

THE GOULSTONIAN LECTURES ON PUERPERAL FEVER.

Delivered at the Royal College of Physicians, London.

By ROBERT J. LEE, M.D., F.R.C.P.,

Assistant Physician at the Hospital for Sick Children, Great Ormond Street.

LECTURE II.

ONE fact, as Cullen's remarks have suggested, had gradually become evident in his day to those who were interested in the question of the cause of the disease; it was the great mortality that characterised its occurrence in public institutions and hospitals. Long before 1788, when Tenon gave a report of the serious condition of the wards in the Hôtel Dieu, several epidemics, as they were termed, had been heard of. For more than a century before Cullen's *Præticæ* was published—that is, between 1652 and 1783—we have notices of this kind. Every author referred to them more or less directly when writing on the subject in the latter part of last century. One of the most distinguished observers of the disease almost commenced his treatise with the words: "The mortality attending the puerperal fever is truly lamentable. In the year 1750, at Paris, none who were seized with it recovered. In one hospital in London, in the space of two months, thirty-two patients were affected with that disease, and all except one fell victims to it." (Gordon's treatise on the *Puerperal Fever*.)

This fact did not, however, appear in the same light to all. We have, among many other examples, that of an excellent physician, Dr. Hulme, whose work I have already mentioned, who makes the following benevolent remarks in the dedication of his treatise on puerperal fever to the Governors of the City of London Lying-in Hospital:—"Public hospitals", he says, "for the reception of the sick and hurt are the grand seminaries of practical knowledge in the art of medicine. The utility of these institutions is so apparent, that they are now universally received all over Europe. Great Britain in particular hath not been behindhand in promoting such humane designs. Buildings of this kind, or which incidentally promote the same end, are to be seen in almost every part of this great metropolis. Among the rest, the City of London Lying-in Hospital rises up a simple, yet elegant monument of her beneficence." There is a foot-note, to the effect that "this hospital is entirely supported by a voluntary annual subscription, whereby four or five hundred poor objects are admitted every year, and relieved with all necessaries during childbed".

As Hulme was an authority on puerperal diseases, we may briefly notice that "the immediate cause" of the fever, in his opinion, was an inflammation of the intestines and omentum, and the "chief predisposing cause the pressure of the gravid uterus against the intestines and omentum". On similar principles of what we should hardly agree to call humanity, but with the additional object of teaching his pupils to "acquire true practical knowledge", Dr. Leake founded a hospital in Westminster, and Dr. Osborn and Dr. William Hunter both enjoyed opportunities of studying the disease in special hospitals.

We may conclude from this that, though Cullen's views were probably generally entertained, yet in the minds of many who were engaged in active professional duties, and who were certainly not deficient in intellect and benevolence, there was no distinct idea that the mortality in lying-in institutions might depend on causes over which there could be exercised very considerable control; so that the question of treatment appeared to be much more important than that of the etiology of the disease. I have mentioned one example of this in the recommendation of doses of ipecacuanha by Dr. Doucet. The report of the French Government on this plan concludes with the observation, that "the cure of a disease so active, and generally fatal in the Hôtel Dieu, and which allows so little time for reflection or hope from medicine, performed by a method so simple as that practised by M. Doucet, the success of which has proved so certain and uniform, is one of those extraordinary phenomena of nature which form an epoch in medicine".

We are not told exactly what other remedies M. Doucet employed; but, from the remark, "that those medicines most frequently administered in putrid diseases approach nearest to the method practised by M. Doucet", it would seem probable that his success may have depended on some other principle than that of promoting the action of the liver

and intestines by emetics, which was one method, in his opinion, by which the effects of a poison might be eliminated.

As it is our object to trace the progress of exact knowledge rather than to review ephemeral views, we may content ourselves with the list of remedies which Dr. Meigs has introduced into his well known work as a quotation from M. Baudelocque, the younger of that name. This is the list:—Blood-letting, emetics, purgatives, sudorifics, antiseptics, tonics, particularly camphor and bark; blisters and other revulsives, cold douches and cold stupes or hot stupes to the belly, subcarbonate of potash, oil of turpentine, mercurials: he ought to have added opium. Such were the differences of opinion on the subject of treatment.

As we have brought our review of the theories and different kinds of practice which prevailed at different periods, from the time of Hippocrates to the latter part of last century, when William Hunter, Cullen, Denman, and many other celebrated men, were contributing in their several departments to the advance of medical science in this country, I shall ask your careful attention to a work which apparently produced no great impression at the time it was published, but which has since had an important influence on our views of the nature of puerperal fever.

