make our ignorance of how the special remedy cures a bar to our use of it. Our knowledge of all remedies, in the gradations and progress of their working, is incomplete; so that, after all, perhaps the most just distinction between specifics and cure on the one hand, and non-specifics and treatment on the other, would be, that of the former we know absolutely nothing but the bare result, and of the latter we know both the result and something of the way they travel to it. But this something may be very much or very little; yet, be it much or be it little, we are naturally proud of it. We prefer the implements which we in any measure guide by our eye and impel by the force of our right hand, to those which we must leave from first to last to their own self-working, and trust to bare experience for the result. But be it always remembered what is our calling. Human lives are given us in charge; and so the amount and certainty of benefit within reach of the remedy may become grounds for its preference before any knowledge we have of the way in which that benefit comes to pass. It will not do to set up the conceit of a little philosophy against the moral obligation of doing good.

In medicine, this inscrutable region is not only a pretty large one, but there is often as good reason for trust of things transacted therein as of things done in more open day. And no disparagement of our profession is contained in this truth. A vast experience of results is needed as the groundwork of a sound faith in cure by specifics and special remedies; and surely such experience is a thing at least not opposed to knowledge.

Specifics or special remedies always seem to me to contain

within them a promise or prophecy of future knowledge, waiting for time and opportunity and suitable minds to work it out. But, in the meanwhile, the results are so full of benefit to mankind, that they are allowed to have a premature accomplishment; and so we are put in trust of them and their great power and mystery of doing good, and find ourselves able to use them aright upon the simple condition of a large general experience. But the work is greater than the workman. It must be so at present; and there is good reason to be content. It may take ages to bring out the explanation how bark cures an ague; and the explanation, when it comes, may carry with it the discovery of new physiological truths. But these truths, known at last, may leave the practical uses of the remedy where they were. Thus, for wise ends, things seem to work in an inverted order; and the uses of knowledge take precedence of knowledge itself.

IV.—Cure.

THE VAST USE OF CINCHONA BOTH AS A SINGLE SPECIFIC AND AS A SPECIAL AUXILIARY TO OTHER REMEDIES.—THE USE OF OTHER SUBSTANCES AS SPECIFIC OR AS SPECIAL REMEDIES.—THEIR SECRET OPERATION IS THEIR COMMON CHARACTERISTIC.—OF POISONS, AND THE CONDITIONS WHICH HINDER THEIR SUFFICIENT TRIAL AS SPECIAL REMEDIES.—OF COMMON REMEDIES, WHICH SEEM OCCASIONALLY TO HAVE A SPECIAL POWER.

It is wonderful how multitudinous are the uses of cinchona; and in all its uses it is still a specific or special remedy. It is the type of remedies that *cure*, in all their powers, degrees, and ways of *curing*, from the greatest to the least. It cures single-handed and alone far beyond what can be ascribed to any other remedy. Thus it is the greatest of specifics. And it supplies, by a virtue of its own, the shortcoming of other medicines to remove the disease and restore to health, oftener and more effectually than any other special remedy. Thus it is the greatest of special auxiliaries.

Here and elsewhere, and in all the known world, experience has the same report to make of cinchona and its specific power to cure alone and absolutely, and also of the special help it has to give other remedies in curing diseases which would go uncured without it. The renown of cinchona is ubiquitous, because the diseases which it cures are found everywhere—diseases bred of noxious things, which come from earth and sky and changeful seasons.

If a man visit the fairest regions of the earth, he takes with him plenty of quinine as the safeguard of his existence; for it is in those regions that he is sure to encounter, in greater frequency and force than at home, all the diseases over which it exercises a specific or special power. The fruits of the ground, springing up without the helping hand of man's labour, seem to witness against the truth of the primeval

sentence. Their gift is eminently gratuitous; it is neither worked for, nor paid for, nor asked for. There is no sweat of the brow in procuring it. But "the sweat of the brow" spoke and prophesied of other and higher industries than the labour of our hands, to become ministers of our well-being. The good gifts are fatally tainted. To free them from their evil, or to find the antidotes; to take them and use them without hurt; to live by them and not die by them,—this has been the labour of man's mind—the sweat of his better brow—for centuries. And thus the primeval sentence stands firm as ever.

Now bear in mind what ague is, and what are its kindred diseases; and what the remedial power of quinine over it and them; and then think of other diseases common to the whole earth—more frequent, indeed, and oftener epidemic, and oftener fatal, in some than in other climates, but met with, and epidemic, and fatal, in all by turns. Think of dysentery and cholera and influenza, as they are called, and all their formidable kindred; and, finally, conceive that for dysentery, cholera, influenza, all the world over, we had as sure a single-handed specific as quinine for ague, and as hopeful a special remedy for their kindred diseases as quinine for the kindred of ague; verily, three such remedies added to our stock, each in its own large sphere of operation the rival of quinine, would immensely diminish the amount of premature death and early incapacity in the whole human race. But quinine stands alone; there is nothing like it or second to it.

Now my experience of what quinine will do as an absolute specific goes for nothing, compared with that possessed by surgeons of almost every British settlement abroad. Therefore I shall say nothing more about it as such.

But here in England there is immense use made of quinine by physicians. As an absolute specific, indeed, for a particular disease, we have employed it less and less as aguish districts have been more and more drained of their endemics. As an occasional specific or special remedy for diseases which at certain times and seasons are unaccountably found curable by it, we still employ it largely; and yet more largely we find ourselves resorting to it as the special auxiliary of other remedies in our management of various diseases.

But there are other substances used from time to time by

all of us, for the sake of the special power they have of dealing with diseases curatively, either single-handed or as auxiliaries to other remedies. All physicians have given mercury for syphilis, and cured it; arsenic for leprosy, and cured it; colchicum for gout, and cured it; iodide of potassium for bronchocele and periosteal nodes, and cured them. Here is a formidable array of diseases, and a goodly array of special remedies and cures; and if these several remedies were absolute specifics for the several diseases, there would be, as far as they are concerned, an end of the matter, and a happy end too, as between physicians and mankind. The world would have nothing more to ask of us. Our duty would be complete, and we might hand over these same diseases to the pure pathologist, hoping at the same time that the remarkable fact of each having in Nature one sure antidote would be for him a great point to start from in search of their essence, and thus science and humanity be both gainers alike in the end.

But of remedies such as these, even if they were absolute specifics, and we had nothing to do but prescribe them and register their curative effects, it would look strangely incurious to have nothing more either to think or to say.

Now the first thing to be remarked is this, that of such remedies—remedies specifically or specially curative in the sense understood—physicians have greatly increased the number and the use of late years, upon grounds, I presume, sufficiently safe and trustworthy. And if the fact of one and the same disease cured uniformly or almost uniformly by one and the same remedy, as in the single instance of ague and quinine, be taken to point to some great pathological truth hitherto undiscovered, the like fact, when it comes to be predicated of many several diseases and many several remedies, denotes a whole region of still occult pathological truths. But wherever it be within our bodies that these truths have their place, wherever they live and exercise a living force and agency, *common observation* has not reached; yet it has searched hard, and done its best within its own possible sphere. But there *is* a sphere beyond; and this science has found the means and implements to penetrate, and there it has traced and grasped a few elements of disease and a few substances, whether remedial or poisonous. The means used are chemistry and the

microscope; the sphere reached and unlocked is that of the blood and the tissues.

Doubtless, great things, ministering both to health and to disease, are being constantly transacted within this sphere, while they give no notice of themselves during their progress, but only by their accomplishment. And it may be well believed that remedies also which *simply cure*, have this for their secret laboratory. Specific and special remedies seem to have it almost as a condition of success that their operation should be secret.

We see how great a subject is this cure of diseases by special remedies, and how it becomes fuller and fuller of interest the more we think of it. But the interest is of different kinds. We feel sure that it contains a mine of physiological and pathological truths, if we could but reach it; while it displays upon the surface abundant proofs of the weakness and credulity of medical men, from which we would willingly turn away if we could.

But the subject cannot be systematised as a whole. Some of its most important facts can only be fairly stated, without any attempt to force them into compliance with prevailing theories.

In our times poisons have been largely pressed into the service of medicine; but upon what terms? Are they to be considered as special remedies for *curing* certain diseases secretly and mysteriously, it matters not how, if experience only give sufficient warrant that they *can* cure them? Or as remedies for *treating* diseases, remedies rationally chosen for the sake of operations, which we form some notion of from our knowledge of their common, *i.e.* their poisonous effects? These may seem idle questions; for practically it all comes to the same thing. And physicians, however it may please them to account for the results, have all one care in using these substances; namely, that in their way to become remedies they do not fatally show themselves to be poisons. By some mode or measure of prescribing them, we seek to cheat them of their poisonous and insure their remedial effects.

It is a hard sentence to utter, but nevertheless true, that there is nothing of which the physician need be so jealous as his knowledge. Let the knowledge be ever so sound, and consequently the desire and temptation to turn it to use ever so

great, the conditions of medical practice are often such as to make its use dangerous or impossible. We have illustration at hand in our present subject of what is meant, which it is worth while to pursue.

The doctrine of poisons, or toxicology (as it is called) has greatly helped pathology. But how far has it helped the practice of medicine? Toxicology, making search for the substances in which it deals, after their application to the body, has traced and found some of them deposited and at rest within the tissues, and some moving and circulating with the blood. Thus it has disclosed a new sphere of their operations. And as of many poisons, so of many medicines; for poisons and medicines are oftentimes the same substances given with different intents; and, being the same, their operations, whether for evil or for good, must, one would think, be always in the same sphere. Thus, beyond these particular substances, a clear intimation arises concerning some others, even our most powerful and summary, and hitherto mysterious agents of cure, that they possibly may have their operations in this sphere also, and that here they meet the first and finer and governing elements of the disease, and here they counteract them.

Now, one would think that medicine and its effects would derive abundant illustration from poisoning and its effects; that medicine, even practical medicine, would learn skill and exact aims from toxicology. Both bring their means to bear upon the same living body, and those means are often the same and often much alike. They dwell (so to speak) in close neighbourhood. They urge their labours, and ply their experiments, within sight, and hearing, and knowledge of each other. It cannot be, one would think, but that instructive hints must be continually passing between them, and toxicology be most helpful to medical practice. But the fact is otherwise. The purposes and ends of each are so totally different that you cannot reason or practise from one to the other. What does all we know of poisoning by opium teach us of the uses of opium as a remedy? How to kill by it is quickly learnt. The few cases of poisoning by it, accidentally or designedly, which occur at any large hospital in the course of a year, are enough to teach the student how to kill by it without fail. But a long life, spent amid the emergencies of disease and the constant

obligation of ministering to them, is required to teach physicians the infinite remedial uses of opium and the skill to make those uses good.

Take arsenic, strychnine, corrosive sublimate, and think what the genius and experimental industry of toxicologists have taught us, within these few years, of the sphere and mode of their operation as poisons. Then, take arsenic, strychnine, corrosive sublimate, and think of them as medicines, and how physicians, whenever they use them, are obliged to handle them as if they had still no knowledge of them whatever, and still took them for dark mysterious specifics. For, understanding all the characteristic effects of each as poisons upon the living body, they are daily and hourly on the alert to bar and exclude them. They so measure their dose as to keep it down below the possibility of producing the least sensible effect. They exclude all that is known of them as calculable, for fear of poisoning, and they rest in what is unknown and inappreciable for the sake of curing. Poisoning and healing work by the same means to different ends; but the end of each so imposes its own conditions upon the mode and measure of dealing with the means, that the use of them lays open knowledge to us in the one case, and closes it against us in the other.

Poisons, be it always remembered, are esteemed poisons, not because they are found in the blood and in the tissues after the man is dead, but because, while he yet lives, they damage or spoil the working of organs by which pre-eminently he holds his life; because they kill the brain, they kill the spinal marrow, they kill the heart, or because they go near to killing them. Poisons are poisons in their living signification; and, however much more may be known about them, this living signification must always be the exact rule and measure of their use as remedies.

All medicine is practised upon this condition, "Harm not man's life;" or if present harm must needs be, let it be such as will be made up for by good to come, and so man's life suffer no detriment in the end. But toxicology has life abandoned to its experiments—brute life absolutely; and as for human life, toxicology is at hand to witness all forms of violence which crime or accident can inflict upon it, and to study the ruins they have left when life is gone.

My own experience in this class of remedies amounts almost to nothing; for I confess myself altogether a coward in the use of them. But as what a man does only once in his life is memorable to himself, and he is naturally apt to make much of it; so to me is the single occasion in which I ever employed strychnine as a remedy. Let this be my excuse for mentioning it; for the result only shows how magnanimous the patient may be, and how pusillanimous the physician.

About a quarter of a century ago, there came into St. Bartholomew's Hospital a fine, tall, intelligent fellow, at the prime of life, with both his wrists paralytic and useless. He was a painter. He suggested his own remedy; the same remedy which, eighteen months before, when he was just as bad as he now was, had, by his own account, perfectly cured him. This remedy was strychnine. He had been in the Manchester Infirmary, under the excellent care of Dr. Bardsley, who had prescribed it and superintended its use. The result was the happiest possible. He returned to his trade, and handled his brush as well as ever.

Upon such showing, it was impossible to deny the man his request: so, committing him to the charge and frequent visits of a trusty student, I prescribed him strychnine. Well! things went on bravely with the patient, but nervously with me. Never was man so happy as he. He had perfect faith in the remedy, and was constantly interested in speculating upon the progress of his recovery. At my visits, he was always urgent with me to increase the dose of strychnine, and get on quicker with his cure. And now he began to have involuntary twitchings of the extremities, especially by night, which he thought denoted the favourable operation of the remedy, and entitled him to have more of it. One day, I found him happy above measure; for, in the course of the preceding night, a convulsion had lifted him clean out of bed, and flung him bodily on the floor of the ward. This was to him a sure presage of recovery; and must be to me, he thought, an irresistible argument for a bolder use of the special remedy. Great, therefore was his disappointment, when he found me pusillanimous enough to order that for the present it should be suspended altogether. The next day my patient was gone; he had positively run away. He left me his good wishes at his

departure, expressing at the same time the small opinion he had of a physician "who," he said, "could cure his disease if he would, but he would not."

What a subject of contrasts and opposites is this of special and specific remedies! We find ourselves dealing rarely and reluctantly with deadly poisons. We wait and look on with fear and trembling, while they are at work, upon their doubtful and dangerous trials. And then we learn, on credible report, that certain harmless familiar things, which we have used all the days of our lives, doing no wonders with them, but yet a great deal of good in a common way, turn out to have special or specific virtues which nobody ever dreamt of. Remedies of common use have gained from time to time the credit of a specific or special use; valued and familiarly employed for their known power over certain indications, and frequent sharers in the *treatment* of whatever diseases, be they many or few, are wont to present these indications; they are all at once reputed to possess, moreover, a *curative* power over some one disease in particular. The authorities or sponsors for these facts are beyond all question; and thus old remedies gain renown for their new virtues, and come largely into use for new purposes.

Some years ago, my friends at Guy's announced to me that lemon-juice was a cure for acute rheumatism; and my friends at St. Bartholomew's, that tinctura ferri muriatis was a cure for erysipelas. I make no doubt that the present evidence was sufficient for the fact thus far. There was the disease, and there was the remedy. The remedy was given, and the disease ceased sooner than according to common expectation.

I earnestly hope that the experience of these two great hospitals has continued to run in the same groove ever since; that lemon-juice still cures acute rheumatism at Guy's, and tinctura ferri muriatis still cures erysipelas at St. Bartholomew's. If the case be really so, then can these two remedies justly claim the rank of specifics, each for its disease respectively. If it be not so, then they are no specifics for the particular diseases, but remedies *only upon conditions*, though the conditions were past finding out. Here we have great mysteries. We have remedies analysing diseases into something more than we know, or diseases analysing the operations of remedies into

something beyond the uses of experience. At all events, the things we witness are realities ; not visible and tangible, indeed, but still realities to be traced and acknowledged in their practical bearings, and to be dealt with and used from time to time, but very cautiously reasoned upon.

They suggest that the diseases spoken of, as well as other and even all diseases, are often more than their symptoms declare them to be, and that they contain some element beyond our knowledge. This element may be occasional only ; and yet, when it occurs, the part which it plays may, for the time, be so essential to the entire disease, that the remedy which can reach the single element deals curatively with the whole.

And further, they suggest concerning remedies, even the commonest and those of which, as we fancy, we know the most, that we do not know all ; that each may have powers in reserve beyond the reckonings of our maturest experience, and a possible sphere of operation beyond our deepest research.

But suddenly let the element which we have supposed be withdrawn, and at once the medicine which did wonders is inert, the disease becomes again its common self, and the specific is reduced to a common remedy.

V.—Cure.

NEW SPECIAL REMEDIES: TO BE RIGIDLY TESTED BEFORE THEIR TRUSTWORTHINESS IS ADMITTED.—EXAMPLE OF M. LOUIS.—SUCH REMEDIES, LONG USED AND TRUSTED, AND AT LENGTH PROVED WORTHLESS, ARE DISCREDITABLE TO THE COMMON SENSE OF PHYSICIANS, AND DENOTE THEIR FAULTY EDUCATION.

How far we are called upon to employ remedies which come to us from time to time with the repute of a special power to *cure* certain diseases, is an important and difficult question. I say emphatically, remedies *to cure*. New remedies to act upon this or that particular organ and its functions—new anodynes and sedatives, new diuretics, diaphoretics, or purgatives—are put to quick and easy proof, and the commonest observation cannot well err therein. They need only be given to a few different patients, and the question is soon settled whether they have or have not the virtues ascribed to them. Besides, we can easily understand how there may be substances unknown, or little known, or unaccountably neglected by the profession at large, which, being put to the proof, may turn out to have a power over certain organs of our body and their functions. These may be at length taken up by particular physicians, who, from practice, learn to use them skilfully; and in their hands they are made to serve the purposes of procuring sleep, or assuaging pain, or raising or depressing the pulse, better than the remedies ordinarily in use for such purposes.

Indeed, any new remedy of this sort is well worth knowing and trying; for it may help us in the successful treatment of twenty different diseases. But the remedy which can achieve the summary *cure* of the single disease is the thing with which we are now concerned.

Now, an uncomfortable sort of suspicion possesses me that physicians from time immemorial have prescribed, and still go on prescribing remedies, upon the credit they have got of

curing particular diseases, without the least reasonable evidence that the remedies ever did anything of the sort. The improved education of which we boast in our day ought to have made us less easy recipients of traditional errors, and more cautious in fabricating new ones of our own, and handing them on as evil gifts to our posterity. But it is not so.

When I was physician to a large hospital, having then the opportunity, it seemed almost a duty incumbent on me to gain an independent experience of sundry remedies which were much employed, with various degrees of credulity or faith in their curative powers. Indeed, I tried to do so; but (doubtless, from some fault of my own) I did not get on well with my task. Perhaps I was unduly sceptical, or not patient enough; or perhaps I was both. For men do not go to work with the same good will to detect what they suspect will turn out an error, as to confirm what they hope to find a truth. Moreover, in medicine (what men are scarcely aware of until they become somewhat severely practical), it requires as much labour and time fairly to lay hold of an error, and uproot it, and have done with it, as to learn and settle a truth, and abide by it. It is commonly to (what are called) chronic diseases that remedies said to *cure* have their application; each case spreading itself over a long time, is a case of mixed evidence; and, though a simple matter of fact is at issue, that evidence must be equally gone into, on whichever side the verdict may be in the end. Hence nothing is more certain than that numerous articles still encumber our materia medica, and still pass for being able to do something or other extraordinary, simply on account of the infinite time and trouble that would be needed to prove them utterly worthless.

When remedies claiming, however falsely, a special curative power, have been long in use, it is almost impossible ever to get rid of them. Their use then becomes a good deal like what the game of shuttlecock would be played by a crowd. The shuttlecock need never fall to the ground; there are so many to knock it about and keep it up. But, on such terms, it is a stupid game, or rather no game at all, and any left-handed clown can play at it.

Now it is only upon their first introduction into practice that special remedies are to be impartially examined and dealt

with. In their trial, physicians of hospitals collectively have it in their power to become to some extent the safeguards of the profession; and even individual physicians might do something to help us. Those especially who have bestowed much time and thought upon the investigation of particular diseases, whatever positive truths they may fortunately discover or explain, should consider their task still unfinished until they have pointed out the stumbling-blocks of false facts, false opinions, and false remedies; and made a clean sweep of them all. Strong in our esteem for the truths they have taught us, they would have equal authority in exposing error. But then they must not do it magisterially. They must admit us into their confidence, and make us sharers in their modes of proceeding.

There is a living physician who has made his profession doubly his debtors; for not only upon whatever subject he has written has he imparted to us new knowledge, but, what is better even than new knowledge, he has by his manner of dealing with it given us, moreover, a constant pattern to follow, whereby each of us may pursue his own subject safely and successfully. This physician is M. Louis. Now that disease which is pre-eminently the scourge and terror of mankind has been to M. Louis the chosen subject of thought and of research during half his life, even pulmonary consumption. And when, for this pulmonary consumption, he comes to take account of all that in his experience art can do, he is constrained to admit that its resources are limited to the one sphere of *treatment*. The sum of his own experience is in *treating*, not in *curing* it. Nevertheless, he deems his work incomplete until he has reviewed the professed experience and success of others in the way of *cure*. Thus, with a patience and impartiality worthy of all praise, he passes in review ten different substances which, under the shelter of good names and of seeming matters of fact, had gained the credit of a special power for the cure of phthisis.

The ten substances are—1, protioduret of iron; 2, chloride of sodium; 3, carbonate of potash; 4, sal ammoniac; 5, chloride of lime; 6, chlorine gas; 7, digitalis; 8, hydrocyanic acid; 9, creasote; 10, iodine. Of hydrocyanic acid, creasote, and iodine, he considers it only necessary to remark the utter

insufficiency of the evidence by which they come recommended to us as having power to cure consumption, and then leaves them without a trial. Of chloride of sodium, induced by his respect for M. Latour, who had introduced it and emphatically recommended it, he made large trial. He gave it to every phthisical patient admitted under his care into the Hôtel Dieu for five months successively; and in all that time he did not observe any change or amelioration in their condition which could be fairly accredited to the remedy. He saw nothing, in short, which is not constantly seen in phthisical patients placed under the same circumstances, and submitted to different treatment. He complains of the scanty and insufficient details in M. Latour's cases brought forward to recommend the remedy. Of carbonate of potash, recommended by M. Pascal of Strasburg with such poverty of detail as to leave it uncertain for what disease it was really given, though professed to be given for phthisis, he fairly excuses himself from making any trial. Of sal ammoniac or hydrochlorate of ammonia, recommended by M. Cless of Stuttgard, and of chloride of lime by Dr. Hirzoy of Posen, as curative agents in phthisis, he points to the brevity with which their cases are related to be such that they could hardly expect either to be believed or followed. Three substances remain, upon which he dwells at considerable length. These are protioduret of iron, digitalis, and chlorine gas. The care and patience and earnestness with which M. Louis betakes himself to treat of the first of these, the protioduret of iron, are remarkable. He starts with an evident desire and expectation of a favourable result; for he knew and highly regarded M. Dupasquier of Lyons, who introduced it into practice, and had employed it with much reputed success for several years. Having first taken care to make sure of the purity of the remedy, M. Louis administered it to upwards of sixty patients labouring under phthisis in all its stages, and in no single case did he observe any improvement which could be ascribed to the new agent. He cannot refrain from confessing his honest surprise and disappointment that, after the fairest trial, it should have utterly failed in his hands. Of digitalis, it appears that M. Bayle had reasoned himself into the belief in it as a cure for phthisis—not that he had reached it by experiment. He had collected the evidence of twelve eminent phy-

sicians, resulting in the conclusion that, of a hundred and fifty-one cases treated by digitalis, eighty-three recovered, thirty-five experienced benefit permanent or temporary, and thirty-three no benefit at all. And this conclusion (or something near it) M. Bayle thought himself in all reason bound to accept, upon a choice of alternatives; namely, to believe that eighty-three cases (or thereabout) of phthisis out of a hundred and fifty-one were cured by digitalis, rather than believe that twelve men of reputation and honour were mistaken in their diagnosis of the disease. But all these eminent authorities seem to have lived prior to the use of auscultation; and it is no disparagement either of their honesty or their skill to believe them quite mistaken, since they did not possess the means of a sure diagnosis. This is M. Louis' view of the question, and he does not think it necessary to make experiments purposely to settle it. During a dozen years he had prescribed digitalis to a few patients from time to time, without any positive result; and the occasional trials of others had been alike unsatisfactory. To make the heart beat slower by digitalis, and so believe oneself remedying consumption, is about as wise as it would be to think of improving the weather by playing tricks with the barometer, or altering the real time of day by tampering with the clock.

Chlorine gas, administered by inhalation, once gained much repute for the cure of consumption; and M. Louis takes certain cases, twelve in number, brought forward by M. Cottereau, as the most interesting within his reach, and makes careful examination of them one by one. To these examinations I would invite attention, as specimens of what is rarely met with in medical literature, and of what in the present state of our knowledge could be brought to bear on no organ of our body except the lungs. Each of them is a study in itself. It makes exact and discriminative analysis, by auscultatory signs and general symptoms, of consumption and of other diseases, as pneumonia and pleurisy, actually co-existing together during life; of their unequal rates of progress, and of the different effects wrought upon each by the impression of medicines, and the cessation and disappearance of the one while the other remains in its chronic state. Thus the conclusion results, that many of the cases in question were a complex of pneumonia or

pleurisy and a certain amount of tubercles: the acute and remediable part, pneumonia or pleurisy, being remedied, left the patients in a satisfactory state for the present; while the few tubercles or incurable part still remained: further, that a few of the cases were simple unmixed pneumonia.

The analysis of each of these cases, so clear, so discriminate and exact, I have recommended as a study. But it will be a study not alike profitable to all, but to those only who are already accurately instructed and well exercised in diseases of the lungs, and, moreover, who have love and jealousy enough of the truths of practical medicine to think great pains well bestowed in disentangling them from constantly besetting errors. Verily, there is nothing so self-deceiving and evasive, and upon the whole so mischievous, as this love of specific remedies. It is akin to the love of the marvellous. If it get strong hold of a man, it incapacitates him from becoming a good physician. And why this continual search after a specific remedy for consumption?

If I were to name any single disease in which medicine has pre-eminent claims upon the gratitude of mankind for the largeness and certainty of the benefits received at its hands, I should name pulmonary consumption. True, I may believe pulmonary consumption to be incurable in the sense of having no remedy which can be brought to bear with counteractive effect upon its own nature; and I may greatly fear it to be incurable, in the more popular sense, that once begun it can never be brought to cease, and so entirely to disappear as if it had never been. But still, by patient study of its pathology and of what it is in its own nature, by patient scrutiny of what it shows itself to be in individual living men; of its seat and its extent, whether small or large, and its several stages; now, of its oneness and simplicity, and now, of its mixtures and complications; of its rates of progress, whether rapid or slow; and what it gives to the vital being of those whom it befals, and what it takes from it; upon these several foundations we raise our great landmarks of the treatment of this disease. And thus, it has come to pass, that of the things in nature capable of acting upon man's body for good, every one, in its turn, has its just claim to be reckoned among the remedies of consumption in the way of *treatment*, but not one to be reckoned a

remedy of consumption in the way of *cure*. There are a hundred remedies for consumption, but no single remedy.

The result of these investigations of M. Louis is to raise the curtain and to make us see an unwelcome truth where we would much rather not have found it. It puts in a very strong light the fact that the origin and spread of these lying wonders, and the mischiefs they carry with them, must be largely ascribed to physicians themselves.

This is a serious matter. Yet it would be well to look it in the face, could we only make sure of doing so honestly and without offence.

To insure success to those who practise it, medicine does not require that their understanding should be of the highest order, but it does require that it should be sound. Of all human pursuits, medicine suffers the greatest detriment from a small intellectual flaw. Not that practical medicine does not present work fit to engage the highest understanding; not that it does not even require the highest to carry it to its greatest perfection. But, then, the higher the understanding so engaged, the more need there is that it is quite sound; otherwise, it will do the more mischief.

Whether we suppose a sort of flaw, or a certain vicious element in the original mould of a man's mind, something there is which all the world knows and speaks of as a reality. And what the world makes sure of, and is familiar with, it is apt to call by some homely vigorous name; and this it calls wrong-headedness. The name is almost descriptive of the thing itself; for, wherever it is found, it is always ready to hinder whatever is best within the man from doing its perfect work, and to lead to perverseness of opinion and conduct.

Now, this (so-called) wrong-headedness is found in various measures, from small to great; but, unfortunately, in practical medicine, a little of it goes a long way; it leavens a large lump. In every profession, business, and calling, with all the advantages of education and special training, men continually fail, from some touch of wrong-headedness in their composition, some trace of defective common sense; and truly that a number of clever, industrious, learned men should meet with nothing but bad luck in life is lamentable enough. Still, it would be well for the world if the worst misfortune that could be

reckoned were the misfortune of individuals; but the professions, businesses, callings, themselves suffer detriment and discredit and hindrance of advancement at the hands of those who are daily practising them and spoiling them. And, above all, the medical profession; not because there are more wrong-headed men in it than in other professions, but because a less amount of the fatal ingredient goes to vitiate its conclusions. Medicine has very few scientific tests at its command, and almost all its great questions are left to the sole judgment of well-instructed common sense.

Therefore, we need not be surprised that, taken upon a comparison with other professions, and having no more than its average proportion with the rest, of that which is naturally antagonistic to common sense in them all, medicine should suffer a far greater practical evil from it.

I have spoken of *well-instructed* common sense; for common sense has somehow (perhaps from its name) come to be taken for something coarse in itself and its operations, and only fit to be coarsely used. But there is not a thing in the world more delicate, or more easily spoiled, or requiring to be more delicately handled. Left to itself, it is not good for much. It must have culture and education; but very different are its issues, according as it is wrongly or rightly educated. It may be sophisticated into folly, or it may be cultivated into sagacity, which is its highest perfection.

Sad tricks have been played with our professional education of late years. Medicine justly claims kindred with all things that are best in human knowledge. But this is not a reason why every man who is to practise medicine should be set to learn them all. Let a man learn them all, if he can wield them all; or as many of them as he can lightly bear and freely use. But a small overweight of knowledge is often a sore impediment to the movements of common sense.

If half the scientific things now insisted upon as essentials of medical education were dispensed with to-morrow, the knowledge of them would not perish from among us. They are much too attractive objects of study not to find many of the best minds always ready to take them up and pursue them zealously and profitably for themselves and their profession; but by those who *must* learn them they are only half learnt, and serve them for no use whatever, but for a great hindrance.

Should ever a few of the most right-minded and experienced among us set themselves to settle, what is much wanted, a scheme of medical education the fittest and best of all, or for the great majority, and be quite agreed about it, and commend it to the world, it would offend by the fewness of its requirements, and so be unacceptable, and be rejected, and come to nothing; where the hitch and hindrance really lie is plain enough, and ought to be plainly told. Men are vain only about themselves; but about their clan, their calling, their profession, they are more—they are vain-glorious. Hence the extravagant expenditure of all the world upon *mere show*, socially, morally, mentally. The evil is absolutely incurable; and, alas! it bears most heavily upon the medical profession, whose basis is common sense.

Common sense is in medicine the master workman. With it a few good solid materials become the ways and means to practical results infinitely various and important; without it, materials ever so many and ever so good come to nothing or come to mischief.

Moreover, medicine has (so to speak) no one grammar, or orthography, or formal logic of its own. Men may spell it, and write it, and reason it, how they will, and have none to take them to task. It is the result only that is seen or appreciated. But, then, the more each man is left to himself, the more strict should he be in his self-questionings; for every man then, being (but in no evil sense) a law to himself, becomes responsible to himself for exact compliance with its requirements, according to his own consciousness of what is right *for him*. And, might we not add, according to his own conscience, too? For, often times, a man's intellectual self and his moral self must be mutual safeguards and fellow-labourers, if he is to work his way without mischance to the best conclusions. This is pre-eminently the case in practical medicine.

VI.—Cure.

ONE CHIEF CAUSE OF DAMAGE AND DISCREDIT TO MEDICINE AS A PROFESSION IS TO BE FOUND IN THE NOTIONS POPULARLY HELD OF DISEASES AND REMEDIES; AND IN THE USE TO WHICH THOSE NOTIONS LEAD OF PRETENDED SPECIFICS OR THINGS CLAIMING A SPECIAL CURATIVE VIRTUE.

The abuse of specifics or special remedies has been spoken of as it rests with physicians, when they choose them, and apply them, and misjudge their effects from evidence which, upon sober examination, turns out good for nothing. Now, should anyone be inclined to cultivate a little plausible ill-will towards physic and physicians, let him dwell for awhile in this unfortunate region of our practice, and he will easily pick up enough to confirm his own prejudices, and to convict us of weakness and credulity. There is nothing edible or potable in the world, which has not found somebody or other to eat it or drink it as a sovereign remedy for some disease, and upon the recommendation of some physician. The records of Medicine show all this; and ludicrous enough they are.

But there is an expenditure of worthless things for special remedies constantly going on, and never more than now, far beyond what can be accounted for from any mere fault of physicians. The world is much more in fault than we are. Indeed, all the world practises medicine beside physicians. And it holds pretty strong opinions on its own line of practice; and its opinion and practice influence our's more than may at first appear, and more than we may think or care to allow. Now its line of practice is chiefly in *curing*, and it deals largely in specific or special remedies.

Supposing, from the aggregate experience of ages, medicine, as an art, to have now gained a certain level of perfection, and supposing it to be always in good and skilful hands, there is that constantly at work in the world which is calculated to keep its practice below what it ought to be and what it might be, and

prevent the best physicians from doing their best, and lead the wisest among us to do all sorts of foolish things, or allow them to be done.

There are always two parties to the management of the disease—the physician and the patient. But the share of each would seem to be defined plainly enough; and that it was the physician's business to direct and the patient's business to submit. This, however, rarely happens in fact. Unless when he is so ill as to have no will of his own, the patient always interferes with the physician in some measure, great or small; and it is hardly possible that he should not do so. And truly his interference is sometimes for good, and the physician finds help in it. Too often, however, it is for evil, and to the hindrance of the physician; and its mode and measure may make the evil and the hindrance great and serious. Between patient and physician there arises a perilous chance that some such practice will result as is found to come from consultations between physicians themselves, bred in totally different schools, and of very unequal experience. Unluckily, the physician of perverse views and of little or no experience, is apt to be practically pertinacious and dogmatical. He sees no difficulties, and will have his own way. On the other hand, the physician of sound views and well tried experience, while he is ever ready to give you the full benefit of them, if you choose to trust him, is not the man to be for ever doing battle in their behalf, and forcing a way for them against strong prepossessions. Hence, in the management of any disease shared between such two physicians, it is odds but the handiwork of the former will be visibly predominant throughout.

The world, or, as we should say, "all those that are without," and that have not the estimable privilege of being physicians and yet keep their common liability to become patients; all mankind, in short, except physicians, even this entire world conceives all diseases that have a name as distinct essences, and of all remedies as distinct antidotes. Hence it has no sympathy and common understanding with us and our proceedings. The world, the educated world, has no other notion than that each disease has its own remedy; and that thus the whole practice of medicine really consists in knowing the disease and its antidote, and matching them fairly against each other. "You physicians

have not yet found out *the remedy* for hydrophobia." "Here is cholera come again, and you physicians know nothing more about *the remedy* for it than the first time it appeared among us." "Here is this diphtheria making cruel havoc, and you seem to understand a good deal about it as a disease, but none of you has any conception *what* is the *cure* for it." All such common impeachments of our shortcomings plainly imply the prevalent notion, that physicians have but two things to learn, and have learnt only one of them, and not always that.

It has been a matter of painful concern to me half my life, to see my profession lightly esteemed by many wise and good men; owing, I believe, to the utter impossibility of anybody being brought to understand what that profession really is in any other way than by partaking of its peculiar experience—*i.e.* by practising it. The distinction, plain, simple, and needful as it is, between *curing* the disease and *treating* the patient, is one of those things which cannot be comprehended otherwise than by being daily conversant with diseases and remedies. It requires to be wrought into us by force of the facts, the peculiar facts, of practical medical experience.

I had a friend, who was a lawyer, and had held a judicial office many years; and he had a son, also a lawyer, about 30 years of age. The son was taken ill of a fever, and was put under my care a few days after its commencement. The fever was what we now call typhoid. I took some pains to make the father understand all that he was most interested to know about it; how that it had a certain course to run; that it was the business of medicine to avert or to mitigate its contingent evils, and so carry it safely to its end; but that there was not the thing in the world which could cut it short, and at once and directly *cure* it. He seemed to comprehend all I meant perfectly, and to assent to it; and, upon the terms of this preliminary explanation, I proceeded to do my best for his son.

The case went on hopefully in the main, but not without circumstances of anxiety enough to make me glad of having another physician called in consultation. Yet, notwithstanding the two physicians, such circumstances did not become fewer or less pressing as time advanced. And then the father announced to me his resolve that he would have physician after physician, until he hit upon one who knew the *remedy* that could *cure* his

son's fever. Luckily, on that very day the symptoms began to clear up, and hope was more legible in them all; and so the son was spared the peril of the paternal experiment. In due time the young lawyer got well, leaving the old lawyer to meditate the mystery, how in the world it is possible for mortal man to get rid of a disease for which there is no *cure*.

This instance strikingly shows the fact, and might have shown the mischief, of the common belief, that the practice of medicine consists altogether in the giving of antidotes. "Try this. This not succeeding, try that. That and this both failing, are there not yet things which are reputed sovereign for the malady? Try them all one after another. None of them succeeding separately, still do not despair; try them all at once." The matter, thus plainly put, looks ridiculous in the eyes of every experienced and thoughtful physician. But the world—the greater part even of the well educated and intelligent world—would see nothing at all ridiculous in it; it is simply their notion of the practice of medicine. The popular notion has, and it cannot help but have, a momentous bearing upon medicine and its fortunes, as a profession. The world has great faith in physic upon the whole; but its faith is of a wrong sort. It is not a belief, but a superstition.

To find the particular antidote for the particular disease being the popular notion of the practice of medicine, it unhappily comes to pass that no small portion of mankind have actual motive enough for demanding these antidotes at our hands, in cases where they can never be forthcoming—cases of irremediable disease. And it is these, even these, that furnish the principal field of trial for all worthless things, and the principal source from which they issue and pass current for specifics or special remedies. In truth, the amount of irremediable disease in the world is enormous. There are small tuberculous deposits at the apex of a single lung or elsewhere, which may be slow to advance, or remain stationary for years and years. And there is acute fungoid cancer sweeping and destroying all organic structures before it, and killing in a few months. And between these two there are diseases of various kinds—some limited in their seat, some wide-spreading and diffusive, some slow, some rapid—all admitting of various degrees of palliation and postponement, but not to be got rid of, and all in the true

sense irremediable. Now no diseases have been so well studied as these; none have had, none still have, so much thought and experimental industry bestowed upon them by the best minds. And all for what?

