REMARKS

ON THE

INCUBATION PERIOD OF ENTERIC FEVER.*

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THE object of the present paper is to relate some facts pointing to the conclusion that the incubation period of enteric fever may be longer than the two or three weeks which it is generally supposed to be. But, before doing this, I will consider briefly some of the instances upon which the belief in short incubation periods has been founded. The evidence in favour of these appears to me to be either insufficient, as in the cases mentioned by Professor Griesinger (*Handbuch der Speciellen Pathologie und Therapie*, Band ii, Sect. ii; Virchow, 1864, p. 149), or to consist of observations, inaccurate in respect of the etiology, or the diagnosis, or of both of these together, as in the Clapham cases recorded by Dr. Murchison (*Continued Fevers*, 2nd Ed., p. 472), and the Cowbridge cases recorded by Dr. W. Budd (*Typhoid Fever*, p. 72. Longman and Co., 1873).

To enter into a consideration of these cases in detail would require more time than is now at my disposal. I must therefore limit myself to a very few remarks on each, and shall consider first the Clapham cases recorded by Dr. Murchison. In these, the main points to be noted are sudden onset by vomiting, early death, and enlargement of the mesenteric, the solitary, and Peyer's glands, with slight ulceration of the latter. Now, whatever these symptoms and these appearances point to, they do not point to enteric fever. Even admitting that in certain circumstances this fever might run as rapid a course as was the case at Clapham, yet, as we of the present day know it, it is not a disease which is fatal within a few hours or days. Death during the first week even, is extremely rare. Sir William Jenner, Dr. Bristowe, and Professor Trousseau each record but one case in which death occurred within the first week. Dr. Murchison, in his exceptional experience, met with only one clear case, and I know of but one other, a case which was admitted into the Homerton Fever Hospital. The history of sudden onset is not the history of enteric fever; on the contrary, no acute disease, of all our English acute diseases, creeps so insidiously upon its victims; and not only the actual suddenness, but the mode of its manifestation by vomiting, is unlike enteric; and, finally, the *post mortem* appearances were not the *post mortem* appearances of enteric fever. For these reasons I submit that the diagnosis of the Clapham cases as cases of enteric fever has not been established, and is, moreover, strongly negatived when we reflect that the symptoms and the *post mortem* appearances, such as they are, correspond with what is known of scarlatina, a fever to which children are specially liable, and of which they often die at an early stage. To begin with the incubation, it is, in scarlatina, in a large number of cases from one to two days; the onset is sudden, and is nearly always attended by vomiting. Death is not unusual on the second or third day, and may occur within a few hours. Indeed, no acute disease, of our English acute diseases, not even malignant small-pox, is so rapidly fatal as scarlet fever; and the *post mortem* appearances described in the Clapham cases are exactly such as are sometimes met with in scarlet fever. I therefore maintain that the symptoms and the *post mortem* appearances described, if they prove anything, prove that the Clapham cases were cases of scarlatina; and have, therefore, no bearing on the question of the incubation of enteric fever.

An error in exact diagnosis applies to the Cowbridge cases described by Dr. Budd. There is, in my opinion, no evidence that these were cases of enteric fever; and the evidence such as it is, whilst insufficient to enable us to form a correct judgment, points to scarlet fever, or, more probably, to poisoning from eating putrid meat, of which there have been notable instances in Germany, rather than to enteric fever, a disease, permit me to remark, not characterised by vomiting and purging.

It would be interesting now to consider the assigned causes of the Clapham and Cowbridge cases, but time will not permit this; and I am compelled to content myself with saying that, in my opinion, these were not proved; and thus from this second side the conclusions as to the shortness of the incubation period were not justified. And here I should like to say that, although I differ from Dr. Budd in this particular instance, his views on the etiology of enteric fever are, in my

opinion, in the main, right, and those of his opponents, in the main, wrong. He is probably wrong on minor points; but in the main, I think he is right; and he deserves no little admiration for his bold advocacy of truth at a time when it was almost sneered at by nearly all the leading authorities, with the notable exception of Sir Thomas Watson.