This work is by Dr. Alexander Gordon, and contains evidence that the fever depends on a particular poison, which may be communicated by direct contact from one person to another, and that the occurrence of a number of cases of the disease is to be explained in that way rather than by atmospheric or other conditions; that is to say, that the disease does not occur epidemically. It is usual for modern authors to mention the names of Denman in England and Semmelweiss in Germany as the chief contributors to this important fact. When I say that these names are associated by modern authors, I mean simply that in our text-books we find some such sentence as the following:—"As a result of clinical observation, on the one hand, the febrile puerperal diseases were proved with certainty to be due to secondary infection first by the English (Denman), afterwards by the Germans (Semmelweiss)."

I am now quoting from the German text-book on *Pathology* by Klebs. It may be presumed that the gratifying priority thus accorded to us by German authorities has induced our own writers on the subject to be content with this division and distribution of fame. If it were just, it would be still more gratifying. By way of a little diversion from the direct path of investigation we have been pursuing, I shall ask your attention to this question of priority; not so much because it is of any great importance to determine how or when the fact was first brought to light that puerperal fever depends on a special poison, as to relieve the mind from an uncomfortable feeling that somebody may have suffered neglect. It always appears to me to be better to omit in educational text-books as much as possible all mention of names; but, if they be mentioned, it is necessary that whatever is stated about them should be as certainly true as possible. It is a subject deserving of the consideration of the learned Fellows of this College, why there is such a scarcity of really good works for educational purposes at the present day. It will be answered probably, that science has made such rapid progress as to render it difficult to compose such works as text-books. This is not the case in other sciences; and I have not the slightest doubt that, if there were a general feeling of gratitude to those whose practical knowledge most fits them to undertake such difficult and thought-requiring labour, we should not be long before we could boast of possessing first-rate works of the kind I allude to. But to return to the question of Denman and Semmelweiss. Let us take Denman first.

It appears that we have more than one edition of his works, and that considerable periods intervened between their publication. In the last of these, there are the following remarks. "There is another consequence of an epidemic, or even a sporadic puerperal fever, on which it would be criminal to be silent. This is the contagious nature of these fevers, it having been long suspected, and being now fully proved, that they may be, and often have been, conveyed by midwife or nurses from one patient to another. This fact explains the reason why persons practising for many years with the most enviable success have at one or more periods of their lives, without any change in the principles or manner of their practice, met with a number of unfortunate cases when perhaps an adjoining neighbourhood has been entirely free from such diseases," etc. (Denman, p. 644, ed. 1805.)

There is no doubt about the meaning of this passage; and, as it is contained only in the third and last edition, and not a word is said upon the subject of contagion in the former two, we may conclude that our German admirers have quoted from the most recent one. We can easily understand that such a general statement as "having been long suspected, and being now fully proved" is not quite satisfactory to their

love of detail. However, Denman was quite right in saying so; for, in the very year (1795) that he published his second edition, which preceded the third by ten years, the work of Dr. Gordon appeared, and, curiously enough, was dedicated to his intimate friend Dr. Denman. Now, Gordon's treatise contained the results of his observations on an outbreak of the fever in Aberdeen, which continued from 1789 to 1792, and then he allowed between two and three years to elapse before he wrote upon it. It is very probable that he communicated those results to Denman before the year 1795, when, as we have seen, his own work and Denman's second edition were published. If this were true, it would certainly lead us to conclude that Denman was not very willing to believe in Gordon's contagious theory when first proposed to him. If it be asked why did he not refer to Gordon's treatise in his last edition, we can only reply that, as he followed the example of the dogmatic teachers of his day, he omitted on principle the mention of names, restricting himself closely to careful judgment of reported facts, applying to them the test of extensive personal experience. We may also conclude, from the circumstance that Denman was satisfied with the reasons adduced by Gordon for his contagious theory, that, in the interval of nine years between his second and third editions, that theory had been more or less generally accepted.

A very considerable change, in fact, had taken place in the state of knowledge of the nature and cause of puerperal fever. A careful analysis of the kind of evidence brought forward by Gordon to prove his point will show that it was much of the same character as that furnished by Semmelweiss. Neither of them used pathology or morbid anatomy to any great extent. The method they both employed was a species of close clinical observation, assisted by correctly logical deduction, and was rather of the nature of proof by circumstantial evidence than of actual demonstration.