In this working-day world of ours, we must take things according to their kind. If the greatest good be not attainable, we must not count all that is short of it for nothing worth. If the highest mark be beyond our reach, we must content ourselves with a lower aim. By all his knowledge and experience, the physician is convinced that, in certain diseases, he can prosecute neither cure nor treatment to the restoration of perfect health. But his office does not therefore cease. Still he can palliate; still he can postpone; still he can procure years and years of life and usefulness for the subjects of irremediable disease. And I will be bold to say, that the prime condition of his ability to do all this, is his knowledge that the disease is irremediable.

Further; it is no paradox, but an experimental truth, that the physician holds his power of doing good in incurable diseases so long only as he does not attempt to cure them. And his power of doing good is very great.

The world may think, naturally enough, that the knowledge (howbeit only to be procured by hard study) of what is incurable and intractable in the nature of diseases, must indispose physicians for putting forth the full power of their art, and lead them to make too easy and premature surrender of its possible masteries. But let mankind have no misgiving on this point: our fault is the common fault of all. Oftentimes, in particular cases, we catch ourselves at work fabricating a sort of fictitious faith, and setting it up against all the experience of our lives, sooner than give the disease up for incurable. We believe, or try to believe, against knowledge; and so we run a perilous chance of *acting* against knowledge, and doing mischief.

There is hardly a man living, be his disease what it may, who will bear to believe himself beyond the possibility of restoration to health. He will allow the physician to profess palliation, and postponement, and relief; and, when the physician does all which he professed to do, he will be thankful to him, but he will think it strange that he can do no more. And so he lets go his faith and his allegiance, and goes in search of some one to cure him by a lucky remedy.

Thus the comparison of what we *can* do with what we are expected to do, is often mortifying enough. But it cannot be helped. People are free to expect what they like; while we cannot do more than can be done. Our ability is bounded by the nature of things, at all events.

The physician gains strange insights into the weaknesses of poor human nature. Men are found to cultivate credulity, and take refuge in it against the convictions of common sense, until they bring themselves to believe impossibilities. An eye, or an ear, or a large joint irreparably damaged by some bygone disease or injury, is doubtless an evil hard to bear; but, the fact being demonstrably certain, nothing remains but to acquiesce in it. Therein, however, consists the moral trial, of which we physicians are the privileged spectators and judges; and, summarily, this is the result of observation. The poor, and those who live by their labour, and to whom the loss of an eye, an ear, or a joint, is a serious hindrance in their daily work, *do* acquiesce in it well upon the whole. They do the best they can without them. They learn new shifts to supply their defects, and so contrive to forget them, and get on pretty comfortably altogether. Not so those who are delicately brought up and are well off in the world; not so those especially who have no indispensable duties waiting upon their each day's thought and each day's labour; yet they have their own (perhaps many) resources always at hand, and always ready to serve them if they choose. But when the mind needs physic, the dose must be compulsory to do any good. So many come to cultivate credulity as a better expedient, and some with such success as to cast anchor upon downright undeniable delusions, and rest there with much content.

I knew a man who was totally blind of one eye. The eye was gone. Disease had abolished it years ago; and, in its stead, all that now remained was a little round button of gristle or cartilage, to which the muscles still had an attachment, and moved it about. He was a man of fortune, and had had the best professional advice, and must have been made acquainted with the true state of his case. He was also a man of accomplishments, and his society was much sought and cultivated. Yet he was going twice a week to a notorious quack, and having something dropped upon what was once his eye. Thus he had

brought himself to believe that lucid glimmers followed each application of the nostrum; and this happy delusion kept alive in him the hope of returning sight.

One would willingly believe this to be a very rare specimen of self-deception; but really it is not so. The experience, I find, of many physicians has furnished them specimens to match it. But surely it cannot be that any man, educated for our profession, should have the requisite ignorance and the requisite dishonesty to share and to promote so vast a delusion. The transaction must always lie between the patient and his favourite quack.

But let us concern ourselves only with actual diseases, diseases existing and in progress. And of these let us ask whether the fact that they are, or are deemed to be, incurable or intractable—the fact that there is no medicine or method of treatment known by which they have ever been successfully managed—whether this fact be enough to warrant physicians in doing and trying anything or everything indiscriminately upon them?—enough to justify or excuse us in falling in altogether with the world's notions, and adopting the world's practice of medicine, as far as they are concerned? I think not; for this would be mere gambling with drugs, and not the practice of medicine. Gambling, too, it would be of the silliest kind; the chances being incalculably against you. For thus to try this, that, and the other thing, implies an expectation of finding in some one of them a specific or special remedy for the particular disease, well knowing how very few such remedies exist in the whole world. The absurdity is neither more nor less than that of a man, who should trust the payment of his bills to the chance of finding a bag of money.

But there is, it must be confessed, a good deal done every day by eminent physicians which we cannot honestly compliment and call it the practice of medicine. Bankruptcy of means puts good men often upon strange shifts to stave off pressing obligations; and so they must be excused.

The conditions under which we are called to act are not seldom such as to disturb all sober reckonings of the means and methods which we employ, confounding even their choice, their uses, and their effects. Irreparable disease, with its pains and its perplexities, its fears and its despair, is apt to raise a very

importunate cry. Ask what it will, in very pity it must be granted. But it asks impossibilities; and when it does, the physician in very pity must attempt them for it.

But where nothing is expected, the least thing is exaggerated by surprise, and comes to pass for something great. Morally, no extremes so nearly meet as credulity and despair. Thus cures are reputed for incurable diseases, which confessedly remain as incurable as ever; and wonders are accredited to particular remedies, which can never be got to do the like wonders again. Physicians, who have worthily achieved great reputation, become the refuge of the hopeless, and earn for themselves the misfortune of being expected to cure incurable disease. They find a necessity laid upon them of attempting something when they have faith in nothing. Thus, for all who resort to them, they must needs prescribe a remedy. But acquiescence for long in any labour felt to be quite useless is intolerable. And so physicians themselves are driven to get up a sort of fictitious faith in certain remedies which hard necessity forces them often to use; even if it be but a faith to this small amount, and thus faintly expressed, "that they do not know but they may have seen them do good in a few cases."

What a penetrating insight had they of old into occult moral truths; and how they sketched them to the life in their beautiful mythologies! Thus they fabled some curious "penal servitudes" in their world below, typifying the torment of labour with utter uselessness. But, could the unhappy ladies have stopped only one of the hundred holes in the leaky cask, they might perhaps have deceived themselves into a little heart and hope under the circumstances. And surely many a sensible physician must half-willingly deceive himself, or he never could go on prescribing the things he does under any necessity.

Now even we physicians have a motive for being careful of acting foolishly, lest possibly we may find followers. Remedies known to be prescribed habitually by the best of us, but *not* known with what little or no faith, are apt to make sad havoc with the faith and practice of the profession at large. A physician plausible, clever, and very active, a favourite and an authority, perhaps, with people of the world, may, if he be but right-minded, be a credit and a gain to us all in the position which he holds; but a little wrong-headedness, or a little

credulity, or his own opinions half worked out, and consequently a facile compliance with the opinions of others and even with the will and pleasure of good people who can know nothing about the matter, must in the main greatly damage his own usefulness, and make him, all unconsciously perhaps, a worker of mischief to his profession; as, in other respects, so especially in this, that he keeps alive within it a talk and a fuss and a barren interest about a pack of foolish things, which are utterly worthless.

Thus it is beyond question that the adoption of special remedies for diseases upon insufficient grounds is a main hindrance to practical medicine, constantly withholding it in its progress to that degree of perfection which it is capable of. A motive for their adoption, and a strong one, has been seen in the sway which the world is ever exercising upon us in the manner explained. Another motive, also a strong one, has not been concealed—the acquiescence and credulity of ourselves. There is no doubt of the fact that what may be taken up to-day without reason or proof by a plausible enthusiast or two will have the multitude of the profession using, and believing in, and swearing by, a few months hence; and thus it will gain a credit from which the contrary experience and sober dissent of well-judging minds will never be able to set us free. Unfortunately, where there is no experiment of exact science to settle the matter, it takes as much time and trouble to pull down a falsehood as to build up a truth. Only let the most worthless nostrum get backed by the credit of some good name, and it will never cease to pass current for something in the world, and will never be altogether got rid of from our materia medica. Thus, upon the whole, it is sad to think how much of the practice of medicine is blindly engaged in a busy, noisy workshop of impossibilities.

VII.—Cure.

REMEDIES TO BE ACCEPTED FOR SPECIFIC OR SPECIAL ONLY ON THE CONCURRENT TESTIMONY OF THE BEST AND THE MOST EXPERIENCED MEN.—SUCH REMEDIES FEW.—GREAT VALUE OF THOSE FEW: 1. FROM THEIR POSITIVE EFFECTS; 2. FROM THE HINTS THEY FURNISH OR CONFIRM OF THE DEEPER AND MORE OCCULT ELEMENTS OF DISEASES.—THEIR MIXED USE.—FAITH IN THEM GAINED IN ACUTE RATHER THAN IN CHRONIC DISEASES.

The abuse of specific or special remedies, whether by physicians or by the world, is a heavy clog and drag upon the credit and fortunes of our profession. How it came to pass, and how it always has prevailed largely, and always will and must prevail, may be gathered from the explanation which has been given. Should this be deemed too much of an explanation, let its motive be its excuse; namely, that thoughtful physicians may be made more fully aware how great the evil is, and may guard against it in their own practice and discountenance it by their example, and thus hope to abate it a little. But they must never hope to put it down until they can persuade all their brethren to practise medicine wisely, and all the rest of the world not to practise it at all.

Now let us resume the subject of *cure* and special remedies where it was left off, and let us speak no more of their abuse. For have we not seen an obligation to use them in their own unaccountable success; and have we not seen within themselves promises and prophecies of future knowledge? (*Vide* article III.)

The *specific* remedy and its certain *cure*, and the *special* remedy and its probable *cure*, will bear to be called "great facts"; and, indeed, they are much to be admired. But, to be so called and so thought of, they should bear the royal stamp —the stamp of experience. They should be current at sight among the wise and prudent, and pass with them for as good

as gold. And, verily, such specifics and special remedies there are. But they are few; few and precious—precious in themselves and beyond themselves. For it cannot be that they should stand alone; they must have kindreds enough of worth and value, if we could but find them out.

True it is, that these same remedies officiate something which we do not understand, and in parts of the body where we cannot follow them. We know neither what they do, nor where they do it; yet the effects of their operation are positive and unequivocal. They have been fairly surmised to have to do with those first elements of disease, which are peculiar objects of interest and research in our own times, and which have their seat within the blood. Perhaps there would be no harm in following this surmise a little way in the direction to which it points.

For, to be long busy about some spot where truth has seemed to dwell, and to take up often and examine often the curious things that are to be found there, and join them and separate them, and separate and join them again and again, skilfully or unskilfully, is not this the common lot and probation of those who have happily made them fit at last? And this fitness have they not welcomed for a notice of some truth, which they sought, being not far off, and have gone on their way rejoicing.

In like manner we would deal with these great remedies, even the special instruments of *cure;* and, before we take leave of them as a formal part of our subject, we would take some of them up and handle them a little, and try what fitness they have, or seem to have, with the things they approach, or touch, or mix with, and so gain what better insight we may into the manner and place of their surprising operations.

Let us begin with the most familiar instance. No remedy does its work so summarily and well as Peruvian bark when it cures an ague. And when it cures other diseases, as sometimes it does, it still does its work summarily and well. But how and where does it do it? The man who finds out that, will make a great discovery. But already one cannot help but see much plausible fitness of things that cluster about the matter, which tells of truth being somewhere near at hand.

All know it for a fact that, by accident or design, noxious

elements are constantly gaining entrance within us, from which it is the perpetual business of the body to set itself free. If the element be such as specific or special remedy can reach *curatively*, the body is spared a world of trouble, pains, and perils in regard of it.

There is a certain noxious element of great activity and power, and wide-sweeping operation, which seems to be *sui generis*, and Peruvian bark is its antidote.

It belongs to intermittent fevers essentially and inseparably, making Peruvian bark their remedy always and everywhere, and in every body. And it seems to mix itself with other fevers and other forms of disease accidentally and occasionally, calling for Peruvian bark to aid their cure at certain places and seasons, and in certain men.

But this same material morbid element, which has Peruvian bark for its antidote, goes, after its entrance within the body, nobody exactly knows whither; yet we guess it goes into the blood, and that there the special remedy reaches it and deals with it, and somehow renders it harmless. And whoever chooses to adopt this theory of its *curative* operation, may have the satisfaction of knowing it to be in respectable conformity with pathological views now commonly entertained.

Let us briefly run over these views, and so judge of their fitness to furnish us convictions or surmises of the place and manner where and how our specifics and special remedies have their operation.

Pathologists are coming more and more to the belief that diseases are developed from elements which have their first existence in the blood. And, indeed, there is sure proof of the fact in a few instances, as where sugar, or urea, or pus, are found therein; and there is next to sure proof of the fact in contagious eruptive diseases. We know that they have a material element. Entering the body, whither can it go but into the blood? Where can it germinate and increase but in the blood? Whence can it be diffused throughout the body, and be finally separated by the skin, but from the blood? Further, by a very strong analogy, it commends itself to our reason to believe that malarious diseases, from the least to the greatest, from the headache a man has suffered for a few days while a drain has been offensive in his house, to the fevers con-

tracted in the Pontine marshes, have all their origin from a foreign and noxious element gaining access to the blood.

All this, and more than this, the popular language of medical men has already sealed and certified as true. We talk of almost all diseases as blood diseases, and of their elements as poisons; of the scarlatina poison; the typhus and typhoid poison; the cholera poison; the poison of gout, of rheumatism, of erysipelas; the poison of boils and carbuncles. We speak even of chronic constitutional diseases as live-long poisons, as the poison of struma and cancer.

Without doubt this language may be taken in some extent to express plain and almost demonstrable truth; and, again, in some extent to express truth of very high probability. But then, in a much larger extent, it means nothing beyond surmise.

Now surely there is an evil in using language more precise than our knowledge. If this were the place, it would be easy to show that medicine has partaken especially of it. Its language has almost always outrun its knowledge. And the evil has been nothing less than this: our language, by ever persuading us that we are wiser than we are, has wedded us to many a capital error for half a century.

The theory which we have been propounding is sober and plausible enough; but it has its weak points. Yet some would think it complete throughout. Let us, however, beware: for, accept it in its totality and without reserve, and give it free play, and see what a wide extravagant sweep it will presently take. The theory is this; that the bark and all other remedies whatever, which *cure* without any intermediate operation being apparent, do in fact work their *cure* by finding each that element of disease in the blood for which it is the natural antidote, and there dealing with it and casting it out. But let us beware: for the theory would go to creating as many elements or quasi-elements of disease in the blood, as there are specific or quasi-specific remedies in the world; and a precious nosology we should have of it! It is all very well to hold the belief that diseases and cures are largely transacted within the blood; there are facts enough pointing to it and confirming it. But there are not facts either enough or of a fit kind to furnish out anything like the perfect theory contemplated. The thing must wait.

The number of facts within the knowledge of every experienced physician is enormous; and every thoughtful physician has it perpetually in mind to put them in their natural order, and to learn their true meaning. But then comes the temptation common to all minds that are speculative (and what mind that can think at all is not speculative in some degree?) —the temptation, namely, to press known facts a little further than they will bear, when that little is all that is wanted to establish a theory. Even sober minds are too apt to yield compliance to this temptation. But we tread upon tender ground. Let it be enough to notice the extremes which are, and always will be, found among the best of those who study and practice medicine.

There are those who are ready to work every bit of new knowledge they pick up into the common fabric of what they have in daily use and exercise; this is a great temptation and a great fault. And there are those who, always busy in acquiring knowledge, are yet so religiously on their guard against erroneous theory, that they are afraid even of generalising. Is not this sometimes the case with Heberden? Doubtless this too is a fault; but then it is so rare, and where it exists, it so often springs from that charming union of a modest with a powerful mind, that it easily claims for itself a merit, and even passes for virtue, in our esteem. The great and frequent fault of our profession is of the other kind—to be premature and precipitate in systematising new knowledge; for thus some of the best minds have been lost to the advancement of practical medicine.

Something remains to be added respecting Special Remedies; but it would be altogether a mistake to attempt any orderly account of them. A pretty sure insight into things, be they what they may, is required to systematise them profitably. To be exact beyond our knowledge is the worst of pretences. All arrangements of facts aim at being didactic; but then they must be arrangements into which they naturally fall, not into which they are forced. The facts which concern Special Remedies are far too interesting and important not to be spoken of; but they must be spoken of somewhat discursively.

Now, have these remedies anything like a domain to themselves, be it large or small, in the cure of disease. It is difficult

to say whether they have or not. The disease pre-eminently deemed curable by its own specific will, in a few instances, refuse to be cured by it without the help of some remedy suited to emergencies of the individual patient. Ague commonly yields to quinine; but occasionally it does not until some mercurial be given to redress a present wrong-doing of the liver. Gout is often cured by colchicum single-handed; and erysipelas single-handed by bark. But gout from colchicum, and erysipelas from bark, often obtain something short of cure, which remains to be completed by common or non-specific remedies. Or take the converse, which perhaps shows the fact as it more frequently appears. Gout is treated by remedies suited to the requirements of the particular case; and they fulfil this, their immediate intention, and bring the whole disease *nearly* into subjection. The patient is almost, but not entirely well; a trifle of gout still lingers, and vexes him pertinaciously; you give him colchicum and at once he is well altogether. In like manner, erysipelas is treated by remedies suited to the particular case, and they answer their immediate purpose. Moreover, the whole disease is manifestly kept in check; it is reluctant to spread; it fades; but it does not disappear until the patient takes quinine, and then it at once disappears altogether.

Thus, brought to the test of how they demean themselves under the trial of certain special remedies, diseases have much that is mysterious to tell us concerning their nature. Truly, in this regard, they would seem to change their natures from time to time; or the same disease to partake of a double nature, as when it is curable in part by a special remedy, and treatable in part by common ones; or to share that double nature in variable proportions, being now almost curable by a special remedy with very little help from any common one, and now almost treatable by common remedies with very little help from a special one.

How the two orders of remedies seem conjointly to analyse the nature of the diseases with which they have to do!

There is a good deal of difference how we regard the effects of the same special remedy, according as it is brought to bear upon an acute or a chronic disease, and as its own operation is rapid or slow. The nearness or remoteness of the result

strengthens or weakens our faith in the power of the remedy. If some sure earnest of the coming result follow the first use of the remedy, and the result, even the *cure*, be complete within a few days, then we have all that reason can ask for faith in the special remedy from first to last. But if the result be postponed for weeks, and still postponed for months, and the earnests in the meanwhile, upon which hope likes to lean and to refresh itself, be few and long between—then, though the result arrive at last, even the perfect cure, the reason gives but a hesitating faith to the special remedy for its share in procuring it.

So when quinine is prescribed for an ague, or for any rapidly progressive disease to which fever belongs, such as erysipelas or acute rheumatism, and it ceases abruptly or soon, or in a few days, our faith in the remedy as of special efficacy is complete. But when quinine is given for strumous ophthalmia, or for swelled absorbent glands of the neck, or for any complaint of whatever name which has the accompaniment of a weak and cachectic constitution, and the ophthalmia or the swelled glands disappear and health follows after the lapse of months, then our faith in the special remedy, as such, gets a good deal frittered away or is exchanged for the more moderate expectation of good which waits upon the use of tonic medicines. It sinks from a specific to a tonic, which is a great fall. In like manner colchicum, given for the paroxysm of gout, and at once abating its severity and finally abolishing it in a few days, is justly regarded and trusted as a special remedy; but given and continued week after week for chronic pains of doubtful pathology, and those pains in the meantime slowly and reluctantly yielding, and at length giving in altogether, we thus lose faith in colchicum as a specific for gout. If we allow it any credit in the result, it is of another kind. It sinks from a specific to an alterative, which is a great fall.

But let us beware of all debate about "tonic" and "alterative"; which, whether applied to remedies or to their operation, are most without meaning of all the terms in use among physicians. Tonics and alteratives include a countless heap of things, and get much of the credit they have as a class from a few remedies, passing by their names, which better deserve to be called special or specific; as cinchona when it cures other

diseases besides ague, and sarsaparilla when it cures chronic syphilis. As to quassia, and gentian, and cascarilla, and cusparia, and orange-peel, they should rather be reckoned as condiments for food than as medicines.

This may seem a hard sentence to pass upon time-honoured remedies, and so to dissever them from the art altogether. Within the memory of man, bitters were in high esteem, and their virtues were nicely studied. There were those who could tell you which was to be preferred to which, and were not to be caught confounding their effects, and giving one when they ought to give another. The abundance of drugs to be taken was a chief characteristic of medicine as it was practised forty years ago, and not unnaturally it did its best to believe or make-believe in their efficacy.

Here we take leave of Cures and Special Remedies as a formal part of our subject; but they will still be meeting us at every turn. In truth, what we have called *special* and *common* remedies never so well display their powers as when they are seen at work concurrently or in conflict with each other. Remedies, indeed, are our great analysers of disease; common remedies more plainly and sensibly, special remedies more intimately and profoundly, but both alike in fact. Of *living* disease hardly any analysis is possible but by help of both.

VIII.—Treatment.

TREATMENT DISTINCT FROM CURE.—CONVERSANT NOT IMMEDIATELY WITH THE DISEASE, BUT WITH INTERMEDIATE PURPOSES.—WHAT IS MEANT BY INDICATIONS OF TREATMENT.—CHOSEN FROM PRESENT SYMPTOMS.—NOT NECESSARILY THE MOST PROMINENT SYMPTOMS.—OR NEAREST AKIN TO THE PARTICULAR DISEASE PATHOLOGICALLY.—OFTEN COMMON ALIKE TO MANY DISEASES.—AND THE LARGEST AND MOST COMPREHENSIVE.

We come now to consider *Treatment* as distinct from *Cure*. Treatment seeks to abolish the disease after fulfilling one or several primary and intermediate purposes aimed at and intended by the physician. These intermediate purposes are called Indications of Treatment. They are, upon the whole, of much variety, and in each particular case may be a single one, or few, or many; and, accordingly, the means or remedies by which indications are sought to be fulfilled are various upon the whole, and in each particular case may be a single one, or few, or many.

Now, it is in the *Treatment* of diseases as distinguished from their Cure, that our knowledge comes chiefly into play. Here it is that by our knowledge we discern and choose the aims or indications, be they many or few, which are intermediate and conducive to the ultimate scope of all, the restoration to health. And here, again, it is by our knowledge that we select, and apportion, and apply the means and remedies, be they many or few, which are fitted for each particular aim or indication we desire to compass. Often, indeed, we thus feel ourselves (as it were) working with our knowledge experimentally and in detail, leaning on it, trusting it, step by step. Further, we are thus often able to survey retrospectively (and it is a most profitable exercise) the course and management of particular cases, and their results; and see where we have done well, and where we have done ill; where, from treatment

perfectly carried out in all its details, restoration has followed as the natural and necessary consequence; or where, from mischance or mismanagement, from defect or misuse of knowledge in this or that practical detail, restoration has been postponed, or left imperfect, or has failed altogether.

The practice of medicine, when it is thus concerned with *Treatment*, and proceeds upon the choice of aims or indications, and of means and remedies fit to compass them, has pre-eminently been called "Rational." It is a boastful appellative; well enough for those who are full of hope and expectation, at the onset, of what knowledge is calculated to do and should do, but too high-pitched for those who have made honest trial of what it can do and has done. Men feel and speak differently in putting on their armour and in taking it off. Truly, the practice of medicine best deserves to be called "Rational" when it is concerned with *Treatment*. But then, as such, it has had its limits of success, which leave room enough still for plenty of hard, well-directed research, but none at all for vain boasting.

On the other hand, the practice of medicine, when it is concerned with *Cure* has been pre-eminently called *Empirical*. And it has often made its boast of the name, and been pleased with its significance as contrasted with that of *Rational*. It has rejoiced to acknowledge elements of disease deeper and simpler and more powerful than any which it can sensibly apprehend. It has used remedies given it by chance; and, knowing no natural aptitude they have for their purposes, it has trusted bare experience for their effects, and has not been disappointed; remedies surer and further reaching, and of more special virtue, than any which industry, and science, and skill, and use, have enabled "Rational" medicine to know, to choose, and to apply.

Thus, from time to time, the extreme "Rationalists" among us have professed to know more than can be known; and the extreme "Empirics" to believe more than is credible. Each, in their turn, have done signal detriment to practical medicine. The physicians who have most benefited mankind have been neither rationalistic nor empirical by pre-eminence, but a fair mixture of both; for faith and knowledge lean largely upon each other in the practice of medicine. The mainstays of our faith would be often giving way but for the contrivances of our

knowledge; and the insufficiency of our knowledge would often leave us helpless but for the resources of our faith.

It lies at the foundation of all successful Treatment to understand the intermediate purposes which medicine has to fulfil, in order to reach the disease remedially. And these are to be sought for in some present conditions of the individual patient. To determine which they are, and determine how far they can be trusted for guidance in the Treatment of the disease, this it is which constitutes the doctrine of practical indications.

Now, all conditions of the patient, which belong to him as such, fall within the category of symptoms. And by his symptoms it is that we get what knowledge we have of his diseases, and what guidance we need for their Treatment.

But what is it that makes the *practical* indication? What are the properties which, belonging to certain symptoms, mark them out as guides for treatment.

Are the symptoms which suggest the remedy more prominent and more striking than others from their mere magnitude? Sometimes they are, and sometimes they are not. Often, indeed, they are so faintly marked, that they might easily escape the notice of those unaccustomed to search after them. The patient wonders at the indifference with which the physician regards his greatest grievance or pain, and at the attention he gives to some paltry circumstance (as it appears), or to some minor uneasiness which he is himself hardly conscious of, and makes it the scope of the remedy. Yet, although symptoms are not made guides to practice by their mere magnitude, do they not become such by virtue of the real pathological import belonging to them? Now, their pathological import must come from some essential connexion between them and the disease; and this connexion, by how much the stricter and closer it is, must make them pathological in a higher degree. Names have been given to symptoms for the purpose of marking the nature of this connexion. Some are called pathognomic and some diagnostic; names which almost carry their own meaning, and scarcely need explanation. The symptoms which characterise diseases in their essence (as far as we have an insight into what that essence is) are called pathognomic; and the symptoms which distinguish diseases from

one another in their nature, or which distinguish them in their seat, are called diagnostic. These have all been analysed and arranged, and classified, with some show of philosophy and some success, and hence our knowledge of diseases, within a certain range, has been made easier and more precise. But then comes the question, do these symptoms which claim for themselves the highest pathological import, even these, the proper pathognomic and diagnostic symptoms of the disease, claim, therefore, to stand pre-eminently for guides and indications of its treatment?

As to pathognomic symptoms sometimes they do and sometimes they do not. The heat, the redness, the swelling, which are pathognomic of external inflammation, largely suggest and regulate the methods of managing it. But the pustules of small-pox and the rash of measles, which are pathognomic of each and due to the proper essence of each respectively, have little—I will not say nothing—to do with the right management of either.

As to symptoms called diagnostic (as we have only to do with the practical uses of things, and have no further regard to names than as they can help us in that behalf) it is fit to remark that they and the pathognomic are commonly the same. The symptoms which, under the name of pathognomic, have already served to denote what the disease is in itself, are the same which now, under the name diagnostic, serve to distinguish it from other diseases. The pustules of small-pox and the rash of measles having bespoken the essential nature of each, were rightly deemed pathognomic. But, pathognomic as they were, they did not suggest the methods of treating either. And now, the pustules of small-pox taken to distinguish it from measles, and the rash of measles to distinguish it from small-pox, are rightly called diagnostic. But, diagnostic as they are, they do not suggest different methods of managing the two. The same symptoms do not alter their practical value by changing their names. Neither do the symptoms, which are diagnostic in the sense of discriminating the seat of diseases, demand for them, except subordinately, a treatment respective to the parts they occupy, or require that the same disease—inflammation for instance—should be dealt with differently in each of the many organs it may chance to befall.

Yet all these symptoms (I would repeat) are eminently pathological; all of nearest akin to their diseases, specialising, severalising, localising them; the symptoms by which we gain all the knowledge we have of what diseases are in their own nature, all our discernment of one disease from another, and of the part which any disease occupies during the life of the patient.

Small-pox and measles have been mentioned for the sake of their contrast. But what has been said of them may be equally said of almost all other diseases, especially febrile, which have a distinct essence, and symptoms flowing from that essence which are properly and exclusively their own. It is emphatically true of them, when you come to treat them, that the symptoms proper to each and distinctive of each, nearest akin to its essence, and giving to each its stamp of individuality, recede and give place to others which they have in common; and that these latter become the just indications of practice, and serve for guidance and discretion in the use of remedies.

Well! but touching the symptoms which, as indications of treatment, practically supersede those of the highest pathological import: what are they, and where are we to look for them?

Typhus fever, typhoid fever, scarlet fever, variolous, rubeolous fever—all these diseases have severally an element of their own; and the element of each one is different from the element of every other. But why do we call them all fevers? Fever is the proper element of none of these. Yet fever is, so to speak, the motive power of them all, and common to all. Assuredly, their outward presentment has always the accompaniment of fever. Fever is conditional to their known existence as diseases. They cannot come into known activity without fever. Yet each has a pathology of its own prior to the fever. We are quite sure of the fact, although we can make no use of it, or extract anything from it which may serve as a guide to practice. Our treatment must needs wait for the fever. It can only begin with what it sees. It cannot take aim until it catches sight of a mark.

Of these several diseases, the proper pathology and its ingredients (whatever they may be) are unapproachable by medicine. It is through their common pathology, through

their fever and its ingredients, that they are brought within our sight and our knowledge, and the reach of our remedies. At present, let it be enough to declare of fever, that it is constituted of cold, heat, and perspiration, in various measures and in various forms of combination; and to point to the entire vascular system and nervous system, now acting and suffering beyond and beside the uses of health, as engaged in carrying it on. Fever thus regarded is full of indications for the treatment of all such diseases as have been mentioned. And it is with the two great systems, the vascular and the nervous, in their largest extent and reach, that we have to do, whenever we deal with these indications.

Fever, or its ingredients of cold, heat, and perspiration, take the first place for comprehensiveness and exigency among symptoms which are guides to treatment.

It is through the vascular system as a whole, and the nervous system as a whole, that the body makes chief display of its vitality.

VIII.—Treatment. Part 2.

INDICATIVE SYMPTOMS OFTEN SINGLE AND SIMPLE.—THE QUALITY OF A SECRETION.—THE BEAT OF AN ARTERY.—A LOCAL PAIN.—CONTRAST BETWEEN THESE AND THE MORE COMPREHENSIVE.—EXCELLENCE OF SIMPLE TREATMENT, GROUNDED ON AS FEW INDICATIONS AS POSSIBLE.—ATTAINABLE ONLY BY LONG EXPERIENCE.—SUCCESS OF SIMPLE TREATMENT NOT A SURE EXPONENT OF THE DISEASE PATHOLOGICALLY.

It happened, at first, in the mechanical arts, that every operation had its own proper instrument, until, in process of time, some simple power was successfully applied to many different purposes. Something of the same kind may be remarked in the progress of practical medicine. (The things themselves have no natural alliance, but still enough of analogy to assist in explaining what I mean.) There was formerly an evident industry for combining in the remedy whatever was thought to be of virtue for each of the symptoms which constituted the disease. Hence, various nostrums were in daily use containing twenty or thirty ingredients. But, in our days, a remedy of two or three ingredients—often, indeed, of a single article—is safely trusted for treating diseases of complex and multifarious symptoms.

Such simplicity of Treatment has taken much time, and some of the wisest minds, to bring it to the perfection which it has reached. Physicians, however, have, for the most part, been content to record their practice and its results; and, without analysing its principles, have left it to recommend itself by its own success. But it is worth while to ascertain, if we can, the sources of any power we possess which results from our knowledge, and thus to obtain the surest means of still keeping it and still enlarging it. And such a power as that of bringing diseases under easier and better management, ought especially to be had in possession and with the hope of increase.

Indicative symptoms, or those which guide the Treatment, may have to be sought for in a very narrow as well as in a very wide sphere. Thus the unhealthy quality of the fluid secreted by some one organ, as the liver or the kidney, will present itself as the single indication for treating a complex disease, made up of various and multitudinous symptoms. Thus, some preparation of mercury adapted to the kind and amount of bilious derangement; thus, some alkali or acid, suited to the quality of unhealthy urine, has been the sole remedy which has brought a crowd of concomitant disorders into subjection, and led the way back to recovery. In this way, does a single symptom become, for remedial purposes, the practical representative of the whole disease.

A strange quality of the pulse (it may be a hardness, or it may be a softness; but whichever it be, a thing very distinct and palpable), has served for the single scope of the remedy. The disease has involved half the organs and structures of the body, and accordingly its symptoms have been of great complexity and variety, and the peril of death has been urgent and instant, from opposite conditions in some certain cases. But all other indications have, in each case respectively, been passed by, but one. The soft pulse has been, practically, the whole disease in this case, and the hard pulse the whole disease in that. In this, all Treatment has been bent to the one purpose of raising the soft pulse into firmness and consistence, and in that to the one purpose of bringing down the hard pulse to softness.

A pain, or a disordered sensation of some certain part, has been singled out from a multitude of such in various parts, which, together, make up the whole complex disease, and it has become the one scope of successful Treatment. Shall I cause surprise by proclaiming my often proved experience of opium being found at last the sole and simple remedy of disorders which have been long abiding and long intractable?

Among a multitude of symptoms which have practically served only to lead astray, pain, referable to the stomach and bowels, has proved the one just index of Treatment. Sydenham, perhaps, might not be displeased to find that his experience still stands good, and that he has a disciple among physicians in these later days.

But it has been said that Treatment may seek its indications from symptoms made up of actions and suffering, which are spread widely and diffusively throughout the whole body, and consist of cold, and heat, and perspiration; of excitement and depression; of conscious strength and conscious weakness; and all varieties of evil sensation and pain. These are found among the ingredients of very many diseases, and call for remedies of wide and diffusive operation.

Now there is a good deal worthy of thought in the contrasts here presented. That the practice of medicine, when it is guided by rational indications, should be so different at different times in what it intends and what it does; that at one time it should be so exact and eclectic and so restricted in its aims, and have to do (apparently) so altogether with small things, and at another take such large and general views, and have operations alike large and general. These are remarkable facts; and it is well to notice them.

But then, does it in truth often happen that the Treatment of diseases is brought within the compass of one well chosen indication and one well directed remedy? Is such simplicity of practice often attained? I believe often. Happily for mankind, there are, and always have been, physicians who have sought to practise their profession with as much exactness as its nature will allow. And to this end, among the many uncertainties of medicine, they have been always in quest of those few things belonging to it which are more sure and stable, and how and where in every case to lay hold of them, and to use them, and to make the most and the best of them. But it is no easy task to pick one's way from truth to truth through besetting errors. The man who to cross a dangerous stream must trust to stepping stones, had need have a sharp eye to which is the broadest and firmest before he rests his whole weight upon it, and takes his spring from it to another. Indeed, this is a tottering exploit until every day's crossing makes it easy, safe, and bold.

To physicians, exact indications are the stepping-stones of practice. At first, perhaps for years, they go softly and distrustfully, waiting to examine each, and trying what each will bear. It is only from time and experience that they tread firmly and fearlessly. And this is the result: that the older

they grow, their practice becomes simpler and simpler, and at the same time surer and bolder. Indeed, theirs is the only practice which is at the same time bold and safe; and all because it is simple. Moreover, time and experience convince them of this particular truth, that, taking all diseases and their Treatment, the cases are numerous beyond all belief, in which safety and success are concentred in one chief indication and one chief remedy.

But where is the constant proof one would desire of simplicity being the condition of its success? The best physician is found, oftentimes, busy about a dozen things. Yes! but, even then, is he not *really intending and doing* only one thing? The sagacious looker-on sees that he is, and the physician himself would acknowledge it. He is intent upon making sure of his one indication, and of the effect of his one medicine addressed to it. In the meantime, perhaps, he willingly admits the use of other medicines for good but subordinate purposes; it may be as helps and auxiliaries, it may be for comfort's sake, or it may be for the prejudice's sake of patients or friends, which it is innocent, or wise and needful, to satisfy.

Or a physician may fancy he has twenty remedies for every disease, and as many cunningly devised notions of purposes (that is, of indications) which his remedies are fulfilling. And thus, lavish of aims and lavish of means, he may go on practising half his life, and ascribing what success he has to the nice adaptation to their end, and the equal co-operation of all his twenty remedies at once, in almost every case he meets with, while, in fact, he has been doing the whole business with one or two. For Nature has been practising upon him a happy deceit all the while. She has been choosing her own single and sufficient remedy out of his multitude of useless ones, and keeping it true to its mark, and insuring its sovereign purpose, and ridding herself from the incumbrance of all the rest. How wise and indulgent is Nature, who thus contrives to vindicate herself and her powers of reparation against the art of the physician without hurting his vanity!

Practice among the poor is a good school for teaching simplicity of Treatment, and testing its worth. There is a necessity in the case. Means are few, and they must not be thrown away. What is absolutely needed must be done, and

that only; for nothing more can be afforded. Hence arises a constant industry, to make sure what it is, and what will tell upon it. Hence, though the poor man's disease is the most severe, and calls the loudest for relief (for he does not succumb to trifles), its Treatment lies often within a narrow compass, and even the single aim and the single remedy are often sought after and found. The best physicians have begun by being the physicians of the poor.

So, too, the soldier's physician, when his patients are an army in the field or on actual service, must work as best he can with the means at his disposal; and these are often few enough. And so he betakes himself to reduce all that concerns their diseases and their Treatment to the utmost practical simplicity, and learns from hard necessity how much successful treatment is concentred in one chief indication and one chief remedy. The practice of medicine is, upon the whole, largely indebted to army physicians.

On the other hand, among the rich and well-off classes of the community, the Treatment of diseases is seldom as simple as it might be. Superfluity of means leads to their useless expenditure. Besides, the will and pleasure of others are ever tempting and influencing the physician to do many things where few are needed, and to divide and scatter abroad, and, upon the whole, so to weaken the power of his art, rather than to strengthen it by concentration. It is a fine thing pretty soon in life to be employed and patronised by rich and great people; but it is not the surest way of making a good physician.

But simplicity of treatment may be pushed to a faulty extreme. A conceit may lurk where it is least expected. When, in the confusion and tumult of disease, life has been poised, as it were, on a pin's point, and a single effort of skill has saved it, the exploit speaks great things for the glory of our art and its simple methods. But let not our over-zeal for simplicity fetter our freedom of thinking and acting in the management of disease. Oftentimes not one, two, or three, but many well-considered indications, must be fulfilled; not one, two, or three, but many well-directed remedies, must be brought to bear upon them, before the disease can be successfully treated. Still, to know the import of single indications and the power of single remedies lies at the root of all sound practice; and I am

persuaded that no man can clearly see and prosecute many indications together, or can safely and successfully use many remedies together, who has not begun by studying both indications and remedies one by one.

