PUERPERAL FEVER.

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I READ with great interest the report of the debate on Puerperal Fever at the London Obstetrical Society. Amidst a good deal of somewhat vague theorising as to the essential nature of the fever, and its relations to other zymotic diseases, one practical point seemed to stand out rather prominently; viz., that a (if not the) principal mode of the spread of that terrible disease, is its conveyance by the medical attendant, from one patient to another. That this impression was produced upon others as well as myself, is proved by the fact that two letters at least have since appeared in the JOURNAL attempting to fix a period of quarantine for all doctors who have a patient so attacked, in which attempt they have been seconded by a coroner's jury, whatever its opinion may be worth. Such a question, therefore, cannot be confined to the debates of the Obstetrical Society, but must percolate through society generally, creating great alarm and seriously injuring medical men. If this opinion be true, of course, those consequences cannot be avoided; but, before coming to such a conclusion, it appears to me that we should possess more extensive and more precise information than we have at present, and, if an old practitioner be of no other use, he may at least be allowed to plead for delay and further consideration.

Let us see upon what ground we stand.

1. We know that a person going direct from a *post mortem* examination to a lying-in case may excite puerperal fever in that patient. But how often does the opportunity for such mischief occur? I should suppose that most obstetricians do as I used to do; viz., ask a friend to make the *post mortem* examination for me; but, if in the country parts this be impracticable, surely it must be very rarely that the *post mortem* examination and the delivery succeed each other immediately.

2. We know that medical men attending cases of erysipelas have excited puerperal fever from attending lying-in women at the same time, especially if, as has happened, due precautions had not been taken.

3. We know that nurses going from attending on puerperal fever to lying-in women have conveyed the disease. But it must be remembered that a nurse attending on a case of puerperal fever comes into very close contact with the patient, and her hands have to do with the discharges and with whatever may be unhealthy about the genitals, and this at least once every day; so that it would require extraordinary care to avoid conveying morbid matter to the vaginal mucous membrane of the parturient woman.

4. We also know that, in some cases, the disease has followed in the track of some particular doctor, and that, in spite of minute and thorough precautions, as in the case of Dr. Gooch's friend and the American physician mentioned by Holmes and Dewees.

How to explain such cases, I confess I do not know. But very few such are on record, nor do I think they can be used to prove the necessity of quarantine; they rather show its failure.

5. All men in large obstetric practice meet occasionally with cases of puerperal fever. Dr. Dewees met with a number of such; but, in a passage too long for quotation, he has given the dates of their occurrence, showing conclusively that he could not have transported the complaint; and he mentions that he met in consultation the physician who was haunted by the disease, with perfect immunity as regards his own patients.

6. In the course of forty years of practice, I have had puerperal fever among my patients; but the most rigid inquiry has failed to shew me any connection between any two patients. The precautions I adopted were these. I never attended a confinement in the clothes I had worn on my visits. I always saw recently confined patients before seeing the fever case, and, on returning home, I always changed my clothes. I was scrupulously careful about the frequent and thorough washing of my hands before leaving the patient's house. So far as I was concerned, these precautions were successful; for I certainly never carried the disease from one patient to another. If they had failed, it would have been difficult to say how the disease had been conveyed. If the dress and hands were out of the question, it must have been by some absorption of the "contagium".

7. If the ordinary attendant is to be put in quarantine, what about the consultant? Is he not as likely to carry infection? Yet we never hear of his doing so, and no one refuses to see these cases and yet attend his own lying-in patients.

Taking into consideration all these points, and before we assume the portability of the disease, I hope that some effort will be made to col-

lect examples to show how often, notwithstanding due precautions, the sequence of the disease proves it to have been conveyed from one patient to another by a third person. If well authenticated cases of this kind be numerous, then puerperal fever is more infectious than typhus, scarlatina, or even plague, and the doctors must go into quarantine. If not, neither the profession nor the public need be alarmed, and, with proper precautions, the doctors may pursue their avocations in peace and security.

