nates presented, and the child was also extracted alive." "Dr. H. Davies induced premature labour at the seventh and a half month of her fifth pregnancy, and the child was born alive, but died soon after in convulsions."

Occurrences similar to what happened in the preceding case must, observes Dr. Lee, "originally have suggested the idea of bringing on premature labour artificially in cases of distorted pelvis, and probably led, in 1756, to that consultation of the most eminent practitioners in London, at which the practice was approved of, and soon after successfully carried into effect by Dr. Macaulay;"* and by a similar study of the modes of delivery occasionally adopted by nature in the same cases of distorted pelvis, we have, I think, suggested to us likewise the propriety of artificially turning and extracting the infant as a footling presentation. I shall cite only one other case from the same writer, shewing the degree of difficulty in contracted pelvis to be less when the child passes by a pelvic or footling presentation, than when it presents and passes by the cephalic extremity.

CASE VII.—A patient, with the outlet and brim of the pelvis, both considerably contracted, was delivered of her first child by craniotomy, after being more than forty-eight hours in labour, and extremely exhausted. "The difficulty," Dr. Lee remarks, "experienced in extracting the head with the crochet, after it was opened, *proved* that delivery could not have been completed by any other method." At her third labour she was again delivered by craniotomy, after the labour had endured for some time, and four fits of convulsion had supervened. "I found her," says Dr. Lee, "completely insensible, with dilated pupils and constant convulsive movements of the muscles of the face. The pains continued with such violence, and recurred at such short intervals, that I dreaded rupture of the uterus. * * * The small size of the pelvis, the impossibility of applying the forceps to the head, the imminent risk of rupture of the uterus, with the result of the former labour, were the circumstances which made me to determine to open the head" At her fourth confinement, the child's head was again perforated. Labour had commenced on the 2nd of December; the liquor amnii escaped soon after; the pains continued strong and regular during the whole night. On the morning of the 8th, (six days after labour "had commenced,") the head was still wedged in the brim, the abdomen tense, the patient occasionally delirious, &c., craniotomy was performed. The patient was thus delivered by embryulcio in her first, third, and fourth labours. The history of her second confinement is this,—“I proposed to induce premature labour on the 21st of July, 1835, when she was seven months and a half pregnant; but she would not consent to this. Labour came on spontaneously at the commencement of

the ninth month of pregnancy; a foot presented, and the child was extracted dead, without craniotomy.”*

These, I repeat, and similar instances, (and I shall have occasion to quote cases still more striking in the sequel,) appear to me to point evidently to this probable conclusion,—that in particular forms and instances at least, of distorted pelves, the passage of the child by the feet or pelvic extremity, affords to it and the mother some special facility of transit which is wanting when the head or cephalic extremity forms the presenting part. And we have thus placed before us this problem,—upon what does this greater facility and safety of footling, as compared with head presentations, in such cases, depend? Let us proceed to the consideration of this point, and attempt to ascertain how this result is obtained.

* Clinical Midwifery, Case 81, p. 39.

(To be continued.)

THE HISTORY OF AN EPIDEMIC CONTINUED FEVER,

AS IT OCCURRED IN THE PARISH OF GREAT FINBOROUGH, SUFFOLK, IN THE AUTUMN OF 1846; RELATED MORE PARTICULARLY AS ILLUSTRATIVE OF THE DOCTRINE OF CONTAGION.

By C. R. BREE, M.R.C.S., Stowmarket.

It is not my intention to write a treatise upon fever; or to frighten my brother associates with a lengthened disquisition upon disputed points, connected with what Dr. Graves has termed, the most intensely interesting of all subjects connected with medical science. My object in the following remarks is to state broadly, clearly, and I trust concisely, some of what I think will be found not uninteresting facts, connected with the all-absorbing subject of continued fever.