Something more needs to be said of certain single indications which experience commends to our notice as guides to practice in complex diseases, for the sake of the attestation they furnish to the general fact or principle already noticed. For these single indications of Treatment have often their value as such rather because they point to organs and functions which medicine can easily reach and deal with remedially, than because they are known to hold any proper pathological kindred with the diseases themselves.

Now, of all organs of the body, those are most within the reach of medicine whose function is to secrete, and the products of whose secretion consist largely of refuse matter to be cast out of the system. And what has experience not learnt to make of these organs, and their excesses and defects and errors of function, as indications of Treatment?—of the liver, and of the stomach and bowels; and of the skin and of the kidneys; and of bile, and excrements, and perspiration, and urine, redundant, or scanty, or vitiated? There are forms of disease (*i.e.* groups of symptoms) which directly flow from the wrong-doings of these several organs, and pathologically belong to them. And then, when the physician addresses his remedies to the liver singly, or to the stomach and bowels or skin or kidneys singly, he does not only what is safest and best and easiest to do, but he does it like a pure pathologist. He pursues the disease through its own channels. He tracks it to its spring-head, and takes hold of it there, and puts an end to it. But there are diseases which certainly have not their origins in any of these secreting organs, and which, nevertheless, are successfully treated by remedies addressed to them in part or altogether. They spring from none of them; but they involve injuriously the functions of one or more of them, sooner or later, in the course of their progress. Hence, in fevers of every name and every climate, and from every source, malarious, contagious, and accidental, whatever other and larger and more comprehensive indications of Treatment need to be pursued, particular indications present themselves, arising out of what is amiss in

the functions of secreting organs; and these must be pursued also. Indeed, the disease can hardly be named, whether acute or chronic, in which the liver, or stomach and bowels, or skin, or kidneys, do not present something to be redressed, and constituting an indication of Treatment. And this may be a small, or it may be a large part of the whole Treatment. It may be by much the largest part. Indeed, it may itself be the whole Treatment, rationally and of necessity.

The gravest diseases, which are of a specific nature, and are accompanied by fever, and have a definite course to run, can none of them be *cured*. They must all of them be *treated;* and, according as they are well or ill treated, their termination is in recovery or in death. And their Treatment turns altogether upon the choice of the right indications, and of the right remedies to reach them withal. But in how many cases of such diseases are the more general indications so doubtful that, in all prudent calculation, they go for nothing! We have no sufficient guides to tell us whether to sustain or to deplete. To do one or the other would be to strike a fearful blow in the dark, and so we wisely do neither. And thus it comes to pass that, among many things obviously going wrong, we are left to choose what are most within our reach to set right. Accordingly we make the liver and stomach and bowels the scope of our remedies; and, as they reach and take effect upon them day by day, we see the formidable conditions of the whole disease constantly kept in check, and are sure that thus many a life is saved.

What a temptation to easy and often erroneous theorising upon the nature of diseases has come from the success of these simple methods of Treatment! There is no denying the facts or their value—their plain, solid, appreciable value, practically; and, at the same time, there is no denying that they have set speculation upon a wrong scent, and led it far astray in its search of causes pathologically.

Thus fevers of various types and various names have from time to time, and by well-esteemed physicians, had their cause and point of departure assigned to them in the liver and its vitiated and redundant bile, and congestions of blood, and I know not what besides, therein; also in the stomach and bowels, and their foul and corrupt secretions. Physicians, taking these

for certain truths, and entirely believing in them, and conforming their practice to them, have saved innumerable lives. They have given mercury in every dose, from the largest to the smallest, from ten grains of calomel to two or three grains of hydrargyrum cum cretâ; and seldom or often, and with or without aperients; and this has been all their treatment. Or they have applied leeches seldom or often to the abdomen; and this has been all their treatment.

Men who have had great duties in hand, and who know *how* they have done them, and feel that they have done them well, naturally hold strong practical beliefs; and these beliefs soon take the place of absolute truths in their minds. The process is this. Being daily called to act, they read and study the facts before their eyes, and trust to them for guidance; and so acting frequently and successfully, and under the same guidance, they confide more and more in the facts by which they have been taught, until they hold them to contain the whole truth, and deem it needless to look or reason further about the matter. And these earnest assertors and followers of what they believe to be the truth, seen and verified in the great facts of daily experience, are the men by whom the world has been especially benefited in the practice of medicine. Give me one of these men for my physician, and I will take care not to discuss with him the abstract truth of his beliefs until he has set me upon my legs again; and then (most ungratefully on my part), when I have had the full benefit of his practical wisdom, I may venture to call in question his theory.

We are concerned to clear the subject from misapprehension, seeing that it presents such notable instances of the all-sufficiency of single indications and single remedies in the management of complex diseases.

Modern research has brought us back to the blood as the source and the distributor of disease throughout the body, and thus taught us respect for the surmises of our forefathers by furnishing us with reasons for them which they had not. What were surmises to them have become truths to us. Nevertheless, what science has brought within our knowledge, science has not placed within our power. By the tests of organic chemistry and the microscope, we have proofs of elements of disease in the blood; but we cannot reach them there. In this sense, there is

no such thing as taking disease at the fountain-head. Elements of disease, pure and simple, within the blood, present no possible aim for medical treatment. Accordingly, we have to wait and look for things which are beyond, and choose from among them, as they arise, possible objects of interference and fit indications of practice. We have to wait and see how the blood itself behaves within its own vessels; whether it moves with unwonted forces or unwonted rapidity; how it makes distribution of itself throughout the body, whether equally or unequally; and all that the pulse and local congestions may have to notify as indications of Treatment; then to see how the blood maintains its great diffusive functions of animal temperature, whether regularly or irregularly, and what of cold or heat or perspiration ensue declaring the common phenomena of fever; and then to see how the blood supplying the pabulum of its functions to every organ and system of organs throughout the body now hurts or hinders or variously disturbs those functions; and so from the nervous centres and from the nervous system diffusively there arise pains and painful sensations, and torpor or excitement, and sleep or wakefulness or delirium. Thus we find and follow up indications.

IX.—Treatment.

DISTINCTION BETWEEN THE PROPER AND COMMON ELEMENTS OF DISEASE.—ALL TREATMENT CONFORMABLE TO THIS DISTINCTION AND ILLUSTRATIVE OF IT.—SMALL-POX.—SCARLATINA.—MEASLES.

A general outline has been given of what Treatment is; showing it at work within its own sphere, which is a pretty large one, and upon its own objects, which are numerous enough; and how from among all the things within its reach and power to deal with, it seeks and finds the fittest to become practical indications, and then actually dealing with them produces great results.

But some of these objects are worth a nearer inspection, if we would understand how all good Treatment falls in with the nature of disease pathologically. Besides, remedies have been spoken of as the "great analysers of disease" (Art. 7). But how so? Verily in the sense of testing and discerning the elements of which disease consists, even its *vital* elements, or the things comprised in the doings and sufferings of those who are the subjects of disease, while they are yet alive. All this, for the sake of its importance, let us now consider a little carefully, and try to explain it, relying upon the plainest and commonest matters of fact as we go along.

It may be doubted whether the theories of clever men have not done more harm to the practice of medicine than all the mere blunders of the ignorant put together. At all events, purely speculative conclusions about the nature of diseases have heretofore done quite enough in confusing and misleading practice to make us cautious by this time, how we receive and trust what comes to us in the same questionable shape.

But pathology has its general facts nevertheless; that living pathology, I mean, which is the physician's concern and study, as he sees and watches and ministers to disease, and all that

belongs to it, in living men. *This* pathology contains facts of high generalisation. And if among them there be any which point to practice, it were a pity to pass them by and not follow them in the direction to which they point. For ultimately they may stand for practical principles.

The distinction between the *proper* and the *common* elements of disease is a fact of this kind. It is plain and unmistakeable enough in certain conspicuous instances. And these may be taken advantage of to introduce and explain the general truth, which pathological research seems in the way of verifying by new instances continually.

And, first, for the *proper* elements of disease. *Proper* to what. *Proper* to the disease itself in its own nature, and distinguishable from whatever else may attach to it in this, that, and the other living body wherein the disease is found.

We take something on a pin's point, and with that pin's point we just prick the skin hardly enough to be felt or seen. But by this little prick the something has gained its entrance, and is gone its way whither within the living body we do not exactly know. They say it has gone at once into the blood, has mingled with it, and is already distributed with it everywhere. Indeed one can hardly believe otherwise. But wherever the thing be gone, and whether moving or stationary, we can find it no more. Still wait for a certain time, a time forereckoned from the known quality of the thing upon the pin's point; wait, and a disease will come forth forereckoned also from the very same thing, a disease having its own proper characteristics and different from all other diseases in the world. And that disease is small-pox.

Here the element of the disease is the smallest that can be *sensibly* made sure of, the least that can be palpably taken and kept apart; taken, that is, from one living body and kept apart for some while (I know not how long), and then conveyed to a second, in which it becomes the beginning of a disease after its own kind, where there was no such disease before. Here, I say, is the least element of disease which is obnoxious to the senses. And from this example reason cannot refuse to admit the possibility of elements too minute for the senses to reach. Indeed, the possibility is made a certainty by the proved fact, that the same disease which is propagated by inoculation is

propagated also by the contact or near approach of one living body having the disease to another living body having it not.

Through what channels and by what processes we do not punctually know, but instances innumerable of the like experiments and results attest it for a truth, that the thing almost or altogether too minute for the senses is the primary material element of the whole disease which follows. Further, this primary element in producing its disease multiplies itself into new material elements which are divisible to infinity and separable from their own living body, their native body (so to speak), and ready to be transferred to other living bodies innumerable, and so to bring vast communities into the same conditions of disease.

It is a great subject this of the *proper* elements of disease, seen or unseen, apart from the body. Their reality, the conditions of their existence, their spread, their limit, also the substances to which they adhere, and much more besides; all these things require a pretty comprehensive mind to deal with them to much purpose. Then the mixture in this subject of equivocal facts and testimonies with the true, needs, moreover, a pretty shrewd as well as a candid and wary mind to pick its way safely, a mind not over-credulous and seeing contagion in everything, yet not over sceptical and seeing it in nothing.

But our concern at present is with the *proper* elements of disease *within* the body.

Their entrance within the body is one thing and the visible outbreak of the disease is another. And between the two the patient's consciousness and the physician's observation testify either absolutely to nothing, or else to nothing distinctive, Until diseases make outward presentment of themselves we know not *what* they are or whether they *are* at all. And yet it is certain that many diseases have a long period of latent being within the body. This is the case with small-pox, scarlatina, measles, and all that spring from a proper contagious element of their own. Well, but is there nothing being transacted all the while? Is it a passive incubation altogether? Nature contradicts the belief by all its analogies. And all observation of these same diseases goes to testify that the process, though unfelt and unseen, must be an active process; and not only active, but that (be it what it may) it works and moves by a

living law, and has stages and degrees and regular progressions filling up the definite interval, until the time is full and ripe for the disease to declare itself by its outward presentments. What else can mean the average periods of latency in small-pox, scarlatina, and measles, but that things are to be done therein, which require just those periods for the doing of them? The things themselves, be they what they may, unseen, unknown, and out of reach, but timed, and ordered and forwarded by nature with exact care, must be taken, upon the faith of nature, to carry some weighty meaning and to contain more than a formal preamble to the coming disease. The disease stands at bay (so it should seem) within the blood, and there it defies our scrutiny of what it is and our attempts to meddle with it remedially.

But in due season, when the foreknown period, according to the nature of the disease, is complete, the proper elements come forth into broad daylight, and make of themselves their clearest, fullest display. They may be pictured to the eye and defined in their forms, succession, duration, growth, completion. They claim now pre-eminently to be called *the disease*, and group themselves into small-pox, or scarlatina, or measles.

Nothing in the world can produce small-pox, or scarlatina, or measles, but the proper element of each. And the disease being produced is simply a multiplication of the element from which each sprang, the same in kind and unaltered, be its quantity much or little.

And now as soon as seen, surely now if ever, the disease may be reached and laid hold of remedially in its *proper* elements; the small-pox in its crops of pustules, the scarlatina and measles in their diffused and figured rash. No! it cannot, even now. As when their proper elements, visible or invisible, were in the smallest germ; as when they were diffused, we believe, through the blood during the period of their latent progress and incubation; so now do these same elements, made visible and displaying this, that, or the other phenomenon in succession until they have completed their course, equally refuse to become the *direct* aim of any medical treatment.

Still medicine is constantly busying itself about small-pox, scarlatina, measles, and oftentimes, it believes, for good. But

in *what way* does it busy itself with them? That is the question; and a question it is of great concern, and not to be answered off-hand.

The disease, be it what it may, if it have a distinct nature of its own, is never found pure and unmixed and its bare self and nothing more in any *living* body. There is, moreover, always something, which is *not* the disease and yet exists because of the disease. It may be much or it may be little, but there is always something. What the disease is and how it is identified with its "*proper* elements" restrictively and exclusively, and what they are, we have just seen. All the rest have been left and let pass for its "*common* elements." These we would now take up in their turn and try to make them out.

But how *common*? Because found in all diseases and thus *common* to all, but identifiable with none. And *common* in a wider sense still—common to all diseases, and common alike to whatever the living body does or feels painfully or hurtfully, though it may not bear to be called disease. But all this needs to be made plain by familiar instances.

Suppose a man sound of body and sound of mind and at the prime of life, and ready to do battle with the world for what it deems its best things, for its wealth, its fame, its power. Such a man must not spare himself, or he will have a poor chance of success in this restless competitive age of ours.

Now, in some of his seasons of full activity, and enterprise, let him give a little attention to himself, body and mind, and note what they are doing and what they are feeling in compliance with the uses and necessities, the *hard* uses and necessities, of health to which he puts them. He will remark colds and heats and perspirations coming and going strangely, his pulse varying in force and frequency, perhaps his stomach, bowels, liver, kidneys at fault in the quantity and quality of their secretions; more of thirst and less of hunger than he likes; less of sleep and less refreshment from the sleep he has,—all this, and, besides, many an unusual pain in his head or heart or limbs, which he would be glad to get rid of and seek a remedy for.

Here are transacted, in brief and in little and many times over, much of what the body does and suffers in real diseases.

The *common* elements of disease, and nothing less, are sensibly present and at work, while in truth the body is only engaged in some of the harder uses and necessities of health.

Let a man meet with some injury from without, small or great, simple or complex; presently all that is vital within him will show its resentment of it, and continue to show it little or much for a short or a long while, proportionably to the amount of the hurt and the time needed for its reparation. But how? Truly in some, many, or all of those identical ways in which the body is wont to be affected under the harder uses and necessities of health.

And here, be it remarked, that all the several ways in which the body is wont to show itself aggrieved from the harder usages of health or from mere mechanical outward injuries, are found among the constituents of fever. And being not soon got rid of and being also severe, these come to shape and group themselves into what none would hesitate to call by the name of fever, and they may carry with them its consequences and its perils. Hence of those who have had (so-called) fever thus derived, some occasionally have been very ill, some have recovered by a narrow chance, and some have died.

But there is no such familiar image of disease (if, indeed, it be the image and not the reality) as intoxication. The body is abused into all the conditions of disease for a season. First you have, as of disease, the *proper* elements foreign to the body and without it; and then the same introduced within it, and left to it to deal with and bear with as it may; but totally beyond the reach and touch of remedies. Next you have, as of disease, the *common* elements shown in what the vascular system and the nervous system, and their two great central organs, the heart and the brain, do and suffer according to the modes and measures of which they are capable under any present stress that is laid upon them. And oftentimes in intoxication these actings and sufferings, identical as they are with the *common* elements of disease, come with such rapidity and are carried to such excess that, but for experience to the contrary, we should constantly see in them great peril of life.

Again, let a man suffer real disease; such as has demonstrably an essence of its own and its own *proper* elements. Let it be small-pox, scarlatina, or measles. This disease having

gained entrance within the body is left to its vital powers to deal with as they may. By them it is to germinate and increase, be matured, and be finally cast out. But it is a hard necessity they have to strive with, a hard work they have to do, these vital powers. Yet not so hard but that they often accomplish it perfectly. Sometimes, however, it is so hard that they cannot get through it without hindrances and drawbacks and disasters, and, in the end, restoration is incomplete and not without remnants of damage to the body for a time or permanently. And sometimes so hard, that early or late or midway the vital powers utterly fail and the patient dies.

Meanwhile the ways in which these vital powers sometimes feel and act we summarily call fever. And fever is greatly indicative of Treatment. Not that we can treat fever as summarily as we talk of it. We cannot name it in one single word and name its remedy in another. We cannot ever treat it simply as fever, and have done with it.

If we would find the indications wrapt up in fever, we must resolve it into its constituents. And in doing so we come upon groups and forms and qualities of very common things. And these common things are the same which, one by one or a few combined, we constantly depend upon to tell us what to do, and what means to use when we know of no specific or special remedy and cannot pretend to *cure;* even in all cases calling for our interference from the ailments of abused health to the terrible emergencies of the Plague.

IX.—Treatment. Part 2.

subject continued.—typhus.—typhoid.

Of diseases, as separate things, things which have *proper* and essential elements of their own, we are never more sure than when they are introduced into the body from without. Those which have been mentioned are among the most familiar examples; and they and all other diseases which fall within the same category have more of wonder belonging to them, the more comprehensively they are regarded. They preserve their distinct essences for ever. They pass through millions of living bodies, and come out pure and the same after a hundred generations.

Really, "the blade, the ear, and the full corn in the ear," are not more distinct from the earth in which they vegetate, than are these diseases from the bodies in which they are found. The proper germs of vegetation get all that they want for reproducing and multiplying themselves from the earth, and the proper germs of disease get all that they want from the body. Our concern is altogether with the earth and the body, and with their *common* powers and faculties, in whatever way they may need, or will bear our interference, and not at all with the seeds *proper* and the diseases *proper* which are inaccessible to us. How this teeming earth and this living body take, and keep, and guard, the germs committed to them, and yield them their materials of increase; and how working (as it were) by a model, they bestow upon the growths and products of each that form which is its own and is always the same, and distinct from all others; these are things past finding out.

But analogy must not be pressed too far into the service of practical medicine. To provide that the things belonging to it lose none of their natural simplicity in our hands, should be our constant care. To make them simpler than they are, is an idle conceit and a great mischief.

Now, *proper* and essential elements are terms which present

to the mind the idea of something speculative. But in certain diseases (as we have seen) they are tangible matters of fact. From these the diseases had their beginning, and without them they could not have existed at all. Still, they are not alike plain and distinguishable in all diseases. They are not found out every day. Men can not betake themselves to their discovery of set purpose and make sure of success. It is well, if chance or industry discover one or two in the course of a century.

Not that medicine has no place, power, or opportunity for doing good in diseases as long as their *proper* elements are undiscovered. From what has been seen, it is probable that medicine has them all, and is now using them in as large and complete a measure for good, not knowing the *proper* elements of many diseases, as it ever will when it knows them.

It is a great field of pathological research this, which respects the *proper* elements of diseases. The plainest examples (we have seen) are at hand, and ready to point it out. But the same examples also show how practical medicine may be well aware of this field, and yet not able to enter it; and how, with a pretty good insight into it, it must be content to take up with things without it, *i.e.* things short of the diseases themselves, and how it has done its office well notwithstanding.

Now, if practice, having full knowledge of certain diseases and their *proper* elements, has been able to turn it to no direct account, and yet has continued to do its work well, one need not be surprised that, being wholly in the dark about other diseases and their *proper* elements, it should still often have done its work well; or, at least, should not always have failed for lack of knowledge.

This nineteenth century in which we live had finished half its course, before Dr. William Jenner saw and fixed the *proper* elements of the two diseases called "Typhus" and "Typhoid."

But the two diseases, yet unknown in themselves, had a treatment not ill-understood or unsuccessfully prosecuted long before. For their *common* elements had been always present, and ready to suggest what that Treatment ought to be. For years and years, Typhus and Typhoid were terms which had borne intelligible meanings, and been familiarly spoken in our schools and in the wards of our hospitals. Hitherto, indeed,

experience had noted in them nothing more than the *common* elements of diseases. But, then, they were grouped so constantly into certain forms, and were of such definite duration; they presented such sure reckonings of life and death, and were such intelligible guides to Treatment, that practice, finding in them all that it wanted, was content to take them for the entire diseases, and note them for Idiopathic Fevers, and call them Typhus or Typhoid.

And now let us see how all prior observation was corrected by the readings and decipherings of Dr. Jenner.

Fever having endured for about a week (between the fifth and eighth day they tell us) a rash appears. It consists of distinct spots, of a dusky pink or mulberry colour, slightly elevated. The like spots still come out for three days, and then no more are added to them. But those which have appeared, be they many or few, remain and undergo certain changes. At first, press them with the fingers and they fade; but take off the finger, and they return in an instant. In two or three days, pressure alters them little, or not at all. In some cases, they gradually grow paler of themselves, and so finally disappear. In some, they become true petechiæ. They are generally pretty numerous, and chiefly on the trunk; but they may be also on the extremities, on the face, and every part. In size, they are anything between a mere speck and a large split pea. Those of the larger size having become true petechiæ, I have seen upon the face of a patient, almost as black as ink, at the further end of the ward as I entered.

This spotted petechial rash, though not unknown to us before in alliance with fever, took the physicians of hospitals by surprise in 1837 and 1838, from its unwonted frequency. It has, since that time, undergone patient investigation, and is now believed to be as much "the proper element" of a distinct disease as the pustules are the "proper element" of small-pox. And the name Typhus is given to this disease by pre-eminence, and is restricted to it.

Again, Fever having reached its second week, a rash appears of another kind from that just described. It, too, consists of spots, or rather specks, a little elevated. But they are bright or rose coloured, and often so like flea-bites as to be easily mistaken for them. Yet the absence of the little black

point in the centre shows that no flea had the making of them. They go and come again, as you put your finger upon them and take it off. They never go on to any other form. They never become petechial; each particular spot does not last more than three days, and then it vanishes away. But spot after spot may continue to come out until the end of the third week. In several cases, their number varies from one or two only to a hundred from first to last. And, seeing they last so short a time, and yet may be so few in number, the physician had need be sharp-sighted and of pretty fair experience always to make sure of them.

This non-petechial rash, whenever *post-mortem* examination has gone in search of the fact, has been found in alliance with the swelling, ulceration, and sloughing of Peyer's intestinal glands, well known to the physicians of hospitals for half a century. This rash and this intestinal disease are thus justly held to have an essential connexion pathologically, and both to be equally "the proper elements" of a distinct disease. And to it the name "Typhoid" is given by pre-eminence, and is restricted to it.

Behold, then, in these dusky petechial spots, the proper elements of one disease, Typhus; and behold, in these bright non-petechial spots, and in this peculiar affection of Peyer's glands the proper elements of another, Typhoid. And the one is as distinct from the other as small-pox is from measles, and as measles from scarlatina.

Further, in respect to treatment, these (so-called) Typhus and Typhoid maladies are just on the same footing with small-pox, measles, and scarlatina. Their proper elements afford no direct indications for the management of any one of them. That wherein their own essence consists can be at once touched or handled remedially in none of them.

It is a great subject this, of the *proper* and *common* elements of disease. To illustrate it, I have chosen examples where the distinction is plainest and nearest at hand. In some of these the *proper* elements are familiar and unmistakeable. Small-pox, measles, and scarlatina, display themselves what they are to all the world. But in the Typhus and Typhoid diseases the *proper* elements are sure only to the practised eyes of physicians. And even physicians have but lately been taught when and

where and how to look for them. Yet these must always have existed, and must essentially have played the same part in all past times which they do now. But heretofore they lay covert.

Now, I do not pretend to announce new things in pathology. I only wish to put in a little better order things that are known; and, perhaps, I may make them easier to handle for practical purposes. To tell the truth, most medical books have been a puzzle to me all my life long. Their descriptions of disease have been (to my thinking) needlessly complex and multifarious, mixing what is proper, and constant, and first in order, with what is common, and variable, and consequential; both being real enough in themselves, but meaning different things. Again, their lessons of practice were conversant with too many aims and too many implements for me ever to make myself master of them. Hence, for itself and its own needs and uses, my mind has always been in search of something simpler and easier. Dr. Jenner has given me the key to unlock many a speculative puzzle; and I thank him for it.

In this place, I would make a remark which is of some practical concern. It is this. Here are two diseases, each constituted of its own proper elements, and both distinct from every other disease in the world. One is called Typhus, and the other Typhoid. Now, it is a misfortune that they, being new to our knowledge, were not designated from the first by new names. Each having a nature of its own should have had its own name restrictively. But Typhus and Typhoid were names already bespoken for other and commoner uses. They had been current time out of mind, and were prefixes of fever, denoting it, not with much strictness perhaps, to be of an evil character. For, in truth, they had no more definite meaning than what was implied by their etymology. Typhus was the fever that *smouldered*, but did not burn. Typhoid was the fever that neither *smouldered* nor *burned*, but of the two it was rather given to smouldering. Take small-pox, scarlatina, measles, erysipelas, dysentery, boils, carbuncles; take these, or any other diseases you please which have fever belonging to them, and you will find that fever becoming Typhus or Typhoid in the old sense, according to times, and places, and circumstances. Whatever diseases get an entrance into camps, and into crowded squalid

quarters of the poor in large towns, are sure soon to be Typhus or Typhoid. Their fever *smoulders*, and is of an evil character. And thus it was by an unlucky chance that the names, which had been taken to signify accustomed groups of the *common* elements of I know not how many diseases, came all at once to be restricted to the *proper* elements of two diseases in particular.

The terms pervade all the best records of practical medicine for the last century. But the radical difference between their present and past meaning will be too likely to perplex future readers of those records, without help of a glossary.

Now, all the five diseases which have been mentioned, small-pox, scarlatina, measles, the typhus and the typhoid maladies, commend themselves to the study of the physician for their own sakes, and for much more besides. Having their *proper* and *common* elements distinctly legible, they exhibit specimens how diseases of several names and natures come by a single characteristic in which they agree, to find their place within the same category, and to fall within the easier reach of the physician. The category to which these belong includes, together with them, many of the most formidable diseases throughout the world. And so they become our examples near at hand of what all the rest need practically, and, in a vital sense, of what they are pathologically. Thus, knowing these five, go where we will, we find ourselves not altogether strangers to the nature of diseases which we may never have seen before; and not quite unapt for their management, though we may never before have been called to treat them. Of these five, and the rest that fall within the same category all the world over, none admit of *cure;* none other require or tolerate a special remedy. With them it is all an affair of Treatment, and of choosing the right indications in particular cases, and dealing fitly with them by whatever means they require.

These are a few simple things rescued (so to speak) from a great throng. They claim, however, the nature of principles, being verified by the numerous instances which they cover and explain. And, indeed, the times we live in call upon physicians for new and more strenuous endeavours after principles in the exercise of their art. For it is being undone for want of them.

Further, these five diseases have been all recognised as fevers, and all so denominated; variolous fever, scarlet fever, rubeolous fever, typhus fever, typhoid fever. And it is likely enough that many physicians, whatever speculations they may have in reserve, take their familiar notion of fever from what it has appeared to them to be in these examples. And no bad notion either. But, however this may be, now is a fair occasion for what remarks I presume to make on the same subject, even Fever.

X.—Fever.—what is fever.

Stahl's Phlogiston looked very like the truth for its time. Chemistry wanted a something in aid of its facts, to complete a theory of combustion; and it had recourse to this figment. And figment as it was, the facts and it fitted into one another so well, that it soon passed for a reality; and Phlogiston kept its ground for half a century.

Now the world in general invests fever with a quasi materialism and personality, and regards it as offspring and parent by turns of almost all diseases. And a great many facts fit in curiously with this conceit, and make it a plausible piece of pathology. Thus Fever has been our Phlogiston.

But one would not like to let drop a hint that Fever is nothing better than a figment! Far be it from me. Only take me round a hospital, and you will have me acknowledging Fever in half the poor fellows I see. Just so. But what is Fever, and what the thing or things contained in it? If a single thing, it must be very large; if several things, they must be very numerous. But, without saying which it really is, let us, for the present, be allowed to take it as a *single* thing; for there are facts of practical moment belonging to it, which can be most conveniently regarded from this point of view.

It would have a formidable look, if we began by requiring a definition of Fever. There are things which will not be defined, and Fever is one of them. Besides, when a word has passed into everyday use, it is too late to lay a logical trap for its meaning, and think to apprehend it by a definition. Vain, too, is it to hunt after it in dictionaries, or consult antiquaries and etymologists about it. After lapse of time, even common language comes to vary its signification from mere custom. It is not likely, therefore, that in a profession like ours, which, itself and all belonging to it, suffers perpetual change, language should be constant to its meaning, and the same word continue for ever to express the same thing. Practical medicine must

not be overnice about the language it uses, or it will be brought to a standstill altogether.

The term Fever is as old as the hills, and it must remain. What it meant formerly, it may not mean now. But our business is with the sense it bears at the present day, and to give it and take it according to common acceptation. We *must* use it. So let us make the best of it, and ask no curious questions about it. Listen to the fellows in the street, how they bandy about some vulgar word from morning to night, giving and taking it in its now unmistakeable sense, and little thinking of all the trouble some antiquarian philologer has had with it. It, a mere monosyllable perhaps, has cost him a chase of centuries over half the world to find out whence first it came, whither next it went, and where it has been, and what it has been doing and meaning ever since, down to the present time.

Without retrospect or regard then of other times, if there be any sense in which Fever is now popularly understood by physicians and the world, the same may be safely adopted. And such a sense there is.

In casting our eye over the subject of fever historically, we find this remarkable circumstance, that it fills a much larger space in medical books, both ancient and modern, prior to the century in which we live, than it has ever done since, and than it does at present. In these days, physicians talk about Fever as much as ever. They have it perpetually on their lips, and keep up still a pretty vociferous report of it throughout the world. But they do not now *write* much about it under *that name*.

A fact like this must have its reasons. Verily, writing and talking are different things. Is it that they of old studied the subject more, and we less? No! But each of us has studied it in our own way; and our way is different from theirs.

They dealt with the subject more bravely and adventurously than we, and more comprehensively too; also, more with the aim of coming to conclusions and constructing systems; and, so far, more successfully than we. For this bold and intrepid thought of theirs upon the large subject of Fever took with it such a sweep of things innumerable, and things of every sort near and remote, as contrived to drag in almost all diseases. And so it was that, readily and almost naturally, their theories,

and cure, and treatment of Fever, became their theories, cure, and treatment of all the rest. It is, indeed, plain that most of the doctrines, which aforetime have borne grandiloquent names, and made professors and universities famous in their day, have been expansions and reflections of what had been thought and taught, and practised, concerning Fever.

But our studies are, and have been for some time past, more about it and about it than of Fever itself. And no disparagement to us on that account! Search has been made by us after places that engender Fever, or rather the noxious things which are the proper elements of diseases that involve Fever; and this search has engaged some of the best minds. And it had need of them; for it is difficult beyond measure. And it has had great success. Nevertheless, it is not exactly the study of Fever; perhaps, it is something better. For better, indeed, it may well be to prevent the whole pernicious work from being set agoing, which carries Fever along with it, than to understand what this Fever is when it arrives. Think what it is to know the elements of certain diseases before they have entrance within the body, and then to be able to lessen them and reduce them to nothing; or, failing the ability to do either, to give warning where they are, and save communities their perilous or fatal exposure to them.

Our studies have also been of contagion and its doctrines; and thus we have become conversant with the proper elements of certain other diseases which, however originally engendered, are now kept existent in the world by having human living bodies to accept, and nurse, and multiply them, and impart them to others. Many of these have Fever involved in them from first to last. But the study of them, and the conditions of their spread, is, strictly speaking, something beyond and beside the study of Fever.

Further, our studies have been of diseases which begin and end with the individual man. With naked eye and scalpel, and microscopic and chemical tests, we have been hard at work of late years, and have searched out many wonderful things within the body, claiming to be the *proper* elements of diseases, which in their vital progress involve Fever; but they are not Fever themselves.

Behold us, then, circumventing and closing in upon our

subject on every side. And are we none the wiser, after all, what Fever is? Yes! far the wiser. For thus we have come to know all that can be known of it; and that it is neither malarious, nor contagious, nor scissile, nor microscopic, nor chemical; but that it is vital. Fever is not the man's disease, but his life assailed by his disease; even his life on a large scale.

Let me, however, be careful what I say. Pure life is a thing inapprehensible and unimaginable by human thought. But the actings and feelings of the parts and structures which compose our body are, many of them, apprehensible enough. And, as it is by them that the man shows that he lives, we must be content to deem them identical with his very life. For we cannot penetrate further.

With this explanation, let us come close to the bedside, and there see and judge for ourselves what ails the patients who are said to have Fever.

Well, then, some are said to have Fever, while, to outward view, nothing ails them more than that they are hot above measure. Others, that they are hot and cold by turns, after some strange fashion. And others, that they are perspiring too; hot and perspiring or cold and perspiring at the same time; or none of the three together, but each by turns.

All this is apparent enough; and in outward show there is nothing more than this. But there is something more, and no trifle either. Question the patients about their feelings. And, if it be really fever that they have, whether their heat, cold, or perspiration, be much or little, or in whatever manner occurring, they will all testify to a consciousness of something wrong within them. What it is they feel they cannot exactly tell. Perhaps they can say no more than that "they are ill," or "that they are downright ill."

But people in perfect health are hot and cold and perspiring by turns; and all because they *are* in perfect health. Take them whose lot is labour. Whatever they have to do is by voluntary muscle and limb; and they do it with all their might. This is their outward life put forth day by day and every day in all its power and energy. And their inward life shares the vigour of their outward life, and co-operates with it. They put forth glowing heat and fresh perspiration; and as for cold, if it

be felt at all, it is but for a short season, and as a stimulus for more heat and more perspiration. They testify to nothing wrong within. All their consciousness is of health and power.

Here, then, we have our specimens of life healthily actuated, and of life injuriously assailed. In each case, it is through its same embodiments that we are enabled to judge now of its health and now of its illness; viz. through the entire vascular and nervous systems, and the great central organs of both. For these, as far as mortal insight goes, are the prime movers and ministers of all function, and feeling, and consciousness, within us, ready to answer whatever calls or emergencies await them, as they are able. If, from things not out of harmony with themselves, and not of more force than they can bear, they answer (as we have seen) boldly, according to the nature of their office, by putting forth the simplest and surest manifestations of health; if, from things essentially baneful or simply overstrong, then they answer (as we have also seen) still according to the nature of their office, but strangely and confusedly.

It is upon life that we are looking; upon life embodied in the entire vascular and nervous systems, and on this large scale injuriously assailed, whenever we contemplate Fever.

Is Fever, then, one thing or many? The question has been answering itself as we have gone along. Show me Life as a single thing, and I will show you Fever as a single thing taking forcible hold of it. But to contemplate either this or that as a single thing is utterly impossible. Fever must be taken to stand for the sum and aggregate of many particulars.

But all general terms in use among physicians had need to be jealously watched. Even strength and weakness, simple and innocent as they look, have by turns been the marring of us. The terms themselves cannot be dispensed with; and as long as they truly represent (as they always ought) an aggregate of things real, and verifiable in their details, they serve an useful purpose. But, failing this, they become pernicious abstractions. In my time medicine has been going through a speculative crisis of two opposite kinds by turns; and strength (so-called) has ruled the one, and weakness the other. Beyond all question, during its critical ascendency, abstract strength cost the world many lives by its practice of blood-letting; and abstract weakness, in its turn, has cost the world many also by its practice of

brandy-giving. I have not statistics to show with exactness which of the two has levied the larger mortal tax upon mankind. My impression is that the bad pre-eminence belongs to the latter.

Now Fever is a general term, and as such, carries with it the danger, as far as its meaning is concerned, of degenerating into a vague abstraction. And this danger is the greater, the more it is thought of and talked of, apart from the patients who suffer it. Therefore, in what remarks we have yet to make about Fever, let us still keep close to the bedside. And not only so; but let us choose some large hospital, and conceive ourselves to make daily visits to it for some three or four years in succession, and there and then make acquaintance with the real things, which Fever represents in its natural and necessary alliance with bodily hurts and diseases.

X.—Fever. Part 2.

INFLAMMATION AND ITS FEVER.

Let it be constantly borne in mind that Fever is not the man's disease, but the man's life assailed by his disease; his life, not as a single thing which is inapprehensible and unimaginable, but as embodied in the functions and feelings of the entire vascular system and nervous system, and seen and read of all men.

Now our best insight into the nature of Fever is likely to come from contemplating it in diseases with which it is most conspicuously allied, beginning with any one that we know the most of. And the most familiar thing bearing the name of a disease with which we are acquainted is Inflammation. The organic processes, which concern it, have been minutely scrutinised and had their meanings interpreted by pathologists. They are all visible on the surface of the body, and many of them are audible as they transact themselves in some important organs within. Then of the antecedents of Inflammation, we have a pretty good notion which they are that may be set down for its exciting causes. Such are things that can hurt mechanically; such are unwonted transitions from heat to cold; such are overtaxing or too hard usage of the bodily powers in health, and of the mental too sometimes. Of some of these we are so sure that we could choose them, and apply them, and so procure Inflammation at will.

At all times, and especially at sickly seasons, Inflammation abounds in large hospitals. And cases of Inflammation are the best studies we can have of Fever. Above all other cases they represent: 1. The relation of Fever to the disease as a whole from first to last. 2. The correspondence of Fever with the disease in its intermediate changes, stage for stage. Further, above all other cases they denote the bearing which it belongs to Fever to exercise upon the management of the disease. Here,

if ever, the vital movements of the vascular and nervous system, now assailed and aggrieved, stand out as indications of treatment. Here, if ever, they first suggest what that treatment should be upon the whole; and, then, from its effects upon Fever and Disease correspondently, they suggest what it should be from stage to stage until the end. And all with some evident exactness.

Now, Inflammation is naturally the simplest of Diseases, and Fever annexed to Inflammation is the simplest of Fevers. But then they must meet with fortunate coincidences to make them and keep them such.

The artist, whatever may be the material he uses for his projected work, whether wood or stone, takes care to have the most perfectly sound specimen of it he can get; knowing that, otherwise, the work itself is like enough to turn out a failure, and be neither so good nor so durable as his ambition might hope. And we physicians, when we want Inflammation for a study of Fever, would like to choose the subjects whom it should befall, and have them of the soundest material. But we must take cases as they come. We are practitioners by necessity; we are pathologists by choice or by chance. It is well when we *can* be both, but the first we *must* be.

In a large hospital, the cases of Inflammation are not all of the right vital stuff for making the disease the thing which (so to speak) it naturally ought to be. For entertaining Inflammation and its Fever, and insuring them their natural simplicity, and carrying them to the perfection they are capable of, the body must be previously healthy. There must be the antagonism of health to show to the full what the disease naturally is.

