THE CROONIAN LECTURES

ON

SOME POINTS IN THE PATHOLOGY AND TREATMENT OF TYPHOID FEVER.

Delivered before the Royal College of Physicians of London.

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LECTURE I.

IN taking for the subject of these lectures the pathology and treatment of typhoid fever, I fear I may seem to have chosen one which has been completely exhausted, and about which, therefore, little or nothing new remains to be said; its general pathology, its mode of origin and dissemination, its distinction from other diseases, have indeed been thoroughly investigated, owing in no small degree to the researches of eminent Fellows of this College, among whom I need only enumerate the names of Stewart, Jenner, and Murchison.

Of late years, too, there has been a pretty general agreement as to the principle on which the disease should be treated; but, notwithstanding, upwards of seventy-three thousand persons died of typhoid fever during the past nine years in England alone. In the face, then, of this great mortality, I think it may not be without some degree of interest and advantage if we again pass in review some of the received opinions as to its nature and treatment. I do not, however, propose again to discuss the distinction between typhoid and other fevers, or to give any clinical account of the disease, but to confine myself to the consideration of some points about which more or less difference of opinion and practice still prevails. And I shall first consider the causation and mode of dissemination; secondly, some points in the pathology; and, lastly, the treatment.

In proceeding to consider the mode of origin and propagation of typhoid fever, we are at once arrested by the question, What are the nature and properties of the poison which is supposed to give rise to the disease? Now, the typhoid poison has up to the present time eluded all attempts to isolate it or to demonstrate its nature either by microscopical examination or chemical analysis; we are only conscious of its existence by the effects which it produces on the human organism. Nevertheless, some of its properties are known with tolerable certainty: (1) when introduced into the system it multiplies; (2) it is contained in the alvine discharges of persons suffering from the disease; (3) it retains its activity for an indefinite time after it has passed out of the body, when placed under favourable conditions, these conditions being the presence of decaying animal matter and moisture. Hence its usual habitats out of the body are drains, sewers, cesspools, dung-heaps, wet manured soils. And there are some grounds for supposing that, in these situations also, it may possess the power of multiplying. Lastly, in all probability, it is particulate, and not either liquid or gaseous. The actual nature of the poison—whether it be, according to the hypothesis most generally accepted, some kind of fungus, or microzyme, or protoplasm, in a word, a *contagium vivum*; or whether, as maintained by others, it is some derivative of albumen, capable of exercising a catalytic action on other albumen—I do not propose to discuss, as it is a question at present rather of theoretical than of practical interest; and it is one, moreover, for whose final determination the data are hardly yet sufficient. A subsidiary question to this, but one of considerable practical importance, is whether the poison can be generated *de novo* from decaying organic matter, whether it be pythogenic, as was ably maintained by Dr. Murchison, or whether it can only arise by continuous propagation, as was maintained no less ably by Dr. William Budd.

The arguments on both sides may be very briefly stated. In favour of its origin *de novo*, it is asserted that typhoid fever has often broken out in isolated situations—as solitary farmhouses, far removed from, and holding no communication with, places where the disease exists; and many such instances are given by Dr. Murchison. On the other hand, it has been proved incontestably by many instances, both in this country and on the Continent, that all the conditions supposed to be required for its generation may be present for an indefinite time—as percolation of sewage into wells supplying drinking-water—and yet the disease does not show itself till the poison is introduced by the arrival of an infected person, when an outbreak at once takes place. Now, I would submit

that this latter argument far outweighs the former; for otherwise, if it be proved that all the conditions necessary for the origination of the poison are present, as shown by its subsequent development when the germs are introduced, and yet it does not develop, we should have to admit that the same causes are not always followed by the same effects. The instances in which persons in the latent stage of typhoid fever, or actually ill with it, have carried the disease to distant places, and caused it to spread, are so numerous that I believe that this mode of propagation is now universally admitted; the communication of the disease taking place not by direct contagion from the sick to those brought into immediate contact with them, but by the ordinary mode of sewage-contamination.