WHAT IS THE DURATION OF QUARANTINE REQUIRED AFTER PUERPERAL FEVER?

By A. D. L. NAPIER, M.B., C.M., Fraserburgh, N.B.

IN the BRITISH MEDICAL JOURNAL of January 22nd, Dr. Braxton Hicks has invited discussion on the above-named question. The disease is inferred to be always contagious; Dr. Hicks evidently ignoring the idea of epidemic puerperal fever as a reality. Without wasting time in specious arguments, I simply deny the correctness of this hypothesis. I could easily prove that the disease was generally epidemic. I do not wish to systematise or theorise, as most authorities on this subject are too apt to do, but will simply give unvarnished facts.

I have in my possession notes of thirty-one cases, of which twenty-three were under my own care. I have formerly published twenty-two of the thirty-one cases. I do not think it necessary to instance midwifery cases which were attended by myself shortly after treating a case of fever, when these patients were in course of time seized with fever; of these I had a few examples, but that is not the point raised.

The question to answer is, How long must we abstain from obstetric practice after attending a case of puerperal fever? As Dr. Braxton Hicks uses the word *accoucheur*, I presume the question more exactly is, If a medical man deliver a woman who is attacked with puerperal fever, how long time must elapse before he can with safety deliver another patient? To prove the extreme uncertainty of the accuracy of any hard and fast law to guide us in our rules of the requisite quarantine, I will cite a few instances.

On December 8th, 1874, I delivered a primipara, who died of puerperal fever on December 13th. I treated a case of puerperal fever from December 16th to December 31st. This woman had been delivered by a midwife on December 13th, and died on December 22nd. I refused all midwifery till January 24th, 1875, when I attended another woman. She had a tedious labour, which was terminated by forceps; she grew ill on January 25th with puerperal fever, and was cured on February 7th, after a severe illness.

On February 20th, I attended a difficult case, which was followed by good recovery; no fever. On March 5th, I delivered a woman. This patient had puerperal fever, with subsequent oophoritis; she was cured in May. Having had several cases prior to this, I left off practice from March 9th till April 29th, when, returning home, I delivered a patient, who died six days after her confinement. On May 21st, I had a severe forceps case; on May 29th, a breech case, besides others; all did well. These are cases with long intervals, let us analyse them.

The first was delivered on December 8th, 1874; the second I did not deliver; the third was delivered on January 24th, 1875; so that there was an interval of forty-seven days. The next case, less than a month afterwards, had no fever, but made a good recovery; the fourth, a woman confined on March 5th, forty-one days from January 24th, took fever; the fifth case quoted, took fever fifty-six days after the case preceding it.

In July, 1875, puerperal fever again broke out after we had enjoyed a perfect immunity from it. Thus, we had an interval of three months. At this time, as at the former, cases occurred not only in my practice, but in the hands of other practitioners also.

Now for the other side of the question. On July 21st, I attended a woman in childbed, who took puerperal fever. I treated her till August 8th, when she had recovered; I had also under my care from July 29th to August 2nd, a mild case of the fever. On August 4th, I attended a case of midwifery; the recovery was excellent. On August 28th, I delivered a woman who took the fever, and recovered on September 9th. On August 31st, I had a very difficult forceps case, in a second labour; the woman made an excellent recovery. On September 15th, I attended an accouchement, which was followed by a good recovery. On September 16th, I attended a primipara, a forceps case; the woman had puerperal fever, and died September 23rd. On September 20th, I attended a case of labour, followed by good recovery. On September 30th, I treated a woman for puerperal fever, who had been delivered by a midwife on September 28th; she reco-

vered by October 9th. On October 6th, while treating the above, I attended a woman in labour, whose husband was lying dangerously ill of typhus fever; recovery was very rapid. On October 29th, I delivered a primipara of twins; she made a very good recovery. No more cases have occurred since the one in September.