This is no paradox; but a truth put (I believe) into fitting words, and worthy to be noted.

Now, it is where their natural simplicity is preserved, and perhaps in consequence of it, that Inflammation and its Fever do, if ever, display a superlative energy, and require and tolerate the most powerful remedies. The cases typifying this course of things make a great show in medical books. The disease is then severe and unmistakeable; and its Treatment heroic and successful. But such cases are the few. Perfect health, like perfect beauty, is a rare thing; and so, it seems, is perfect disease.

Still, not to speak of *perfect* health, good health is common enough, and Inflammation does not befall those the oftenest who have a fair share of it. Yet, when it does, then, if ever, the case is demonstrative of the points most sought after, both pathologically and practically, and is an easy and satisfactory study.

But let us take the experience of a large hospital year after year for a succession of years, and run over what records we have of those who have suffered Inflammation there. Of some patients, we shall find that they were old; of some, that they were half-starved; of some, that they had chronic liver-complaints; of others, that they had habitual coughs; of others, that their constitution was bad at the best, and (what they called) their health was no better than other men's disease. All these many patients had Inflammation. The pneumonia, or pleurisy, or peritonitis, was diagnostically the same disease in them as it was in the few. But observe the difference. In the few, Inflammation found Life unharmed and vigorous. In the many, Inflammation found it hurt and deteriorated. In the few, Life, embodied in the vascular and nervous systems, being now assailed, was ready to resent and react according to its strength; in the many, according to its weakness.

By strength and weakness, is here meant nothing more than the completeness or incompleteness of men's habitual health. And these furnish us the measure or gauge of their vital sufficiency or insufficiency to encounter whatever of an extraordinary kind may befall them; for instance, to bear inflammation when it comes, and bring out its natural characteristics, and help us to make good its treatment.

But, taking still the experience of a large hospital for a few years, let us review as well what has been our treatment of Inflammation as those who have been our patients, and note its results. In so doing, we shall doubtless be able to give a goodly account of some easy and successful experiments, in which all went on well from first to last; of Fever and Inflammation proceeding in perfect harmony together, and preserving a constant course, and presenting indications of treatment safe and sure, because distinct and intelligible; and of simple and powerful remedies, all telling for good, and all producing the effect for which they were given. But this goodly account, if

it be honestly reckoned, will not be a numerous one. In fact, the much more numerous account we shall have to give will be of experiments in which our treatment has been successful indeed, but not easy. All came out well in the end. Meanwhile, however, from intervening accidents, or some weakness of the subject, Fever and Inflammation did not keep exact concord together, and presented but obscure and half-legible indications of treatment; and these were adopted with much care and circumspection. And our treatment became all along a compromise between what the disease required, and what the patient could bear. Nevertheless, it was successful in the end, and largely too. And then will come our inventory of unsuccessful experiments, made up partly of cases in which our treatment saved life indeed; but, being unequally matched with the disease, could not bring it to an end on better terms than that it should leave the part it occupied permanently damaged, and the whole man permanently deteriorated; and partly of cases in which our treatment absolutely failed, and life was lost.

Thus, let Inflammation and Inflammatory Fever be naturally ever so simple, they cannot exist without life and organisation. And so variable a thing is Life, and such disguises does it throw over them, and whatever else that can befall it from time to time, that they are not always to be apprehended in their entire natural simplicity.

Still we do but begin with the beginning, when, in seeking to know what Fever is, we try first to trace it in its connexion with Inflammation. For Inflammation is, pre-eminently, a fundamental subject of practical medicine, and its great vital ingredient is its Fever. It is to us a sort of grammar, which helps us to construe half the things we see and deal with as physicians. And therein our great grammarian is John Hunter. For it is from him that at this day we have our most comprehensive teaching in all that Inflammation includes, and especially its Fever. Observe how constantly sedulous he is to keep apart what is local and simply organic from what is constitutional and more eminently vital. As soon as the entire vascular system and nervous system show themselves assailed and aggrieved, *i.e.* as soon as Fever arises, he lays aside his scalpel and every foreign implement, and abstains religiously from all meddling and interference. He becomes, in the

strictest sense, a physician. He sits by the bedside, and takes note of chilliness, and heat, and perspiration, and all the unwonted movements of the vascular system and nervous system, as they follow the visible shiftings and changes of the Inflammation; and so he seeks to understand better the meaning of both, and how they serve to interpret the Inflammation and it them.

RHEUMATISM AND ITS FEVER.

Within a large hospital, and side by side with cases of Inflammation and Inflammatory Fever, are cases of Rheumatism and Rheumatic Fever. The characteristics of these latter have been well studied and are well marked and well known. The thing which, seated in the joints, and in this part and that, is called Rheumatism, if regard be had to the organic processes which concern it, will just as well bear to be called Inflammation. And the manner in which, this Rheumatism being present, the vascular and nervous systems show themselves assailed and aggrieved, can be called nothing else but Fever.

But there is a great difference between them somewhere. And it lies deeper than sight, or hearing, or microscope, or stethoscope, or any instrument we have to help us, can reach. Let us review our cases of Rheumatism and Rheumatic Fever, and collate our experience of their treatment for a few years. Such heat, perspiration, and pain; such general vascular action and nervous distress as we witness in Rheumatism, had they occurred in Inflammation, would have been taken by us to hold some correspondence with its amount, its extent, its stages, and been used for plain indications of treatment, and the patient's safety been thought to turn upon the just fulfilment of them. But now, in their alliance with Rheumatism, we did not, and we dared not, take them literally for what they were, and apportion the force of our remedies accordingly. Further, with all our observation, be it ever so correct, of times and places and seasons and of antecedent and concurrent things, we cannot pitch upon certain of them at our command, and take and apply them, and so produce a Rheumatism in individual men and women as we can produce an Inflammation. Where many or most of them are present, there must still be something over and above them, or there will be no Rheumatism. And oftentimes,

where none of them exist, there will still be Rheumatism. But what this thing is, which, favoured by coincidents or by its own sole agency, is the very cause of Rheumatism, no man knows.

All men, however, guess pretty unanimously *whence* it comes. Sagacious observers and experimenters have, in these later days, gone nigh to show that there is a chemistry within us which is co-operative with life; that making good its work, it gives to our bodies the materials of their health; and that doing its work faultily, it suffers noxious things to form, which become the elements of their diseases.

Already a miscellaneous throng of diseases is fairly conjectured to issue from this vital laboratory; many trivial enough, and of smaller interest, and coming and going with little interference on our part; and many of deeper pathological and practical concern, and more abiding. Among these, one of the most momentous is Rheumatism with the Fever that accompanies it.

The disease cannot be named which makes a handsomer figure nosologically than this Rheumatism. Yet there is none about which we are more in the dark. For nosology, at the best, is but a sort of provisional pathology, telling us something, little, or nothing, as it may happen, about the nature of the disease, and the management thereof.

This Rheumatism once begun, we can make no reasonable calculation when it will end; in a few days, or in a few weeks, or in a few months. Begun in a single joint, we do not know how many more joints it will visit, and what other and nobler parts besides; the heart, the lungs, the brain. Further, this Rheumatism calling for medical management, we are divided between (what I have called) cure and treatment, between the use of special remedies whose aim is nothing less than to be counteractive of the entire disease as such, and the use of any remedies whatever best calculated to satisfy indications which present themselves in individual patients. Hence upon a retrospect of years, in the management of Rheumatic Fever, cinchona, colchicum, guaiacum, nitre, lemon juice, alkalis, and alkaline salts, will claim the authority of our experience for their occasional and inconstant success as special remedies for Cure. And so will bleeding and purgatives and opium, as common remedies in treatment.

ERYSIPELAS AND ITS FEVER.—BOILS.—CARBUNCLES.

In a large hospital, and side by side with Inflammation and Inflammatory Fever, are cases of Erysipelas and Erysipelatous Fever; also cases of Boils and Carbuncles. And these, together with Rheumatism, regarded from the physician's point of view, group themselves into a class, different as they are in outward aspects; for they are all alike favoured by external causes with which we are not unacquainted, while they are essentially due to something over and above—to something which we have no exact knowledge of; and hence they are beyond our power to produce. Further, from time to time they all in their turn alike surprise us by the success and the failure of the remedial measures we adopt. Cure and treatment, special remedies and common remedies, have gained for themselves the praise or the blame of unaccountable results. But they all show their vital import in one and the truest sense, by the power they have of assailing life in the entire vascular and nervous systems, as seen by the Fever of various degrees and durations that may or does accompany them all.

The physician, finding these diseases—Rheumatism, Erysipelas, Boils, Carbuncle—all beyond the power of known external causes to set them a-going, has with good reason transferred them pathologically to the sphere of those that are engendered within. But in so doing he has pledged himself to a task of great, perhaps insurmountable difficulty.

Touching external causes of diseases, there could have been no certain proof given that any such exist, short of specialising some of them, and showing that it was this, that, and the other cause which set a-going this, that, and the other disease. And this proof has been abundantly given in specialising many contagious and malarious elements, and tracing their several diseases infallibly home to them.

Now, even such philosophy as we physicians pretend to cannot be satisfied with less, when the question is of causes engendered within. Here there is even more and more need of first specialising things into realities before fixing upon them as germs of disease.

This is the work to be done. But the science which must help us to do it is new to the age in which we live, and has

almost taken us by surprise. One may be pardoned for being a little jealous of it at starting, for already it has been made to stand sponsor for some fantastic imaginings.

Diseases (so the history of medicine tells us) have many of them been named and nosologised so luckily and so well from their external characters, as to betray the physician into the belief that he had a perfect understanding of them. But of those among them whose proper elements are self-engendered and within, he now sees that he knows little enough, and he fears a natural interdict to his knowing more, until he has specialised the cause or causes he is in search of; and this may be the work of ages yet to come.

But our subject is Fever. And it was for the sake of gaining some better insight into its nature, that we came to meditate upon the several diseases which have been named; that in Inflammation, seeing Fever allied with the disease of which we know the most, and then allied with others of which we know the least, we might perhaps gain some surer reckoning of what it is from contrast or comparison. Whether we have really done so, I cannot tell. But this is certain, where the question is of Life (and, if the phenomena of Fever be rightly interpreted, it is now pre-eminently so), we must look for enlightenment anywhere and everywhere. Even between Inflammation and its Fever the proportions were not found exact. Large allowances had to be made for age and for constitution, and for hereditary and individual strength and weakness. And all this implies nothing less than some power beyond the reach of science, that can calculate what their life is worth in individual men and women before it suffers the disease. And this power belongs to practical experience—to busy, thoughtful, multifarious experience—the honest and just judge that ultimately settles the worth of everything for physicians.

XI.—Treatment.

TREATMENT OF ALMOST ALL DISEASES—HELPED AND EXPLAINED BY THE STUDY OF FEVER.—SPECULATIVELY AND RUDIMENTALLY FEVER PERTAINS TO THE VASCULAR AND NERVOUS SYSTEMS.—PRACTICALLY AND CONSEQUENTIALLY TO ALL PARTS AND ALMOST ALL DISEASES.—LIMIT OF SPECULATIVE THOUGHT IN PRACTICAL MEDICINE.

It was altogether accidentally and by the way, that we came to discuss the great subject Fever. We were simply in search of the symptoms which should serve us for indicators or guides of *Treatment;* and we found them not among those which are *proper* to the particular disease, as being nearest akin to it and its real or supposed essence; but among those which are *common* to it, and to many another disease besides.

Take the five Diseases which served us for our first and plainest illustrations. They were fitly called small-pox, scarlatina, and measles, and typhus and typhoid, from their *proper* elements, which differenced them one from another, and from every other disease in the world. And still were they fitly called Fevers from their *common* elements, which kept them all still related one with another, and with every disease in the world of which Fever is predicated.

But I do not presume to tell medical men what they mean, or ought to mean, when they talk of Fever. Some may mean more, and some less, and many may not exactly know what they mean. Only we have strangely misnamed what we have all so called, if this one and the same appellative denote now one thing and now another in every different disease where Fever is found. The name should stand for some pathological reality common to them all. And so it does, and plainly too and unmistakeably, in the many diseases of various types through which we have just been tracing this (so-called) Fever.

Let it then be repeated, that Fever, wherever it is found, is not the man's Disease but the man's Life assailed by his disease;

Life as embodied in the functions and feelings of the entire vascular and nervous systems. For my part I can go no deeper than this, and can generalise no further. But if, as far as we can see, the vascular system and the nervous system are first and principal in acting or suffering wherever disease most tries and imperils life, it would be well to consider what they are in themselves prior to our more practical dealings with them.

In speaking of the vascular system and the nervous system, I pretend to no greater accuracy than the nature of the subject allows. Doubtless, to the eye of the anatomist, the vascular system and the nervous system are things apart one from the other. But to the physiologist, the pathologist, and the practical physician, they are always mixed. One cannot fulfil its natural and healthy functions without the other. One cannot be drawn out of its natural and healthy habitudes, and display the actings and sufferings of disease without the other. Nor can one be subjected to the impressions of medicine without the other being subjected also. While each taken alone is vitally inert, both taken together exercise the great vital forces which move and actuate the whole body.

Nevertheless, the vascular system and the nervous system have each its own proper instruments and power to act with, and each its own proper part to play. Though they cannot work but by mutual help, their offices are not interchangeable. Some little may be learnt of these separate powers and offices by observing their operations in health; but much more by attentively looking on when accident or disease forces them out of their accustomed conditions into others which do not naturally belong to them; and much more still by noting accurately the results of experiments of our own choosing in the treatment of diseases. But for diseases, and the treatment of diseases, we should know but little of the powers and offices of the vascular and nervous systems.

Now, all this is in analogy with the nature of things around us, with the material elements of the world, and their separate existence, and their co-operative agency in accomplishing their great designs.

It is remarkable how few are the elements which nature makes use of in her grandest operations. Yet she never works with them singly, but always in combination. By single ele-

ments she does nothing; by two or three together she moves the world.

These elements, inert in themselves and giving no self-notice of a separate existence, have been hard to find out; and hence their discovery has been postponed to a late period in the age of mankind. Yet have they always been chief objects of human curiosity; and it has ever been reckoned among the triumphs of science to win a new element from the jealousy and concealment of nature. But such discoveries have not been made by simple observation of things as they are; but men, by contrivances of art, have forced them out of their natural combinations, and transferred them into others of their own choosing, and then noted their behaviour under new conditions presented to them beyond the first intent and purposes of nature. Thus, injury, or disease, or whatever, either by accident or design, has been brought to bear upon the functions of our bodies, so as to force them out of their natural course, has given the first hint, and become the very test and experiment, and ultimate proof of new and otherwise unattainable knowledge concerning them.

Further, the vital functions of our bodies are in analogy with the intellectual faculties of our minds. A mixed operation and a separate essence belong to them both. The memory, the imagination, and the reason do not, perhaps cannot, work but in union one with another. Yet, it should seem, they are three separate elements of intelligence. Lord Bacon, at least, regarded them as such; he accepted this ancient truth, and gave it pre-eminently his sanction by the use he made of it. For, in accordance with it, he made a threefold division of his universe of knowledge, and found for its several parts a store-house (as it were) and natural laboratory, either in the memory, the imagination, or the reason. Thus, the three together go essentially to make up the one mind; and thus the one mind becomes capacious of all knowledge. Where all three are of equal power, and that power is great, and all are harmoniously blended, and all duly co-operative, there is the perfect mind. But the difference between mind and mind is mainly seen in the different measures in which they are conjoined. And thus He who made us "has divided unto every man severally as He willed." But a total defect of any one of the three elements

of intelligence can hardly consist with any mental operation at all. The working of any one can scarcely be begun, certainly it cannot be sustained, without some degree of impulse and support derived from the others.

But this utter spoiling of intelligence by one capital defect is (I presume) very rare. And, on the other hand, the perfect mind has been the lot of very few. To come near to possessing it has not been the lot of many. And those who have the greatest repute for intelligence, like those who seem to have the best health, are often so self-conscious of some weak part, that they are glad to resort to remedies from time to time to patch themselves up mentally, as others take physic to patch themselves up bodily.

There are men whose business is thought. They come soon to learn that it is above all things important for them to keep in good working order the several implements which serve to execute the operation of thinking. And these implements are no other than the several faculties of the mind. Follow great thinkers into their studies. In every age there are a few who are inwardly prompted to give a life's labour, that they "may, perhaps, leave something so written to aftertimes as they shall not willingly let it die." All such abide under the burden of a heavy task. But the labour of each is different according as Nature has distributed the strength or the weakness of their mental constitution. The labour of one is to keep his memory in full grasp and possession of all the materials essential to his great design; the labour of a second, to draw upon his imagination for all that is suited for its illustration and adornment; and the labour of a third, while he has memory and imagination ever ready and ancillary to his will, is to reason out his conclusions, and raise it, and build it, and settle it, upon the sure foundation and conviction of truth.

It happens with our body and its life, and health and disease, in like manner. Life is only known as the complex of many functions, and health as the integrity of these functions, each in itself, and their harmony among others. Every organ has its proper function, which it only can fulfil, and no other organ. Yet can no organ live and move, and be vitally at work upon its own health in perfect singleness and independence. And what knowledge we have of disease (of living disease) runs parallel

with our knowledge of life and of health. We see in disease organs or systems of organs acting and suffering against their healthy conditions and uses. But each organ or system of organs then acts and suffers in its own way, and as no other can act and suffer besides itself; yet not one of them can be vitally at work upon its own diseases in perfect singleness and independence.

Now, medicine mediates between health and disease, and must needs comply with the conditions of both. Hence it is chiefly conversant with partial aims, and contrives single purposes, well knowing that these only are fairly within its reach and handling. Thus, it seeks to bring this, that, and the other organ or system of organs in subjection to its remedies. It deals with the vascular system, and reduces its plethora by venesection; and with the nervous system, and quiets its irritation by opium. It deals with the stomach and bowels, the liver and the kidneys, and by acids, and alkalis, and aperients, and astringents, and by whatever remedies have especial power to reach and modify the functions of each, it sets them right when they are wrong. Such are its aims, and it sees them clearly, and is pretty sure of hitting them. But they are partial aims, after all.

Within the range, however, of such partial aims, medicine affects and prosecutes a sort of scientific exactness, and claims and reckons some amount of certainty in the results. And if all life were the life of distinct organs and nothing more, and if health and disease were nothing more than what distinct organs could work out for themselves through their own functions singly and independently; and if, moreover, nature had supplied the means by which all organs and their functions could be reached, and man had learnt the skill of using such means with perfect safety and success; then, indeed, would medicine be a certain art.

But the domain of medicine is wider than its positive command. What it cannot reach it can reckon upon. True, its aims are partial, and their range is narrow. Yet within that range beginnings are made, and first impulses given, which are felt and resented far beyond; even further, and wider, and more variously, than we can see, or trace, or follow them. But

medicine looks to results, and calculates and often foretells them, trusting to experience.

Now, the Treatment of all diseases is helped and explained by the study of Fever. And well it may; if Fever be comprised in the actions and sufferings of the vascular and nervous systems, and they denote the measure and manner in which life is assailed by the present disease.

The study of Fever! Why, *speculatively*, we have gone as far as it is safe to go in search of a principle, and perhaps have found it. But, *practically*, we must go much further, if we would show that there is any truth and comprehensiveness in the same principle; and that, simple as it looks, it is anything better than a pathological puzzle.

For all, then, that more concerns Fever we must go and remain at the bedside and study it there. In so doing I wish we could fix the limit of speculative thought in its bearing upon practical medicine.

It is a delicate and difficult subject, and worth dwelling on for a moment. The limit cannot be defined absolutely. If it could, it would mark the boundaries of the Art itself. Nevertheless, all sagacious and experienced minds do, in fact, arbitrate and settle it for themselves. And in so doing they hold a pretty near agreement with one another. This agreement is not to be put in words; but resting in their general conceptions, it is brought out and tested by practice.

To understand, then, tolerably well what is this limit of *practical* thought in medicine, and to walk circumspectly within it, is the secret of all the power we have of doing good. And really that power is very great. But not to understand what it is, and wander heedlessly or rashly beyond it, is the secret of all that mistaken energy of ours, which makes the evil we do weigh so heavily in the scales against the good. Within this limit lie all the realities of medicine, both small and great, which are at present available for its use. And outside it there lie other realities, which are (as it were) upon trial, and waiting for proof of their use and bearing on practice, that they may thereupon claim entrance and a place within it. These outside realities, too, are both small and great; many as great as any which are already found within, and comprising some of the

highest pathological truths. Physicians, therefore, must not be forbidden all interest and inquiry about things (and such things!) lying at present beyond this limit of practical thought. For then it would never be enlarged. Only they must be admonished, in their daily work of curing and treating diseases, to keep jealously within it for the sake of present safety and success.

As to the realities themselves, some are solid, substantial matters of fact, and some have rather the nature of feelings, qualities, powers. These two have, by philosophers, been taken and studied separately, and used in acknowledged contrast. Not so by physicians. Medicine has not only to do with both; but the practice of medicine is a mixture of both, bringing them together, and making them harmonious and co-operative to fulfil its ends. Thus they both, in their kind, claim equally the name and nature of realities; and, tested by use, both have their place within the just limit of practical thought.

What is disease? and what is it not? These interrogatories have a formidable look. They seem to threaten deep thoughts about the nature of diseases, or doubtful disputes about their names. But I would keep clear of such hazards, and leave the nature and names of diseases where I find them. Still whatever may be the compass of our knowledge, or the limit of our art upon the whole, we must hold with ourselves some sort of questioning and answering in particular cases "what disease is, and what it is not," if we would keep in view such aims as are possible in the practice of medicine; if we would have any fair understanding what it is and what it is not, which day by day we pretend to cure or to treat.

Think of three, four, or five hundred cases in the several hospitals of London, and of the physicians that have them in charge. Those living embodiments of disease teem with realities which crowd and press upon their minds, and all with some show of meaning and significance. Of these they try to make sober reckoning. Yet not of them all; but only of such as each physician can fairly bring within his own limit of practical thought. And these and only these he takes and trusts for guidance and direction in the use of remedies.

But what of all the rest?

Now, all strenuous and earnest men, let their habitual work be what it may, if it have but a serious purpose, carry home

with them plenty of matter for thought springing out of that work every day of their lives. It is eminently the case with physicians. And the more truly practical they are, the more things they find suggested by their practice but lying at present beyond it. Let them be sure they have done their best, and that the Art itself has done its best in their hands, yet their success is ever suggestive of a greater possible success beyond it. From things of easy and familiar use they gather intimation of truths which lie too deep for common observation. Thus, besides turning what can be turned to the best practical account according to present duty and necessity, they put by facts and (it may be) thoughts for further inquiry and experiment, and hope thereby to gain new knowledge, or new adaptations of knowledge, and so render that which they have in exercise more sure and trustworthy.

But every much employed physician passes his life in the midst of speculative temptations. It is not, therefore, much to be wondered at that sometimes a plausible faulty theory has stolen into an otherwise sound and simple mind, and got mixed with its practical reckonings, and corrupted them. Without much care this may happen to any of us, and we may become emblems of our profession in this respect. What is too often seen and lamented in individuals is to be more seen and more lamented in the profession at large. A theory of easy comprehension has obtained its ready acceptance, and so has become the common tyrant and impostor of medical practice, dominating over it and throwing it back for half a century.

It would be well for those of our profession who have a touch of intellectual vanity in their composition, to make a modest compromise between it and the satisfaction of doing good. On the question what disease is and what it is not, if they are to answer within the limit of *practical* thought, they must commonly keep below the mark of their knowledge, and be content to speak of it truly indeed, but simply and superficially; while they have much more to tell which is still true, but recondite and beneath the surface. It must be confessed that what is very showy and creditable to possess, and to talk about, and good for many worthy purposes, may not yet be good for the purpose of curing and treating diseases.

Physiology, pathology, and practice, often part company just

where an intelligent looker-on would make sure of their becoming sociable and co-operative. The practice of medicine is a perpetual compromise between what we know and what we can do, between our knowledge and our power. There is a portion of the nervous system, a little particle of matter at the top of the spinal cord, which, in a plain, intelligible sense, is the most vital of the whole body. In man, and in any animal like unto man, hurt it substantially and life is gone at once. This part, which is pre-eminently the cord of life, and which Nature has well protected from mechanical or accidental harm, she would take care (one might think) to put especially within the power of man to influence for good, that remedies would go directly to it, and might be aimed directly at it. Not so. Remedies cannot otherwise reach it than through the whole nervous system and the entire body.

The great discovery has been made within our own times, that the nerves of sensation and motion are different nerves, visibly different in having distinct origins within the spinal marrow, and still visibly different as they emerge from it, until both are included in a common sheath, and distributed together throughout the body. But this great discovery has been no help to us in the choice and application of remedies. True! there is a palsy of motion and a palsy of sensation, when they are distinct diseases. And there is a palsy of motion and sensation combined, and the disease is mixed. But whether the palsy be one or the other or a combination of both, it has not, therefore, its own treatment; and remedies cannot reach either order of nerves except through the entire nervous system and the body at large.

XII.—Treatment.

EXPERIENCE.—MUCH INSIGHT INTO ITS NATURE TO BE GAINED FROM A SINGLE DISEASE AND ITS TREATMENT, ON A RETROSPECT OF YEARS.—LET THE DISEASE BE ERYSIPELAS.

Upon the whole, men agree better about what is subjected to the senses than about anything else; and they agree best of all about what they see with their eyes. Certainly physicians do so. Show us a redness of the skin, with some degree of swelling, and a margin irregularly circumscribed and gradually enlarging itself; and at once, without doubt or disagreement, we call it Erysipelas. But there are plenty of things in the world which, while they are seen by all in the same light, and because they are so seen, are rightly enough called by the same name, have moreover a great deal belonging to them beside their visible or any other sensible properties, and consequently a great deal not admitting of the best agreement among men. Such are pre-eminently the subjects with which physicians have to do. Physicians know Erysipelas at the first glance, and agree forthwith that such it is and so it shall be called. But Erysipelas includes much that lies within reach neither of the sight nor any other sense; and upon all this physicians are found in no constant accord.

Wrapt up in that visible thing which we call Erysipelas, there is a living thing which does not tell itself to the eye; and yet a real thing nevertheless, pre-eminently real. For, more than all that meets the eye, it denotes the character, course, tendency, and probable end of every case of Erysipelas that occurs, and suggests and regulates its treatment withal. This living and invisible thing is a compound of action and suffering; and this action and suffering have qualities and degrees which vary in different cases, and vary in the same case from time to time.

Now, to know Erysipelas as a living disease—*i.e.* as a compound of action and suffering—and to test their qualities and

degrees, and to catch their variations as they occur in several cases or in any single case, not all the observation, thought, and experiment which can be brought to bear upon the mere part will help us in the least. To know the living disease, we must search the whole body. We must look for signs in the Vascular system, and take note of cold, and heat, and perspiration; for signs in the Nervous system, and have regard to excitement and depression, to conscious strength and conscious weakness, and to pains; for signs in the Pulse, and count its number, and reckon from day to day its varying qualities.

These are the vital ingredients of the disease, its vital histology, so to speak—the living web upon which it is spun, woven, and held together.

Taking these elements, and reckoning the sum of them, you will find that in one case they amount to strength, and in another to weakness; and that strength or weakness is the characteristic of the disease from first to last. In one case you will find the disease implicating the Vascular system principally, and in another principally the Nervous, and in another equally implicating both. And (what is remarkable of Erysipelas beyond all other diseases that can be named) you will often find in one and the same case the strength of yesterday become the weakness of to-day—nay, the strength of midday become weakness at midnight; and also, more than in any other disease, rapidly and in a few hours the stress of the symptoms transferred from the Vascular system to the Nervous, and then as rapidly transferred back again from the Nervous to the Vascular. The more predominantly the disease is of the Vascular system, the more of strength does it contain; and the more it is of the Nervous, the more of weakness; and the more equally it is shared between the two, the more manifestly is it a disease of moderate force, needing moderate treatment, and involving small hazard.

As to Treatment, the bare announcement of the remedies severally recommended by the experience of the best physicians must astonish any thoughtful mind not yet let into the secret of all that is involved in practical indications. The surprise will be that remedies of such power and such opposite effects should ever be brought to bear successfully upon a disease of one and the same name. But, in the treatment of Erysipelas, it is the

living disease with which we have to do; not with the mere mechanism of the thing in the part from which it takes its name, but with the actions and sufferings of the entire Vascular system and Nervous system, and all their possible contrarieties in different cases, and all their possible shiftings and inconstancies in the same case.

Now bleeding has a right to be put down as a remedy for Erysipelas, even bleeding in all the ways in which it is practised—venesection, cupping, leeches. And so has wine, and so has ammonia, and so has any diffusible stimulant nearest at hand which you choose to name. And so has tartar emetic, acting according to one or other of its several modes of operation, whether upon the skin as a diaphoretic, or upon the stomach as an emetic, or independent of any notable effect upon the skin especially or the stomach especially, controlling the force and modifying the character of the pulse by its gradually augmented doses. Opium, too, claims justly to have a place among the remedies of Erysipelas, both in large doses and in small, and by the several ways it has of exercising its impression for good through the Nervous system; by abating pain, or by procuring sleep, or by (what can hardly be described in words, and is intelligible only to practical experience) doing the double office of a sedative and a tonic, and at the same time quieting irritation and sustaining strength.

Oftentimes nothing more surely tests the nature of the disease than the remedy that restores the man to health. But then your remedy must be one of plain and unmistakeable effect, and of such power that it can never be nugatory, but is always either for good or for evil. Of such a stamp are the remedies just mentioned—remedies which, in reviewing the records of past experience, I find myself to have employed for the most part in the treatment of Erysipelas, adapting them (as I thought) to the calls and emergencies of particular cases, and nothing doubting, then or now, of their instrumentality in the restoration to health.

Well, then, my experience for the most part would go to placing Erysipelas among diseases to be *treated*, not among diseases to be *cured;* to be managed by remedies having regard to things which may chance to be in the man, not by remedies proper to the disease. I say "for the most part," not alto-

gether; for I have met with cases of Erysipelas in which I have been as sure as common sense could make me that, had there been no Peruvian bark in the world, the patients must have died. They have been cured by quinine—cured outright and in the strictest sense, without the fulfilment of any intermediate purpose seen, known, or aimed at, whatever. The specific has even seemed to work its cure unswayed by present conditions, and indifferently, whether the sum of concomitant symptoms has denoted strength or weakness, and whether their stress has fallen upon the vascular or upon the nervous system especially, or equally upon both.

What shall we say, then? That when our knowledge, as it were, breaks down under us, it is well to have something to supply its place? or rather, that our experience of things in their detail has sometimes to give way to our experience of their gross results as more trustworthy, and that laws which we understand the more intimately must sometimes bend to laws which we understand the less? All this may be justly said. But it does not therefore follow that our ignorance is better than our knowledge, or our lesser knowledge better than our greater. The truth rather is, that our common sense—*i.e.* our prudence—sitting in judgment upon our knowledge, discerns its imperfections, and decides how far it can be trusted, and how far it cannot, and so finally allows free play to some secret law of life which is greater and stronger than any law within the reach of our understanding.

It would not be incorrect to state that, when I entered the profession, bark was regarded by all physicians of mature age and experience as a specific for Erysipelas. The majority would not have hesitated so to call it; and the rest, without professing a faith thus absolute, would have been sorry to hold themselves responsible for what might happen if in any case they omitted its use. Surely my own experience has not run parallel with theirs; and what I have said of it may be taken fairly to represent the experience of physicians practising in London during my time.

But how are we to be just to the experience of others? Even by making a fair reckoning of our own. Remember there is a secret inscrutable region wherein the things belonging to health and to disease are largely transacted, and wherein causes

are vitally at work into which we have no insight. Our experience, therefore, must be left very free; for it cannot be fettered by any known rule. It must needs be our guide and master, but not in such sort that the greater experience should always tyrannise over the less. The less contains its own truth, as well as the greater. The greater need not convict the less of falsehood. My own far greater experience represents Erysipelas as a disease to be *treated;* my far less experience, as a disease to be *cured.* The first is conversant with very many realities of things, and familiarises me with their use. The last brings fewer realities within my view; but, fewer as they are, they are certain. Therefore it is not thrown away; at least, it is largely suggestive of what is possible. And no man can have grown old in the practice of medicine, and not allow that things which he once looked upon as barely possible have become with him frequent realities at some after time. His rarer and his more common experience have changed places altogether.

Wherefore then serveth experience, and of what use is it? Its first and best use is for the guidance of him that has it. Its next, and hardly less important use, is that it enables him to judge rightly the experience of others. And this is the fair reckoning by which to judge it. If, at the same place, our own experience has differed from itself from time to time, surely it is abundantly credible that another's may differ from ours often or always, at other times, or at other places.

I am not of the number of those who rest in a self-satisfied belief that diseases never were, never will, and never can be, other than I have seen them; and that they never were, will, or can be, successfully treated or cured, otherwise than I have treated or cured them. The physicians of one time have been too prone to make a mock of what was deemed the sound practice of another. This is neither generous nor wise.

But I wish to make a still larger use of this instance of Erysipelas in showing what the practice of medicine really is; its differences and contrarieties in the same diseases; its trust in remedies of opposite effects; now in treatment, now in cure, now in non-specifics, now in specifics; and in showing, nevertheless, that the practice of medicine is a rational practice.

The vital pathology of Erysipelas, regarded as a disease of individual men, is full enough of conditions to account for the

success of many and various ways of dealing with it. Erysipelas, however, has a vital pathology, I will not say of another kind, but reflected from another aspect; viz. from its natural history. For every disease has its own place, kindred, and relationships, near and remote, among the whole family of diseases belonging to mankind. And so has Erysipelas. This may be called their natural history. And the same is the natural history of Erysipelas; and wrapt up with it is a portion of its vital pathology, as important and as suggestive, from time to time, of its right treatment, as present conditions of individual men.

Now, the great physician found in Erysipelas the marks characteristic of what he called an intercurrent disease. Its cause might be purely accidental and from without, from exposure to cold, or exposure to heat, or from mechanical injury. Yet once formed, in its measure of fever, its strength or its weakness, its excitement or depression, it ran parallel with the stationary or dominant epidemic, and its successful management was in nothing different from the management which the epidemic demanded. Thus, in what it was pathologically, and in what it needed, and what it could bear remedially, Erysipelas was of close vital kindred with it.

Again, Sydenham put down Erysipelas as a disease which was intercurrent, coming on in this way. And yet, there would be no Erysipelas, but there would be fever, which was in nothing different from the dominant epidemic. And when it had continued for a few days, and been regarded as the epidemic itself, and nothing more, Erysipelas would be added to it, and be, as it were, among its accidents; and no other treatment would be required of the whole disease than if Erysipelas had formed no part of it, and no treatment of the Erysipelas, but what was included in the treatment of the whole disease.

Perhaps Erysipelas may serve for the most signal example of all that is most mutable in diseases, from whatever point of view you regard them. If you want the best representative of all that the different constitutions of men can do in diversifying the aspect of diseases, you will find it in Erysipelas. If of what times and places can do, you have it in Erysipelas. If of how opposite remedies can save life and restore to health from the same diseases, according to times, places, and men, you have it in Erysipelas.

XIII.—Treatment.

Difficulties and Anomalies of Practical Experience.

Ευριφων ὁ ιατρος ερωτηθεις τον διδασκαλον, παρ' ᾧ επαιδευθη, παρα τῳ χρονῳ, εφη. (*Stobei.*) This being interpreted means, "Euriphon the Physician, being asked what preceptor had the teaching of him, made answer that his preceptor was Time." These words are quoted by Dr. Heberden, and stand the last of his *Commentary on the History and Cure of Diseases.* Dr. Heberden's volume contains the condensed experience of his life carried on to his seventy-second year, and left by him for publication after his death. He lived until he was ninety. In this remarkable work the preceptor Time is plainly seen ruling and guiding throughout.

And time keeps school among physicians still. But he is not, and never was, a popular lecturer. He is slow in coming to the point. He has a cautious, hesitating, self-correcting manner, which is not altogether pleasant, and makes him difficult to follow. Hence a good deal more patience is required to understand his teaching, and profit by it, than most men care to give. That his disciples should value highly what their venerable preceptor has hammered into them is natural enough. But then they are themselves few and grey-headed, and have often caught something of their master's manner, and so do not obtain a ready ear from the world. And I can hardly expect a better success than the rest while I rehearse some of the leadings or misleadings, the teachings, and corrections of my preceptor Time.

Nothing is so difficult to deal with as a man's own Experience, how to value it according to its amount, what to conclude from it, and how to use it and do good with it.

I had been physician to St. Bartholomew's Hospital eight years, and eight years previously physician to the Middlesex, when I ventured to write a book; bestowing upon it as much

care and thought as I was capable of. Indeed, it cost me a world of labour, and, at least it was honestly done. This book contained my own sixteen years' Experience of Fever, and what conclusions as to its nature and treatment that Experience would (I thought) fairly sustain; and I was not without a reasonable confidence in their truth.

Fever in those days, written or spoken of singly and without adjunct of any sort before or after, stood for an idiopathic disease. Gaining, however, an accession of many and evil things in its progress, it became a fearful complexity; and earned, before it came to an end, the name of Synocha, Synochus, or Typhus, as the case might be, but without prejudice to its original idiopathic rights.

It was Fever regarded from this point of view that I undertook to write about. But my book never saw the light. For abruptly, or within the short space of a year or two, all that was called Fever suffered the nature of a revolution, and so did its Treatment. What was life and safety to Fever, as it had been, became death and destruction to Fever as it was. Present Experience was in conflict with past. And my poor work, with its truths, its beliefs, its opinions (call them what you will) all rudely shocked and scattered, was ashamed to show itself, and so came to an end. It was a sort of violent end it came to. It seemed as if some force (for such forces there are) indwelling in the nature of things, but latent, and so unseen and unreckoned upon, had suddenly come into action and demolished it; just as one's foot demolishes an ant-hill, scattering the population within, and bringing ruin upon all its toil, and all its instinctive wisdom, and the fruits thereof, in a moment.

We physicians had need be a self-confronting and a self-reproving race; for we must be ready, without fear or favour, to call in question our own Experience and to judge it justly; to confirm it, to repeal it, to reverse it, to set up the new against the old, and again to reinstate the old and give it preponderance over the new.

Belief, opinion, truth! When we cast up the sum of a long Experience, by which of these names shall we call it? Its subject is one of the worthiest, even nothing less than the life and well-being of man. Let it then be spoken of in the handsomest terms it deserves. And it is hard if there be not some-

thing in the results of Experience that is worth the name of Truth. But then it is an infirmity of men, finding a fragment of Truth, to take it for the whole Truth; or, intent upon what may well pass for Truth now and for a few years to come, to account it the Truth which stands fast for ever and ever. We must be cautious, then, not to deceive ourselves, or take the Truth there is in the practice of medicine for what it is not. If what can be apprehended by fragments but not completely, if what lasts safe and sure and trustworthy for times and seasons but not for ever, can be called Truth, this is the Truth, which is vouchsafed us to know and to use in the practice of medicine, and this only. And we must make the best of it, and be content.

