he was in bed.—On February 12th and 13th, it was absent; he was in bed.—On February 14th, he was allowed to sit up; and, at 7 P.M., albumen appeared, which continued until February 16th, when I put him to bed again.—On February 19th, he got up. At 2.30, after a small meat dinner at 1 P.M., his urine was loaded with albumen, quite one-third on depositing; he was, therefore, put to bed, and allowed milk only.—On February 21st, he had meat, and remained in bed.—On February 25th, he got up, and had meat at 1 o'clock for dinner. At 3 P.M., his urine was again loaded. He was, therefore, put to bed.—On March 2nd, he got up, and had milk only. There was no albumen.—On March 4th, he got up, and had bread and milk for dinner, and an egg and bread and butter for tea. Albumen again appeared, about one-sixteenth.—On March 7th, he got up again but had milk only. He had three visitors, and albumen was found in his urine at night.—On March 11th, he was washed limb by limb, while in bed, at 10 A.M. Albumen was found at 11 A.M.—On March 15th, he got up (as his parents wished him to go home soon) at 6 P.M. His urine contained albumen, about one-third. The albumen was deposited by nitric acid in cold urine, and also by heat and nitric acid. The specific gravity was 1019. At 9.30 P.M., the urine contained about one-sixteenth of albumen.—On March 16th, at 8 A.M., there was only a trace; at 9.30 A.M. about one-fourth; at 12.45 P.M. a trace; at 5 P.M. one fourth; and, at 9 P.M., also one-fourth, of albumen.—On March 17th and 18th, albumen was present all day.—On March 19th, he travelled home, and was under treatment for three months at the seaside.

CASE IX. Mr. I., aged 16, never had scarlatina. During vacation he fainted in church, and was sent to school, suffering from general debility, for which a tonic was requested, and I was asked to see him about it. Feeling his hard slow pulse, and observing his muddy countenance, and furred tongue and headache, I immediately examined his urine and found it albuminous. On September 28th, 1878, he was put in bed, and allowed milk only. The albumen disappeared, and it has not returned, although the urine is examined regularly once a week.

CASE X. Mr. X. aged 15, had scarlatina in 1869. On November 7th, 1878, he came to me saying, "Please, sir, I always feel so faint when I have been playing football or running"; but he had never actually fainted. Feeling his pulse, I found it extremely hard and firm, and I had him stripped to examine his heart. There was no valvular disease, but its impulse was heaving and hard. From the above conditions of pulse and heart, together with his pallid dirty countenance and furred tongue, I suspected albuminuria; and, on immediately examining the urine, I found about one-fourth albumen. He is now under treatment.

I am fully conscious of the imperfections of these notes, which are only hurried notes, taken at the bedside.

REMARKS ON THE ORIGIN OF TWO CASES OF ENTERIC FEVER, IN THE HOMERTON FEVER HOSPITAL.

By ALEXANDER COLLIE, M.D., Medical Officer to the Hospital.

MARIA ALLEN, aged 21, a laundry-maid in the service of the hospital, was admitted into the enteric ward on the 12th of May, with well-marked enteric fever of from eight to fourteen days' duration. Convalescence was established on the 10th of June, and on the 12th of July she was discharged recovered.

Sarah Allen, aged 25, sister of the preceding and also a laundry-maid in the service of the hospital, was admitted into the enteric ward on the 30th of July, with well-marked enteric of about ten days' duration, and is now (August 19th) still under treatment. The former had been upwards of two years in the service, as ward-servant and occasional laundry-maid, in both of which situations she would be exposed to enteric effluvia; and the latter, wholly as laundry-maid, for about two years and a half.

The interest of these cases lies in the question: How did they arise? In attempting to answer this question, one thinks of the laundry at once, and particularly in reference to the Allens, whose special duty it was to sort all the linen and to wash what was soiled. The origin of an infectious disease from linen soiled by the discharges of the sick is a generally received opinion; but it has not been observed to arise in this way at Homerton: for, of upwards of one hundred persons, mostly young women, who have been engaged in laundry-work there during the last seven years, one only contracted any fever, a laundress aged 40. With reference to the Allens, when we consider that Maria

had been engaged daily more or less at the soiled-linen tub for upwards of six months continuously, and for about two years off and on, and her sister for upwards of two years and a half, and both with impunity; that they were both attacked at the time of year when enteric fever is at its minimum; and when, therefore, as was the fact with us, there would be less soiled enteric linen to wash than usual, I think this mode of origin may be set aside, especially if any other more reasonable mode can be shown. As regards drainage, the drains of the laundry were in good order, and had not been once stopped during the year.