To examine these cases: on July 21st, I had delivered a woman who took fever, I attended her up to August 8th, and, in addition, another case up to August 2nd; during frequent visits to these cases, I delivered a patient on August 4th, who took *no fever*. Again, on August 28th, I delivered a woman, who had afterwards puerperal fever; she did not recover till September 9th. On August 31st, only three days after the former case, I had a very severe case, which was followed by a most excellent recovery; in fact, the woman was so well, that she imprudently went out on the fourth day.

The three following cases, *per se*, are sufficient to upset many dogmatic opinions regarding the contagion of the fever. One woman, confined on September 15th, had a capital recovery; one, confined on September 16th, took malignant puerperal fever and died; the third, confined on September 20th, had no fever, but made a most favourable recovery. And, for another example: while I was on daily attendance on the next patient mentioned, who recovered on October 9th, a woman delivered by me on October 6th had no unfavourable symptoms.

Lest the question should be raised, I add, that no prophylactic treatment was used in the cases which escaped, beyond attention to cleanliness and diet; and, in the case which occurred on September 20th, the patient used vaginal douches of tepid water; also, that I was more careful regarding personal disinfection in the earlier than in the later cases.

The profession at large will be greatly indebted to Dr. Hicks for having raised this question; but it is only those men who have suffered from the depressing effect of so hideous a bugbear that can, with sufficient appreciation, realise the import and weight of its bearing.

CLINICAL MEMORANDA.

PECULIAR ALBUMEN.

DR. HADDON has carefully described certain reactions which he has observed in testing a sample of albuminous urine, and which have led him to doubt whether the "abnormal ingredient" which gave these reactions be albumen at all. As I have had the opportunity, in the laboratory of King's College, to work for some months exclusively upon the subject of albumen and its chemical behaviour, and as I hope that my remarks may help to clear up some of the confusion which now obscures the subject, I will do my best to offer a satisfactory explanation of the apparent anomalies noticed by Dr. Haddon, and to prove that the substance which has puzzled him so much is really albumen.

In his memorandum which appeared in the *BRITISH MEDICAL JOURNAL* of February 12th, Dr. Haddon describes the following reaction. A highly acid urine containing one-third of albumen yields only a slight cloud on the addition of a single drop of nitric acid, which disappears on shaking, leaving a clear liquid which is unchanged by the application of heat. This by no means novel observation was to my mind quite sufficiently commented upon and explained by papers which were published in the *BRITISH MEDICAL JOURNAL* for February 26th, though I shall have more to add on this subject presently.

But Dr. Haddon does not stop here. In his paper of March 4th, he mentions the results of some new experiments made upon another sample of the same urine mentioned in the *JOURNAL* of February 12th, but he does not tell us whether this sample, like the former one, contained one-third of albumen: a point which is of some importance in connection with at least one of the reactions he describes, as we shall see. He also omits to mention the reaction of this sample with litmus. He writes: "A few drops of nitric acid in the cold urine made no change." He probably means no permanent change; for, if the acid employed in this experiment were of the same strength as that used in the first, he would surely have observed a faint cloud, which speedily redissolved on agitating the liquid. However, the explanation of the absence of any permanent turbidity in the liquid is the same in both cases; viz., a small quantity of nitric acid only was added, sufficient to form the compound, soluble even at 212 deg. Fahr., which I have ventured to call nitrate of albumen, and insufficient to render the albumen insoluble, *i.e.*, to coagulate it.

Dr. Haddon then proceeds to add acid in quantity equal to a quarter of the volume of the urine, *i.e.*, a moderate excess. The result (as might be expected) is whiteness and opacity of the liquid, produced, of course, by coagulation of the albumen. To this opaque liquid nitric

acid was further added, and, when a quantity equal to half the bulk of the urine had found admission into the test-tube, the liquid was quite clear. Surely Dr. Haddon must be aware that a large excess of strong nitric acid will dissolve coagulated albumen, especially that which has been coagulated without the application of heat; and that there is consequently nothing in all this which can throw any doubt upon the nature of the "abnormal ingredient" whose properties he describes.