It is impossible, however, on comparing the works of Gordon and Semmelweiss, to resist the conclusion that, although they were both possessed of powers of original thought and admirable truthfulness and courage, there was a considerable difference in their mental calibre. It requires a full appreciation of that peculiar instinct which is so rarely exhibited, and to which we owe all important advances in scientific knowledge, to estimate the great superiority of Gordon's intellectual and moral character. We can measure various kinds of force, but this mental force has never yet been properly valued. We cannot, of course, but admire the talent and industry which enable one man to work out such a problem as whether or not puerperal fever depends on a contagious poison; but, when the idea is the suggestion of another, we cannot admit that he has any claims at all equal to those which we must accede to the one who first conceived the idea, and who, as far as he was able, applied experimental tests to it.

This was the great difference between the total results of Gordon's observations and those of Semmelweiss: the one possessed original powers of perception, which led him to trace such a connection between the various phenomena of the disease as to enable him to deduce a general principle or law by which they were controlled.

Now it is necessary, before we allow to Gordon the merit of this superiority, to examine whether his theory was simply a conjecture, or well supported by observation. I should not have introduced this subject to your notice, if I had not felt pretty well assured that you would agree with me after such an examination. I had another reason, and that was, that I wished to prove a fact of which we ought to feel certain as well as properly proud, and that is, that no country in the world can surpass ours in the production of men of that peculiar genius which in all departments of science characterises their chief pioneers.

Some recent authors have not been so generous to Denman as Klebs. The author of one text-book, which has been translated into English, gives to us the credit of ascertaining the fact, that puerperal fever is contagious or specific; but he claims for Semmelweiss the merit of having worked out the subject carefully, and of being essentially the author of all we know of the etiology of the fever (Schroeder). The growing tendency to copy such statements from foreign authorities into our own literature, many will agree with me, is not likely to encourage that kind of work and thought among us upon which the scientific merits of our profession depend.

Let us examine the data on which Gordon established the fact, that, to use his own words, "the cause of this disease was a specific contagion or infection", of which he asserted "he had unquestionable proof". He first undertakes to lay before us evidence that "every person who had been with a patient in the puerperal fever became charged with an air of infection, which was communicated to every pregnant woman who happened to come within its sphere". He submits to us a table of seventy cases, containing the name, age, and residence of each patient, and the name of the midwife or practitioner by

whom she was delivered. Of course it is the latter column of this table which we are tacitly required to analyse. We are expected to notice certain names, either that of Dr. Gordon himself or those of midwives, in connection with several cases of the disease. Thus we find one midwife entered as having attended three cases on days closely following one another, and the same thing we observe to have happened to two others. It cannot be said that this table is altogether conclusive. It is not difficult to perceive that it only contains one part of the evidence which is necessary to make it so. At the time that these cases of puerperal fever occurred in a place like Aberdeen, it must have been, and was, remarked that there were other practitioners and midwives to whom such accidents did not occur, as Gordon mentions in his work; but it was impossible to reduce this important negative evidence into a tabular form. To make up for this deficiency, we have the evidence in another part of the treatise. As one instance, we may take the following:—"Now it may seem remarkable that the puerperal fever should prevail in the new town, and not in the old town, of Aberdeen, which is only a mile distant from the former; that it should prevail at the Printfield, Gilscomston, and the Hardgate villages, in the parish of the old town of Aberdeen, and not in the old town itself. But the mystery is explained, when I inform the reader that the midwife, Mrs. Jeffries, who had all the practice of that town, was so very fortunate as not to fall in with the infection, otherwise the women whom she delivered would have shared the fate of others."

It appears, from this and other parts of his treatise, that Gordon fully appreciated the importance of being able to account for apparent exceptions to the principle he was working out, and that he proceeded with the caution characteristic of his countrymen in venturing to publish the very important conclusions at which he had arrived. It is impossible for any one to analyse Dr. Gordon's treatise fairly, for it is made up of the closest reasoning and the most distinct data. He never supposes anything, but looks carefully to see that his premises are correct, and leaves the reader to draw his own conclusions.

A few extracts from the work will show you what sort of a man he was. Of the nature of the poison, he says simply:—"With respect to the physical qualities of the infection, I have not been able to make any discovery." Of himself, he remarks: "It is a disagreeable declaration for me to mention that I myself was the means of carrying the infection to a great number of women."

He says of typhus, as compared with puerperal fever: "The cause of both is undoubtedly infection; but the two infections are of a very different nature." Of the cure, he says: "There is no disease in which less is done by nature or more may be done by art." As he based his treatment on the view, that the chief effects of the poison were of an inflammatory nature, he directs attention strongly to the benefits which follow early bleeding and purgatives. In his own words: "On this head, I speak with proper confidence, because I speak from experience—the surest test of medical truth. And, as I have already mentioned, I found myself disappointed when I trusted to those means which have been recommended by some authors of considerable respectability; for neither antiseptic nor tonic medicines, nor such as obviate sensibility or irritability, were found effectual."