I will quote in illustration three instances which have occurred in this country. One is the famous epidemic at Over Darwen. The water-supply pipes of the town were leaky, and the soil through which they passed was soaked at one spot by the sewage from a particular house. No harm resulted till a young lady suffering from typhoid fever was brought to this house from a distant place; within three weeks of her arrival the disease broke out, and 1,500 persons were attacked. Another well-marked instance occurred at Calne. A laundress occupied the middle one of a row of three houses supplied by one well, into which the slop of the laundress's house leaked. She on one occasion received the linen soiled by the discharges of a case of typhoid fever, and after fourteen days cases occurred in all three houses. At Nunney, a number of houses received their water-supply from a foul brook contaminated by the leakage of the cesspool of one of the houses, but no fever showed itself till a man ill with typhoid came from a distance to this house. In about fourteen days, an outbreak of fever took place in all the houses.

Many equally striking instances might be quoted from foreign sources; I will, however, only adduce the well-known one of Lausen, as it illustrates some other points of great importance in the etiology and prophylaxis of the disease. Lausen is a village through which I have no doubt most of my hearers have passed, as it lies on the railway between Basle and Olten, shortly before coming to the great Hauenstein Tunnel. It is situated in the Jura, in the valley of the Ergolz, and consists of 103 houses, with 819 inhabitants; it was remarkably healthy and resorted to on that account as a place of summer residence. With the exception of six houses, it is supplied with water by a spring with two heads which rises above the village at the southern foot of a mountain called the Stockhalder, composed of oolite. The water is received into a well-built covered reservoir, and is distributed by wooden pipes to four public fountains, whence it is drawn by the inhabitants. Six houses had an independent supply—five from wells, one from the mill-dam of a paper-factory. On August 7th, 1872, ten inhabitants of Lausen, living in different houses, were seized by typhoid fever, and during the next nine days fifty-seven other cases occurred, the only houses escaping being those six which were not supplied by the public fountains. The disease continued to spread, and in all 130 persons were attacked, and several children who had been sent to Lausen for the benefit of the fresh air fell ill after their return home. A careful investigation was made into the cause of this epidemic, and a complete explanation was given. Separated from the valley of the Ergolz, in which Lausen lies, by the Stockhalder, the mountain at the foot of which the spring supplying Lausen rises, is a side valley called the Furlenthal, traversed by a stream, the Furlenbach, which joins the Ergolz just below Lausen, the Stockhalder occupying the fork of the valleys. The Furlenthal contained six farmhouses, which were supplied with drinking-water, not from the Furlenbach, but by a spring rising on the opposite side of the valley to the Stockhalder. Now, there was reason to believe that, under certain circumstances, water from the Furlenbach found its way under the Stockhalder into one of the heads of the fountain supplying Lausen. It was noticed that when the meadows on one side of the Furlenthal were irrigated, which was done periodically, the flow of water in the Lausen spring was increased, rendering it probable that the irrigation water percolated through the superficial strata and found its way under the Stockhalder by subterranean channels in the limestone rock. Moreover, some years before, a hole on one occasion formed close to the Furlenbach by the sinking-in of the superficial strata, and the stream became diverted into it and disappeared, while shortly afterwards the spring at Lausen began to flow much more abundantly. The hole was filled up, and the Furlenbach resumed its usual course. The Furlenbach was unquestionably contaminated by the privies of the adjacent farmhouses, the soil-pits of which communicated with it. Thus from time immemorial, whenever the meadows of the Furlenthal were irrigated, the contaminated water of the Furlenbach, after percolation through the superficial strata and a long underground course, helped to feed one of the two heads of the fountain supplying Lausen. The natural filtration, however, which it underwent rendered it perfectly

bright and clear, and chemical examination showed it to be remarkably free from organic impurities; and Lausen was extremely healthy and exempt from fever. On June 10th, one of the peasants of the Furlenthal fell ill with typhoid fever, the source of which was not clearly made out, and passed through a severe attack with relapses, so that he remained ill all the summer; and on July 10th a girl in the same house, and in August a boy, were attacked. Their dejections were certainly, in part, thrown into the Furlenbach; and, moreover, the soil-pit of the privy communicated with the brook. In the middle of July the meadows of Furlenthal were irrigated as usual for the second hay crop, and within three weeks this was followed by the outbreak of the epidemic at Lausen.