Professor Griesinger's cases require no comment.

With these preliminary observations I will now relate my own cases. Nurse Broom, aged 22½, entered the service of the Homerton Fever Hospital on July 10th, 1879. From the 11th of that month to the 21st of the following August, she was wholly occupied in nursing scarlet fever in the scarlet fever block. From August 22nd to the 3rd of the following September, she was engaged nursing enteric fever in the enteric fever block. On September 5th she was transferred to the scarlet fever block, where she remained in charge of scarlet fever patients until October 31st, when she was sent back to the enteric fever block, where she nursed enteric fever up to November 11th, when she was herself admitted as a patient with symptoms of enteric fever, having been ill about fourteen days. As nurse Broom had been ill some days previously to November 1st, we cannot look for the cause of the illness among the enteric cases which she nursed between the 1st and 11th of that month; and, since previously to this she had not nursed enteric after September 3rd, and began to be ill somewhere about October 27th, this would give an incubation period of about fifty-four days, calculating from September 3rd, the last known time of exposure to enteric fever, and October 27th, the time about when the symptoms began to appear, i.e., nearly eight weeks. Of course it by no means certainly follows that Broom's illness was caught by exposure either to the enteric patients whom she nursed between August 22nd and September 3rd, or to a drain specifically infected by them. There might have been by mistake an enteric patient amongst the scarlet fever patients whom she nursed between September 5th and October 31st, and this possible unrecognised enteric patient might have been the cause of her illness. Or she might have caught it by visiting nurses or friends in enteric wards; or from exposure to fever-air in her bedroom, or in the dormitory closet which she must have occasionally used. Or she might have caught it outside the hospital from visiting friends, from sitting next a person sick of it in a crowded railway carriage with closed windows. She may have drunk somewhere contaminated milk or water, or have made use of a specifically infected closet at a railway station, or elsewhere; or, lastly, the diagnosis may have been wrong. She may not have had enteric fever. These and other more remote possibilities were considered, and the conclusion arrived at was that, granting the illness to have been enteric, about which there might have been some doubt as the case was not a well-marked one, and the woman recovered, its most probable source was the enteric cases of which she had charge between August 22nd and September 3rd. This conclusion led to the further conclusion that the incubation period must have been about fifty-four days, calculating from September 3rd, the last known time of exposure, to October 27th, the time about which symptoms appeared. There is one other possibility, and that is this: sometime between September 3rd and October 27th, nurse Broom might have had an attack of enteric fever too mild to interfere with her ordinary work, and the attack for which she was treated might have been a relapse. This was considered, and the nurse was specially examined on the point. And, although she could not be certain of the commencement of her illness to within a week, which might throw the illness back to October 20th, and thus reduce the incubation period by seven days, previously to October 20th, she was clear that she was perfectly well. This would give an incubation period of forty-seven days at the least, and it may have been more, because the fifty-four and the forty-seven days are arrived at by assuming that the disease was caught on the last day when she had a known chance of doing so, that is on September 3rd.

Albert Ingham, aged 42, was admitted into the Homerton Fever Hospital, on April 13th, 1880, suffering from enteric fever. He was one of the porters of the Homerton Small-pox Hospital, which had been used for the treatment of enteric fever from September 29th, 1879, to January 27th, 1880, on which day the last enteric patient was admitted there. Between this last date and February 7th, the whole of the enteric patients in the Small-pox Hospital were either discharged or transferred to the Homerton Fever Hospital, and the Small-pox Hospital, having been disinfected, was opened for small-pox on February 8th. Ingham's duties at the Small-pox Hospital, temporarily used as an enteric fever hospital, were to bathe the male patients on their admission; to help to carry these and the female patients to their respective wards; to help to carry the dead to the mortuary, and generally to take charge of the latter. He was in these ways brought into close contact with persons sick of enteric fever, and it is here important to note that he was the only servant, not a nurse, an assistant-nurse, or a ward-