The parish of Great Finborough is situated upon rather high ground, three miles in a south-westerly direction from Stowmarket, on the road to Hadleigh. Like almost every other rural village in the county of Suffolk, it is distinguished by stagnant ditches, neglected horse-ponds, and ill-arranged cess-pools, which are more or less to be discovered surrounding the thin, cold, damp, confined, lath-and-plaster-built cottages, which are allotted as residences to our labouring poor. Its geological characters are simple and easily described. Fifty, sixty, or seventy feet of alternating blue and yellow diluvial clay, or drift as it is termed, with the chalk formation at the bottom, and from four to ten feet of heavy loamy soil at the top.

I will not say that any of the above points had anything to do with my first case of fever, as I was not able to trace it to contagion. It will be found that with most of the other cases there was no difficulty in settling this point. For convenience, I have drawn a rough diagram of the parish, numbering the infected houses corresponding to the cases in the order in which they occurred.

* "Clinical Midwifery," p. 62.

The first case which occurred in the parish was on the 25th of August, 1846, at least, this is the date of my first visit. As the symptoms in all the cases were as similar as could be expected from the same disease affecting different individuals of various ages, I will content myself with briefly describing this case, which will do as a type of the rest. *Ex uno disce omnes.*

CASE 1.—A. Garrad, the wife of a labouring man residing in the cottage marked below, No. 1, sent for me on the 25th of August, 1846. She had, I found, been *drooping* as it is emphatically termed, for several days; a general sense of debility, weariness, pain in her bones, loss of appetite. To these symptoms were added when I saw her; severe pain in her head; dry tongue, furred in the centre, with the edges, and small islets here and there, dry, smooth, and perfectly clear; a quick pulse, 120, small and irritable; flushed cheeks, and a hot dry surface; abdominal tenderness, and diarrhoea; urine dark coloured, depositing a thick dusky-coloured sediment.

These symptoms gradually increased in intensity, but they were all present when I first saw my patient. In three days, however, the headache had gone on to delirium, and on the 31st my notes record—violent delirium; great depression marked by loaded tongue; teeth and gums covered with sordes; picking of the bed-clothes; cold extremities; quick irritable pulse; in fact, a tendency to death by asthenia. But there were no spots or vibices, or discharge of blood from the bowels either in this or any of the other cases.

Up to this period my treatment had been, cold to the head, and sponging the surface with vinegar and water; the daily exhibition of an enema of a cupful of starch, containing half an ounce of the syrup of poppy, (which will generally control the diarrhoea;) mustard poultices daily to the ankles; and the exhibition of ten grains of the Pulv. Sodæ Comp. of Guy's Hospital every six hours, with a draught containing ten grains of carbonate of soda, and three of carbonate of ammonia, in an ounce of water, every three hours.

On the 31st, however, it was quite clear that the patient was in great danger, and would die, unless I could *keep her alive*; I therefore discontinued the cold and the medicine, and having procured a bottle of wine, I directed the nurse to give my patient a wine-glassful every three hours, or oftener, according to circumstances: my directions being, "the more delirium, the more wine." These directions were well followed up, and the best part of the bottle was gone next morning. The patient was still very ill, but we had established a footing, and our enemy was half vanquished. There was more warmth in the extremities; the pulse was firmer and fuller, though still ranging from 120 to 130; and there was a little moisture on the skin. I kept up this advantage. On September 5th my patient had taken three bottles of wine, and was safe.

I need not describe the process of amendment,—the gradual abatement of delirium, replaced by sleep, the increased action from the skin, the lowering of the pulse, and the gradual cleaning of the tongue,—these are familiar features, and pleasing ones, too, in the diary and the retrospect of all practical men.

But my patient had a long convalescence; although after the 5th of September she was safe from the imminent danger which had threatened her, and from which she had been snatched by the all-powerful agency of port-wine, she had yet to encounter the effects of disease. Emaciation, great debility, irritability of mind, are, I find, the prominent features of my notes of September 11th, and it was some weeks before nourishing diet and suitable tonic remedial agents had restored her to health.