In order to demonstrate the connection between the water-supply of Lausen and the Furlenbach, the following experiments were performed. The hole I mentioned above as having on one occasion diverted the Furlenbach into the presumed subterranean channels under the Stockholder was cleared out, and 18 cwt. of salt were dissolved in water and poured in, and the stream again diverted into it. The next day, salt was found in the spring at Lausen. Fifty pounds of wheat flour were then poured into the hole, and the Furlenbach again diverted into it; but the spring at Lausen continued quite clear, and no reaction of starch could be obtained, showing that the water must have found its way under the Stockholder in part by percolation through the porous strata, and not by distinct channels.

Besides showing the necessity of the introduction of the specific poison in order to render sewage contamination capable of giving rise to typhoid fever, this case is remarkable as an instance of the extreme dilution to which the poison may be subjected without losing its potency, and also the uselessness of irrigation and any ordinary filtration in separating it or rendering it inert. Here the dejections of two cases of typhoid are thrown into a stream; the water of this stream is used to irrigate extensive meadows; a portion of it sinks through the superficial strata, and probably finds its way into subterranean channels, and passes through a distance of many thousand feet under a mountain, partly, no doubt, by mere percolation; it then takes an insignificant part in feeding one head of a copious spring, which has another head that is not contaminated, and, nevertheless, it gives typhoid fever to 130 persons out of a population of 800. The dilution must have been almost infinitesimal, unless we assume that a multiplication of the poison took place after its discharge from the intestinal canal of the first two cases—possibly in the reservoir at Lausen.

The apparently spontaneous origin of the disease in isolated places may, doubtless, be in part explained by the very long time the poison may lie dormant, still retaining its essential properties, and capable, under favourable conditions, of developing its activity.

The poison, too, may be introduced in quite unsuspected modes. It is well known that the typhoid poison by no means always produces what is usually recognised as typhoid fever. In very many cases, its only effect is to cause some malaise, together with slight intestinal catarrh, which is not necessarily attended by diarrhoea. These cases are only recognised as typhoid when they occur during an epidemic, and among persons who have been exposed to infection; but if we may judge by the analogy of scarlatina, diphtheria, and other contagious fevers, where similar slight unrecognisable forms are not uncommon, these apparently trivial cases should be as potent in communicating the disease as the severe well-marked forms. Hence it is quite possible that the typhoid poison may be introduced into a locality by a person whom no one would suspect of being infected with the disease.

It has been ingeniously suggested by my colleague, Dr. Robert King, that the typhoid poison is derived solely from decomposing albumen, which is not present in healthy stools; and that it can therefore only arise from morbid stools which contain albumen, as is the case in some forms of catarrhal diarrhoea; and he has published a case where the poison seemed to be generated in this manner. This theory might certainly account for long-continued sewage contamination of water without the production of typhoid fever, as the special material from which the typhoid poison is generated might be absent. But it is hardly likely that any such derivative of albumen would remain undecomposed in a drain or cesspool for so long a period as we have reason to believe to be the case with the typhoid poison.