I must not omit to mention that, as far as the pathology of his time allowed him, Gordon was correct in saying on that subject: "The puerperal fever is a disease which principally affects the peritoneum and its productions, and the ovaria". He distinguishes between "the most effectual means of preventing the infection from being communicated" and the method of "preventing the action of that infection after it has been communicated". He gives sensible hygienic rules to gain the first point, recommending in particular the purification of infected chambers and the fumigation of infected apparel. We can judge, now that three-quarters of a century or more have passed, of the value of this work of Gordon's. It is just the result of that kind of rare combination of originality, energy, patience, and good feeling, which makes us dissatisfied with inferior productions, and is peculiarly characteristic of the leading minds of this country.

We might reflect a little upon the singular fact, however, that every point, except the most important one in this treatise, has been taken up, first by one author and then by another, and all have praised Gordon's work in the very respects for which he least deserved it. We might pause, I say, to reflect upon the question why it is so difficult to get rid of old notions and accept new ones, when they are true and important, or why it happens that error is so much more favourably received than truth when presenting itself as a candidate for support. With what kind of a feeling do we read the conclusion to Gordon's preface? "The benevolent reader must observe with displeasure the ungenerous treatment which I met with from a very sex whose sufferings I was at so much pains to relieve; for, while I was using my best endeavours to mitigate the calamities of many miserable sufferers,

several others were busy in traducing my character, who, prompted by prejudice, very uncandidly proclaimed the deaths and concealed the cures, to raise an odium against my practice."

(To be continued.)

ON THE MEDITERRANEAN COAST OF THE SOUTH OF FRANCE IN ITS MEDICAL ASPECT.

By WILLIAM MARCET, M.D., F.R.S.,

Late Assistant-Physician to the Westminster Hospital and Hospital for Consumption and Diseases of the Chest, Brompton.

THE subject of the present communication has lost, I fear, some of the freshness and attraction it used to possess, from the extent of its literature. My apology in troubling the reader on the present occasion is a desire on my part to show which are the resources of the Mediterranean coast in a medical point of view, and to what extent a winter residence in the sunny South can and will benefit those who suffer from consumption. I shall in no way propose to establish the comparative merits of any particular localities, but endeavour to point out and insist on certain rules which, in my humble opinion, might be useful in the selection of a winter-station on or near the Mediterranean coast. A residence of over three winter seasons on the Riviera has given me a fair number of opportunities of judging of its climate and its influence on the progress of disease; hence, in the following communication, I am writing from personal experience.

The usual circumstances inducing, on the part of a medical adviser, a recommendation to winter in the South, will probably be first of all the fact that his patient is invariably worse in winter than in summer. This will apply to chronic cases of phthisis, or cases of threatened consumption, chronic bronchitis, asthma, debility from long continued rheumatism or gout. Many patients apply to us, in some of the usual resorts on the Mediterranean coast, broken down in health from business, anxiety, and mental work. The tide of invalids moving south in November, and northwards again about the end of April, brings with it sufferers of every description, in fear and trembling of cold, which, indeed, their weakened frame has no power of resisting. They leave home upon the first signs of autumn setting in, with the enjoyable prospect of country life in winter, and exercise in the open air, under a bright sun, and without either frost or snow.

It is with consumptive patients that we are mostly concerned, and to that class of sufferers let us now turn our attention.

A certain number of those who come to us for medical advice are anxious to avert an attack of bronchial inflammation, which might lead to the softening of a pneumonic deposit. The applicant may be in apparent good health, strong, able to go about, and fairly full in flesh, although exhibiting a suspicious dulness at one apex, and perhaps some slight laryngitis, attended with a purulent secretion. He or she has been seen by several medical practitioners, and, after obtaining varying opinions, is advised to go south, rather as a means of precaution than with any other object in view. These patients reach their destination often considering themselves in perfect health; and, acting upon that erroneous assumption, subject themselves to many risks, or rather dangers, joining dinners and evening parties, and taking no precautions against the powerful rays of the sun or the sudden cold at sunset. The result is an attack of hæmoptysis, and the disease assumes at once a very serious aspect. If, on arrival, a residence be taken up in a proper locality, and certain rules be strictly observed, the case will, in all probability, do well, the patient returning home in the spring with many a pleasant recollection of his winter season abroad.