Now, my own sixteen years' Experience of Fever and febrile diseases and of their treatment, may be taken as a fair sample of what medical Experience really is. It was no fable, but made up of undeniable matters of fact. Time went on, and proved them more and more, and showed them still consistent and trustworthy. And they were trusted accordingly, and the practice grounded upon them had abundant success. But the facts were constant and consistent only for their own season. They did not hold together long enough (for what is sixteen years after all?) to become one solid Truth. They were but fragments of Truth after all. Yet, fragments as they were, they were not worthless. They go to make up my stock of good working materials, and are still of use to me every day. They do not serve me for building systems. But they help me to treat diseases and to save lives.

How is it that in medicine Truth is thus measured out to us in fragments, and we are never put in trust of it *as a whole*? How is it that Experience, upon which alone we can depend for all the good we do, is continually breaking down under us as a *system*?

Well, one cannot indeed account for it; but one is not surprised that so it should be. What with inscrutable things within the body and inscrutable things without, and incalculable and incontrollable withal, it is no wonder that Experience should sometimes find itself at fault, and be not always able to shape the future out of the past upon a large scale.

The one fact has been already noticed, of things within the

body belonging to life and health and disease, which are hitherto untouched by human knowledge. All medicines give intimation of it more or less in the course of their operation. Specifics present summary proof of it from first to last.

The other fact, too, deserves to be well pondered, of forces without the body exercising dominion over all that is vital within it—forces of some of which we know neither what they are nor whence they come. But they are full of reality and power.

On my first entrance into my profession, and for some time afterwards, nothing was less intelligible to me than the writings of our great Physician. All his discoursings about epidemic constitutions, and stationary fevers, and intercurrent diseases, and the same diseases needing different treatments from time to time, were to me dark and mysterious. And just the same, I am persuaded, is the case with every man who studies medicine; and the more so, the more he finds himself thus early in the presence of real disease, and in the society of those who, like himself, are daily watching it, and ministering to it, and comparing notes about it. For then the mind is at its freshest and its best, and is most apt for observation. It holds strongly to its own conclusions: and why should it not? It takes for absolute truth what has already come to it authenticated by a hundred instances: and why should it not? It is not yet upon the look-out for what may gainsay or invalidate its own well-observed facts concerning diseases and remedies: why should it? Tell, therefore, the student and the physician of a few years' standing, that there is something more in diseases, and in the effects of remedies upon them, than can be seen and known from any number of individual patients during any given period of time; and he will neither understand you nor believe you. Yet there is something more, nevertheless; and physicians at length come to own it.

Choose almost any febrile disease you please, and question the Experience of honest and well-informed physicians about it, and what it was, and how it was best treated at their own time and place of observation; and you will find them tolerably well agreed. But take this Experience and agreement of theirs to determine its nature absolutely, and fix the canon of its treatment for all places and all times; and you will run into a great

practical delusion. Still a man must have lived long enough to see many phases of medical practice, and been himself engaged in it all the while, before he can be in a condition freely to acknowledge what is here implied.

Take Pneumonia. It has been treated by bleeding, and got well. It has been treated by brandy, and got well. It has been left to itself, and got well. And the bleeders, the brandy-givers, and the doers of nothing at all, respectively have had a vast deal to say for themselves and against their rivals. And which of them are to be our guides and masters in the Treatment of Pneumonia? None of them for a single day, much less for always. Besides, the Treatment of Pneumonia happens just now to be a matter of high controversy; and from controversy often comes exaggeration. And exaggeration often does the work of falsehood unawares. Therefore I would willingly have chosen some other instance, but that I wanted such *internal* febrile disease for illustrating my purpose, as, whenever it exists, declares itself infallibly. And, since Erysipelas well served my purpose, because all about it was plain and unmistakeable to the eye; so now Pneumonia will do the same, because, of *internal* diseases, it and all about it are best ascertained by the ear.

Now, we have the best testimony which the ear can afford to the fact that Pneumonia has gone through all its processes of disease and reparation, now advancing, now receding, until the lungs have finally recovered the conditions of health, when it had received no formal medical treatment whatever. Again, we have the sure witness of the ear to the same fact, when the Pneumonia had been treated by brandy; and yet again to the same fact when it had been treated by venesection.

Here the means (for even the doing of nothing may now be deemed to exercise a positive influence)—the means, I say, and the end, are things so obvious, and are found so closely following each other, that it is impossible not to put down their sequence to the account of natural connexion. And this being the case, and there being such vast odds between doing nothing, giving brandy, and drawing blood, there is no wonder, if the current of a man's Experience had run generally in favour of any one of the three, that he should hold one to be absolutely and always right, and the other two absolutely and always wrong.

No wonder, indeed. But then practical medicine is unlike all other things in the world. It has its own conditions; and they prohibit all such summary conclusions as these. Let a man use his own Experience as best he can for the present; but let him not, upon the strength of it, rebuke the Experience of all past times, and dictate to the Experience of all future; for, if he live long enough, nothing is more likely than that he may find himself fallen under his own reproof, and inconveniently confronted by his own maxims. From having been a bleeder, he may become a stimulator or a passive looker-on in cases of Pneumonia. He may find himself interchangeably all three in the course of thirty years.

And what is the explanation of all this? People not over well disposed towards us and our profession (and there are many such) find a sufficient key to the whole matter in the caprice of medical men; and they take this key of theirs, and turn the lock, and bar the door against all further question! "It is neither more nor less," say they, "than the caprice of physicians; so let us have done with it and them."

But let the world think of us as it pleases, there is a rational practice of Medicine nevertheless. And let medical men themselves still be right and wrong by turns, seeing clearly and doing successfully what is at present to be seen and done, yet mistaking the same for something more sure and permanent than it really is; finding (as it were) a fragment of truth, and learning well what it will now bear, and now safely trusting their own weight upon it, let them foolishly deem it a rock and build a castle upon it. Whatever physicians may be, or the world may think of them, there is, I say, a rational practice of Medicine nevertheless, and there are causes sufficient to explain its greatest anomalies. A cursory glance can only here be given at what these causes are.

The vital being of every individual man has its qualities. What is summarily called his Constitution is full enough of its own conditions to require in almost every case some deviation from the standard treatment of the particular disease. Not seldom are these conditions sufficient to justify great differences, and even contrarieties, in practice. They are never to be lost sight of at the bedside of the patient; and we consider ourselves as never practising more rationally than when we practise in obedience to them.

Then there are the things which are the commonest and the most inconstant in the world, yet which do and must surround every man as long as he lives; cold and heat, moist and dry, dense and rare, foul and fair. All these reach and rule and qualify the vital being of us all, and so vary the aspect of our diseases and necessitate changes in their methods of Treatment. Here is the very subject which in our own times has especially attracted the interest of the world at large; viz. the influence, upon the well-being of mankind, of the air which they breathe and the places where they dwell. Governments, military and civil, national and municipal and domestic, have expended abundance of study and labour and money upon it; yet not more than it deserves. Practically, what physicians have to do with the subject may lie in a small compass and be reckoned by a few items, or it may expand itself infinitely, and run into particulars innumerable. Our patient may be an individual, a family, a community, or an army. Our concern may be with the cleanliness and ventilation of a single room, the drainage of a town, or the site of an encampment.

It is difficult to speak rightly of things which have no body or form presentable to the senses. But we *must* speak of them, and dwell upon them. Though we know hardly by what name to call them, they cannot be ignored; for they have oftentimes a greater sway over the world without and the world within us, than all things of substantial being put together.

Now, bearing upon diseases and upon their medical management, there is a certain thing which I have thus far designedly suppressed; for I was afraid of darkening the subject with a mystery, and so I kept it out of sight. But this mystery is a truth. Mystery indeed it is *to us*, for we know neither what it is nor whence it is; but it demands notice from its effects, which are full of reality and power. It has been called the "Epidemic Constitution" of certain years, or series of years. And so let it still be called, rather than by any name of more scientific pretensions. For it intimates nothing of the nature of the thing itself. But it acknowledges a reality and a power. This is enough.

The most eminent characteristic of this "Epidemic Constitution" is its levelling influence. Where it exists, and as long as it lasts, it subdues into conformity with itself all that is

peculiar to the individual man's Constitution, as well as peculiar to places and external things. These, which are wont to be so potent in modifying the forms of diseases and ruling and qualifying their treatment, now disappoint our reckoning. Behold, for a season mankind in various places and circumstances require a Treatment for their diseases contradictory to the experience of former times. Then wait for a season, and behold, mankind, in the same places, and in the same external circumstances, will require a Treatment for the same diseases contradictory to the Experience of the present times. But neither now, nor formerly, nor hereafter, will there be found in the vital being of men themselves, in their places or circumstances, anything to reconcile the contradictions or at all explain them.

But it is not only that diseases, having this, that, or any name, are rendered severally unlike themselves of other times by the Treatment which they now need and bear. This, indeed, would show an agency at work of great power—an agency, however, only of special and partial bearing. But these same diseases, which are thus made severally unlike themselves as they were at other times, become all like one another as they are at the present, by reason of the need they have of now receiving, and their capacity of now bearing, the same or similar modes of Treatment.

Thus febrile diseases—diseases, I mean, which require Fever for their development, and which altogether make up a large number of the sum total of all that flesh is heir to—become types and tests and interpreters of one another at the times wherein they are found, in what concerns their Treatment. Thus all so-called inflammations, in every part of the body, also diseases the products of different contagions and contrasted in their outward forms of presentment, and so bearing different names—Measles and Scarlatina, the Typhoid Typhus-Fevers—all give and take, impart and accept illustration, mutually and interchangeably. You read them through one another. In treating one you learn to treat another. What one needs and bears, another needs and bears. These are facts of great significance. They seem to point to some one wide-spreading and pervasive influence ruling and dominating over them all alike.

But do the diseases named denote the limit of the influence

in question? I believe that it may be traced much further. I believe that even those diseases which more than others are proper to the individual man, traceable to what he eats and drinks, or does and endures, such as gout when it is a febrile disease, the gout of distinct attacks and paroxysms, even they often own subjection to times and seasons. Now for a series of years the indications, which guide the treatment of particular cases, will generally demand remedies of greater power and of one kind; and then for a series of years the indications will generally be satisfied with remedies of less power, and of another kind. But only let present indications be justly chosen, and fulfilled according to a fit measure, and then the Treatment which they suggest, while it is variable at different times, will be at all times uniform in its success.

XIV.—Pain.

WHAT IS PAIN?—IT MAY KILL.—ITS RELATION TO DISEASES VARIOUS.—PAIN SOMETIMES A DIRECT INDICATION OF TREATMENT.—SOMETIMES NOT.

It would be a great thing to understand Pain in all its meanings. To understand it, first of all, in its largest or universal meaning; what it is in its own nature; how allied to the various morbid states and conditions in which it is found, how formed by them, how evolved from them. Then to understand it in its diagnostic meaning, as a symptom for practical use; how it serves to interpret the seat and the kind of any present disease. And then to understand it in its therapeutic meaning, as a symptom for the most practical use of all; how it may be taken to indicate the choice of remedies, and to direct their application and their aim.

An immense field of inquiry is thus opened to us. And, indeed, it would be an affair of high philosophy to expatiate worthily upon every part of it. But I come to the subject in the temper of one who witnesses Pain under the constant obligation of doing the best he can to mitigate or abolish it. Therefore I must be pardoned for passing briefly and lightly over much that presents itself of great speculative interest, if I can but convey to practical minds some just notion of what Pain is in relation to disease, and of its value and bearing as an indication of treatment. There are, however, a few general remarks which I first desire to make.

To anyone who should insist upon its being stated in terms what Pain is, it would, I hold, be a sufficient answer to say, that he knew himself perfectly well what it was already, and that he could not know it the better for any words in which it could be defined. Things which all men know infallibly by their own perceptive experience, cannot be made plainer by words. Therefore, let Pain be spoken of simply as Pain.

Again, of the things which belong to simple perception,

there is a wonderful agreement among mankind, as long as we are content not to talk too much or too curiously about them. If we do, people get doubting and misunderstanding their own feelings. And this is another reason for speaking of Pain simply as Pain. It is safer to appeal to men's perceptions than to their logic.

But, while upon these pleas, we are very fairly let off from defining Pain; we may perhaps be further admonished not to make it appear other than it really is, by the mode we speak of it. This, however, is a rather difficult business. Natural sensation suffers offence from some cause or other; and hereupon it is altered, raised, exasperated, and so it becomes Pain; but it is sensation still, and nothing more. Nevertheless, it is and always will be something more in common apprehension. No man, wise or foolish, ever suffered Pain, who did not invest it with a *quasi* materialism. I have known many a philosopher, outreasoned by his feelings, take to rating and chiding *his Pain*, as if it were an entity or quiddity of itself. Really, for practical purposes, we must often let people think and speak of things as they seem to be, and not as they are, making a compromise between philosophy and common sense. We must let them so speak of Pain. There is no help for it.

These are some of the considerations, interesting and puzzling at the same time, which are apt to occur to us when we think of Pain in the abstract. But let us now suppose ourselves at the bedside and within hearing, when Pain raises its cry of importunate reality. And the first thing to be noticed is the difficulty we have in judging of Pain as a symptom which we do not find in respect of other symptoms. The fact of pain being suffered at all must always be taken on the patient's own showing. For there is such a thing as shamming Pain. Then, admitting our patient honest and his Pain real, how are we to make sure of its degree? Its degree cannot safely be reckoned according to the patient's own estimate of it. Some make much of a little, and some make nothing of a great deal. Pain, indeed, has or seems to have its many degrees, and its extremes of small and great. There is a Pain which barely disturbs the complacency of a child, and a pain which is too much for the strength of a giant. But are these really the same in kind, and different only in degree? Does

the smallest Pain contain all that essentially belongs to the greatest, as the minutest atoms of matter have separately the same properties as their largest aggregates? Familiar language would seem to attest the common belief that it does. But small and great are spoken of Pain as in a figure. They belong primarily to things of bulk and substance. But the things of life and feeling are different from all things in the world beside. They can neither be weighed in a balance nor made to give answer of what they are to tests and reagents. Pain, itself a thing of life, can only be tested by its effects upon life, and the functions of life. And whether it be small or great (so to speak), or of whatever degree, it is to its effect upon life and the functions of life that we must look, if we would know the part it acts pathologically, and what it requires remedially in individual cases.

Playing upon the nerves as upon an instrument, the force which Pain can exercise over the movements of disease is very various. It can raise, and it can depress them. It can quicken them into twofold activity and liveliness, and it can subdue and bring them down as low as death. Moreover, still playing upon the nerves, it can so jar and spoil the concord of symptoms as to falsify the disease, and make it appear other than it really is.

In such manner, rather than by complaint of the patient, does Pain tell itself and its amount of great and small to the reckoning of the physician.

Pain may kill. It may overwhelm the nervous system by its mere magnitude and duration.

A poor fellow had suffered painter's colic for several days, when, by the use of certain remedies which he took, his bowels were scantily opened, and the Pain subsided. In a few days, however, the bowels became again constipated, and the Pain returned with more severity than ever. Then it was that he was admitted into the hospital under my care. I ordered him warm bath and laudanum. The next day, at my visit, the Pain was unabated, and the constipation remained. I did not omit to examine the abdomen carefully. I made equable pressure upon it with both my hands, and almost with the whole weight of my body; and, as long as that pressure was continued, he experienced relief. In the course of this day, he

went several times into the warm bath. While he was in it, his Pain ceased; but it returned as soon as he was removed to his bed again. The constipation was not yet overcome. In the course of the night, he became first cold, then delirious, then pulseless, then insensible; then he vomited some dark fluid, and then he died.

Upon examination of the body, neither within the abdomen nor elsewhere did any visible appearance of disease present itself to which his death could be attributed. There can be no doubt, I presume, that the man was killed by Pain, and died from exhaustion of his nervous system.

In vol. xiv. of the *Medical Gazette*, Dr. Borrett reports the case of a lady who died from the Pain of gall-stones. Her nervous system was overwhelmed by the agony arising from a gall-stone impacted in the cystic duct. After continued suffering for sixty hours, she became cold and pulseless and delirious, and so died. After death, nothing was found to account for it but this gall-stone.

In Dr. Merriman's *Synopsis of the Various Kinds of Difficult Parturition* is a case, furnished by Dr. Gooch, of a woman in whom the severest Pain and the most violent expulsive efforts still continued after the child was born, until she became cold and delirious, and her pulse gradually went out, and she died. After death, the Pain and the expulsive efforts were found to be owing to a polypus, which had its attachment to the mouth and some way up within the neck of the uterus. Dr. Gooch remarks upon the case "as a striking proof that mere Pain can destroy life."

There is an eminent instance of death from Pain—death by excitement and torture of the nervous system, followed by its fatal exhaustion, to which I will just allude. Perhaps it has often occurred to many minds.

In all civilised nations calling themselves Christian, capital punishments are now so contrived that death should result from a sudden arrest being put to the functions of an organ essential to life, and thus the criminal be mercifully spared (as far as possible) the pains of dying. But there was once a manner of capital punishment, esteemed the most ignominious, and reserved for the most criminal, which, when the Roman empire became nominally Christian, Constantine thought fit to abolish; I mean

crucifixion. It is obvious that crucifixion did not at once put a stop to any vital function, but brought on death by pure, unmixed, protracted torture. The victim died from slow exhaustion of the nervous system by Pain.

"It was the *third* hour, and they crucified Him. And when the *sixth* hour was come, there was darkness over the whole land until the *ninth* hour. And at the *ninth* hour, Jesus cried with a loud voice, and gave up the ghost." (Mark xv. 25, 33, 34, 37.)

Six long hours of dying agony! The thought is very awful.

In that complex of action and suffering which belongs to injury and disease, small things and great, accidents and essentials, are often so crowded together as to defy analysis, and causes and effects get so mixed that one can hardly tell which is which. Therefore it is an occurrence to be made much of, when some great fact stands out and claims its sure and unmistakeable agency in the great result. Here we have Pain and Death, and the sure connexion between them. And the rare and extraordinary cases, displaying the connexion, have their value mainly in this, that they show what, under given conditions, is possible, though it seldom come to pass. The result, complete and accomplished in the few, denotes the tendency which is wrapt up in the many; even the tendency of Pain to death, ever working towards it, and often making near approaches to it, but seldom arriving at it.

From the same or the like causes as the Pain that kills also comes the Pain that brings to the verge of death, and yet allows to live—from hepatic, renal, and vesical calculi—from morbid growths involving nerves—from some forms of colic. Pain from all these can produce the cold skin, the collapsed features, and the failing pulse. And when it does, who can mistake their meaning? Who can doubt what should be the treatment? The parts are various; the present actuating causes are various. But Pain is the common adjunct of them all; and so crying and so urgent as now to become practically in each case the representative of the whole malady—the sole indication of treatment, superseding and excluding all other indications for the time.

In this range of cases, the whole business of the physician

is with the nervous system. All the power and all the expedients of his art are needed to lull it, and, if possible, to lay it to sleep in its agony, and at the same time to hold it up in its weakness, and prevent it from sinking into death.

But let us fully understand what it is that we do, and what it is that we do *not* do, in such cases, by administering exclusively to the pain. We abate an accident, but we do not cure a disease. The disease remains; if curable, to be cured by other treatment; if incurable and uncured, to have the same Pain engrafted upon it on a future day. The Pain is neither more nor less than an accident resulting from the mere mechanism of the parts, which renders them obnoxious to suffer offence from foreign bodies within them or about them. The foreign bodies happen to be the products of diseases within the body itself; and the parts are affected by them not otherwise than they would be by foreign bodies introduced from without.

But, in the vast majority of cases to which Pain belongs, it is no superadded accident, but a true part of the disease. It is involved in the morbid processes going on. It grows out of them, and is sustained by them, and lasts as long as they last.

Now the Pain which waits upon the essential movements of disease would be a pretty large theme to discourse about comprehensively, and too much for our present purpose. The view we now take can only be partial, but it need not be erroneous.

Whatever sick-bed we stand by, and hear severe Pain complained of, and find it the accompaniment of febrile or sudden or rapidly progressive disease, we must not leave that bedside until we have satisfied ourselves whether anything, or what, is to be done expressly for the Pain. The result may be, either that we altogether decline to meddle with it, or that we make it our chief indication of treatment; but not without due deliberation in either case.

What a crowd of symptoms go to make up that best known and most distressing of all remediable maladies, the sick headache! I mean the thing which is acute and occurs in paroxysms. And the symptom which takes the lead of them all is the Pain. Yet, when we come to treat the disease, we seek no direct remedy for the Pain as such. Experience has warned us that *thus* neither would the Pain itself receive any mitigation, nor the whole malady be advanced towards its cure in the least

degree. Symptoms there are, besides, pointing to those organs of the body which are above all others within the reach of remedies—to the stomach, and bowels, and liver, and imputing to them scanty, torpid, or disordered functions. Accordingly, our trust is in medicines addressed to them, and not in opium. And, the pain being thus got rid of, we do not merely abate an accident, we remedy a disease. The entire complex disease is swept away, and the Pain inclusively. True, the same patient may have the same sort of Pain again; but then he must first have the same disease.

But have we here a constant rule of practice? Whenever (to re-state the premises) Pain is involved in the morbid processes going on, growing out of them, sustained by them, and lasting as long as they last, can it be laid down as a principle that it does not need a remedy for itself; but has its treatment always included in the right treatment of the parent disease? Indeed, it cannot be absolutely so laid down. The *duration* of the disease and of the Pain, as a part of it, according as it is long or short, must guide the judgment what to do, or what to abstain from doing, in respect of the particular symptom. In the cases just referred to, the disease is short. The pain, severe as it is, is endurable, while the disease runs its brief course and hastens to its end.

But there is still a wide range of cases equally displaying the subjection of the Pain, as in its nature so in its treatment, to the nature and treatment of the disease; equally showing the final cessation of the one dependent upon the cessation of the other, yet urgently calling in the meantime for something to be done specially for the Pain, for the sake of present palliation and relief. And it is still the *duration* of the disease which compels this practice too. But now its duration is *long* (it may be), very long, while the pain being severe (it may be), very severe, cannot be borne, until the disease has accomplished its entire course and brought itself to an end.

Take a severe attack of acute Rheumatism in a man previously healthy and at the prime of life. There are extreme force of vascular action, extreme heat, profuse perspiration, swelling of many joints simultaneously, and Pain, which is agonising and unremitting.

Now, if this disease had a certain stated period to run, and

that a short one; if it could be trusted to come to an end, or to conditions of sure mitigation in two or three days; then we might be content to treat it as a whole, and need not be fearfully calculating the tendency of its particular symptoms, or seeking remedies for them; we need not be making much of, and be specially treating, its Pain. Pain, severe though it be, so long as the nervous system bears it well and reacts upon it, may add celerity and force to the existing disease; it may quicken and invigorate all its actions, without disturbing their natural course and harmony.

But acute Rheumatism has no stated period to run. It is oftener long than short; oftener an affair of weeks than of days. And Pain is its accompaniment from first to last; Pain (it may be) of the greatest severity, and such as the nervous system cannot long continue to bear well and to react upon. Two or three nights and days made sleepless by Pain tell fearfully through the nervous system upon the whole bearing of the disease. In proportion as the nervous system is brought under the mastery of Pain, and is depressed, or subdued, or exhausted by it, the entire disease suffers fluctuation and change. The pulse, hitherto steady to a certain number, varies between great extremes; and hitherto strong, it becomes weaker. The heat of skin, which was uniformly high, becomes lower, and, at the same time, unequally distributed. And now strength begins to pass into weakness; and now one vital organ and now another gives notice of something wrong. This may be the semblance only of disease; but it may be the reality.

Observation, I think, may be trusted for the fact that Pain can do all this. Most rationally, therefore, does it become a special indication of treatment in acute Rheumatism whenever it seems likely to do it; that is, whenever it is in excess. Thus, in the conduct of a case of acute Rheumatism, the chief and greatest remedy sometimes is opium. It is almost always a necessary safeguard.

What has been said of the Pain of acute Rheumatism holds good of the Pain which belongs to other diseases acute in their kind, and of uncertain duration. It holds good of the Pain which attends acute Gout and acute Erysipelas; which attends the Inflammation, acute, severe, and involving many and various structures, produced by large burns, and lacerations,

and fractures, and sprains, and by the complex of several in the same man. We know, indeed, that Gout, and Erysipelas, and Inflammation, are curable diseases, and that their Pain will cease whenever they come to an end. Nevertheless, in the meantime, their pain being more than the nervous system can well bear, calls aloud for relief and remedy. For the exhaustion conducing to death, and even actual death may come as much as from the attendant Pain as from the diseases themselves.

More need not be said. For I am not specifying rules, but declaring a principle—a principle that receives abundant illustration from the fact to which all successful practice can testify; namely, that oftentimes the medical treatment of these formidable affections has turned, from first to last, upon the right administration of opium.

What an easy and simple affair would be the practice of medicine, if we could always find out what and where the disease was, and then had only to lay hold of it (so to speak) in its seminal essence, and to treat it, and cure it; and thus could treat and cure exclusively all that belonged to it; its fever and all its disturbed functions of parts, and all its excitement or depression of blood-vessels and nerves, and all its Pains!

And, indeed, one has now and then seen all this accomplished. One has seen a hard and bounding pulse laid hold of as the sole indication of treatment, and an heroic bleeding brought to bear upon it as the sole remedy. And, forthwith, an acute Inflammation, just emerged, with its many formidable accompaniments, has been summarily swept away. Here surely a great deed has been done; enough to make a man proud of the power of his Art, and a little vain, perhaps, of his own skill in wielding it.

But disease in its progress, *after it has some time emerged*, is apt to split itself into fragments, and each fragment to become a several centre for new morbid actions and suffering, to radiate from, and to present a several scope, at which new remedies must take their aim. Pain is one of these fragments and one of these centres.

XV.—Pain.

REMEDIES FOR PAIN.—ANÆSTHETICS.—ANODYNES.

Upon the all-important subject of Pain and its Remedies, there is still something that I wish to say—a little in the way of speculation, perhaps, but yet soberly. For I cannot help believing that physiological, pathological, and medical research is now at work towards a better knowledge and a better treatment of diseases of the Nervous system, and especially of Pain.

Surprising facts have within these few years become familiar to us respecting insensibility to Pain and the modes of procuring it. Already they have carried with them to the mind and hand of man a knowledge and a power so full of mercy in their use, and so stamped with the character of a divine gift, as to call for praise and thankfulness. Think of the world of Pain that has been spared by the use of chloroform! Think of the lives that have been saved by operations of surgery, which never could have been accomplished without it!

This would, indeed, seem enough, were nothing more to be expected. But there is probably wrapped up in these facts something more yet to be learnt and used for beneficial ends, though it may be of a less splendid sort and pretension. For they must have their own place belonging to them in the sum of our knowledge. And to ascertain what their just place is, and how they fall in with our prior experience of diseases and of remedies would seem to be a first step to any reasonable calculation what more they are likely to teach us.

The observation is as old as Hippocrates (lib ii. aph. 6), that in states of disease and injury which ought (so to speak) naturally to produce Pain, even extreme Pain, it happens sometimes that none is complained of, and, there is reason to believe, none is felt. But in all such cases, whatever be the present disease or injury sufficient to produce Pain, and yet not producing it, there is, moreover, some present harm sustained by

the sensorium. Thus, in the insane, casual hurts and accidents often lead to disease of great amount remaining unnoticed, because uncomplained of. And thus, too, in those who suffer fevers, when they continue long delirious, and so for the time are on a level with the insane, deep ulcers often appear on the back, or erysipelas spreads over a limb, unaccompanied by the least expression of Pain on the part of the patient.

Here nature offers us her own pattern of things; a sample (as it were) of her own handiwork for us to study. She blunts or she abolishes the perception of Pain; but, in the meanwhile, she strikes the brain with confusion or incapacity. And it is after this copy that we find ourselves working, whenever we would bring Pain into subjection to the power of medicine. There is nothing which we can administer with such intent but will make itself felt by the sensorium, while it operates remedially upon the suffering part. Be the effect proposed anæsthetic in the completest degree, or anodyne only in the ordinary sense, the sensorium must still be interested.

Hence, when we use such remedies, two things come to be considered and weighed against each other in almost every case; what does the pain need, and what will the brain bear? When all that is needed by the one can be borne by the other, then no restraint is put upon the just use of the remedy. But oftentimes the susceptibility of the brain limits its use to some inadequate measure, or entirely prohibits it. There are some unfortunate cases in which you cannot use opium to abate Pain, because the brain will not bear it.

But an anæsthetic remedy is a new thing under the sun. Therefore let us be sure that we know what it means. And as many things are better understood in contrast or comparison with other things than by themselves alone, so is this. And, therefore, we will still bring the anæsthetic remedy and the anodyne together, and mark the distinction between them; and thus contrive to get a better notion of both.

To make myself better understood, I will define what I mean by an anæsthetic, and what by an anodyne.

The Anæsthetic annuls the capacity of feeling Pain altogether, *without limit* as to its degree. It defies all the powers and methods of torture, all that fire or steel can inflict for the production of Pain.

But the Anodyne mitigates or abolishes Pain *within limits*. Its remedial power is restricted chiefly to Pains which are the adjuncts and consequences of disease. A sharp inflicted hurt from without is enough to break down all the power of defence or resistance it can offer against Pain. Your anodyne has no chance with fire or steel. Chloroform may be taken as the representative of one class of remedies, and opium of the other.

Thus far, tested by their effects upon Pain, the anæsthetic and the anodyne have the same operations in kind—only the one can do more in that kind than the other. The anæsthetic has the greater, the anodyne the less force. Mind, I say tested *thus far*, and as to the power which each has over Pain.

Well, then, if *thus far* the anæsthetic can do all that the anodyne can do, and much more, what need have we of both? Why, in our dealing with Pain, should not the remedy which has the greater force be always chosen before that which has the less? And since, for such purpose, the greater force and the greater certainty go together, why should not the anæsthetic entirely supersede the anodyne? Why, as a remedy for Pain, should not chloroform beat opium out of the field? Because, in the very treating of Pain, more is to be taken into account than the mere pain itself. Because, as aforesaid, the two questions are always to be asked, both, What does the pain need? and, What will the Brain bear? Truly, as regards this intermediate operation upon the Brain, each remedy, anæsthetic as well as anodyne, is tied to its own conditions of doing its proper work. These are such, in the case of the anæsthetic, as will not allow it to fulfil the manifold office of the anodyne; and such, in the case of the anodyne, as will forbid it, by any augmentation of its dose, to reach safely the full power of the anæsthetic.

This sensorial operation is now the thing to be considered.

Prior to experience, one would have thought that, when absolute incapacity of feeling was produced summarily and suddenly at our will, and by an agent at our command, and by one and the same agent, as by chloroform, one would have thought that its sensorial operation would be always the same in every case. But the fact is not so. In one instance, as

soon as the insensibility to Pain is complete, and as long as it lasts, there is a profound coma—no sense, no consciousness, no voice, no motion. The lungs respire; the heart contracts; and the blood circulates, telling us that the man is not really dead. The anæsthetic dose is discontinued, and it is not renewed. The sensorium throws off its coma. Sense, consciousness, voice, motion, all return; and with them returns the capacity of suffering Pain. But all has been a blank to the patient, from the moment that the chloroform had its anæsthetic effect to the moment that it lost it.

In another instance, as soon as the insensibility to Pain is complete, and as long as it lasts, there is no coma. Sense, consciousness, voice, motion, all remain; but they exist divorced from the present scene. The man is altogether in some visionary world. He sees and hears and talks with phantoms. But this world is always a happy world, and its phantoms are happy phantoms. And, indeed, it is passing strange to witness the dreadful business of a surgical operation going on, and see something of anguish and doubt and fear in every other countenance, and the patient himself the only person unconcerned and apparently happy. But the anæsthetic dose is not renewed; and its effects are unsustained, and wear themselves out. And then the sensorium wakes up from its dream, and the man returns to the real world; and he brings back with him his senses and his consciousness, his will and his intelligence, to live and dwell and converse again with things as they are. And, moreover, he recovers his capacity of feeling Pain. But he has to be told of all that has happened during his dream.

Ecstasy, trance, somnambulism (for we want a collective term for this class of affections), were things of rare occurrence, until we learnt that the means of producing them were within our own power. Accordingly, beyond the few striking phenomena, which bear the name, nothing was known about them. The instances were too few to furnish *an experience*. Nobody could tell what conduced to them, or what or how much more than the mere ecstasy, the trance, or the somnambulism, was wrapped up in their essence — what, in short, was the state of internal functions and feelings, what

were the capacities and susceptibilities *of pain* in persons so affected.

Whichever of these two remarkable states be induced, the coma or the ecstasy, the anæsthetic effect, which was the thing desired, is equally certain and complete: equally, whether the brain has its actings and its consciousness extinct for the time, or has them still alive and energetic, but under such forms and guises as characterise ecstasy, trance, somnambulism.

The operation of chloroform is one of those things which deserve to be dwelt upon and studied, for the sake of the light it throws upon the effect of medicines which are our daily resource and reliance in the treatment of diseases. Chloroform, administered as an anæsthetic, while it displays much that is properly and exclusively its own, exhibits something that it has in common with other remedies—something, for instance, in which it affords a magnified representation of what occurs whenever we give opium as an anodyne internally. Besides its local effect of abating or abolishing pain, which we desire and aim at, opium has a sensorial effect annexed, which is no part of our purpose, and which we had often rather be without. And it is remarkable that, incident to its operation as an anodyne, the sensorial effect of opium is of two opposite kinds, just as the sensorial effect of chloroform has been shown to be, which is incident to its operation as an anæsthetic.

Opium, administered in sufficient dose to abolish the severer degrees of Pain, such, for instance, as attend the passage of biliary or renal calculus, does not fully compass the purpose until it has made itself powerfully felt by the sensorium in one of two ways, either until it has subdued the sensorium into sleep, or roused it into delirium. The Pain remains in subjection to either of these conditions as long as they last. But when the sensorium awakes up from its sleep, or reposes from its delirium, then the Pain is free to return; and it will return, the cause still existing which first produced it.

But chloroform and opium, both possessing this vast power for the control of Pain, and both exercising it through the sensorium, are nevertheless not convertible remedies. Each can do what the other can not. Each can go beyond the other within the range of its special power. And it is the mode

and conditions of their operation upon the sensorium which prevent them from being employed *convertibly* as remedies for Pain.

That appalling impression upon the sensorium, which is essential to its complete anæsthetic effect, chloroform can be made to produce in a minute or two. But the impression does not abide. As in a few minutes it reaches its full amount, so in a few minutes it totally disappears. And if, for the sake of a longer anæsthetic effect, a longer sensorial impression be needed, the chloroform must be applied afresh; and so on again and again. Thus, when we use chloroform for its great purpose of procuring insensibility to pain during a tedious operation, we seem to be playing upon the sensorium, as it were upon an instrument, sustaining its effect so long as we keep the pressure upon the keys, and letting it subside when we take the pressure off.

Arising spontaneously, or from causes independent of our interference and intention, there are forms of disease, which, in popular language, and, indeed, in medical language, are called Fits. They are of great severity, of great present alarm, and of *short duration*, and belong most frequently to the brain. But Fits, severe as they are, and terrible as they look, and having their root, as they often have, in incurable disease, are themselves seldom fatal. Yet they are so sometimes. A man may die in a Fit. But before this happens, his disease has generally lost the character of a Fit. It has lost its shortness of duration, and its distinctness of attack. Each attack abides longer; and when it is considered to have subsided, something still remains as a memento of it, and occupies the interval until another attack takes place.

Let us take Epilepsy, for instance. Epilepsy is made up of elements which give it, apparently, a fatal stamp. Yet it is not fatal while it retains the character of a Fit; while it is an affair of minutes, or not of more than a quarter of an hour. But when it is lengthened out for half an hour, an hour, or more, it thus loses its character of a Fit, and a fear begins to arise of its proving fatal. Indeed, but for the shortness of its attack, Epilepsy (one would think) must be always fatal.

And, thus, chloroform does not produce its vast impression

otherwise than *fitfully*, and the impression itself may be said to consist of Fits of coma or Fits of trance. From its own nature the coma or the trance is ready to pass away every few minutes; but chloroform applied every few minutes succeeds again and again in keeping and retaining it or bringing it back. And numerous Fits procured in this manner serve to give an apparent length and continuity to the impression. But let the impression hold on spontaneously, let it sustain and prolong itself independently, and so lose the fitful character, then peril is nigh at hand.

This sensorial impression, then, produced by chloroform, which looks so formidable, has in its *fitful* character the great condition of its safety. And its subsidence, or inclination to subside, again and again, after being again and again renewed by fresh applications of chloroform for one, or even two hours, is still a condition of its safety.

Doubtless, you can produce as great an impression upon the sensorium by opium, if you only give enough of it, as you can by chloroform; but not an impression as great in degree, and, at the same time, as short in duration. You cannot play upon the sensorium with opium, as you can with chloroform. You cannot urge and take off the pressure of your opium, and so raise, or lower, or stop its effect once produced. Opium once given is gone beyond your power to recal. It must be then left to work according to its nature. And its nature is to exercise its impression, not *fitfully*, but continuously.

Now, *inflicted* Pain is, beyond all experience and conception of Pains, the most severe. No Pain can come near the Pain of the knife. To counteract it, to stop its cry, requires an insensibility which shall equal the insensibility of death. But the world has grown old before the medicine has been found which can procure the insensibility of death yet not procure death itself. But it is found at last; and that medicine is chloroform; and thus chloroform is peculiarly the remedy of inflicted Pain.

Further, *inflicted* Pain is as short or as long as, guided by the present necessity, we choose to make it. Generally it is short; or if it be long, it is rather several successive inflictions than one continuous Pain. In the meantime, we want to have

that deadly insensibility, which is counteractive of it, as much at our command as the injury which we inflict is at our command. To make the insensibility short when we make the injury short, and long when we make the injury long. Chloroform is the medicine which can both procure the insensibility and can put it thus wonderfully at our command. In this respect, also, chloroform is peculiarly the remedy for inflicted Pain.

But opium is no remedy for *inflicted* pain. The conditions of its operation forbid it. Its operation, whether great or small, is always continuous. It can produce the insensibility of death, but not without producing death itself.

XVI.

WHAT IS MEANT BY A MAN'S CONSTITUTION?—GREAT BREADTH OF THE SUBJECT.—FULL OF THINGS INDEFINITE AND DIFFICULT TO NAME, BUT REAL AND INTELLIGIBLE, AND REQUISITE TO BE STUDIED FOR THEIR BEARING UPON THE COURSE OF DISEASES AND THEIR TREATMENT.