We have next to consider how long the typhoid poison can thus retain its activity out of the body in a suitable locality. One of the clearest pieces of evidence on this point is the well-known instance related by Dr. von Gietl. To a village free from typhoid an inhabitant returned suffering from the disease, which he had acquired at a distant place. His evacuations were buried in a dunghill. Some weeks later, five persons who were employed in removing dung from this heap were attacked by typhoid fever; their alvine discharges were again buried deeply in the same heap; and nine months later, one of two men who

were employed in the complete removal of the dung was attacked and died. Here we have distinct evidence that the poison retained its powers for nine months. Dr. Murchison, in his work on *Fever*, gives an instance in which six cases were spread over a period of eight years. I have recently seen an instance in which an interval of two years occurred without apparently any fresh importation of the poison. Supposing the germ-theory to be correct, there is, of course, no reason why the poison should not preserve its vitality, by continuous propagation, for an indefinite time. On the whole, though the point cannot be regarded as finally determined, I think the weight of evidence is against the *de novo* origin of the disease.

We have now to consider a question about which much difference of opinion still prevails; viz., the contagiousness of typhoid fever. That this disease is communicable from the sick to the healthy is, I believe, universally acknowledged; the point in dispute being the mode in which the transmission takes place, whether directly by emanations from the patient, or from his fresh evacuations, or indirectly from eating or drinking, or inhaling the emanations from the stools, modified by their having undergone some kind of decomposition or fermentation outside the organism. Although the point in dispute is a narrow one, it is of considerable importance, and erroneous views may lead, on the one hand, to the adoption of unnecessary restrictions, which may seriously incommode the patient and his attendants, while at the same time it is only too likely to cause a neglect of the really essential precautions. I have myself witnessed an outbreak of typhoid fever where the belief in its contagiousness—shared, I may say, by the medical practitioners—was so strong as to excite quite a panic, so that difficulty was found in procuring attendants for the sick. In consequence, I had on one occasion myself to carry a patient from one room to another, and a man who volunteered to help me was regarded as having done something rather heroic. I need hardly say that, at the same time, the utmost recklessness is commonly shown with regard to the real causes of its dissemination. On the other hand, a disbelief in the direct contagiousness of the disease might, if not well founded, lead to an unnecessary exposure of the patient's friends.

The arguments against the direct contagiousness of the disease are in the main these. 1. In hospitals, we rarely find that typhoid fever spreads either to the attendants on the sick or to the other patients. When it does thus spread, persons in other parts of the building, who have never been brought into contact with the typhoid cases, are attacked about as frequently as the immediate attendants themselves. Dr. Murchison has published the evidence on this point afforded by the London Fever Hospital for a period of twenty-three years, up to 1870. During this period, 5,988 cases of typhoid fever were admitted, and seventeen of the resident staff took the disease; but of these seventeen only five were in communication with the typhoid cases, and twelve occurred at a time when there were serious defects in the drains. Twelve patients also admitted for other diseases became infected. But since 1861, when the patients were so classified that the typhoid cases and the patients suffering from other acute diseases, not fever, were placed in the same wards, not a single instance has occurred in which the infection has spread to the non-typhoid cases, though the same night-stools and water-closets have been used by both classes of patients, and the use of disinfectants has been exceptional. This shows pretty conclusively that the emanations from the recent stools are not capable of communicating the disease. Since 1871, 1,447 cases of typhoid have been admitted and treated in the same wards with 692 patients suffering from other diseases; and during this time only three nurses and no patients have acquired the disease. On several occasions, however, cases of scarlet fever lying in other wards have become infected. So far as the London Fever Hospital is concerned, it would seem that the risk of becoming infected with typhoid fever is rather less in the typhoid wards than in other parts of the building. The experience of other hospitals is for the most part in accordance with that of the London Fever Hospital. Liebermeister states that, up to 1865, neither at Greifswald, nor at Tübingen, nor at Berlin, had a single instance been known of the disease spreading to other patients in the hospital or to the attendants on the sick. The Hôtel Dieu and La Pitié at Paris, the City of Glasgow Fever Hospital, and the general hospitals of London, show much the same results.