Dr. Haddon next adds half its bulk of nitric acid to some of the urine in which the albumen had previously been coagulated by heat. The result is a clearing up of the turbid liquid, but with a slight residual deposit of small white flocculi. The explanation of this reaction is the same as that of the last, the slight insoluble residue being accounted for by the fact that albumen which has been coagulated by heat is always far more difficult to bring into solution again than that which has been coagulated by other reagents. Probably a further addition of strong nitric acid and a more prolonged application of heat would have dissipated even these white particles which Dr. Haddon observed.

The next experiment mentioned does, I confess, puzzle me to explain. Undoubtedly, one drop of nitric acid added even to a boiling solution containing suspended coagulated albumen would not render the solution clear, nor even clearer than before, unless phosphates were also present. Unless, therefore, the amount of albumen present were very small, this observation does appear startling. I notice, however, that Dr. Haddon confesses that the liquid was "slightly turbid" after the subsidence of the white flocculi. The explanations of Dr. Haddon's remaining observations are sufficiently simple.

To half a tube of urine he added a drop of (1) nitric, (2) hydrochloric, and (3) sulphuric acid. No change took place in either case even on boiling. The soluble nitrate, hydrochlorate, and sulphate of albumen had been formed in each case respectively, and, there not being sufficient excess of acid present to coagulate the albumen, the liquids remained clear even on boiling; for it is a law, which Dr. Haddon will find to be true, that a clear liquid holding in solution albumen and a free acid will remain clear on boiling.

Dr. Haddon next emptied the tube, and added a fresh half-tube of urine, omitting to rinse out the adhering acid. Heat failed to coagulate the contents of the tube, for there was sufficient acid present to combine with all the albumen present and to hold it in solution. But, when the tubes were emptied the second time and half-filled with urine, though they had not been washed since the last experiment, coagulation of the albumen was produced by heat, the reason being that there was not sufficient free acid present to form the soluble compound previously mentioned.

Dr. Haddon's next paragraph is a beautiful proof of the combination of albumen with acetic acid. Each drop of the acid which he added to the albuminous urine diminished the turbidity produced on boiling, until, when he had added sufficient to combine with the whole of the albumen present, the liquid remained quite clear on the application of heat.

Since Dr. Haddon has "not seen the compound" formed between nitric acid and albumen, nor "tried its reactions", perhaps he may be interested to hear what those reactions are, and to verify my statements (as he readily may) for himself. The following, then, are the chief differences between ordinary albumen and the nitrate of albumen.

(a) The solution of the latter is not coagulated by heat.

(b) The following reagents which coagulate ordinary albumen do not coagulate a solution of its compound with nitric acid; viz., mercuric chloride, silver nitrate, tannic acid, lead subacetate, alcohol, and carbolic acid.

If, however, Dr. Haddon or any of the readers of this *JOURNAL* should feel interested in noticing the various reactions of the different compounds of albumen with the acids, and the proportions of acid to albumen present in each, I would refer them to a paper which I published on this subject in the *Journal of the Chemical Society*, August 1874, a paper which was referred to by Dr. Rayne in the *BRITISH MEDICAL JOURNAL* of February 26th; and I may here mention with what pleasure I find that that gentleman corroborates my views on this subject, though he has attacked the matter from a different point of view.

Finally, Dr. Haddon thinks he has proved that the present methods of testing for albumen in urine are unsatisfactory. But perhaps the assertion is not too bold that, with a judicious employment in experienced hands of the two methods (heat and nitric acid) now at our disposal, no sample of urine containing sufficient albumen to be of pathological importance could escape detection. Neither method is trustworthy *per se*; but the combination of the two will give results sufficiently accurate for all practical purposes.

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