WHAT do we mean when we talk of a man's Constitution? For, indeed, we are continually talking of it, both among ourselves and to our patients; and thinking of it, too, when we come to treat their diseases. And, moreover, the older we get, and the more conversant we have become with diseases, patients, and remedies, the more stress do we find ourselves laying upon a man's constitution. But this thing so talked of, and thought of, and practically regarded, what is it? Is it not a man's individuality, in a physical sense? Thus much it is, at least; and a great deal more than this expression would include.

Yet might not some lucky definition be hit upon, which would carry the whole meaning of the thing, and give us a rule to work by, that might be invaluable? But it is this very thing, even a man's constitution, which fills the whole subject of practical medicine with such endless circumstances and conditions as to spoil it entirely for definition. And strange indeed would it be, if what snatches everything else from the grasp of definition should itself submit to be defined.

True! the practice of medicine is sometimes narrowed to a point, and curiously eclective; and sometimes it is broad and free. But, whether narrow or broad, it cannot be fixed by definition, or worked by rule. The physician's success in the treatment of each particular case depends not upon the exact definition under which he can bring it, or the exact rule he can bring to bear upon it; but it comes from his own free choice of what is now best to be done, guided by the sum of all his former experience.

But if this (so-called) constitution of the man meet us thus at every turn in the practice of medicine, though it do not bear definition, yet will it not bear, and does it not demand, some careful thought? Such let us try to give it.

A man's constitution, taken for his individuality in a physical sense, may be that with which the physician has most to do upon the whole. But it is only part and parcel of his entire individuality; and the physician will find that he has to do with every part in its turn, and even with all of them together. The subject, therefore, needs to be considered a little comprehensively.

Now, all intercourse with the world as it is, and acquaintance with contemporary men, all study of history and of the characters of men still traceable upon its pages, would lead to the conclusion that never, from the foundation of the world until now, have there existed two individuals who, in outward presentment and in intellectual and moral attributes, were the exact counterparts of each other. Being first stated, this sounds a monstrous paradox. Being pondered a little, it begins to appear probably true. Something like the same bodies and minds have been traced in different ages, furnishing historical parallels. But no man of any age could be taken for either a re-embodiment or a metempsychosis of any other man of an age preceding.

There are certainly as many varieties of man as there are individuals; and these varieties reach to his entire nature. Peruse men by items, and you will see that, however much they resemble one another, no two were ever found each other's counterparts. Take two brothers as near alike as any two brothers were ever known to be. Being apart, they are continually mistaken for each other. In size, and form, and face, and feature, they are the same; but no man, the least observant, can live with them for a day and not ever afterwards distinguish between the two with as much ease as if one were white and the other black. He will distinguish them, and perhaps not know how it is that he *does* so distinguish them. But it is the expression of countenance—that wonderful indefinable reality—which lets out the secret, and tells which is which. From things of every moment, things without and thoughts within, there peep out or flash out gleams and changes which are the proper living

light of every human countenance, and, going and coming, photograph its individuality.

View men intellectually. Take two men educated together, and in the same way, and in the same things. Let all tests of competitive knowledge have found them equals. Let it be fairly allowed that neither is better or worse than the other. But there *is* a difference between them; and, if we could come at the secret of the manner in which the mind of each addresses itself to its work, herein we should plainly discern them different. One has had most trouble with his memory, the other with his reasoning power, while they have worked up their minds to the same mark, and have compassed equal results. Truly one may be neither better nor worse than the other, but they are intellectually different. Accordingly, put them both into the same profession, and one will succeed and the other fail. Put them into different professions, luckily chosen and suited to their secret but real intellectual difference, and they will both of them succeed.

View men morally, and still take them by pairs. Take two as morally the same as men were ever seen. They have become fast friends for life, from like dispositions and tastes. They seem to joy and sorrow, hope and fear, like and dislike, in equal measures and on equal occasions. The same accidents of life make both happy or both miserable. But they differ; and they know it well, though nobody else may see it. And their very friendship, however it began, is now maintained and strengthened by both of them well understanding wherein they differ, and being naturally indulgent of what each may think the other's weakness, perhaps by never touching it at all, perhaps by gently handling it, perhaps by seasonably playing with it, but never by assaulting it. True friendship has its own delicate tests of the moral attractions and repulsions of man's nature and its own intuitive skill in dealing with them. It is the best touchstone of moral individualities.

But when physicians speak of the constitution of a man, and make so much of it in reckoning the issue of diseases and directing their treatment, they are understood to have in view not what he is intellectually or morally, but physically. Yet how physically? Surely our thought is not of a man's body, and his body alone, when we speak of his constitution; but

of his body *with the life* in it, at least. As to size and bulk, shape and make of body and bodily organs, the differences between men are pretty obvious, and need not be pointed out. In the treatment of diseases, not much (I will not say nothing) depends upon what these differences are either wholly or in detail. But be the body or the body's organs of what size and bulk and shape and make they may, it is how and what they can *do*, and how and what they can *endure* under the ordinary wear and tear of existence; and again under the stress of unusual circumstances; and still again under the harder stress of diseases; it is thus, and thus, and thus, that men make show of their several constitutions and their physical or vital individuality.

It is a misfortune to have a great subject fall in one's way to handle, and have the fear of not handling it worthily; and, moreover, to feel at every step how possible it is to fail in explaining to others what has been in *practical exercise* by one's self during half a life. And such is the subject of man's individuality, which, being restricted to his *material vital* nature (σωματικως), is especially called his constitution.

There are more realities in medicine than we have language to describe—realities known and acted upon and put to the proof every day, yet not apprehensible as things which have definite forms and substance; and we have been obliged to find terms suggestive of what they are in their own kind, not as forms and substances, which they are not, but as living energies, which they are. Temperament, susceptibility, idiosyncrasy, diathesis, are such terms. They are in very common use among us; and they bear a stricter or a laxer sense, according to each man's habit of thought who uses them. But, stricter or laxer, there is a practical need of them; for they denote something real. Every living man has his health qualified by something which one or other of these terms may be allowed to stand for; and every living man has his diseases tinctured by the qualities of his health.

Temperament, idiosyncrasy, susceptibility, diathesis, are indeed terms of portentous sound; and they all stand (it has been said) for realities; yet not for definite forms and substances, but for living energies. And their meanings, thus denoted, seem transcendental and mysterious withal. But it is

the great mystery of life itself which is at the bottom of all the mysterious language we are obliged to employ concerning it. Yet it is far better for us to use these terms, even temperament, idiosyncrasy, susceptibility, diathesis, which belong exclusively and restrictively to vital things in their first and proper and only sense, than any plainer and commoner terms, which could only be made suitable to them in some secondary sense. For the plainer and commoner terms, having (as needs must be) their primary sense always adhering to them, would carry a peculiar hazard of misteaching us in our conceptions of those things to which, in their secondary sense, they were transferred.

This matter concerns us more than is usually supposed; therefore it is worth while to pause, and try to clear it up a little. Almost all language is figurative, and so far may obscure as well as illustrate the subject which it is used to denote. Inflammation and fever contain the figures of *burning* and *boiling;* and, in all our conceptions of their nature and treatment, the ideas of *burning* and *boiling* have been continually mixed up. But this is to make the name of a thing stand for a very part of it; and thus it is even possible that the name may come to dominate in our minds over the thing itself. Is it not true that the popularly prevalent notion of treating inflammations and fevers turns to the purposes of extinguishing and refrigerating? Hence the popular term *antiphlogistic*, which really takes one's understanding by storm; also *febrifuge*, which is startling enough.

Now, inasmuch as there is abundance of true inflammation and true fever which does not bear the faintest analogy to *burning* and *boiling*, and abundance of successful treatment of the same which is as remote as possible from our conception of *antiphlogistic* and *febrifuge*, it would have been well, perhaps, if the terms both for the diseases and their remedies had never been used. But the names are too ancient and venerable not to remain for ever. Let it then be enough to beware of their furtive figurative sense, as applied to diseases and remedies which may belie the nature of the one, and confuse the application of the other.

The realities, which these portentous terms, temperament, idiosyncrasy, susceptibility, diathesis represent, are nothing

less than a man's constitution, or the individuality of his physical and vital nature, and its distribution into certain species which are to be understood but not defined. And truly, in using the terms, we find ourselves in tolerable agreement concerning what they represent, and probably shall continue in this agreement as long as we are content to leave their meaning to our general conceptions, and to measure it by no stricter formula.

But in thus leaving the terms and their meaning free, we should always remember that there is no greater fault than the habit of employing them familiarly. It is an offence against good taste, and has incurred and deserved some ridicule. Moreover, it is excused by no necessity. Nevertheless, if we were denied their use entirely, the most sober-minded among us would not be able to communicate together upon many of the great practical realities of our profession.

Then there are other terms, in constant use among us, and of sure meaning enough for men to be agreed about the realities which they denote, yet not to be technically defined. Such are sanguine, nervous, bilious, gouty, scrofulous. And when these come to be added to temperament, susceptibility, idiosyncrasy, diathesis, they must be taken to mark (as they really do) the more special individualities of men.

All this, at least, "is meant when we speak of a man's constitution"—at all events, it is as much as can be conveyed by language. To venture upon describing more would be unsafe. But there *is* more, and most physicians know more. By observation of individuals in sickness and in health, they come to learn that this, that, or the other man, albeit in no outward respects different from the majority of mankind, holds his health upon conditions which none beside himself are tied to, or gets rid of his diseases by methods of treatment which none beside himself would require or bear.

Nothing so vitiates the practice of medicine as that over-refinement, which either pretends to see more than can be seen, or fritters away what it *does* see into endless idle particularities.

But it is not to over-refine, in either sense, when we patiently study the constitution and individuality of men respective to the treatment of their diseases. The practical end in view is ever admonishing us to make sure of what we see, be it obvious

or occult, near or remote; and, instead of multiplying the particulars which we find, to reduce them to as few as possible, that so they may be more easily grasped, and handled, and turned to use.

A man's physical individuality, when it is written and displayed in any of its larger and broader characters, warns and prophecies of things which threaten and beleaguer his mortal state as long as he remains in the world. It tells of one man that his health in its uses and duties and enjoyments, his real health, will always be tied to conditions of place, of regimen, and employment. It tells of another, that there are certain forms of disease to which he will always be obnoxious. In another, whatever be the disease he suffers, it points to the likelihood of certain intercurrent accidents and events. And thus does this individuality, which is written in larger characters, and is seen and read of all men, call for and deserve the special attention of the physician.

But a man's physical individuality may be written in a much smaller character, or in a character legible only by the physician, and not always by him, but only as circumstances chance to bring it out, and put it in points of view that make it visible. Yet this smaller thing, seen but by the physician, and by him occasionally and seldom, being, however, an undoubted reality, may become a vast event in its practical bearing and use. As thus, let a man be overtaken in the midst of health by some acute disease, by Typhoid Fever, or Pneumonia, or Enteritis, or Erysipelas; or let him suffer an extensive burn, or meet with some grave accident, lacerating and contusing soft parts deeply and widely; and after the lapse of weeks, made up of days and nights full of emergencies, met and managed according to their kind, and by remedies of definite aim and purpose, let the man be restored to perfect health. This single attack will serve the physician, who watched and treated it, with information, which will ever afterwards be his guide to the safe and successful treatment of every attack of whatever disease the same individual may suffer as long as he lives.

But how so? Even thus. In the individual case, the physician has not been studying a typhoid fever, nor a pneumonia, an enteritis, an erysipelas, a burn, a laceration, or a contusion.

He knew well enough before what they were in their own nature, and this case has not added one jot or tittle to his knowledge of them. And he never expected that it would. Practically, he has been turning away from the disease, and fixing his attention almost or altogether upon the patient who suffers it. He has been studying an individual constitution; and studying it under the surest test and trial that can be conceived of all its living powers. And what has the physician found? In one case, he has found the disease lightly and patiently borne; not so much suffered as complied with; and carried duly and seasonably through all its stages to its termination in health; and, whatever remedies were needed in the meanwhile, he has found them, too, as well borne as the disease, and answering their intended purposes, and helping the progress, and insuring the fortunate termination of the whole. In another case, he has found the disease impatiently borne and violently resented; the vascular system and the nervous system reacting with steady energy and power; and strong counteractive remedies, such as blood-letting, needed and successful in rescuing from death and procuring recovery—not without hazard. In another case, he has found the disease impatiently borne; and the vascular system and the nervous system making a show of reaction fitfully and capriciously, but mainly without power; and remedies as ill-borne as the disease; and the treatment of each day waiting upon each day's events; and depletion and support, stimulant and sedative, given interchangeably, yet suitably to their indications, and manifestly instrumental to the saving of life.

Thus, day and night, for two, or three, or four weeks together, have experiments, as if instituted for the very purpose, been going on to bring out of the several men what they are vitally made of. And the sum of what is noted in them under this sure test of acute diseases and their remedies, may be taken as the sum of each man's constitution, of his physical and vital individuality.

Now, whoever may be the physician that has seen me safely and well through an attack of Typhoid Fever, Pneumonia, Enteritis, or Erysipelas, or an extensive burn, or some grave accident, let me have the same to attend me ever afterwards in any severe disease which I may suffer. He may not absolutely

be the best physician in the world; but he is the best for me; for he knows my constitution. But the discerning public delights in nothing more than a change of physicians for the novelty of the thing. And for this, as for other novelties, it is apt to pay dear.

Well! but is it quite true, that either written in larger characters, and under all conditions of health or disease, and so seen and read of all men; or written in smaller characters, and brought out only by the emergencies of disease, and so seen and read only by the physician; is it quite true that this physical individuality is always ready to show itself and to play its part in modes of acting and suffering, and always to distinguish man from man? Not always! There are diseases which level all individualities. The plague, the cholera, yellow fever, make all men alike. Temperament, idiosyncrasy, susceptibility, diathesis, sanguineous, lymphatic, bilious, nervous, gouty, scrofulous, even strength and weakness, youth and age and sex, go all for nothing. The disease is as a poison, making no difference between life and life, but destroying all life, or bringing all into equal jeopardy.

Again, there are diseases which have a way of levelling and reducing to nothing the constitutional differences between man and man, otherwise than after the exact similitude of poisons. Poisons are poisons always and everywhere; while diseases may not have it for their constant power and property to level, and destroy, and jeopardise, widely and promiscuously, but they may have the power and property for times and places. It may be for a short or for a long season, over small or wide extent, perhaps for a year or part of a year, and over a certain district only; or, perhaps, for many years in succession, over a whole country, or over a continent, or over half the entire world. All febrile diseases which are contagious or epidemic have exemplified these truths in their turn. A single one is enough for our present purpose.

Of Scarlet Fever, consulting my experience of what it was during the few first years of my professional practice, and taking this for my guide, I must have pronounced that its danger to life was as small as it is possible to conceive, and that all the difference between case and case was manifestly due to the difference of constitution in several men. But of

Scarlet Fever, viewed by the light of a new experience, my experience of what it was during a series of immediately subsequent years, I must have pronounced it the most perilous of diseases, and that, no matter whom it might befall, it levelled all to a parity of suffering and an equal hazard of death.

The sum of our experience carefully reckoned is consistent enough, upon the whole, to allow of our making fair anticipation of the course of diseases, and of the effects of remedies in particular cases. Without this there could hardly be a rational practice of medicine at all. Certainly, there could be no agreement among medical men as to what is expedient to be done in any case.

Still of the simplest diseases and their course, and of the simplest remedies and their effects, our experience is not uniform enough to make us always sure of events, and excuse us from attentively watching all cases, if perhaps something new or unusual may occur in some, requiring changes of remedies and readjustments of treatment according to men's constitutions.

And thus, to comply with men's constitutions gives exactness and success to the treatment of their diseases, while it does no prejudice to any sound practical principles, but rather confirms them by showing their capability of yielding and adaptation to present necessity.

Considering how much individual constitution has to do with the treatment of diseases, a good deal more might, and ought to, be said of it, if it could only be said intelligibly. But it is difficult to deal with truths which are most important to know, and yet cannot be submitted to scientific tests. The truths of medical practice are many of them such, and in this predicament. There are some of the most important realities in the world, which, in all their fulness, can only be pictured to the mind by the help of analogies and similitudes. Then, "with what comparison shall we compare" men's constitutions?

As there is an atmosphere without the man by which and in which who ever lives *does* live; so there is, what may be called, by a just analogy, an atmosphere within the man by which and in which whoever lives *does* live. The external atmosphere is the air we breathe; and somewhere in the world,

perhaps, it has and always preserves its purest possible conditions, calculated to maintain existence at its best and its healthiest. But, throughout the known world, heat and cold, dry and moist, dense and rare, fair and foul, are variously distributed, and calculated, according to their measures, to subject life to degrees of deterioration, and to induce, and prolong, and perpetuate forms of disease. The internal atmosphere (so-called from analogy) exists diffusively within us, and feeds our life. And as the external atmosphere may have perfect purity in some places, but one cannot say where; so this internal atmosphere may have perfect purity in some men, but one cannot tell whom. Being, however, at its purest and its best, it maintains (we may suppose) in each and every part the power and capacity of acting and feeling according to the perfection of their nature. But it is variously qualified in different men; and his vitality so qualified is each man's constitution.

Now, there are nicely constructed instruments, which pretty faithfully denote to us the states of the atmosphere without from time to time. Indeed, for any accurate information about it, we depend entirely upon such instruments. So, diseases and remedies, and all sorts of emergencies, physical and moral, the wear and tear of life, are the instruments which test men's constitution for us; and we should know little about it but for them.

XVII.

FURTHER REMARKS ON PRACTICAL EXPERIENCE.—EXPERIENCE COMES FROM EXPERIMENTS.—SHOWN IN THE TREATMENT OF ACUTE DISEASE—AS PNEUMONIA.—AND OF CHRONIC DISEASE—AS PULMONARY CONSUMPTION.—HOW SUPERVENING ACCIDENTS MAKE THE TREATMENT OF DISEASES MORE EXPERIMENTAL.—IN (SO-CALLED) "EXPECTANT MEDICINE," NO SOUND EXPERIENCE ATTAINABLE BUT BY EXPERIMENTS WITH THE SIMPLEST REMEDIES.

THE highest praise which the world has to bestow upon the physician is that he is *experienced*. There must, therefore, be a good deal worth knowing about this experience, which is deemed his characteristic excellence; as, how he goes to work in search of it, and how he gains it, experimenting after his manner, and with whatever helps of science he can muster, or with none at all; but still experimenting. For in Medicine nothing that deserves the name of *experience* can be otherwise gained than by what deserves the name of *experiment*.

According to the common notion, experiment is usually concerned with inert matter, or, at all events, with things which have not life. But we need not hesitate to transfer it to vital actions and processes. Medical practice satisfies the requirements of an experiment, when it puts the vital frame, with which we have to do, under conditions of our own choosing, and takes note of the results. The sum of these results becomes our experience. And this is suggestive of future experiments *in eâdem materiâ*, how best to set about them, how best to conduct them, and the fittest means and instruments to employ upon them. These experiments are, in fact, *the* practice of physic, perfecting itself by use, and consummated in experience. His experience is each physician's depository of whatever principles, plans, and means he possesses for executing his future experiments; his foundations to work upon, his diagrams to work by, and his tools to work with.

Now the practice of medicine, when it is engaged in *treating* disease, acute disease especially, comes pretty near the current idea of an experiment. There is, then, always something within view and within reach, taken for the immediate object of experiment, and so taken because it is within view and within reach. Also there is test and trial made of things one by one, and note taken of single consequences and effects as they arise under our hands, promising, promoting, and ending in the ultimate effect, which is the departure of disease and restoration to health.

But, in *curing* disease, the current idea of an experiment is upon the whole less apparent. The one ultimate effect is the thing contemplated, and the thing aimed at by the remedy. There are no tests or trials of things one by one preparatorily, no note of single consequences and effects intermediately. Yet, when the one ultimate effect is near at hand, and follows rapidly upon the use of the remedy, as does the cure of ague upon the use of quinine, then there is nothing wanting to the idea of an experiment in the completest practical sense. Thoughtful men are at liberty to penetrate deeper into the secret, and find out intermediate effects, if they can. The rest of us are content with the simple result, and can repeat it any day by the same instrumentality. We can all cure an ague by quinine. The arrow leaves the bow and hits the mark. We see that, and we see this. But between that and this the way of the arrow was through the air, and for an instant we lost sight of it altogether.

Now if it be true in any large sense that medicine, to have sound experience for its conclusions, must have sound experiment for its practice, there should be plenty of instances at hand to prove it. And so there are. Since the least fallible of our senses has been brought to bear upon the two most vital of our organs, their diseases have come more and more within the reach of our scrutiny, and more and more within the compass of our knowledge. And so, too, has correlatively their treatment. The treatment, especially, of diseases of the lungs, both acute and chronic, can now hardly otherwise be prosecuted than as it is made to take the form of an experiment, and to comply with its conditions.

Take pneumonia, acute pneumonia. It tells itself articu-

lately to the ear. The ear follows it as it advances and as it recedes, and the ear dwells upon it as it stands still. Not that pneumonia can syllable its own name, and so tell us what it is. But its living facts speak audibly for themselves; and they are *the* disease, be they few or many, very few or very many; and these same are the objects of remedial experiment. But mind, there are other objects which are mixed with them and come first in experimental order.

There are troubled states of the vascular system, and the nervous system, which are, as it were, the body's signals of distress. Some intimate one thing, and some another. Certain of them bid us specially beware that somewhere within the man there may be inflammation going on. And, then, forthwith we betake ourselves to search for it in any part which falls under our suspicion, by whatever means we can. And thus finding, wherever it be, any unwonted organic movements, such as have part and place in inflammation, forming or formed or progressive, any that lie in the way by which it travels to its well-known events, then we take the same, and, be they few or many, we call them inflammation, and we treat them accordingly.

Thus a portion of the lung, crackling as it breathes, this is pneumonia; a portion crackling as it breathes and another portion giving no sound at all, these are pneumonia; portions crackling and portions silent, and, moreover, the bronchial breathing and the bronchial voice, these are pneumonia. And all these, as they shift and change, and precede and succeed, and variously intermingle with one another, do much more than by themselves singly or collectively, stamp and constitute the disease pneumonia.

Now our treatment takes up pneumonia as it finds it, early or late, and represented by few or by many of its living facts. But this treatment has two aspects. Indeed, I am not refining, being quite sure that, if it lose its simplicity, our treatment is spoilt as an experiment altogether. It has, I say, two aspects, and complies with the conditions of an experiment in both. One aspect is turned towards the lungs themselves; the other looks elsewhere, and almost everywhere, in the body beyond the lungs.

Not from the lungs themselves, nor from the living facts now at work within them, or their number, or their combina-

tions, or their changes, as noted and measured by the ear, does the treatment of pneumonia take its beginning and make choice of its remedies, and apply them with force, great or small; but from the vascular system and the nervous system, and from the present condition of both, as they are taken to mean strength or weakness, reaction or surrender, and all their degrees of much or little, rapid or slow, as noted and interpreted by what the pulse has to tell of them from day to day, or from hour to hour.

This is one aspect of the treatment; and experimental enough it is!

But all the while, and from first to last, the treatment is turned to the lungs, where it finds the aim of all, and the end of all. Here it metes out what it has to do, and then what it has done; and here it again and again metes out what still remains for it to do, and at length it makes its final reckoning of perfect or imperfect reparation. This is the treatment's other aspect! And in this, too, it is experimental enough. And truly, whenever pneumonia is very acute, and the living facts within the lungs are undergoing changes from day to day, and even from hour to hour, and whenever the remedies employed are capable of making a like rapid impression counteractively, then the physician, if he is to proceed safely and successfully, must work his treatment, as he would work an experiment, with his finger upon the pulse and his ear upon the chest, from day to day or even from hour to hour.

Strange enough, perhaps, it may seem to some, that I should find an example of treatment shifting and variable, and curiously experimental, according to the needs of particular cases in that very disease, where braver spirits have made boast of a treatment simple and uniform, and comprehensive of all cases, and to be settled beforehand. Men have held that the proper treatment of pneumonia is by depletion, always and absolutely, and so have bled in every case; and men have held that its treatment is by stimulation, always and absolutely, and so have given wine or brandy in every case. But if there be truth in the principles which have been laid down, the unconditional advocates of either extreme are hopelessly wrong-headed. Nay, should any believe in a golden mean, equidistant from both extremes, and pretend that they know it and can practise by it always and

absolutely, the experimental necessities of every case they treat should convict them of folly.

I have purposely dwelt upon this instance of pneumonia and its treatment, believing it to contain a great practical lesson. When disease is very acute, and its work of injury and disorganisation makes very brief stay in one stage and then advances to another, and each stage is fairly denoted to the watchfulness of the physician, and restoration to health and soundness are yet within reach of treatment, that treatment must be conducted strictly as an experiment from day to day or from hour to hour.

But a large share of what is justly deemed the experience of physicians, is conversant with results brought about by what, in any popular sense, would hardly be called *experiments*. Reparation of the disease, indeed, takes place, not without our interposition, or without means of our choosing, or without the witness of our senses. But then the amount, and the times, and the occasions of interference on our part are less, and fewer than seem naturally annexed to the business and working of experiments.

After a fair examination of their disease and its conditions, and the choice made of some fit place to dwell in, and some cautions given against what is wrong, and recommendations of what is right in diet and modes of living, how many patients are left to nature to do all the rest, and never seen more by the physician! Or seen again after long times, once or twice or thrice, and fairly examined again and again, and the same advice reiterated, or new cautions and recommendations given, according to altered conditions, the patients are remanded to nature as before! And thus, after the lapse of months or years, even many months or many years, nature has done her work faithfully and well. She has restored to health from various forms of chronic disease. Or, without perfect reparation, she has brought even incurable disease to one of those pauses, now understood by physicians, which are sure and lasting, and consistent with a long and an useful, and, allowing for human changes and chances, a happy life.

Physicians, the older they are, come to reckon a large amount of this sort of experience; and, in the meantime, they may seem more like men waiting for results than procuring them,

more as if passively looking on than busied with experiments.

But, experiment or no experiment, call it what you will (and one would not willingly press a word into our service to distort the sense), yet if there be a choice and use of means with a view to certain effects, and those effects be appreciable and actually appreciated from time to time, however slow may be their progress and remote their accomplishment, something tentative and in the way of trial has been going on all the while. And who were its institutors and promoters? Even ourselves. We chose the means, and set them to work, and still continued them at work from observations, made at needful intervals, that the work was proceeding slowly and surely towards its accomplishment. And thus, and thus only, do physicians gain whatever experience they have in the management of many chronic diseases. And, though not exactly in the popular sense, yet in a just and true sense, this experience is the sum and complement of experiments; equally so with their experience in the management of acute diseases.

Take for illustration, the commonest of diseases with which we have to do—a disease of the lungs too, and eminently chronic, even consumption. What was the state of the lungs when our patient went a certain winter to Madeira, and what when he came back? What was it during the next summer, when he remained at home? What when he again went to Madeira, and what when he again returned home, and what after he had remained at home a second summer? Here the treatment of the disease is brought plainly within the category of experiment. It is ruled for years by the double experiment of the good and evil of climate in individual cases.

There is no disease of which the treatment is so simply and delicately experimental as pulmonary consumption. Its success (within all possible limits of success) depends upon keeping it experimental through its whole course; and in particular cases it is best secured when the treatment is allowed to rest from first to last in the same hands. I am quite sure that its failure in an infinite number of cases has been owing to their passing from one physician to another. Let the general experience and practical skill of the several physicians be equal, and equal to the interest they take in the well-being of their patient;

yet every time he passes from one physician's hands to another's, the probability of his recovery is seriously diminished. The reason is this: the treatment thus runs the chance of often halting and often beginning afresh—of being taken up by one, not at the same point where it was left by another, or not with the same views; in short, the chance of being utterly spoiled as an experiment.

Nothing less than this could be fairly said of the treatment of phthisis, if it were a disease always of a certain type, transacting itself in its own way, and admitting no foreign admixtures. But there are in phthisis the accidents of the disease (its separable accidents, you may please to call them. They are scarcely separable, however, but in idea). There is hardly a case in which some or other of them are not present; and, being present, they bear practically upon the treatment according to their kind and degree, and never fail to bring it more and more within the category of an experiment. Thus from time to time, and under stress of its accidents, will phthisis become practically an acute disease.

Inflammation is an accident of phthisis. More than an accident, according to some, who would hold a measure of inflammation needed to help and hand it on from stage to stage. However this may be, enough of it is doubtless sometimes present to require the treatment of inflammation for a little season, in order to save the patient's life.

Hæmoptysis is an accident of phthisis. No fact, which is not of the essence of the disease, occurs more frequently. And it is never a barren accident; it always means something. And, as its meanings are various, so is its treatment, which takes the form of a carefully conducted experiment in every case.

Much employed physicians are never without many cases of phthisis under their care which are to them objects of interest and experiment, at different stages, for years. And their retrospect of twenty or thirty years, read by the needful records they have kept of their experimental treatment, brings forth an experience which is beyond all price.

Surely, for us physicians, it is not a vain exercise thus to run over what knowledge we have of two diseases common enough, yet very portentous, and try it and test it by its uses;

to take pneumonia and phthisis, and their living facts, ordinary and extraordinary, and seek in them notices of times and opportunities and warrants of what to do and what to abstain from doing; and so to gain for the treatment of particular cases the condition of exact experiments. Instances of other diseases might be added, but let these be enough.

And these great experiments, and such as they, make up a great experience. And the experience thus made is rich and prompt and clear in devising and working out other future experiments; and both together stand for nothing less than the active power and truth and faith of practical medicine.

The end of all the thought and labour of physicians is to make experiments with men's lives. Then what mischances, misjudgments, misinformation, what fallacies of all kinds, are apt to interfere with these experiments, and hinder or mar them! And then how difficult to assign them a place and a value among the materials of a sound experience! We study and pore over the essential constituents of diseases, and learn where to look for indications of their treatment. We wait and watch for the customary accidents of diseases, and learn how to encounter them. Precepts, books, authority, which is the experience of other men, serve us for great helps. But, when the cases come, their treatment must be specially and individually our own experiment, and our own hands must have the doing of it.

Excellent things have been spoken of Experience and of its great value to the physician. And justly. But, then, men are apt to talk largely and at random about what they are agreed to praise. And they have so talked about the Experience of physicians. Report would make it a very common thing indeed; as if all who had been in the way of it could not miss of possessing it. But the having to do with diseases and remedies for thirty or forty years does of necessity no more make men experienced physicians, than looking upon the heavens all their lives makes them astronomers, or digging and delving the earth makes them geologists.

It is a divine aphorism (all know whence it comes) that "Patience worketh experience." It is both divine and true; true of all good things, from the least and lowest to the greatest and highest. And surely what belongs to man's mortal life,

and the physician's care concerning it, reaches no mean degree in the general scale of all good things.

It is expedient that medical practice should in every case be conformed to the current idea of an experiment as far as the nature of that case will admit. No Experience worthy of the name can be drawn from any number of cases less accurately followed up than they might have been. In acute cases, when the rate of progress in the disease is rapid yet distinct; so distinct that it can be measured by steps and stages from day to day or from hour to hour, care must be taken that the counteractive impression of the remedies be rapid, and distinct withal; to be seen from day to day or from hour to hour. And to such cases, and to their remedies thus chosen and applied, and to their effects thus noted and appreciated, no one can deny the nature of experiments, and not allow that the sum of such experiments may stand for a safe Experience.

But when the rate of progress in the disease is by little and little, and upon the whole so slow that it can only be seen after the lapse of weeks or months, then the counteractive impression of remedies must needs be also by little and little, and slow, and incapable of being seen and measured except after the lapse of weeks or months. The remoteness of the effect is the condition which especially tends to perplex the procedure, and make void the results of each case as it occurs, and take from it the character of an experiment.

The power of medicine over chronic diseases is a thing hard to get at and appreciate justly. Nevertheless, there is some sound experience of it in the world. And this could only be so far as, in our dealings with particular cases, we have been able to make them read to us the lessons of so many real experiments. Experiments they must be in some sort, if they are to furnish the materials of Experience.

But, in medical practice, as the one ultimate effect is more and more distant in point of time, and little or nothing is to be seen, or contrived, or done preparatorily and intermediately, the current idea of an experiment becomes obscured or well-nigh lost.

It is like a man travelling to some far off place, and finding

no places by the way where he can sit down and rest himself, and few or no guide posts to tell him whether he be in the right direction for it or not. Still he holds on. Perhaps he has been there before, and is pretty sure of this being the direction in which he found it. Or, perhaps he has never been there, but some of his friends have, and they told him of this being the right road to it. And so it may be that, by his own sagacity and the help of well-informed friends, he reaches it at last. Or, after all his own pains, and all his friends can do for him, it may be that he never reaches it at all.

This parable fairly sets forth the sort of Experience which the physician is obliged to content himself with, in seeking whatever good his art can reach within a large field of practice. It is an Experience of summary and ultimate results, with little or no insight or reckoning had of prior and conducive events. It is a general Experience made up of what a man knows himself, and what he takes upon authority. In this field of practice, it is hard to believe ourselves exercising the same experimental art, as when every step we took was made almost sure to us, and we could almost read and analyse the disease by its remedies as we went along.

But there is what is called "expectant medicine." It points to a more cultivated field of practice than that which we have been just surveying. The term is significant, and implies the attitude of expectation which the mind now maintains; waiting upon its own self-experience, and still waiting in faith of (what it deems) trustworthy authority, for a more or less distant event. Now, a rare enough thing to meet with among physicians is this wisely expectant mind. It presumes a study and sound judgment of the sort and measure of evidence which the subject admits, and a decision habitually exercised upon it; not demanding more, but never content with less.

But there is a scepticism common among us, and much fostered by the philosophical part of our training, which is hostile to this state of mind. It requires evidence foreign to the subject matter or more than it admits of; and so not finding it, it believes in nothing.

Again, there is a credulity common enough among us; and this is in sympathy with the larger credulity of the world on

medical subjects, and is strengthened by it. It incapacitates for all patient inquiry; and it may come to believe in everything. Now, if there be no mean between the extremes of scepticism on the one hand, and credulity on the other, there can be no safe and successful practice in this region of "expectant medicine." But such a mean there is, in which all good physicians meet, and communicate, and understand one another, agreeing together in that prudent, patient, hopeful faith which they all perceive, but none can define.

Yet, when we come to test the matter fairly, how can there be any experience in this field of "expectant medicine?" For how can the management of individual cases of most chronic diseases, whether by cure or treatment, be conformed to the idea of an experiment? Verily, to any great degree of perfection, neither one nor the other is possible. But even in "expectant medicine" experiment would be practicable, and experience attainable in much higher degrees than they are, if physicians would only be content to work with fewer and simpler remedies.

I have myself a reasonable amount of faith in the power of medicine over chronic diseases. I have laid up a certain sum of *experience* fairly collected (as I believe) from *experiments* which I have been making all my life. But, then, all my life, I have been careful about my experiments, in this respect especially, I have sought to manage my cases of chronic diseases —in other words, to work my experiments—as much as possible by single remedies. On any other terms, I do not see how it were possible that I should have any faith at all. It is a mystery to me how such prescriptions as the following for any known forms of disease can end in any trustworthy experience:—

Quinine	Ipecacuanha
Steel	Stramonium
Zinc	Colchicum
Valerian	Iodide of potassium
Nux vomica	

Such complex prescriptions render the knowledge of the remedial effects of particular substances absolutely impossible.

Do the prescribers impute a distinct effect of its own to each of the ingredients, and so reckon the separate instalment brought by each to the remedial mass? Or are they content to take it in the lump, and rejoice in the oneness of the effect.

My excellent friend Dr. Chambers, as soon as he had chosen medicine for his profession, did not think it beneath his dignity to work at a great pharmaceutical chemist's, compounding medicines and making up prescriptions. Here he saw what had been carefully preserved, the autograph prescriptions of bygone physicians. And those which bore the initials of the most eminent were remarkable for these two characteristics, their plain and legible penmanship, and the very few and simple articles which they directed. Surely, it is not unsafe to read the men's minds in these documents, and conceive the character of their thoughts and proceedings in the great business of their lives. The men were evidently candid and clear-sighted and of simple purpose; and among them were the best of their time—Dr. Heberden, Sir George Baker, the elder Dr. Warren, Dr. David Pitcairn, and Dr. Baillie. In our day, the profession of medicine needs a little gentle pressure from some such hands as these, to steady it and keep it within bounds.

A gentleman went from Scotland to consult a celebrated watering-place physician. His complaint was asthma. A scheme of diet was laid down for him, scrupulously and minutely strict; and he followed it to the letter. A mixed multitude of medicines was prescribed for him, which had an unpromising look of strife and incongruity. But he took them all bravely and obediently. And verily he had his reward. He obtained relief of his asthma. But the asthma would still return; and, as often as it returned, he betook himself to his dietetic and remedial discipline, and it went away again; and so his faith was confirmed. In process of time, however, whether the diet was too austere or the medicine too nauseous, and so the flesh began to rebel, or whether a laudable curiosity set him to find out the secret of his treatment and relief, he certainly began to question the necessity of *all* the means to the end. So, on his next attack, adhering to his dietetic rules, he bravely took no physic. But the asthma abided, and would not leave him until

he had recourse to his accustomed medicines. On the following attack, he set at nought his dietetic rules, and scrupulously took his physic; and the asthma passed away as usual.

It was pretty plain that the physic-bottle contained the cure. But to which of the many ingredients did it belong? To one or two or three, or to the whole hotch-potch working mysteriously together for good? In a matter which so nearly concerned him, the patient might be pardoned for laying his rash analytical hands upon the mysterious mixture. It contained, among twenty other things, a few grains of iodide of potassium. Ingredient after ingredient was deducted; and, simpler and simpler as the mixture became, it still had equal power to abate the asthma, until the iodide of potassium was deducted in its turn, and then its sovereign power was gone. Again, all the ingredients were tried, excepting only the iodide of potassium; but altogether they did not touch the asthma remedially. Finally, every other ingredient was excluded, and the iodide left alone; and alone it displayed a sovereign remedial power.

Fortunate the man who can get rid of an asthmatic attack on any terms; but unfortunate the art that is content with a rare fortuitous and unaccountable success; it must be either retrograde or stationary. To scatter above twenty remedies, and to let hit which may, is like pigeon-shooting in companies. The bird falls; but whose gun was it that brought it down? Nobody is reputed the better marksman after a hundred volleys.

With all the credit due to pharmaceutical chemistry, and all our obligations to it, I doubt whether, in one chief respect, it has not done some harm. To bring many important remedies together, and unite them by a lucky combination, and compress them within a small compass, and so place them within the common reach, all this gives a facility of prescribing which is hurtful to the advance of medical experience. The facility of prescribing is a temptation to prescribe; and, under this temptation, there is a lavish expenditure continually going on of important remedies in the mass, of which the prescribers have made no sufficient experiment in detail. A simple implement or two, which a man has well proved for himself, is worth a

whole armoury of famous compounds taken upon the general credit. A few thousand years ago, a whole people was in fear and trembling. Their enemy was at the gate. Their hope was turned to a single champion. All weapons of war were at his service. The king's own armour was offered him—his helmet, his coat of mail, his sword. He did not (how could he?) resist the vanity of putting them on. But soon he put them off again; for "he had not proved them." And "he chose him five smooth stones out of the brook," and with one of these he did the deed which saved his country.