During the past year, sixty cases of typhoid fever were admitted into the Middlesex Hospital, and six nurses took the disease; but of these six, five were on duty in the surgical wards, and never came into contact, directly or indirectly, with the typhoid cases. I need hardly say how unlike this is to the behaviour of a contagious disease. I have no doubt that in all these six cases the disease was due to the condition of the drains of the dormitory.

It is also found that when persons suffering from typhoid fever are removed to distant places, it is the exception, and not the rule, for the disease to spread; and when it does spread, it quite as commonly attacks

persons who have not been brought into immediate contact with the sick, as it does the attendants themselves. It may, I think, be laid down as absolutely certain, that an epidemic of typhoid is never caused by the disease spreading by direct contagion, as epidemics of small-pox, scarlatina, and typhus are. On the other hand, it is argued that sometimes, in hospital practice, typhoid does spread to the attendants on the sick or the other patients; and, not at all uncommonly, persons suffering from typhoid, when removed to a distance, do communicate the disease to those brought into contact with them. Thus, in the hospital at Basle, 1,900 cases were admitted in six years; and during this period forty-five residents in the hospital became infected, and a large number in addition suffered from a non-febrile intestinal catarrh, which in all probability was due to the typhoid poison. But of these forty-five cases, many had never been brought into contact with the patients, and of the nurses it was found that those were especially liable to be attacked who occupied a particular room, where they were exposed to the emanations from a leaky choked soil-pipe. Probably there is no city in Europe, not even Munich, and no hospital, so saturated with the poison of typhoid as Basle and its hospital; and, moreover, the hospital drains are or were in a very unsatisfactory condition; and thus there is every reason for believing that the contagion in all these forty-five cases was indirect.

The same argument applies to the spread of the disease in private houses. In the great majority of instances where the fever is imported from a distance it does not spread; where it does, some defect in the sanitary conditions favouring indirect contagion can generally be detected; and it is therefore almost certain that the same is the case in the other instances; otherwise, we should have to admit that typhoid fever is sometimes contagious and sometimes not—nay, that during the same epidemic, the disease is contagious in one house or institution, and not contagious in another house or institution, though they may be situated close together.

I have seen many instances of supposed direct contagion, but have generally succeeded in tracing them to an indirect source. A boy was admitted to the Middlesex Hospital under my care, on March 27th of last year, suffering from a very severe attack of typhoid. For several days he lay in an unconscious condition, and during this time he had very profuse diarrhoea—twelve to twenty liquid motions daily, which were for the most part passed in the bed. In the next bed was a boy aged six, who had been admitted on April 16th, with acute renal dropsy and bloody urine. He was kept strictly confined to bed, and never got up to go to the water-closet down which the motions of the typhoid case were thrown. On May 11th, when he was convalescing, the dropsy having disappeared and the albumen much diminished, he was seized by typhoid fever, and passed through a moderately severe attack, with a well marked rash and characteristic symptoms. This at first sight appeared to be a case of direct contagion, but I have no doubt that the true explanation is this. The bedding of the first patient was constantly kept saturated by his liquid motions, and, though every care was taken to change the linen frequently, it was obvious, from a distinctly faecal smell which was always present, that the bedding or mattress remained contaminated, and thus time was given for the poison to develop its infectious properties. Another patient in the same ward, admitted for acute rheumatism, was also attacked by the fever. He occupied a bed on the opposite side, and never came near the first case, but, being convalescent, he used the water-closet down which the motions of the typhoid case were thrown; and it so happened that at this time the closet was out of order, the contents were retained, and an offensive smell was constantly present. Hence, I think, there can be no doubt but that he was infected by the emanations from the evacuations of the first case.

The first case affords an example of a very common mode of infection; and many of the cases attributed to direct contagion are really produced in this way, especially among the lower orders or where skilled nurses are not employed. The bed-linen or mattress, or the patient's own person, become soiled by the liquid alvine discharges. These soon dry, and are left undisturbed, and in a short time the poison develops its infectious properties. Soiled linen is well known to possess very active contagious powers, and to retain them for a long time.