As regards lodging, they slept, along with ten other persons, on the second floor of the front part of the hospital, in well-lighted and well-ventilated bedrooms, belonging to which there was a closet on a lower landing common to these twelve persons. By their carelessness, this closet was frequently stopped. It was so on the 16th and 30th of April; on the 14th and 20th of May; on the 17th of June; and on the 8th and 22nd of July. In the majority of instances, these stoppages would not have lasted more than a few hours, and in none more than twenty-four to forty-eight, because the engineer examines each closet daily, Sundays excepted, so that the stoppage of Monday, the 22nd of July, might have lasted forty-eight hours, *i.e.*, from the engineer's visit of Saturday morning to his visit of Monday morning. Were these stoppages, or any of them, the cause of the illness of the Allens? Maria was admitted to the ward on the 12th of May, having been ill fourteen days, which would make the commencement of her illness the 28th of April, exactly twelve days from the stoppage of the 16th. Sarah was admitted on the 30th of July, having been ill about ten days, thus making the commencement of her illness the 20th of July, exactly twelve days from the stoppage of the 8th—intervals of time which would suit admirably the incubation period of enteric fever; for they agree almost to a day with the only observations upon which any opinion can be founded about enteric incubation, *viz.*, those of Dr. Buchanan on cases seen by him at Guildford. The stoppages of this closet, it may be urged, were of too short duration to breed fever; but it is impossible to say how much sewer air is necessary to give rise to enteric fever, or what duration of block may be necessary to produce that air; so that an adherent of the pythogenic theory might find, in the blocking of that closet, a satisfactory explanation of the illness of the Allens. Such explanation appears to me defective, because it does not explain the immunity of ten other persons, all exposed to the same influences as the Allens, two only of whom had had enteric fever, and seven of whom, by their ages, which ranged from nineteen to thirty-three, were very susceptible to it—fifteen to twenty-five being the age of maximum susceptibility after childhood. The closet, then, may fairly be excluded. The cause, whatever it was, would appear to be one acting on the Allens alone. Did they catch the disease outside the hospital, either at their own home or at some other house where they might have been visiting? I found, on inquiry, that the only house which they had visited during the last six months was their mother's, No. 50, Brook Street, Homerton. This house is one of a series of blocks of buildings lying between the Homerton Hospitals and Homerton High Street, the sanitary arrangements of which are very defective. Many of the closets have no water at all, and in others the supply is deficient. I found the pans almost black with indurated filth, and heaps of garbage in the small yards; pythic matter in abundance everywhere, and stinks to every one possessed of a nose. Here, if anywhere, are conditions for the production of enteric fever, if the pythogenic theory be correct. Here, if anywhere, one would expect it to be endemic, whereas, strange to say, it is almost unknown. Notwithstanding these presumably favourable conditions, and the poverty of the population, not a single case of enteric fever has been admitted into the Fever Hospital from any of the numerous blocks in the houses referred to; and Dr. Fisher, Dr. Turtle, and Mr. E. Smith, practitioners there, inform me that it is almost unknown, and, as a matter of fact, there is not a case there now. The house which concerns us is one of the worst. In it the closet had no water laid on; the pan was coated with black indurated filth, which was treated to a pailful of water every morning. Of the arrangements generally, it may be said briefly that they were bad. On the ground-floor of this house lived the mother, aged 47; the father, aged 51; and three boys, aged respectively 14, 15, and 21; and on the upper a man, aged 70; his wife, aged 60; and a child, aged 7—nine. All these were in good health, and there had been no fever there for at least eleven years. A little sister of the Allens, aged 3, however, was on the evening of August 5th seized with diarrhoea, and on Saturday the 9th died in convulsions, which Dr. Fisher attributed to meningitis. There was no enteric fever then, nor is there now, in Brook Street, or indeed in the neighbourhood. In these circumstances, I think it may be concluded that the Allens did not contract their enteric in Brook Street. To ordinary filth decomposition they had been exposed in their home visits without result; to special filth in sheets soaked by enteric