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servant, in direct and frequent contact with the enteric sick; and the only servant, not a nurse, an assistant nurse, or ward-servant, who contracted enteric fever. Briefly, the one general servant of the hospital exposed to contagion, in the ordinary sense, took the disease; whereas those general servants not so exposed escaped. To a possible sewer-air contamination Ingham had been exposed just as others of the general servants had been, although to a much less extent, for his duties did not take him to closets and sculleries. To the decomposed stools he was not to any appreciable extent exposed, for he had not to remove bed-pans nor clean out closets or drains. These latter, and decomposition, are in Ingham's case excluded as far as they ever can be, so long as drains and closets form part of our houses. What his duties brought him into contact with, was not a drain, that being the duty of a special labourer, not a decomposed stool to which nurses, assistant nurses, and ward-servants, might be exposed, but sick persons. This etiological digression is necessary to show that the probability is against the notion that Ingham's illness might have been due to drains containing enteric stools, and that, on this supposition, it might have arisen sometime after the Small-pox Hospital was closed for enteric fever, and that, consequently, any conclusion as to the length of the incubation period is founded on a false premiss.

In trying to make out the history of his case two difficulties presented themselves: first, the apparent length of time between exposure and attack; and, second, the impossibility of fixing, accurately, its commencement. The man was generally weakly, and probably liable to be ailing from slight causes. From his own statement, it would appear that he began to ail soon after the admission of the fever cases to the Small-pox Hospital. He said that he was not well when it was closed on February 7th; that two months previous to his admission to the Fever Hospital on April 13th, he had not been feeling himself, but that he had been definitely ill for four weeks previous to his admission to the Fever Hospital, having eaten little or nothing during that time; all which is very unsatisfactory. Ingham is probably one of those weakly people who are never quite well, and, in trying to fix the commencement of his illness, I will set his evidence aside. On his admission, he looked like a person who had been ill for two or three weeks, and on April 30th his temperature was normal throughout the day, and had been falling for days before. Taking then six weeks as the extreme limit of the duration of cases of ordinary severity, and this was one of these, this would lead us up to March 20th as the commencement of Ingham's illness; and from March 20th to February 7th, the last time he was in known contact with enteric sources, would give an incubation period of forty-one days, or about six weeks at the least. In thinking over possible sources of this man's illness, one had to think of the fact that his wife lived with her father in a house near the hospital, and that he sometimes visited there. With this house I was familiar, as one of many noted during the last ten years for two things: defective sanitary arrangements, entire freedom from enteric fever. As regards this house, however, it must be noted, out of respect for opinions in which I do not share, that the drainage had not been satisfactory from January to May. The houses on each side of Ingham's father-in-law's had their closets out of order in April, and fluid from the drain had flooded Ingham's father-in-law's yard about the end of April; be it observed long after the beginning of Ingham's illness. Here possibly some would find the cause of Ingham's fever, but unfortunately for that view the drainage of the neighbourhood is the same, but better generally speaking in this house than in most. As regards Ingham, moreover, one must remember that he did not live there, but only went there occasionally to see his wife, and that there was not a single case of enteric fever amongst the families living in the neighbourhood and exposed continuously to such sewer-air as might exist. I conclude, then, that Ingham did not catch his illness at his wife's home. All the possibilities thought of in the case of Broom were also thought of here, and the conclusion which seemed the most probable was, that Ingham caught his illness whilst in contact with persons sick of enteric fever, which led to the further conclusion that the incubation period was at least about six weeks. There remains one objection, an objection not affecting the incubation except to lengthen it; and that is, that Ingham's illness may have been caused by assisting at *post mortem* examinations, when he would be exposed to the decomposed or decomposing contents of enteric intestines. To this there is a satisfactory reply. My friend Dr. Gayton informs me that, during the time the Small-pox Hospital was open for enteric fever, three *post mortem* examinations were made, one of which was found to be cancer of the rectum, and two others cases of acute pericarditis.