Another class of phthisical subjects frequently appear amongst the winter visitors in the South. They have had one or more attacks of hæmoptysis, and are often troubled with more or less shortness of breath, especially on going up hill; still they appear in a very fair state of health. An inquiry into the physical signs of their chest reveals a harsh respiratory murmur, perhaps some fine crepitation in one or both lungs; while the low and indistinct sound of the murmur shows a deficient supply of air to the pulmonary organ. On percussion, no dulness whatever can be detected. Cases of this kind appear to me as instances of deficient nutrition of the pulmonary tissue, with temporary congestion of the lungs. As a rule, patients exhibiting these symptoms do not progress satisfactorily at the seaside, where a return of the hæmoptysis is always to be dreaded; but they do well on the hills which skirt the Mediterranean coast, at an altitude of from fifty to five or six hundred feet above the sea. On condition of their selecting such a place for their winter residence, and observing certain rules of

hygiene, these patients are pretty sure to be free from hæmorrhage; while their strength and general state of health will improve materially. The prognosis in these cases is very uncertain: some of them recover perfectly; others remain in the same state. They reappear in the South year after year, with an account of several fresh attacks of hæmoptysis, but looking much the same as in the previous winter. The physical signs do not vary, the same harsh murmur or crepitation either at the apex or the base being observed.

It may be, however, that the physical signs of simple congestion, or what I imagine to be a state of deficient nutrition, have another meaning, and are symptomatic of tubercular disease, even without the occurrence of dulness on percussion. Harsh breathing and fine crepitation are but a variety of one and the same sound; and fine crepitation at one apex, without any dulness, may certainly be a sign of incipient phthisis. In such instances, the seat of the abnormal sound is an important consideration. If it be heard at the base of one or both lungs, the inference will be a state of temporary congestion; while the same appearance at the apex will rather favour the idea of early phthisis; and here the thermometer comes into use as an important diagnostic means. In case of an increased temperature of the body and quick pulse, the object of the treatment will be to check these symptoms with as little delay as possible. Should they persist, the weight of the body will be observed to fall off rapidly, together with muscular power, the function of nutrition participating in the general state of debility; and diarrhoea setting in will, of course, seriously complicate the treatment of the case. These various changes for the worse may set in without any dulness on percussion of the chest supervening, and without there being much alteration of the physical signs in other respects. Under favourable circumstances, especially if the patient be sent up to the hills, a great change for the better will ensue, the accessions of heat becoming less severe, shorter in their duration, and perhaps entirely disappearing; while the pulse is reduced in frequency. Strength returns; appetite improves; and food is taken with relish, and readily digested. These cases of phthisis return South on the approach of winter, a little thinner, a little more anæmic, than the year before. Still they keep on, till we hear of their fatal termination in summer or in autumn. The progress of the changes in the lungs is very difficult to trace from the physical signs. In one case which I have closely observed for two successive winters, the most careful examination showed no consolidation or softening. The cough was very urgent, accompanied at times with severe attacks of spasmodic dyspnoea; and the patient died in England four months after I had last seen her. Diarrhoea had set in some months before, and I was informed that a cavern in her right lung had been diagnosed when she died.

In some cases, I really do believe that hæmorrhage may be the cause of phthisis, but not unless a phthisical disposition exist in the patient.

We are constantly asked on a first visit, and naturally enough, what amount of benefit we expect a patient to derive from the climate of the South; and how difficult it often is to give a positive answer! Suppose just such a case as that last considered; we must rely on its history, age, hereditary tendency, pulse and temperature, condition of other organs, and general state of health, as the physical signs of the chest belong, perhaps, to a state of simple bronchitis. The age, condition of the organs of digestion, and muscular power, may yield, however, important indications towards the present object. "Age," according to Dr. Pollock (*Elements of Prognosis in Consumption*), "must enter as a distinct element into all our calculations regarding the prolongation of any chronic disease, or the duration of the pauses between the attacks, which we have been noticing." Again, Dr. C. T. Williams (*Med.-Chir. Trans.*, vol. lv) remarks, that the duration (of consumption) is longer in proportion as the age of the attack is later; the retarding influence of age being more conspicuous among males than among females.

I need not insist upon the importance of taking into consideration the state of the organs of digestion and muscular power. A failing in these functions, although perhaps unattended with any change in the physical signs, is invariably an indication of a downward tendency; and the weighing-chair, which tells no tales, will at once detect a falling off in the weight of the body. This last remark not only applies to obscure, but to all cases of phthisis.

We have to consider, finally, the state of pulmonary consolidation with softening, and also that attended with cavities in the lungs. One of the earliest cases I had during my first season in the South (autumn, 1871), being then at Nice, was that of a lady aged about 35, whom I found very much fatigued with the journey from England. There were signs of softening in her lung, and a few days later a cavern had formed. The case, to say the least, did not look promising. There was, however, one satisfactory sign—the absence of a high temperature of the body. Relying on this circumstance, I ventured upon expressing a hope that, as soon as the patient would be rested from her