The Heart and its Affections, not Organic.

I.—general view of the subject.—the pulse.

It is always desirable to trace out the just limit of a subject, and to note the right places within it for all known facts; and also where kindred facts, hereafter to be known, may take up their places beside them. This I some years ago sought to do, as far as my observation then enabled me, for the subject of diseases of the Heart *(Lectures on Clinical Medicine, comprising Diseases of the Heart)*. It was a sketch only that I attempted. But the facts there considered, numerous as they are, and facts of the like nature hereafter to be known, numerous as they may be, would go to make up a part only of the entire subject. My concern was with structural diseases and disorganisations of the heart; and the facts brought under consideration went to denote them in their kind and in their seat, whether beginning or progressive, stationary or receding, curable or incurable; also their causes, consequences, and events.

But there are numerous facts, unquestionably belonging to the pathology of the Heart, which are of another kind, and which cannot be made to fall within the assigned limit. Therefore we must enlarge its boundaries, that nothing may be excluded which is found legitimately to belong to the subject.

In every organ of the body, and pre-eminently in the Heart, the living actions and sufferings of disease have a compass and a reach far beyond its material framework; and not only beyond it, but independent of it. Excess and defect in the force of the Heart's impulse, excess and defect in the number of its beats; also derangements of their natural order and sequence, and the same made perceptible in the arteries; also varieties of morbid sensation immediately referrible to its seat, from a mere sense of weight and oppression to acute and agonising pain: any of these may exist alone, or a few or many of them in combination, and yet the Heart be perfectly healthy and sound of structure.

But the Heart must act thus and suffer thus from some vital necessity, although that necessity be not demonstrably inherent in its own structure. And not being in the Heart, where is it and what is it? These truly are the very objects of inquiry. Such affections have been denominated nervous and sympathetic. But to call them nervous would seem to imply that they all belonged to one pathological category: and this is certainly not the case. To call them sympathetic, if the term be not taken in any strict sense, would be less objectionable. But if we so designate them, we must be allowed to embrace more than is usually included under that title.

The Heart passes for the most sympathising organ in the body. And it may be really so; or it may be that it does not in fact sympathise more than other organs, only more apparently. For who shall say that every part of the body does not sympathise with every other part, and that its functions are not put out of sorts by every other's detriment or disease? But the functions of many parts are hidden functions, and we know not how it may be with them. Yet the functions of others are apparent enough; of those, for instance, which secrete; and we know that their secretions are ever apt to be altered and vitiated, not merely by diseases of their own, but by diseases, small and great, beginning and ending in other parts, and altogether restricted to them. And so, too, of the heart; it cannot beat a beat too many, or too few, or too strong, or too weak, without its being straightway perceived; and thus we become convinced that it has as many modes of abnormal action derived from diseases without as from diseases within itself.

But it is not enough that sympathies and sympathetic disorders should be admitted as bare realities. They must become objects of familiar knowledge, if the physician is to make any safe use of them for practical purposes.

There are sympathetic affections of the Heart so constantly found in company with certain conditions of disease in the constitution at large and in particular organs, that they have seemed to be naturally annexed to them, and have come to be ranked among their symptoms, and taken for proofs of their existence, and for guides of their treatment. Such are the greater force and frequency of the Heart's action in fevers and inflammations. A fever is hardly thought to be a fever, or an inflammation to

be an inflammation, without them. Nor are these sympathetic affections of the Heart barely *annexed* to such conditions of disease. They are often found to keep exact measure with them. Degrees of frequency and force in the action of the Heart and arteries, and their variations from time to time, follow degrees and variations of febrile and inflammatory movements, as they increase and decrease and rise and fall, and so give notice of their incidents and tendencies and events; moreover, of what they need and what they will bear, for cure or palliation or safe conduct.

Again, there are sympathetic affections of the Heart found in company with certain conditions elsewhere, yet neither constantly so nor in all persons, but occasionally and seldom, and in a few. They look like mere casualties, if such things can be. Or if they come from a real physical necessity (as indeed they may), that necessity does not grow out of the nature of the disease, but out of the constitution of the individual man who suffers it. Such are the palpitations of the Heart and the irregular and intermittent pulse, which sometimes go along with disorders of the digestive and assimilative functions; which attend states of plenitude and emptiness of the blood-vessels and morbid qualities of the blood itself; also which are found in nervous and mental maladies. Of all these I will not say that no orderly account can be given, only that they can hardly be made out to follow exact rules. Nevertheless, they demand careful consideration; for when they come to be viewed in their instances and examples, then will appear their great importance They will not, indeed, be found to claim the rank and value of symptoms, because their alliance with the diseases which they accompany is occasional only and extraordinary. Nevertheless, what is occasional only and extraordinary is sometimes as a light let in from a new quarter, and bringing into view what would otherwise have lain hid. And thus these unwonted sympathies of the Heart denote a possible reach of diseases beyond their common range. Pointing to things which come to pass beyond our thought and reckoning, they serve to abate our wonder at the strange turns and issues of even common diseases, which have taken the wisest and most experienced by surprise.

But sympathetic affections of the heart, in their wide range and in their multitudinous forms and instances, have a very

unequal interest. As there are some which cannot be made too much of, so there are others which cannot be made too little. What can be said of palpitations of the heart, and intermissions, and irregularities of its beats, which come and go during a man's whole existence, neither originating in any known disease, nor terminating in any, nor abridging in any measure the duration of life. They must come from something, but we know not what. We may call them sympathetic, but it must be in a very lax sense.

There are diurnal variations in the state of the circulation which belong naturally to all men. In the great majority, they concern only the force and frequency of the heart's action, raising it a little and quickening it a little at different times of the same day. They must be owing to something, yet they are consistent with perfect health. But in some these diurnal variations go beyond the mere force and frequency of the heart's action. They go even so far as to disturb the rhythm and succession of its beats, making them intermit and flutter at different times of the same day. And these two must be owing to something, and yet they are consistent with perfect health.

The observations which have been made may at least serve to notify the extent of the subject in hand. They do not pretend to have done anything towards setting it in order. If inquiry into the sympathetic affections of the heart is to be fairly and fully carried out, it must begin with those which are the most common and the most constant, and so pass from them to those which are more rare. It must start from the well observed varieties of modes and degrees of action in the heart and arteries, which experience takes and uses as its everyday index of diseases and its everyday guides to practice. The elements of the whole subject are to be found in the Doctrine of the Pulse.

Before we proceed, let us just consider the standpoints from which we must needs make our survey. There is no organ of the body which during life submits its structural condition so freely to our knowledge as the Heart. Of the changes it is capable of undergoing, from age, from accident, or from disease, it is remarkable how many may be fairly reckoned within reach of our diagnosis; its bulk, whether great or small; its consistence, whether hard or soft; its cavities,

whether dilated or contracted, and even which is dilated and which contracted; its orifices, whether free or straitened, and even which is free and which is straitened. All this knowledge is put within our reach by auscultation and the stethoscope. Without them it would be unattainable, and with them it is only attained after a world of patience and industry and infinite trials and experiments. Difficult, however, as it is to get, it is pretty safe and sure when we once have got it. Another circumstance, which greatly helps to make our knowledge of the heart's structural diseases among the surest knowledge we possess, is that their immediate effects upon the organ itself, as well as their distant effects throughout the body, result in a great measure from mechanical necessity. Hence there is a good deal in every case which can be reckoned with much exactness. But it is far otherwise when the Heart being sound of structure is functionally deranged. All that then arises must come from vital necessity. All our reckonings are then made from other points of view, and by other means and instruments, and with less certainty.

Well, but the elements of the subject are then, it has been said, in the Doctrine of the Pulse.

There is not any disease, great or small, in any part of the body, vital or not vital, which the physician does not think to know better than he would otherwise know it, and treat it better than he would otherwise treat it, by interrogating the pulse. The information he thus gains is sometimes much and is sometimes little; but very seldom indeed is it none at all. To be fully aware when it amounts to much, or little, or nothing —that is, to appreciate the pulse at its true worth—is among the chief concerns of medical practice. Very seldom, indeed, I repeat, does the pulse convey no information at all. Even when amid a complex of symptoms, it remains just as it is in health; yet, negative sign as it then is, does it often become, under the circumstances, more pregnant of meaning, and give surer notice of what is going on, and of what should and what should not be done, than the most positive fact among them all. The pulse remaining steady to the force and frequency of health is often the very thing that is wanted to interpret a crowd of urgent and doubtful symptoms; to stamp them all with their

true value, and make them pass just for what they are worth. Thus, an appeal to the pulse is indispensable in every case. There is no getting on safely without it.

But the Heart is the prime mover all the while; and the heart is so far the *pulse*. By its all-pervading sympathy, it feels all that is hurtful throughout the body; and by its own peculiar mode of action it tells all that it feels, and telegraphs intelligence of it through every artery that can be felt.

The Heart using its proper language of sounds and impulses, how clearly and emphatically does it speak of its own diseases to the ear and touch of experience. Then the complete information which we desire comes almost always directly from itself, and further appeal to the arteries is superfluous. On some rare occasions, perhaps, the heart's direct language concerning its own diseases may not be so absolutely indubitable, but that it may gain clearness and explanation by what is further spoken through the arteries. But the converse of this comes to pass when the Heart is perfectly sound, and when it has nothing to tell of itself, but a great deal to tell of every organ and system of organs in the body beside itself, and of all diseases everywhere beyond itself and out of itself. Then its most frequent, precise, and trustworthy intimations are conveyed through the arteries, its own direct sounds and impulses being but of rare and occasional use and significance.

It requires some courage to talk gravely and with a purpose of instruction about common things. For either people do not listen at all, expecting to hear nothing new; or they listen reluctantly, not liking to be schooled about what they understand perfectly (they think) already. And if there be any one thing which medical men may be thought to understand better than another, and to have, literally and in every sense, at their fingers' ends, it is the pulse. And yet I am about to take upon myself to speak even to medical men about the pulse. It lies, indeed, at the very root of my subject to do so. But apart from this formal consideration, I am persuaded that I should be rendering an useful service to my profession if I could speak of it as I desire. I conceive that this same lamp, which we all familiarly use and trust to guide our daily path, may still be placed in a better position, and thence be made to throw a

more certain light upon the great objects of practical medicine.

All that can be known concerning the pulse, we learn either by attending to each separate beat, or to several beats in succession. By attending to a single beat we learn whether the pulse be hard or soft, large or small, that is, its qualities. By attending to many in succession we learn whether it be frequent or infrequent, regular or irregular, or intermitting, that is, its number and its rhythm. And it must be obvious that its number and rhythm alone can be submitted to actual measurement, and that all the rest, the qualities namely of each particular beat, is to be determined by the perceptions of the person who feels it. But men's watches agree better together than their perceptions. Looking to our several watches, we have an index as nearly uniform as possible, and so we are always of one mind about the number of the pulse and its rhythm. Consulting our several perceptions, we use a variable index; and no wonder that about the *qualities* of the pulse we are apt to differ. Yet, after all, practical men are found to agree pretty well about the quality of the pulse in particular instances. And it is because they consult it with no purpose of refining, but with the single aim of finding in it something to help them in their knowledge and treatment of disease. This single aim is mine. Therefore, in every part of the subject, and above all in this, which is naturally open to refinement, I shall avoid new names and new distinctions; for I know full well that if I cannot handle the subject with simplicity I shall make a puzzle of it. Let us, then, first consider the number of the pulse, whether it be frequent or infrequent, and the succession of its beats, whether it be regular or irregular, and afterwards the qualities of the pulse, whether it be hard or soft, large or small—in fact, these are all the distinctions which I wish to insist upon.

Let me add a single remark, lest I should seem to affect an over-simplicity. It is one thing for a man to understand a matter for himself and for his own use, and another thing to understand it and explain it for the use of others. Therefore, I would not exclude a regard to other modifications of the pulse (whatever they may be), which any man's own observation

may have taught him, and which he may have used for guidance and direction in the management of disease. There may be many such with which I have no acquaintance. Still, since there are things which practical men often know assuredly, and employ successfully, yet of which they cannot convey to others either the knowledge or the use; and since this is especially true of our present subject, I would willingly avoid those parts of it which, whatever I may fancy myself to know, I am still incompetent to explain.

The Heart and its Affections, not Organic—(*continued*).

II.—Number of the Pulse.—Is there any certain number, or any limit between certain numbers, that can be assigned as the standard of health generally?

There is a good deal in the nature of things without us which might favour the expectation that the things within us would be found to observe a more exact and undeviating order than in fact they do. Thus, prior to experience, it might not be at all unreasonable to expect that the pulse would be numerically the same in every healthy man; that the Heart would contract within the minute a certain number of times, neither more nor less; that at each contraction it would deliver into the arteries a certain quantity of blood, neither more nor less; and that the exigencies of health in all organs of the body would require blood to be thus supplied to them according to one exact measure of time and quantity. But a very little observation directed to the point will be enough to show that the pulse of health differs greatly as to number in several men. One man's pulse will be found to beat twenty beats in a minute more than another man's; while there is no appreciable difference between them of mind or body, and none of habit or pursuit, to account for it. In some, the pulse habitually reaches 80; in some, it falls short of 60; and in some, and those the majority, it is found at various numbers intermediate between the two. But which tells the best for health and long life—a pulse between 50 and 60, between 70 and 80, or between 60 and 70—my own experience has not yet informed me.

But while it is true that there is no general law of health requiring the pulse to be of any certain number, and that the same in all men; and true also that the pulse may take a wide numerical range in several men, and be consistent with health in them all; yet is there not some certain number which, being found in the great majority, may be deemed healthy by pre-eminence? The world is very curious about this matter; and

physicians have made it so by their constant habit of counting the pulse. Therefore they are bound to give the best answer they can to its questioning.

People in general have no notion of the sort and amount of evidence often needed to prove the simplest matter of fact. To the question—What is the numerical standard of the healthy pulse?—some might answer 68, some 70, and some 72. But I am convinced that, as an ascertained fact, nobody knows anything about it. The least sufficient proof of it which can be conceived would be so laborious, and so beset with possible fallacies at every step, that the result would hardly be trustworthy. It would require that a hundred people at least should be the subject of experiment; and, considering that they would stand as the representatives of all mankind, a hundred is few enough. Then these hundred men should be all nearly of the same age, and all in perfect health constantly and every day as long as the experiment lasted; and they should be under observation for several weeks at least, and in the meantime have the number of their pulse noted and recorded day by day and several times a day. These are a few only of the conditions which the experiment would demand. Lastly would come the formidable calculations of arithmetic, and its severe result, announcing, perhaps, that the number of the pulse in a healthy man at the prime of life was 1¼ beats in a second.

Now, to those who walk about with their eyes open, objects often present themselves with a fidelity and truth which are too apt to suffer diminution and loss when the same objects are submitted to more curious experiments. For Nature herself is wise and discriminative in the conditions she imposes for obtaining different sorts of knowledge; and thus oftentimes she is sternly practical, requiring that what is intended for *use* should be learnt *by use;* bringing all that can or need be known within the reach of daily observation, and presenting it as her own free gift to common sense.

From use, and the daily habit of counting the pulse, all that concerns its numerical healthy standard must have taken some orderly shape in the minds of most physicians. The following is the shape which it has taken in mine. Perhaps, in such a matter, I may appeal to my own case, and not be blamed for egotism. The oracle of old made it the top of

wisdom to know oneself, but did not fix the credit due to that fragment of self-knowledge which enables a man to keep count of his own pulse. My own pulse, when I am quite well and quite free of excitement both from within and from without is 56. And this number of 56 I meet with in others who are in good health often enough to show that the thing, taken alone, means nothing wrong; but seldom enough to show that it is not common. The same I would remark of the pulse of 80. I find it often enough in healthy people to make me sure that, taken alone, it denotes no evil; but seldom enough to make it uncommon. A pulse which is less in number than 56, or more than 80, is so rarely the habitual pulse of health, that I am obliged to regard it as exceptional, and can make no comment upon it from my own experience. Practically, then, and saving exceptions, I am wont to consider 56 and 80 as the two extremes consistent with health. But truly there are eminent odds between them.

Surely somewhere between these extremes, and *distant* from both, are the numbers at which the pulse is most frequently found in the healthy. Now *equidistant* from both is 68; but one would not venture to fix upon this as the number of health by pre-eminence. Thus far, however, one might safely go. The difference between 56 and 80 is 24, or three times eight. Add eight to one extreme, 56, and deduct eight from the other extreme, 80; and eight will remain intermediately, including the numbers between 64 and 72. And between these numbers (it would come as near the truth as experience can bring us to say) the pulse is found in the great majority of healthy people. But *the centre* equidistant between 64 and 72 is 68: therefore it may be added, that the pulse of health hovers about 68, being as often a trifle more as less, and less as more.

This, then, is the best answer I have to give to the general question, What is the numerical standard of the pulse in health?

HAVE INDIVIDUALS A CERTAIN NUMBER OF THE PULSE AS THEIR OWN STANDARD OF HEALTH?

But, while there is no general law of health requiring the pulse to be of a certain number, and that the same in all men,

yet may there not be a special law or (something like it) a necessity of health in each individual fixing his pulse to some certain number, and keeping it constant to the same? Observation must settle the question; and mine leads me to settle it in the affirmative.

It has been seen what a wide numerical range is consistent with health. Now, whatever be the habitual number of the pulse within that range in the *healthy* man, it is, as far as I see, always the same so long as body and mind are at rest and under no present excitement. If its number be 60, it is always 60; if 70, it is always 70; if 80, it is always 80. But a very little stir of body, a very little emotion of mind, even the stimulus of a temperate meal, is enough to add a few beats a minute to the pulse of the healthiest man; and as soon as body or mind is at rest again, or digestion is over, his pulse settles down to its habitual number.

The causes of excitement here contemplated are independent of disease. They belong to the occurrences of every day, such as even many times every day must and do accelerate the pulse inevitably and innocently, and then leave it to reach its standard number again. And it always *does* reach it, if the man be in health.

Observe, *by health* I here mean not merely a state of exemption from positive disease, but of exemption too from those caprices of feeling and function which are called nervous, and are ever making the pulse run wild, and hardly allowing it to remain at any given number for an hour together. Of health thus understood, it is a chief characteristic that the pulse is habitually constant to a certain numerical standard, and that it does not deviate from it but for adequate and appreciable causes. These causes, considered comprehensively, involve within them the whole pathological meaning and import of the number of the pulse both as a symptom of disease and guide to practice.

INCREASED NUMBER OF THE PULSE IN DISEASES.

There is nothing that a man can feel or suffer—feel as a passing incident of health, or suffer as a constant inseparable element of disease, which may not be put down for a cause of accelerated pulse. Of the incidents of health more need not

be said at present. But of diseases, whatever be their kind, if by any avenue of sympathy they can reach the heart, they all accelerate the pulse. All inflammations, all fevers, do so. The inflammation which comes on abruptly from injury, accident, or exposure, and overtakes men in the midst of good feeding and rude health, and calls for blood-letting to arrest it,—this, pre-eminently the inflammation of strength, accelerates the pulse. Again: the inflammation which comes from nothing seen or known, but arises and proceeds furtively in constitutions bad by nature, and made worse by poverty and wretchedness, and admits no blood-letting for its cure,—this, pre-eminently the inflammation of weakness, accelerates the pulse. The fevers (call them by what name you please) which dominated in the hospitals of London thirty years ago, which bore and required and were properly treated by such remedies as exercise restraint upon the movements of the Heart and arteries—by calomel, and antimony, and purgatives, and not unfrequently by blood-letting in its several kinds; also the fevers which now dominate in the same places, and which will bear no such remedies and no such handling, and are properly treated by whatever can raise, sustain, and stimulate—by wine, brandy, ammonia,—these, pre-eminently the fevers of weakness, and those, pre-eminently the fevers of strength, both alike have among their symptoms the frequent pulse.

In short, whatever be the nature of the disease, if the Heart feel it at all, however diversely it may otherwise show what it feels through the pulse, whether in rendering it stronger or weaker, harder or softer, larger or smaller, than natural in several cases, it will surely in all of them render it more frequent. Often the Heart is a more delicate test of something wrong within the man than his own consciousness. His Heart is beforehand with him. It tells of disease covertly but surely progressive by the unnaturally frequent pulse, while his own feelings persuade him that he is well. At length, after the lapse of weeks or even months, the constitution is awaked, as it were, to a conscious alarm, and, by its fever and its nervous irritation, confirms what the Heart has forefelt and foretold by the frequent pulse. Tuberculous disease often forms and germinates and maturates under these conditions.

DIFFERENCE BETWEEN THE NUMBER OF THE PULSE AND THE QUALITY OF THE PULSE, AS SIGNS OF DISEASE, AND WHAT EACH RESPECTIVELY IMPORTS.

Such are the facts, stated generally. We come now to consider their pathological and practical import. This greater number of the pulse in diseases, whence comes it, and what does it mean? What is the nature and amount of help which it contributes towards our knowledge and treatment of the case in hand? For surely the number of the pulse ought to have a vast deal to teach us, seeing what a point we make of ascertaining it in every instance. The pulling out of the watch, and the deliberation which follows, must appear to the patient at least the most solemn part of the interview with his physician. And, if this grave business has no great use in it, it becomes a mighty foolish pretence. Far be it from me to allow that it is so. But let us inquire and see. One thing is plain to common sense; namely, that the number of the pulse is just the sort of symptom which ought to be pushed as far as it will fairly go, and relied upon as far as it can be safely trusted; for the information it has to give, being in itself so easily ascertained and so indubitable, that no two men can disagree about it who are able to count.

Now the frequent pulse, being equally the accompaniment of numerous diseases different in kind, cannot take its origin from any of those proper elements which severalise them. It is proper to no disease; yet its constant presence in almost all forbids it to be regarded as an accident in any. Moreover, the frequent pulse is a passing event of health, as well as a necessity of disease. Its origin, therefore, must be sought in something common to both—something belonging to life and sympathy, in their widest sense—something without which neither action nor feeling nor suffering can exist—something without which there could be neither health nor disease.

For the sake of illustration, let me here anticipate a part of our subject which will be considered hereafter in its turn. Let me take the *qualities* of the pulse and their meanings, and put them in contrast or comparison with the *number* of the pulse and its meanings, and illustrate one by the other. The quality of hardness in the pulse has most important meanings

as a symptom of disease; but in health I am not aware that the pulse ever assumes that quality, or suffers that quality to be forced upon it. Heat, exercise, and stimulant drinks will for a time augment the frequency and force of vascular action. But mere force of vascular action is a different thing from the quality of *hardness* in the pulse. This they cannot produce unless they first produce disease. I am not aware that by any possible contrivance we can make a soft pulse a hard one at will. In order to the change, there must first be disease. Seeing, then, that this quality of hardness in the pulse has no existence apart from disease, I infer it to be in its very nature essentially morbid.

But in health the number of the pulse is continually found to transgress its habitual standard from obvious causes. Every sense and sensation, every moral feeling and faculty of the mind, opens an avenue to impressions which travel to the heart, and may so operate upon it as to raise the frequency of the pulse *without first producing disease*. Thus it is that the ordinary casualties of life accelerate the pulse by exciting thoughts, feelings, and perceptions which are pleasurable or painful in an unusual degree. And thus it is that we can accelerate the pulse at will by subjecting the man to influences capable of being felt above measure mentally, morally, or corporeally. Seeing, then, that this numerical frequency of the pulse, unlike its quality of hardness, may exist apart from disease, I conclude that it is not in *its own nature* essentially morbid.

But though it is not in itself essentially morbid, yet is it found in constant connexion with diseases, and is reckoned among their symptoms. The question then arises, *how* is the number of the pulse connected with diseases?

There is good reason, then, for believing that frequency of pulse is produced in diseases after the same manner that it is produced in health; and that diseases operate upon the Heart so as to accelerate the pulse, *not as diseases*, but as simple irritants. They can and do operate upon it as diseases, when they change the *quality* of the pulse; and they can and do operate upon it as simple irritants when they change the *number*. And they may do both the one and the other in the same instance.

In almost all diseases (it has been already said) the pulse is

raised above its habitual numerical standard. But no disease can be mentioned in which it is always or so far of a certain number, that the nature of the disease can be taken from the number of the pulse. On the other hand, it is only in particular diseases that the pulse assumes certain qualities. But then the quality may be so distinct and unequivocal, and so commonly annexed to the disease, as to admit it to be reckoned among its symptoms. This is the case with the quality of hardness in the pulse of inflammation.

How and whence comes this difference? It comes because the number of the pulse has nothing to do with the disease as such, but the quality of the pulse has everything. It comes because the number of the pulse is annexed to the moral, nervous, vital conditions of the man, which may be influenced by several diseases in the same way, or by the same disease in several ways, according to the varying susceptibility of individuals; but the quality of the pulse is annexed to the nature of the disease, which is the same in all men.

It is, then, the difference of the thing signified which separates and contrasts the quality and the number of the pulse as symptoms. The quality gives information concerning the nature of the morbid processes going on. The number tells nothing concerning these processes, but rather intimates how the system at large is affected by them, be they what they may. The quality notifies disease simply; the number how the constitution bears it, whether it feels it in that degree in which (so to speak) it ought to feel it, or feels it less than it ought and is passive under it, or more than it ought and is exorbitantly affected by it.

With this clue we are prepared to deal with the subject more closely. And, albeit the number of the pulse tells nothing of the nature of any disease; albeit it guides neither the choice nor the aim of any remedy, so far as that remedy is counteractive of morbid processes going on, yet it is not without reason that physicians are ever counting it and making much of it. For it denotes things more important than the diseases themselves, viz. how life and the springs of life are affected by them, whether they will hold out, or whether they will fail. And in treatment it guides us to things just as important as the very remedies, if there be any such, which are directly curative;

viz. the force and extent to which they can be curatively employed.

But nothing can be taken for *absolutely true* in practical medicine. Hence they who profess to teach others out of their own experience, should be careful of using language which may imply more than that experience will justify. At the same time, they must not be continually dealing in doubts and reservations, and so bring their experience below its value by seeming to have small confidence in it themselves. The truth which medicine is concerned with is a *truth of degrees;* the same, nevertheless, which the moral world is governed by. Wise men seek it and cultivate it, and make the best of it. In medicine there is small philosophy, and no use in making it seem less than it is.

That the pulse is constant to a certain number in each healthy man, is a fact already mentioned, and an important one it is. And this fact, it might be thought, would become a standard of reckoning in each man's disease. But in their ordinary practice, physicians seldom come to the management of diseases with the knowledge of this fact. Our patients are too often strangers to us until we are called to treat them; and then we cannot take for our standard of reckoning the exact number which belongs to health in the individual man, but can only assume the healthy limit of the generality, and consider that his pulse falls somewhere within it, viz. between 64 and 72.

It is worth a passing notice, how much safety and success in the treatment of present disease are promoted, when the physician has already known his patient in previous health or in previous illness, or in both. Hereditary constitution, and personal constitution and personal habits; hereditary disease, and personal disease once suffered but now past, may leave a perpetual entail of something able to give a shape or colour to every future malady which may happen to a man, and suggest one mode of treatment and prohibit another. I am even persuaded that any single severe attack of disease, and how the patient bears it, and how he gets rid of it, will furnish an exposition of what power there is in him for bearing and recovering from all diseases as long as he lives. And a physician who has had the treatment of him in one such disease, and brought him safely through it, has gained thereby a fore-

cast of much that will surely be found in all future diseases which he may suffer, as well as of the sort of remedies that they will require. If this, indeed, be true, it were well for people in general that they were aware of it. It might cure them of their fickleness in running from one physician to another, and teach them for their own sakes an honest allegiance to the one who, whatever others may know better than he, knows *them* best, at all events.

The health of individuals may differ from the health of the many in some distinct peculiarity, and be sound authentic health nevertheless. And this individual peculiarity of health may impress an inevitable peculiarity upon every disease which the man suffers, and yet leave it not less the same disease as that which bears the same name in the many. The peculiarity of the man's health may be an uncommon frequency or infrequency in the habitual number of his pulse, and this may be carried forward into all his diseases. Thus, whenever he is ill, his pulse will be much more or much less frequent than in just proportion with other symptoms which constitute the disease than experience says it ought to be. And thus I have known some cases in which the unduly slow pulse of health was still the unduly slow pulse in disease, even in severe disease, and the patient has been thought much less ill than he really was. And I have known many cases in which the unduly rapid pulse of health was still the unduly rapid pulse in disease, even in trivial disease, and thus the patient has been thought much more seriously ill than in fact he was.

In the treatment of disease our business is with the individual, and our experience of the many goes to fit us for our dealings with the one. Yet it may not exactly fit us. But if to it be added a prior experience, had of the individual now the subject of treatment, then it becomes a perfect experience, and as nearly infallible as the nature of the things to which it is applied will allow it to be.

The Heart and its Affections not Organic—(*continued*).

III.—the frequent pulse at its highest degree of significance.—cases.

There are things which, as they ordinarily present themselves in nature, are so beset with collaterals that, but for their sometimes appearing in simple forms, they could never be fairly understood. Such, among the symptoms of disease, is often the frequent pulse. It is one of a crowd, and is lost in a crowd. It is not duly attended to among many as urgent and as pressing for notice as itself. But then it sometimes stands almost or altogether alone. There is the disease; and all that can be reckoned as belonging to it in the nature of a symptom, all that can serve to direct its treatment and prognosticate its event, is a very rapid pulse.

Such cases are rare; but still they occur often enough to afford us opportunities of seeing and noting this symptom of the frequent pulse in its simplest, its most elementary, and yet most prominent form, and the fittest and best to develope its true pathological meaning.

I was called one Friday, at midnight, to visit a young married lady, aged 21, whose state had given just alarm to her medical attendants. Her pulse was too rapid to be counted, and so feeble that it could only just be felt. She made no complaint of pain; and no questioning, no examination by pressure of the abdomen especially, could elicit the least expression of it. The abdomen was soft, and very slightly distended. The tongue was quite clear and moist; the extremities quite warm. Moreover, there was perfect consciousness; and she expressed surprise at the apparent anxiety of those around her.

Here the only present symptom was the pulse countless from rapidity, and scarcely perceptible from feebleness. But there had been another symptom. On the previous Wednesday, at 3 P.M., suddenly and without warning, there had been a long and severe rigor. No reaction followed, when it passed away;

the pulse became extraordinarily rapid and feeble, and it had so continued ever since.

At 10 A.M. the next day (Saturday), the state of things was still the same, except that the abdomen had become flattened in consequence of evacuations which had taken place in the course of the night. There was still no positive symptom, but the pulse most rapid and feeble. At 10 P.M. this symptom no longer remained. The pulse was imperceptible at the wrist. The hands were cold. Consciousness remained, and even intelligence; and there was evidently no feeling on the part of the patient that she was going to die. She died in the night; I did not learn at what time.

Observe: of this rapidly fatal disease (if disease it was) there were only two symptoms—the long and severe rigor, and the rapid, feeble pulse. But they were not, either one or the other, symptoms in the strict pathognomonic sense, being common to twenty diseases different in kind, and diagnostic of none. But symptoms they were, and both alike, of things incident to all diseases, and terrible when they occur. The inceptive rigor denoted the shock suddenly given to the powers of life; and the countless feeble flutterings of the heart, which followed it, denoted their surrender.

Now, as to the nature of the disease in this case, we must seek it in things prior to the inceptive rigor, since it certainly did not appear in what occurred afterwards. This lady had been confined eight days. The labour was favourable. But the placenta was retained, and had to be torn away. Still she had gone on favourably in every respect until the rigor. There was nothing more to be told of previous history; and this, and experience of the like, were enough to show the nature of the disease, and that it was puerperal fever. But shall I say "that it was," or rather "that it would have been?" The morbid element was taken up, and became a quick-working poison to the powers of life. The disease had either only just emerged and was cut short, or it never emerged at all. The disease, so to speak, killed itself; it killed itself in the birth. Its first impression was a fatal shock; and there followed, not the symptoms proper to its own nature, but the one symptom of coming death—this countless fluttering of the heart.

I was suddenly summoned to a lady, about 40 years of age,

who was to all appearance brought at once within the grasp of death. She was pale and cold, and her lips were livid; and her pulse was of a countless frequency, and just perceptible, and no more. Her mind was clear; and she could just be heard to whisper a few articulate words, telling me that she saw all things in a mist, and did not well distinguish one object from another; and that she seemed as if she were going to be suffocated. This looked awfully like dying. And into this condition she had passed in the course of two hours.

I had no other expectation than that she would die immediately. But hour after hour passed away, and she still lived. Days and nights, even five days and five nights, between Tuesday and Sunday, passed away, and she still lived. She lived, but without the slightest appearance of rallying, and without the least token of any positive disease. The countless, scarcely perceptible pulse, comprised all the symptoms. Brandy, ammonia, ether, anything and all things that could stimulate, were given (for the power of swallowing remained), every ten, or every five, or every three or two minutes, night and day. But they produced no reaction. They kept the heart just moving, and that was all. The pulse was sometimes perceptible, and sometimes not; and several times, when it was actually gone, and all was over, as it seemed, life and a pulse were brought back again by a larger supply of brandy.

After five days and five nights thus spent, a change took place; and it was all at once. It was within a certain half-hour. As I sat by her bedside, I found her pulse had for the first time become distinct enough to allow me to count it. Instead of a confused flutter, it gave 120 distinguishable beats in a minute. It continued perceptibly constant to this number for one half-hour, without the help of brandy. Presently what was dusky became red, what was cold became warm; and so much of reaction brought so much of hope.

But how did this frightful mystery end? How did it clear itself up? The tongue became red, then intensely red, then papillated; and then a rash diffused itself over the neck and chest, then over the whole body. And all was consummated in a regular attack of scarlatina, which went through its course without any unusual or unfavourable incident, and the patient got well.

In the first of these two cases, there was the highest presumption *à priori* (perhaps the proof), that the puerperal poison had gained access to the blood, and had entered into deadly conflict with the powers of life. Had the conflict been less than deadly, it would have had its natural issue in some authentic form of puerperal fever, which would have had its own perils. In the second case, there was the positive proof, *à posteriori*, that the scarlatina poison, gaining access to the blood, did for five days and five nights keep up an all but deadly conflict with the powers of life. But life holding out, the disease appeared as the natural consummation at last.

Now these two cases have the force of crucial instances. They are experiments of Nature's own contriving, and exclude all fallacy in the result. They prove the true significance of the frequent pulse; viz. that it is not a thing of disease as belonging to its essence, but a thing of disease and of all that concerns disease only as being a sign and a measure of the degree in which life and the powers of life are then brought to feel and to suffer, and to languish and fail. And this is the interpretation which the frequent pulse will be found to bear in the whole range of diseases in which it occurs, and in all the conditions, few or many, simple or complex, with which it is combined.

FURTHER COMMENTARY ON THE FOREGOING CASES.

But, in these two remarkable cases, everything was uncommon and extreme. They furnished full-drawn representations of the rapid pulse, and, as such, well suited for studying its meaning; not, however, *exaggerated* representations, but strictly those which Nature offered. Yet it is startling to be told that the meaning found to belong to the rapid pulse in these two cases is the same *in kind* which it has in all diseases whatever. The difference between great and small, or of mere magnitude in things of the same kind, is often so considerable, that practically it is difficult to look upon them and believe them to be of the same kind in fact. But, in the things pertaining to diseases, identity of kind, when it is real, ought to be carefully preserved, for the sake of doctrine and of truth. Mere magnitude, however, forces itself into notice. And so it ought; for

life and death continually depend upon it, and it cannot be ignored with safety to practice.

In the two cases which have been cited, we have spoken of *life* and the *powers* of life, and of the rapid fluttering pulse bearing sole and infallible witness to their utter prostration and failure. But these expressions denote the truth popularly, rather than pathologically. Nevertheless, they *do* denote it. Still they are expressions which, being employed about diseases, are suited only to things of the greatest magnitude in their kind. Accordingly, it would be too much to speak of *life* and the *powers* of *life* being reached and implicated in every case of disease wherein the pulse was frequent beyond the standard of health. But it would be neither too much nor beyond the truth to speak of something being reached and implicated which holds peculiar relations with life—something, belonging to our organisation, which communes with life more nearly and at once than through the circle of our grosser and more intelligible functions. And this is the nervous system.

Now the nervous system has the heart for its gnomon or finger of the clock. This notes, by the rate of its movement, the various degrees in which the nervous system is affected, from the least to the greatest. It is the greatest which must still engage our attention, and the rapid pulse the index of them. And the cases which have been just cited will help to explain them. To press the language of our analogy a little further, for the sake of illustration, we have already seen in these two cases the index hurrying rapidly round the dial-plate, and telling that, from some cause or other, the mechanism within was running down, and, if it were not arrested, that it would quickly stop. Even prior to any outward presentments to give assurance of disease, even earlier than its known beginning, we have seen the countless fluttering of the heart and arteries give token of the nervous system already under trial of mortal suffering, and ready to let life go for ever. But there is no reason in the nature of things (as far as I know) why a morbid poison, or any other element of disease, should not affect the nervous system soonest of all, and sooner than the blood and blood-vessels, to which it properly belongs to work out disease into its cognisable products and realities. Only experience says that it is rare, very rare.

FREQUENT PULSE STILL AT A VERY HIGH DEGREE OF SIGNIFICANCE.—TO WHAT CONDITIONS OF DISEASE IT BELONGS.

Not until disease is shaped and realised into something which can be seen or heard or touched, and can be called by a name, is it wont to engage the nervous system perilously; and should the particular disease, according to its nature, have to go through a stated course to a certain end, and so admit a probable measure of its duration, it will not commonly be until towards its decline that extreme peril to life is indexed by extreme frequency of the pulse. But the time is often anticipated. This sign of fearful significance—this countless fluttering of the pulse—may break in upon the disease at any stage of its progress, either early, even very early, or in its mid-course. And when it does, some notable thing has often preceded it, well capable of giving a shock to the nervous system. Sometimes a sudden profuse diarrhœa has been enough to account for it; sometimes a hæmorrhage; sometimes an unhappy error of treatment by larger depletion than the patient could bear; sometimes pain—severe, long, uninterrupted pain; sometimes loss of sleep; sometimes the solution of some organic texture within, as the rupture and perforation of a bowel. Sometimes, however, and oftenest, nothing incidental or casual can be found to bear the blame, but only the malignant nature of the disease itself.

Still there is a hope for these cases; but it is small. Yet small hope is made much of by the hopeful, and often serves them for a great encouragement. And so it must and had need do now, or there will be no saving of life.