My last case, which was published in the JOURNAL of the Association for November 30th, 1878, time only permits me to mention. It was the case of Sarah Allen, a laundry-maid in the Homerton Fever Hospital laundry. For upwards of two years it had been her duty to sort the linen, and to clean what was soiled; and this duty, notwithstanding

standing the exposure to decomposing enteric stools, she performed with impunity, but contracted the disease about six weeks after she had last visited her sister sick of it.

One remark before concluding. My thanks are due to my colleague Dr. Twining for the assistance which he has rendered me in the preparation of this paper.

ON THE ETIOLOGY OF ENTERIC FEVER.

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IN an able paper, which was published in the BRITISH MEDICAL JOURNAL for January 17th, Dr. Collie supports the view that enteric fever is contagious, confining himself to the proposition, that by some means or other, it spreads from person to person; while he leaves the method by which the contagion is carried an open question. This opinion is opposed to the pythogenic theory, which places the cause of typhoid fever in decomposing filth; and which found so eminent an exponent in the late Dr. Murchison. But it is in no way incompatible with the more usually accepted etiology, which maintains that the poison ordinarily enters the system by means of air and drink contaminated with the excreta of patients suffering from the disease. Dr. Collie merely affirms that enteric fever is propagated by contagion passing directly from one person to another; and my experience at St. Thomas's, extending over three or four years, certainly corroborates this view.

In forming an opinion upon this question, it must be borne in mind that the conditions are somewhat different from those in which we are placed when determining whether other fevers are contagious or not. If, on admission of a case of typhus, for instance, into a general ward, those near the patient be attacked with the disease, we look upon this as a proof that it is contagious. But if the patient be isolated in a neighbouring room, and the disease fail to spread, we do not withdraw from the conclusion we had formed: we know that the poison has not had the opportunity of acting. For, experience showing that the contagion of typhus seems to hover closely round its victim, we remove the other patients beyond its range of action, and so prevent infection. Now, in the case of enteric fever, we seem to forget that, if we artificially render a poison inert, we thereby deprive ourselves of the means of judging of its clinical properties. Believing as we do that the poison here resides mainly, if not exclusively, in the excreta, we make every effort to disinfect them at the time, or immediately after they are passed, and carry them beyond the reach of other patients, so that it becomes almost impossible for the virus to take effect. The success of these preventive measures is shown by the rarity of enteric cases arising in hospitals, and the absence of epidemics of the disease in these institutions. Yet, after taking such precautions, we lay great stress on the fact that nurses and patients rarely contract the disease in a hospital, in order to show in how small a degree typhoid fever is contagious. We should rather go to the houses of the poor, where no endeavours are made to prevent infection, and there observe the spread of the disease. A difficulty, however, meets us here. For it is generally presumed that the first case arising in any locality becomes infected by water or air contaminated with the typhoid fever poison; and how are we to know whether cases which occur subsequently are due to the same cause, or to contagion from the person first affected? In fact, our observations, whether conducted on the spot where the disease originates, or at a hospital, are rendered very difficult. Of the two, the latter is the better field for research, as we have a more complete knowledge of our patients' surroundings, and can watch them under fairly well-known conditions. But, in considering the etiology of typhoid fever, great importance should attach even to single cases originating under such circumstances, if we have good reasons for considering the patient to be free from all chance of contracting the disease, except from his companions; and still more if we have done our best to prevent such an occurrence by precautions which are taken in all well-arranged modern institutions.

To show how rarely nurses and patients are supposed to contract enteric fever in hospitals, I will quote the following passages from Dr. Murchison's treatise on *Fevers*. Speaking of the disease under consideration, he says: "Hospital experience lends little support to the doctrine of contagion." "Dr. Wilks informs me that he has never known a nurse in Guy's Hospital contract enteric fever." "In 1856, Dr. Peacock remarked that he had never known enteric fever communicated to the nurses and attendants at St. Thomas's Hospital." "The only instances of enteric fever contracted in all the general hospitals of London which Messrs. Bristowe and Holmes could discover, in their official inquiry in 1863, were those of two nurses in the Royal Free Hospital." "My