stools one of them for upwards of two years without result, and the other continuously for six months and intermittently for about two years. I imagine no one will seriously assign as the cause the "blocked closet", for one is immediately confronted with the difficulty of explaining the immunity of the seven other susceptible persons. The origin of the fever, then, has to be looked for in some circumstance, or set of circumstances peculiar to the Allens. In what respect, then, did their relations differ from their more remote? They differed in this very important and pertinent respect that, on several occasions previous to their seizure, they were in direct and personal communication with persons sick of the disease. The circumstances were the following.

On March 22nd, G. Lowe, aged 29, living at 48, Queensbury Street, Islington, was admitted into the hospital with well-marked enteric fever, of which he was dangerously ill, and this Lowe was the brother of Maria's young man. After the 1st of April, Maria, hearing that he was there and dangerously ill, visited him on at least four different occasions at the height of his illness, and on each occasion remained with him about ten minutes. On or about the 28th of April, she began to be ill, which would give an incubation period of about three weeks. Maria was visited daily by her sister, who fell ill on the 20th of July.

Looking at all the circumstances, it seems to me that the obvious and reasonable explanation of Maria's illness is that she caught it by personal communication with Lowe, and of Sarah's, that she caught it by personal communication with Maria, *i.e.*, by contagion. If these cases stood alone, much could not be made of them; but, taken along with the observations of the late M. Piedvache of Dinan, and those of Dr. Budd, they are, I think, of great importance. They are not isolated cases, and, but for the predominance in this country of the pythogenic theory, under the influence of which impartial observations cannot be made, many other like cases would have been met with here as they have been in France; and, whenever it shall be admitted that the pythogenic theory may be wrong, I do not doubt that contagion will be found to be a powerful factor in the causation of the disease. That this is a mode of origin in many cases I am convinced from my own experience; and, whilst I hope, in conjunction with Dr. McCombie, to consider the subject when leisure permits in detail, I think that, considering the weight of evidence given by M. Piedvache and Dr. Budd, just referred to, along with our own experience, no time should be lost in urging upon the attention of the profession the consideration of this mode of origin, believing that much suffering will be prevented and some lives saved. Without asserting that the contagion of enteric fever is as potent as that of typhus or scarlet fever, it is, nevertheless, a very real—and to persons between fifteen and thirty—a very great danger. Several of our nurses, nearly all of whom were between twenty and thirty, have contracted the disease while nursing enteric patients, just as nurses have contracted typhus while nursing typhus patients; and it is no more an objection against the contagiousness of enteric, that many nurses escape it, than it would be against the contagiousness of scarlet fever, and, for the same reason, *viz.*, that many, perhaps most of them, are protected from it by a previous attack. For not only is enteric fever very common in infancy, but it is so very rarely fatal then, that there must be a considerable proportion of adult persons protected by a previous attack. In treating enteric fever, then, we think that it ought to be isolated in the same way as typhus or scarlatina, especially where children or young adults are about. Enteric nurses, unless protected by a previous attack, ought when possible to be about forty years of age, and, if possible, should never be under thirty. The stools, which are probably specific, and at their maximum of infectiousness when discharged, ought to be removed at once, and disinfected by the only efficient means of doing so—burial; and this may be done by the use of an earth-closet. To allow these stools to go into drains is to risk the communication of enteric fever whenever a block occurs, and it is in this way, I think, that outbreaks of enteric fever in relation to drains which have been blocked for years, may be explained, as in the case of Westminster, quoted by Dr. Murchison (*Continued Fevers*, p. 473, second edition), as an instance of the truth of the pythogenic theory. But how, may one ask, did it happen that the "private sewer or immense cesspool" had been accumulating for years without hurting anybody? Decomposition of sewage was there, existing in abundance day by day for years, and sewage decomposition on the pythogenic theory is the chief cause of enteric fever; but there had been no enteric fever for years. Sewage decomposition, then, may exist in abundance without producing enteric fever, *ergo qua* such cannot be its cause. What recent change had taken place in the surroundings of these Westminster people? Some new thing had happened, for here was a new effect in the shape of a sudden outbreak of enteric fever. This is what we want to know. Were a series of enteric stools certainly excluded? Was a mild case of enteric fever, of