If these views are correct, it becomes an important subject of inquiry as to how soon the stools acquire contagious properties. I am not aware of any observations which would enable us to decide this point with certainty, but the time is probably a very short one. The frequency with which washerwomen have been infected by soiled linen points to a very short duration, as such linen is seldom retained for any length of time. Some facts which have come under my own observation, though not conclusive, are also in favour of the period being very short.

At the Middlesex Hospital, it was formerly the custom to preserve the stools of cases of typhoid, which the physician wished to inspect, in pans which were placed in the water-closets of the wards. Now the time during which such stools would be so kept would hardly ever exceed twelve hours; nevertheless, several instances have occurred in which other patients using these closets have become infected. Supposing the cause of the infection to have been these reserved stools, we should have a period of not longer than twelve hours for the contagious properties to become developed. The experience of the London Fever Hospital pretty conclusively proves that the fresh stools are innocuous.

The practical conclusion to be drawn is, I think, this: that we have in all cases a few hours during which it lies in our power to render the poison innocuous, and to prevent the spread of the disease by direct infection.

[To be continued.]

THE GULSTONIAN LECTURES ON EPILEPSY.

Delivered before the Royal College of Physicians of London.

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LECTURE II.

MR. PRESIDENT AND GENTLEMEN,—The symptoms of the cases of convulsive disease which form the subject of these lectures, the forms of attack, their variations in severity and frequency, the mental and physical condition of the sufferers, and the mutual relation of these and other pathological elements, constitute subjects of much interest. It is useless to attempt in this lecture to traverse the whole of this extensive field, and our survey must be confined to a few points.

One subject which seems worthy of a passing notice, is the relative frequency with which fits occur by day or by night, using the terms as synonymous with the sleeping and the waking states. The investigation of this point in 840 cases has yielded the following conclusions. The attacks occurred only, or almost only, at night in one-fifth (21 per cent.); only, or almost only, by day in rather more than two-fifths (43 per cent.). They occurred by both day and night in rather less than two-fifths (37 per cent.). Fits occurred in the night, partially or only, in rather more than half the cases (57 per cent.); by day, partially or only, in four-fifths of the cases (80 per cent.). In 1 per cent., the attacks occurred only while going to sleep; in one-half per cent. only on waking up out of sleep. In 5 per cent. the attacks occurred only in the early morning. A much larger number had their first attack in the early morning; and many of these patients were in the habit of early rising, a habit which, it thus appears, should be practised with caution by those predisposed to epilepsy. Between the cases of epilepsy and those with hystero-epileptic symptoms, there was no difference in the frequency with which the attacks occurred in the day or in the night. The proportion, for instance, in which the attacks occurred exclusively, or almost exclusively, by night was, of epileptics, 21 per cent., and of cases with hystero-epileptic attacks, 20 per cent. In epileptics, it is somewhat more common for the first fit to occur in the day than in the night—rather more than half commenced in the former, rather less than half in the latter. In hystero-epileptics, it is much more common for the first fit to occur in the day than in the night. Two-thirds commenced in the day, one-third in the night. Nevertheless, in more than half these cases (56 per cent.) some fits subsequently occurred during the night—that is, during sleep. When the first fit occurred in the day, the subsequent fits occurred only in the day in half the cases, only in the night in one-seventh; by both night and day in one-third. When the first fit occurred during the night, the subsequent fits occurred only at night in about two-fifths of the cases; by day and night also in about two-fifths; and by day only in one-sixth. If we may apply to these proportions the doctrine of probabilities, we may say that if a patient who has his first fit in the day has subsequent fits, the probability is as six to one that some of his attacks will occur in the day; the probabilities are equal that his attacks will, or will not, be confined to the day; and they are equal that he will, or will not, have some attacks in the night; while the probability that he will have attacks only in the night is but one in seven. If a patient who has his first fit in the night has subsequent attacks, the probabilities are equal