And there is a treatment of these cases. Death is inevitable if they are left to themselves. None ever recovered but by known, palpable, and adequate means. The treatment is simple enough. A single symptom has absorbed all other indications of treatment into itself, and remains our sole infallible guide what to do. The countless fluttering pulse now stands for everything. Watch must be kept upon it and its meaning. And its meaning is, that the nervous system is ready to sink into death. Accordingly, stimulus is to be given so *often* and so *much* as will keep it going and prevent it from coming to a stop. All this is simple enough in the telling. I wish it were so in

the doing. As much, perhaps, has been told as can be told. But the "how often" and the "how much" are the things upon which life now hangs, and a judgment has to be exercised upon them every hour or many times every hour. Therefore the sick man has a poor chance, unless he can now have the services of one intelligent nurse at least entirely to himself. To have also one intelligent physician constantly within reach would not be at all beyond his needs. But it is useless to insist upon what is generally impracticable. Nevertheless, many cases within my experience, in which life has been rescued contrary to all probable calculation, have had this unspeakable advantage; and some of them have occurred in the family of physicians.

But be it borne in mind that we have now to do with a part only of a very great subject. The nervous system, and its actings and sufferings under disease, this is the great subject; and our small part of it is the frequent pulse. Nevertheless, small though it be, it is the part which especially offers itself as a handle to the whole. The part, by which we can best lay hold of it, and apprehend it, and study it, and turn our little knowledge of it to practical uses and benefit.

I wish still to keep hold of this handle that I may show the share the nervous system has in the graver diseases, and how it often determines their form, their course, and their event.

It is only in the most chronic diseases that the nervous system does not sensibly participate. All which show an appreciable progress from day to day; all to which fever in any measure belongs, either essentially or as an accident; these cannot exist without the nervous system feeling them and showing that it feels them. Now, almost all such diseases have, in respect of their essence, a due course to run and an end to reach. Phlebitis has its mixture of pus with the blood, and its deposit in abscesses within and without; erysipelas has its diffused redness and swelling; scarlatina one form of rash, and measles another; small-pox has its pustules; typhus has its petechial maculæ of the skin, and the typhoid malady has its tumescence and ulcers and sloughs of the intestinal glands. And these several conditions represent the essence of the several diseases, and show it to be different in each. But there are other things belonging to them all in common, which have great

force in guiding their treatment and ruling all concerning them. It is, indeed, by the essential element of these diseases, and by its mode of working, whether diffused through the veins, or collected in abscesses, or sprinkled over the skin, or scattered through the bowels, that each fulfils its course and accomplishes its end. But it is the great vascular system and the great nervous system that are most conspicuously engaged, and acting, and suffering all the time and in all of them, and the sum of their acting and suffering is called fever. The number of the pulse is our chief handle for apprehending the part specially borne by the nervous system.

If the fever be small, with moderate sympathy of the nervous system, there will be moderate frequency of the pulse, with moderate increase of its power, and thus much is natural and perhaps needful. If the fever be greater, with more sympathy of the nervous system, the pulse will rise both in frequency and force. And still all is natural and all is in harmony.

But, as the sympathy of the nervous system continues to increase, and the frequency of the pulse to increase along with it, anxiety begins to arise; yet the strength of the pulse will still be the vital safeguard. Let, however, the sympathy of the nervous system increase more and more, and the number of the pulse run on to greater and greater frequency, then assuredly its strength will languish; and if the balance be not soon redressed, it will become a countless fluttering. And to such extremity is the man now reduced, that this countless fluttering of the pulse will represent all that is vital in him, and all that can be treated in the disease. Gradually or abruptly, in any of the diseases mentioned, this may become the condition of the patient. It is often witnessed in the typhus and typhoid fevers of late years.

This deadly sinking of the nervous system, represented by the countless fluttering of the pulse, this most terrible incident of febrile diseases; coming on at their beginning, or middle, or end, sometimes abruptly, sometimes gradually, not absolutely hopeless, and not absolutely beyond reach of a remedy, deserves to have all told of it that observation has learnt. Well, then, having once begun, it may continue many days or nights (three or four) uninterruptedly. And during all that time, the pulse will continue countless and almost imperceptible; abating

nothing of its frequency and gaining nothing of power for all the wine and spirits administered, but only just kept going and prevented from coming to a standstill. And thus the quantity even of spirits given in twenty-four hours has amounted to pints. And after these three or four fearful days and nights, the pulse may become of a calculable number again and of an appreciable power, and maintain itself at the less number and the greater power with a less amount of stimulus, and henceforth the disease will pursue its course exempt from the alarm of this particular symptom, which is the sure expression of the nervous system collapsing into death. But that the pulse should go on fluttering, and the nervous system go on collapsing into actual death, is the more common event.

Or this terrible incident of febrile diseases may occur after a different manner. The pulse will be countless and fluttering for a few hours, and then, under the use of stimulus, it will recover power and a calculable number for a few hours; and then become countless and fluttering again. And after the nervous system has thus gone on to sink and to rally alternately for a few days, it will rally permanently, and the pulse become slower and steadier, and the disease will proceed free from this peculiar peril. But more often the nervous system will sink irrecoverably, and the pulse flutter and flutter on, and the patient die.

There is a very curious and interesting circumstance well worth our notice, to which my observation, if it do not deceive me, has often borne witness as a matter of fact. It is that the nervous system having reached the degree of collapse, indexed by the countless fluttering pulse, carries with it the pathological consequence of suspending for the time the course of the disease. Whatever be the disease which it attends, whether scarlatina, or measles, or erysipelas, or small-pox, and whatever be the stage it has reached, at that stage it will stop, and all specific morbid actions come to a suspense and standstill; perhaps to be resumed, perhaps not. To be resumed, when (if ever) the pulse comes within some clearly calculable number, and remains steadily within it, notifying the steady recovery of the nervous system. Not to be resumed, if the pulse goes on fluttering, although many days have yet to pass before the nervous system has collapsed into death. It is curious how

disease thus under the all-subduing constraint of the nervous system, and halting for one, two, three, or four days, will, when that constraint is taken off, take up its course again from the stage at which it was arrested, and duly proceed to its end.

What took place in the remarkable case before cited is a specimen of the same thing in kind as that which now engages our attention; only there the disease was latent in its state of incubation; here the disease is manifest and already formed. There the disease had not yet begun, and the failing nervous system still withheld it from beginning during several days; here the disease being already formed, the failing nervous system withholds it from proceeding during several days.

The Heart and its Affections, not Organic—(*continued*).

IV.—the frequent pulse at its lowest degree of significance.

It is a fallacy to conceive that disease, as represented by its symptoms, is difficult to know and to treat in proportion as it is a thing of more danger and severity and altogether more extreme. The contrary would be nearer the truth. In the cases which have been considered, the symptom of the frequent pulse, as the single representative of the disease and of the vast peril involved in it, and as the single indication of what is to be done to save life, if life can be saved, was comprehended in all its bearings at a glance. But, in the vast majority of cases, this same symptom will be found less salient, and therefore more to be searched after; less standing alone, and more one of a multitude, and therefore needing to be compared, contrasted, and weighed with other symptoms, before its value can be ascertained. For, indeed, it always has a value, small or great, though it be a variable value in different cases of the same disease, and variable, too, in the same case at different times. These are things not to be seen through at a glance, but contain matter for patient thought to work upon.

Now, as we have hitherto dwelt upon the class of cases in which the frequent pulse is at the greatest and highest computation of danger, so we will next touch upon the class in which it is at the least and the lowest, and reserve for our last consideration the class which is intermediate between the two. And this last we shall have a better chance of understanding aright by previously knowing the two extremes.

Take twenty cases, and let the symptoms in each be characteristically different enough to show that they represent twenty several forms of ailment or disease; but let all the twenty have one symptom in common, and let that be the frequent pulse. Such are realities which are presenting themselves day by day

to the experience of physicians. Further, let the symptoms differencing the nature of the ailment or disease in each particular case be so little severe in any, and the one symptom common to all—viz. the frequent pulse—be in all so little above the natural standard, that no apprehension is felt about any single case from first to last; and all the twenty do, in fact, according to anticipation, come to a favourable result. And such, too, are happy realities familiar to our experience.

Now what is it which makes the number of the pulse worth the reckoning in such cases as these, that we should take so formal an account of it as we probably should and ought to do in our daily intercourse with the sick? Perhaps some of us might not easily be got to trouble ourselves about the meaning of a symptom which occurred in twenty patients, all of whom got well, as we expected they would, be its meaning what it may. But the frequent pulse, as the accompaniment of disease, is never without a meaning; and, whether it be of great or small degree, whether it denote a great present danger or no danger at all, its meaning is always the same in kind. In all these twenty supposed cases, the pulse, being a little above the standard of health, denoted that the disease was making itself felt by the nervous system. In none did it represent any present peril; but in all it contained the germ capable of possible increase to the greatest magnitude and the greatest danger.

There are other things belonging to feeling and function, in our vital economy, which follow the analogy of the frequent pulse in this respect, that whether they be much or little, or great or small, they preserve the same meaning in *kind;* yet, according to their *degree*, they notify and prepare the way for different results, even as different as life and death.

Hunger and thirst may amount to nothing more than a little sense of physical impatience, natural, wholesome, and not unpleasant to feel, about dinner-time; or they may reach the fierce craving which is ready "to demolish stone walls." But they are the same hunger and thirst, after all. The first are the hunger and thirst by which we live; the last the hunger and thirst by which we perish. And it is the chief business of our lives to provide that the first become not the last; and every day we are acknowledging practically the possibility,

though we have neither thought nor fear about its being realised. Nevertheless, we do sometimes find it realised in single instances of shocking notoriety. And the fate of large armies in war, and of some smaller "armies of martyrs," missionaries for man's enlightenment or for God's truth, shows that hunger and thirst should be the care of nations and governments, and that they who put human life to its hardest uses should study to preserve it.

The same may be said of pain. There is the pain which scarcely disturbs the equanimity of a child, and there is the pain which kills; and the first contains the germ of the last.

THE FREQUENT PULSE OF ACUTE INFLAMMATION IN THE STRONG AND PREVIOUSLY HEALTHY.

There are diseases, then, almost too much for medicine to combat with, and diseases almost too little for medicine to care about; and the frequent pulse is an ingredient of both. In the former, it is the one ingredient which is the very gauge of their formidable character; in the latter, it is that which should at least always prevent us from regarding them with indifference. But there are many diseases far less appalling than the one class, and less trivial than the other, which hold their place between the two, and have a greater practical interest than either. They are uncertain enough in their event to keep both hopes and fears alive during their progress, and amenable enough to remedies to encourage and reward the study of a whole life how to use those remedies aright. These many diseases have still the pulse, and its qualities and number, to present as chief objects upon which such practical study can be profitably bestowed.

Let the patient be in the prime of life and health, and let his disease have nothing specific or malignant in it, but proceed from some common accidental cause, such as exposure to cold, or mechanical injury, or a blow; also let the inceptive rigor or chilliness be past, and the disease have now plainly declared itself, both what it is and where it is—in fact, that it is acute inflammation in some vital organ. And now, before the disease has had any treatment to interfere with it for good or for evil, or to alter it in any way, the pulse will denote the morbid action in which the blood and blood-vessels are engaged by its new

quality of hardness, and the sympathy of the nervous system by the increased number of its beats.

Recollect, it is assumed that the patient was previously in perfect health and the prime of life. At this stage, when the great trial of the constitution from the disease and its treatment is yet to come, the pulse will be more or less frequent from a given amount of inflammation, according as it occupy a part more or less nearly allied with the nervous system by sympathy. To speak generally, the pulse will from the first be more frequent when the inflammation is of the organs within the abdomen, than when it is of the organs within the thorax. Moreover, the inflammation, though just emerged, will produce a greater frequency of the pulse according *to its extent;* and if its extent be very great, and the part it seizes upon be in close sympathy with the nervous system, the coincidence may even be fatal. Strange events occur in the course of a long experience, taking one by surprise at the time, but, when duly considered, finding their place in the order of things to which they naturally belong. I have seen, in the young and robust, inflammation (apparently common inflammation) seizing upon the peritoneum, and the pulse at once becoming countless and almost imperceptible, and rapidly sinking into death; and examination has disclosed the disease pervading the entire surface and every flexure and fold of the membrane. And still it has been in its earliest stage. The membrane has been entirely injected with red; but scarcely a trace of fluid or of lymph—the proper products of inflammation—has appeared upon any part of its surface or in its cavity.

But to what order of things does such an event as this naturally belong? Truly it belongs to the same order with those two remarkable cases already cited. *There* the disease was in its germ or first development; but already the nervous system felt it, and was overwhelmed by it. *Here* the same event is brought to pass in the same way. *There* the disease partook of a specific poisonous quality, pus-poison in the one, scarlatina-poison in the other mingling with the blood. *Here* it is of the nature of common inflammation; but its vast extent and the part it occupies are equivalent for evil with a malignant quality. It is no figure of speech, but a literal expression of the fact, to say that the disease destroys itself and destroys life at the same time.

This is a most rare event of inflammation, yet much too interesting not to fix our attention for a moment. Well, but suppose the inflammation acute, and the reaction complete and vigorous, there will be found wrapt up together in the same pulse these two cardinal symptoms—a quality of hardness representing the disease; and a more frequent beat, representing how the constitution bears it. Such an inflammation will require remedies of much force; and, at every stage and step of the treatment, the pulse will still denote by its quality the need of the remedy, and by its number the power of bearing it. Let *venesection* be the remedy. Upon every use of it, a very strict judgment must be held upon its immediate effects; and that judgment is still to be concentred upon the pulse. What *now* of its hardness, which called for the venesection? What *now* of its number, which said that the constitution would bear it? If, an hour or two afterwards, the pulse has lost much of its hardness and many of its number, then assuredly the venesection has told remedially upon the disease, and not hurtfully upon the constitution; and, should it return to its former hardness and its former number, venesection may be employed again. After the second venesection, the like strict judgment must be directed to the same points; and, should the pulse again show an abatement of its hardness and its number diminished, then has the venesection again fulfilled its immediate purpose of good, with no contingent evil.

I cannot determine how often venesection may be safely repeated as a remedy for acute inflammation, the hardness and the number of the pulse thus rising and falling in constant harmony with each other. But as soon as that harmony ceases to be maintained, or is much disturbed; as soon as, whether it be after a first or second or third venesection, the pulse, while it has lost its unwonted hardness, is found to retain its unwonted number, or to have it increased, even greatly increased —henceforth venesection will become a very doubtful or an absolutely forbidden remedy. Thus far it has exercised a salutary check upon the inflammation. And the hard pulse reduced to softness testifies as much. But it is now beginning to have, or it has had already, some hurtful influence upon the powers of life, or upon the nervous system; and the pulse, remaining as frequent as it was, or becoming still more frequent, is sponsor for the fact.

Now at this point the inflammation may cease altogether; and there may be no further need of remedies properly antiphlogistic, either great or small. The last depletion brought it to an end, and so did its work *upon the disease* well, very well. But it would have done its work, *upon the whole*, better, much better, if, in carrying off the disease, it had not left the pulse as frequent or more frequent than when the inflammation was present and progressive. The thing may be of no consequence when we have constitutions naturally vigorous and healthy to deal with; only, in now reviewing the treatment, our just judgment of it must be, that we have pushed our great remedy a little further than the necessity of the case required. But the inflammation may not cease at this point; it may still call for treatment. And the treatment of the inflammation now making progress, with a pulse of more frequency and less power, is no easy task. Of this something may be said hereafter. At present, let it be further remarked, that not only when we use venesection, but when we take blood in any way, whether by venesection, or cupping, or leeches; and, moreover, not only when we bleed, but when we seek to arrest a rapidly progressive inflammation by a rapidly impressive remedy, be that remedy what it may, whether mercury, or tartar emetic, or colchicum,—the number of the pulse still mediates between how much is needed, and how much can be borne. It balances between the force of treatment, as bearing salutarily upon the disease on the one hand, and as bearing hurtfully upon the nervous system and the powers of life on the other. And yet it is not the number of the pulse taken absolutely. It is not any certain number; but its number as it is increased, or is diminished, or remains the same, day by day, under the working of means and agencies powerful alike for good and for evil.

THE FREQUENT PULSE OF ACUTE INFLAMMATION IN THE WEAK AND CACHECTIC.

But inflammation does not befall the young and strong exclusively; it is much oftener found in the weak and cachectic. In them, too, it is more generally incident to all periods of life, and comes from slighter provocations of injury and accident from without. Indeed, its connexion with any external exciting

cause at all is, in the weak and cachectic, frequently so slight, so uncertain and unapparent, that we are led sometimes to deem it altogether spontaneous and engendered from within, and coming direct from the pravity of the constitution itself.

Now it is a general truth that, in these weak, these cachectic and often seemingly spontaneous inflammations, the frequency of pulse begins earlier and abides longer, and runs up to higher degrees, than in the strong and healthy; or (to look through the sign to the thing signified) the sympathy of the nervous system and the trial to the powers of life are now felt sooner and more constantly, and are carried to greater extremity.

Now, for brevity's sake, let us use the terms "inflammation of strength" and "inflammation of weakness"; for a little more must yet be said of both in their contrast with each other, if we are to get at the true meaning of the frequent pulse in connexion with either, and gain from it all the help it can give us in their treatment.

In all that anatomy discloses after death, and, if the part affected be within reach of the ear, in all that auscultation teaches during life, the inflammation of strength and the inflammation of weakness are the same thing; no mere anatomist or auscultator can tell which is which. But the same treatment which saves in the one case will kill in the other; for their treatment is determined, not by the conditions in which they agree, but by those in which they differ. Now they agree in all that is organic; they differ in all that is vital. What is organic consists in what can be seen, heard, or dissected. What is vital consists in what is felt and acted, and in modes and degrees of feeling and action—pre-eminently in pain; pre-eminently in fever and its attributes of cold and heat and perspiration; and pre-eminently, and above all, in the movements of the heart and arteries, the qualities and number of the pulse.

It is remarkable how health and strength can entertain inflammation, and go through with it, and yet show few or no signs of being vitally hurt by it. I have seen a few cases of inflammation requiring blood-letting, and giving its characteristic hardness in the pulse, where the number has not been increased a single beat beyond the ordinary standard. Indeed, where there are health and strength, while the inflammation

denotes its severity by the force of the remedies needed, the number commonly falls short of 100, and seldom exceeds it. But where there are cachexy and weakness, though the inflammation appertain to no part in strict alliance with the nervous system, the number of the pulse is commonly very frequent throughout. It often rises at once to 120, and in the end it often exceeds it.

Thus strength seems to answer the stimulus of inflammation vigorously, but temperately. It puts its characteristic hardness into the pulse, without greatly multiplying the movements of the heart and arteries. It feels the disease, and bears it well. But weakness seems to answer it with (at most) only a show of vigour, and impatiently. It may give something like its characteristic hardness to the pulse, but not without multiplying exorbitantly the movements of the heart and arteries. It feels the disease, and bears it ill.

But the inflammation of strength and the inflammation of weakness, albeit they are by nature the same things organically and different things vitally, may, by force of accident and circumstances, become *vitally* equalised and the same.

The inflammation of strength is very liable to mischance in respect of its treatment. Opportunity is lost; and when a severe and rapidly progressive disease needs a severe and rapidly impressive remedy, opportunity is very precious, and the loss of it may be the loss of everything. Many a fine robust fellow have I seen who has suffered acute inflammation of an internal organ two or three days without seeking or without being able to procure medical aid; and then, when he has procured it, his disease and its treatment have become a puzzle, from the conflicting indications which have arisen in the meanwhile. Three days ago, a venesection would probably have arrested the inflammation, and the patient have been safe by this time, and in a fair way of recovery; but now, if venesection be adopted, it has been because present symptoms have made us rather afraid to omit it than ready to use it. There has still been an inflammatory hardness in the pulse; but, to set over against it, there has been great frequency. There has been enough of heat and enough of seemingly vigorous action still remaining; but, to set over against them, there has been too much of nervous sympathy, too manifest a trial of the powers of life already begun.

Well, venesection has been adopted; and, happily, it has been just in time. It has abated the hardness and diminished the frequency of the pulse, and raised the nervous system and the powers of life. And thus the inflammation has reclaimed the characteristics of the constitution to which it belongs, and been still the inflammation of *strength*.

Or venesection has been adopted; and, unhappily, it has been just too late. It has taken the hardness out of the pulse, but it has greatly augmented its number. Also it has sunk the nervous system and the powers of life. The inflammation has lost the characteristics of the constitution to which it belongs, and become the inflammation of weakness.

But the inflammation of strength has been turned into the inflammation of weakness in other ways than by unavoidable mischance. Among the perils of disease we must not refuse to reckon the errors of physicians. And this truth has had its instances and its proofs when inflammation was to be treated by venesection or some sort of blood-letting. The choice of the remedy, and its time, and its measure, and its iteration, whether often or seldom, or not at all, are confessedly among the difficult things of medical practice; and he must be either very self-complacent or very oblivious who cannot call to mind that herein he has ever fallen into mistake. Amid many possibilities of error, it would be strange indeed to be always in the right. Nor among the perils of disease must we refuse to reckon the interference of friends with its treatment; and this, too, has been seen when inflammation has had to be treated. And, in charity to human feelings, it is hard to blame them. They see a perilous disease and a perilous remedy. They cannot restrain their anxiety. But it is unfortunate when they show it by distrustful questionings at every turn of the disease and on the use of every remedy; thus always aggravating to the physician his sense of responsibility, at the hazard of overruling his judgment, and of withholding him from doing the full measure of what is right. And too often this has been the sad result. What might have been wholly remedied has been only half remedied; and what has remained has been far less tractable now that it is allied with weakness instead of strength. In truth, there is nothing in practical medicine harder to deal with than this inflammation of weakness, whether it be natural

weakness, or the weakness of circumstances and accident, or strength abused into weakness by the fault or mischance of medical treatment. It is all practically the same thing, however it come to pass.

Let it be remembered, however, that our subject is not inflammation and its treatment, but frequency of the pulse. Yet this frequency of the pulse was not to be understood in its pathological significance, otherwise than by our becoming acquainted with it in those diseases which display it most conspicuously. Such are fevers and inflammations. For in them the nervous system is most largely engaged. And the nervous system, for the share that it has and the part that it plays, has its surest index and measure in the number of the pulse.

Already, as to the inflammation of strength, we have gone so far into its treatment as was needed to show the number of the pulse guiding and regulating it. And just thus far, and with the same intent, we will now touch upon the treatment of the inflammation of weakness.

In the inflammation of strength and its treatment, we showed the number of the pulse arbitrating between the curative and hurtful effects of great remedies, sanctioning their use up to a certain point and no further, and thenceforth forbidding them. We followed the treatment so far as, under this safeguard, the operation of these remedies was successful. But we ceased to follow it, when the inflammation was found still to continue, and the pulse becoming more frequent no longer sanctioned the use of the same remedies for its cure; when, in truth, the inflammation has assumed the characteristics of weakness, and required another treatment, but still under the same safeguard. For in the inflammation of weakness, as in the inflammation of strength, the number of the pulse does not directly suggest the use of any remedy or any mode of treatment; but here as there it still serves for an arbiter. Here as there, when other indications have pointed directly to the remedy or mode of treatment, then it sanctions, or forbids, or moderates their use.

Let it always be borne in mind that the progress of inflammation within the part from one stage to another, until it reaches irreparable disorganisation, need not be less rapid when allied with weakness than when allied with strength. It does

not move slower, because it moves with less force. And thus it calls as much as ever for a remedy and requires as much as ever that the remedy should be seasonably applied. Only the direct and immediate aim of the remedy is now different.

The ultimate aim of all treatment is indeed the same. Whether the disease be one of weakness or of strength, be acute or chronic, be common or specific, the end and aim is to cure it. But intermediately treatment has to do with many an aim which is not the end itself, but only conducive to the end.

When there is much and unremitting heat of skin, and the pulse is hard and yet but moderately frequent, and the vascular system and the nervous system are strongly and steadily reacting, and all that moves and all that feels within the man are (so to speak) doubly alive, then, the fact and the seat of the internal inflammation being once ascertained, it is less absolutely necessary for the success of our treatment to go into its particulars. It is enough, for the present to take the inflammation in its largest sense, without noting its stages and its consecutive phenomena from day to day or from hour to hour. For now the remedy has a less concentrated aim. The indications of treatment are more diffusive and general.

But in proportion as the heat of skin is less sustained and less in degree; and the vascular system, and the nervous system, and all that moves, and all that feels, show less of reaction and less of life, and the pulse is less hard and more frequent, then we have the more need to be acquainted with the exact seat and stage of the inflammation and all its organic conditions; and its shiftings and movements from day to day; its advancing, or receding, or standing still. For now the treatment is more and more drawn towards the part, and more concentrated in its aim.

Between no display of strength in the constitution at large and extreme weakness; between no notable sympathetic reaction and vital surrender and collapse, there are many states and degrees; and the progress, the rapid progress, of inflammation in the part is compatible with them all.

A WORD OR TWO

ON

MEDICAL EDUCATION:

AND

A HINT OR TWO FOR THOSE WHO THINK IT NEEDS REFORMING.

By P. M. LATHAM, M.D.

[Extracted from the "British Medical Journal," Feb., 1864.]

"Science is his forte, omniscience is his foible." This was spoken by one well able to instruct mankind; and with such a lucky way of doing it, that he could wrap up wisdom in a witticism. And this was one of the wisest and wittiest things he ever said. He aimed it at some individual, and perhaps hit him and made him smart. Had he aimed it at our profession, would he have hit it? Perhaps he would; and if he had, we should only have laughed at it. For crowds keep one another in countenance where individuals feel sharply.

The Profession of Medicine, in itself and in the things pertaining to it, is running over with knowledge. The studies proper, preparatory, and collateral to it, are enormous. There are given us things to see which for their minuteness make our eyes crack; and things to understand which for their vastness and infinitude, as they come pouring in upon our minds, make us cry out, "Hold, enough!" Well may they, small and great, all so generously forced upon us in the way of educational culture, turn us poor physicians to sadness, when we think of the return the world may expect at our hands. "Beneficia eousque læta sunt dum videntur exsolvi posse; ubi multum antevenêre pro gratiâ odium redditur." (*Tacit.*

Ann., iv. 18.) Benefits give pleasure as long as they seem within the possibility of repayment; when they have gone much beyond it, dislike takes the place of gratitude. Is not the medical profession at this day accumulating scientific obligations upon itself, which it can never pay in any tangible current coin of usefulness either to itself or to the world.

Having been a good long time physician to a large hospital, I was in the way to see how medical men might be made or marred in the training. So noting or fancying some extravagances in the course of their education laid down by authority, and thinking they boded some harm to the profession, I presumed to bear my moderate testimony against them. This was more than a quarter of a century ago. But my voice was a feeble one. It was no match at all for the edicts of halls, colleges, and institutions, new and old, which were rivalling one another in the ruinous amount of taxation they levied upon the time and thought of medical students, with their hundreds and hundreds of lectures sentenced and inflicted upon them without mercy. At length, however, I am told that our rulers are going to revise their taxation papers, and make their educational imposts with some regard to what their subjects can bear and can afford. Some public bodies, upon a retrospect of this quarter of a century or more, have come to find out and proclaim their error, and are honestly bent upon repairing it. And they shall not want my voice, though now much more feeble than ever, for their encouragement and congratulation.

There is no doubt whatever that the grievance has been a real and serious one, and that the more thoughtful students and the best and more experienced teachers and practitioners of medicine and surgery have been convinced in their own minds that it was so, even for more than a quarter of a century.

It is now two and twenty years since I ceased to be physician to St. Bartholomew's Hospital, and it was a few years before that time, that I had a most interesting conversation with two medical students. They possessed the better order of minds, and had devoted to their professional education at St. Bartholomew's three years with great applause. The day for the annual award of prizes was just passed, and they had swept away between them all of any account and credit. On the

following day, they came to me both together, begging to be told how they were to learn their profession. This was the simple way in which they put the question to me. There was no mock humility in it. Their look and manner were much too serious for me to suspect anything of the kind. Thus the matter stood with them upon their own showing. During three whole years they had been hard at work upon all that lectures and museums could teach them. And verily they had thus far their reward. But they had utterly neglected the wards of the hospital. This was their own honest confession and regret. Their avowed temptation and excuse were the *eclat* that awaited them when they returned into the country, from the distinctions they had gained.

Here are two right-headed, accomplished students, in the hour of their triumph, convinced that thus far they had not gone the right way to work for making themselves good practitioners.

Ten or a dozen years ago, I was present at the annual celebration of one of our medical schools. The distribution of the prizes had already begun, when an eminent member of our profession came in, and, finding a place vacant near me, he took it and sat down, and looked over the printed list of successful candidates which I held in my hand. Whereupon he exclaimed, "Fifty distinguished students! why, they will all be ruined!" He to whom I allude was then our best living example of science and practical experience combined in their just proportions. In him they had done all that they could do for professional success, and he was looked upon by the world as well deserving the high position which he held. He held it without envy. Moreover, he had been a teacher. Now his words need not be taken at the full of what their literal force will bear. But at least they meant this: that the system under which the fifty medical students had gained their present credit was a wrong system in itself, and that so they would find it in the long run.

Take, then, Sir Benjamin Brodie—for he was the man to whom I allude—to represent the best and most experienced of teachers and practitioners, and take the two students who have been mentioned to represent the more thoughtful of their class, and we have the sober judgment of learners, teachers, and

practitioners, that there has been something radically unsound in the medical education prescribed by authority for more than a quarter of a century.

I often think of my profession and its fortunes as of a poor man with a crowd of rich and noble relations. He thinks surely they must help him in the world. And so he cultivates them assiduously; and perhaps they are well enough disposed towards him. But then they are not constantly taking him by the hand, and lifting him over the rough places of life; and simply because they cannot. The truth is, they do not understand his daily objects; and if they were to meddle with them, they would only mar them. Perhaps, however, they may render the poor fellow good service every now and then by chance. Besides he has always the credit of belonging to them; and that is something as the world goes. But let him beware of that very common evil among us, the vanity of large acquaintance; of having, or affecting to have, an intimacy with everybody worth knowing. It is a vanity not fit for all men to indulge in. All cannot afford the expense of time and trouble required to keep it up.

Now here are real truths signified, whether my parabolical mode of putting them be wise or foolish; truths in which our profession is concerned, and would do well to consider. There is nothing honest or of good report in the whole world, nothing of scientific truth or of high speculation, morally and mentally, with which medicine does not claim legitimate kindred. But then there are cunning genealogists among us who would make out the links of kindred to be surer and closer than they are, and the rest of us acquiesce for the glory of the thing.

There is not a more difficult problem in the world than the education for a particular profession. In universities, so that the things taught be good in themselves, education may be as miscellaneous and omnifarious and even as redundant as you please. The object is to rouse the mind and let it make acquaintance with its powers and inclinations, so that it may judge of its own natural fitness by what it is able to do the best. In universities, education is for all professions and for all callings. But when you come to the particular profession or calling, whatever it may be, and seek to educate for it, you must draw to a point, and become discriminate and eclectic,

and above all, very fearful and suspicious of redundance and excess, especially if it be the medical profession with which you have to do.

Now what is this medical profession? It is the art how to treat, cure, and minister all possible relief to men, women, and children, when they have come to harm from disease, accident, or any other adversity. This is the core and kernel of the whole matter. And with this strictly and restrictively in view, a sort of magistracy was given to certain bodies over medical education for the public good. It was empowered to settle, first, the course of training indispensable for all who would practise medicine and surgery; secondly, to test by sufficient examination, one by one, the competency of all, before they enter it: to tell them how they are to fit themselves, and then tell the world who are fit.

Here are two about as delicate and ticklish problems as can well be conceived; and one can plainly see that in both of them the temptation would be to err on the side of excess and in things extrinsic; an error which, of all professions in the world, ours would least bear with impunity.

For the first are instituted lectures on Anatomy, not a few, but by hundreds; lectures on the Principles and Practice of Medicine, lectures on the Principles and Practice of Surgery, on both not a few, but by hundreds; lectures on Forensic Medicine, including Toxicology and Toxicological Analysis; lectures on Materia Medica. Every one of these things, by the very names they bear, claim or seem to claim their right of place in medical education.

Then there are lectures on Chemistry as a Science, lectures on Botany as a science.

But Physiology remains to be mentioned, which, in the hands of a powerful, accomplished, and popular teacher, may be made everything of, and so win the student's mind away from all other teaching. The subject itself is infinite. The difficulty must always be to keep it within bounds, and make it the subject of a separate course. There are physiologies animal and vegetable, and physiologies *de aere, aquis et locis*—physiologies that may be made to comprise the whole world within us and the whole world without us.

On most of these subjects, it is not thought enough to

specify the number of lectures to be attended: prizes are offered annually to stimulate competition, and are given to the most proficient in all of them. The sum of the knowledge which they contain implies a respectable start on the way to omniscience; and the student who, with thus much in his head, can still find room for a trifle of medicine and surgery, may be almost said to know a little of everything. Now I am convinced that none are helped to become good practitioners by this discipline; that the majority are hindered by it; and those who, having gone through it, turn out good practitioners nevertheless, become such in spite of it.

But where is the excess? What are the things extrinsic that can be pared off? Many a great chemist, a great botanist, or a great master of analysis, may be found to contend that what he has to teach is indispensable to the medical student; that the principles of his science are at the root of all rational practice. But beware of great authorities. They have a tyrannous way with them. They make common sense afraid to stand up and say what it knows to be right. Believe them, and they would commit the student to the whole circle of the sciences, and make him unhappy because he finds himself unable to follow his leaders. Surely there must be some shorter and easier way than theirs of making physicians and surgeons, and yet a safe way withal, or it will fare badly with mankind.

Now I have my own little suggestion to offer. It is a humble one, but not unbecoming wise men to adopt: a humble one, but bringing no loss of glory to the profession in the end. Have a very great care, then, of your medical student, and how you guide him at starting. Now especially is the time for good advice, if you have any to give. Take him now into the wards of the hospital at once; fit or unfit, as people reckon fitness, thither take him. He may have learnt all sorts of things already, or he may scarcely have learnt anything at all. But as soon as you have become his masters, thither take him, and there let him remain and make it for the present his sole field of observation and thought, or curiosity, and have a guard that the best things from without do not reach him there, to his hindrance or destruction.

When you would teach a man to read, you do not begin with the history of letters; where and by whom letters were

first invented, and where and by whom the printing-press; but you leap over a century or two without remorse, and put a dainty alphabet into his hand, and tell him something about vowels and consonants. But about labials, dentals, and gutturals, the less said the better. Lips, teeth, and throat make their own division of labour, and settle matters for themselves wonderfully. People do not come to read by being taught the philosophy of reading, but simply by doing the thing itself, simply by reading. So, if you would teach a man medicine and its practice, you must not begin with half a dozen philosophies, or with any philosophy at all, but you must put, as it were, his alphabet into his hand at once, and bid him learn its simple characters one by one, and then help him to join them together and make the best sense of them he can.

Well! but your student in the wards of the hospital is considered to be making acquaintance with diseases, specialising them, learning their right names, discerning between this and that, and getting up his diagnostics and nosologies. Verily this is a good deal more than learning his alphabet, and a good deal beyond it. For technical names and diagnostics and nosologies import the finish and last polish of his perfect work; yet great stress is laid upon them, enough to imply that the common run of individual cases can be finished and polished off in this fashion. But go into hospitals, and see the facts how they stand. There you find diseases mature and complete; and there diseases (and plenty of them) immature and incomplete, and scarcely deserving the names they bear: diseases in their embryo, their travail, their gestation, now coming to the birth, now ending in abortions—their course broken in upon, and their events determined by accident. Truly there are things in these hospital wards better worth learning than the diseases themselves—more simple, more primitive, more alphabetical (if you please) and yet more profound—things which are a key to the very diseases and their agreements and discordances; a key to their remedies, too, their successes, and their failures, and to discrepancies of their effects inexplicable without them; which tell why pneumonia (so-called) is now cured by bleeding, and now by brandy. These primitive, simpler, and more profound truths are now what they have been always. They are the common ground upon which the great physicians of distant

times, Sydenham, Heberden, and the President of the College that now is, would be found to think and act alike, and upon which (were it possible) they could meet and consult together to-morrow, and treat, as well as they could be treated, most of the great emergencies of diseases. But what are the things? They are pains; they are heat and cold, and sleep and wakefulness; and they are the pulse: all easily read, but all vital in their details, and vital comprehensively, including the sentient energy of the nerves and vital motive energy of the blood-vessels throughout the body. In a word, they are the immaterialism of disease, with which a man can only gain acquaintance by constant and familiar converse.

So unlike all other things in the world are medicine and the practice of it, that indications of treatment (simple things, but great mysteries) need to be pointed out to the most intelligent student with the care and patience that you would teach his letters to a child. But, in order to this, you must have the right men for physicians to hospitals, and they must choose the right men for their clinical clerks. And thus there will be gradations of clinical teaching and learning, free and unforced, which every man will look back upon in life as the best and soundest part of his professional education.

Now your student, who is to begin in the wards of the hospital, must continue to frequent them with the mind of a learner during all the years of his pupilage.

But lectures, courses of lectures on medicine or surgery (for of these only would I venture to speak), when are they to come? Not too soon. Not until the student, from such experience as he has, can exercise some judgment upon the subject matter of what he hears. Unquestionably, there is great profit to be had from lectures, if they come in at their proper place, and are properly used. Still, mixed with their good, there is some hazard of evil. They are a temptation to the more contemplative mind to learn diseases by the study of models, rather than of the things themselves. They tend to divorce him from the workshop and the chips and fragments and rude designs that lie about within it, and introduce him into a room swept and garnished and hung round with masterpieces for his contemplation. This may be all very well for gentlemen who patronise the arts; but this is not the way to make the artist.

Here I must stop. The whole subject of medical education is far too large and difficult for me, and I confess myself not up to it; but it is exciting at the present moment a just and serious and painful interest, and presses hard upon the wisdom of those who, fully aware of its past mistakes, are expected to decide what henceforth it shall be. There is one part, however, of medical education in which I was once actively engaged, and had come to hold strong beliefs. Upon it, at least, I thought I might venture to touch. But what I had to say must be said quickly, and now or never. I must strike while the iron is hot, while the interest of the subject is at its glow. Let this be my excuse for meddling with the subject at all, and plead my pardon if the "word or two" and "hint or two" which I have presumed to give my betters should be wide of the mark.

ERRATA.

Page 95, line 3 from bottom, *for* "interruptious" *read* "interruptions."
„ 130, „ 10 from bottom, *for* "perineumonia" *read* "peripneumonia."
„ 166, „ 3 from bottom, *for* "mucus" *read* "mucous."
„ 183, „ 1 from bottom, *for* "Ascultatory" *read* "Auscultatory."
„ 184, lines 3, 19 and 20, *for* "Ascultation" *read* "Auscultation."
„ 192, line 10 from bottom, *for* "right side the chest" *read* "right side of the chest."
„ 273, last line, *for* "ilium" *read* "ileum."
„ 434, line 11 from bottom, *for* "other" *read* "either."
„ 482, „ 7, *for* "as much as from" *read* "as much from."
„ 552, „ 4, *for* "reclaimed" *read* "retained" (?).

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