which there are many, overlooked? And was this supposed case discharging daily specifically infected matter into this choked drain? Or, was it milk? The same remarks will apply generally to the majority of cases in which drains are invoked. Such, choked up, are to be found in abundance without any enteric fever, and when a specific disease arises in the neighbourhood of one which has been in that condition for years, impotent of specific result, we ought, I think, to look for something else as the cause of the sudden outbreak of the specific disease. That enteric fever arises when, to all appearance, contagion is absent, is true; as it is also true that it arises when drains are in good order. Is it absurd to suggest that enteric fever may arise from changes originating within the body independently of specific external cause, and that, having so arisen, it may spread by contagion?

CLINICAL MEMORANDA.

SCARLATINA AND SURGERY.

It is gratifying to one who has paid some attention to the connection between the exanthems and the puerperal state to notice, as shown in the *JOURNAL* of November 9th, that surgeons are becoming alive to the association of scarlatina with surgery. Besides the authors mentioned, I have added three more in my Contribution to our Knowledge of Puerperal Diseases, read before the Obstetrical Society in 1870, and published in the twelfth volume of the *Transactions*: one given me by Mr. J. Hutchinson, one by Mr. Durham, and one quoted from Mr. Bryant's *Clinical Surgery*. A comparison was then made between the surgical and puerperal aspects of the case, so far as our then knowledge permitted, which was not much, and, I am afraid, is not now much more; nor will it be likely to be much more so long as we, both surgeons and obstetricians, confine our attention to those cases where the ordinary symptoms of the exanthem are present. Supposing we say, as some have said, that, so long as the rash and sore-throat are absent, we refuse to consider the case has anything to do with scarlatina. With these words in our mouths, we cannot advance—at least, at any pace. Need one for a moment stop to refute the position? Does not every epidemic point out that variations of the symptoms of an exanthem are notorious, and that not unfrequently we have neither rash in one case nor sore-throat in another? And occasionally neither is present in an undoubted case of scarlatina. What means, then, the division of scarlatina into simplex, anginosa, and maligna? When we see the rash, sore-throat, etc., we are not beholding the disease—only its effects; certainly its more usual, but not always its constant ones. Analysis is very well for us as students of a disease; but when this has done its work, then it is the claim of the advanced observer to employ hypothesis.

No progress in the subject under consideration can be made till, in addition to recording cases of surgery or delivery complicated with an exanthem, we carefully also record the history of those cases of both where an exanthem is notoriously about the patient; and also inquire most carefully, in those cases where a bad recovery or fatal result has occurred, into all the possible influences which may have, singly or combined, affected the patient. I well remember a remark of the late Mr. Solly *à propos* of this question: "Whenever a case of surgery in private takes on a highly phlegmonous appearance, I am always sure to find break out in the inmates of the house either erysipelas or scarlatina." In a small way, in this direction, I have endeavoured to add to our knowledge by publishing the paper above mentioned, and also a paper in the *Guy's Hospital Reports* on an outbreak of diphtheria in my ward at the hospital some years since, where the influence on all is traced. By the light of such investigations, but not without, we may some day gain a nearly accurate knowledge of the part and proportion the exanthemata play in the opposition met by our patients towards healthy recovery.

J. BRAXTON HICKS, M.D.

NOTE OF A CASE OF ARRESTED DEVELOPMENT OF THE DIAPHRAGM.

A HEALTHY lady was recently delivered of her third child. The infant, when born, appeared to be a large and healthy one, but its cry was rather feeble; and a few minutes after delivery, in spite of all my endeavours to assist its respiration, it died. *Post mortem* examination showed a deficiency of the left crus of the diaphragm. A large opening existed, through which had protruded the stomach, the small intestines, the pancreas, and the spleen. The position of the viscera was as follows. The small intestines lay in the left thorax, pressing upon the lung, the growth of which was so far prevented that it mea-