CASE 2.—On the 21st of September, Susan Pegg, the woman who had nursed the above case, told me during my visit that she felt ill. I detected the premonitory symptoms of fever, and sent her home.

By a reference to the diagram it will be seen, that there is a remote cluster of cottages, to which,—by way, I presume, of special honour,—the name of "city" has been given. The city, as I know, from many years' experience, is situated in the winding of a miserable lane, in an isolated corner of the parish. It consists of five tenements, duly marked and numbered in the diagram. From time immemorial it has been celebrated for its open undisguised dirt. Deep ditches filled with stagnant water, open privies, pigsties abutting upon the road, and half obstructing the view of five miserably-built cottages, are the salient objects in this dreary and melancholy picture. And yet it is not with these,—viz., pigsties, ditches, and filth, that I am now about to find fault, or accuse of any participation in what I am going to relate. It was here,—to the cottage marked No. 2,—that Susan Pegg, the nurse of Mrs. Garrad, was brought, and she went through the disease with nearly the same symptoms and intensity as No. 1. The treatment was the same, with one exception,—i.e., I began the wine earlier, on the fourth, instead of the seventh day. She recovered, and her convalescence was more rapid than in the former case.

As usual, in the country, there was a great outcry about "the fever," and no one would go near poor Mrs. Pegg, the mother of eight children, except the wife of John Threadgill, who lives in the cottage marked No. 3. The cases of fever that followed Susan Pegg, in this locality, with the result, were as follows:—

October 28th.

CASE 3. Jemima Pegg, aged 12. } Children of No. 2.
4. John Pegg, aged 4. } All recovered.
5. Rebecca Pegg, aged 2. }

October 31st.

6. Emily Pegg, aged 11. } Child of No. 2.
Recovered.

November 2nd.

7. John Threadgill's wife. } Nurse of No. 2.
Recovered.

November 6th.

8. Susan Pegg, aged 14. } Children of No. 2.
9. Ruth Pegg, aged 5. } Recovered.

November 19th.

CASE 10.—Ann Threadgill, the daughter of No. 7, a girl aged 18, left her place at Stowmarket, to nurse

two families; one by W. Roper, his wife, and seven children; the other by John Sparrow, his wife, and two children.

From the commencement of the fever in Pegg's house, they had kept quite close, declining all communication with the infected premises, and had hitherto escaped. It struck me that if I could remove the two healthy families, and isolate them, I should, even if they did not escape, destroy the focus of contagion, and thus probably cut off the disease. I therefore waited upon the Board of Guardians, and laid the case before them. There was some difficulty upon the subject, which was, however, eventually removed. The Union Fever Hospital, which is a detached building, was given up to them, and after some persuasion, the Ropers and Sparrows departed from the "city," which they left with all the regret of long attachment, and on the 23rd of November, took up their residence in the Fever Hospital of the Union, which is about three miles from the "city." The men were allowed to go to their work as usual. I confess that I felt interested in this experiment, and made frequent enquiries after the emigrants. I had no fresh case, and "all well" was the answer week after week which I received. I began to congratulate myself that there was an end of the epidemic, but I was disappointed.

Thirty-one days after their removal to the Fever Hospital, one of the sons of Roper was attacked with fever, and was followed by five other children in the course of the week; and on the 31st of December, one of the children of Sparrow, making then seven out of the thirteen. These cases fell under the care of another medical man, and all recovered. Their ages were 2, 4, 6, 7, 8, 10, 12, and they will number in my list, Cases 14 to 20, inclusive. They remained three months in the hospital.

January 17th.

CASE 21.—On the 17th of January, 1847, fifty-five days after the Ropers had been removed to the Hospital, Samuel Gardener, residing at the cottage marked 21 in the diagram, was attacked with fever, had it very severely, having been reduced to the very last extremity, but ultimately recovered. *Samuel Gardener had worked with the Ropers daily before their removal to the Union, and this is all the connection I could trace. None of the neighbours of Gardener had the disease, and this is the last case which occurred.*

I have thus given a brief description of every case—the first and the last—in this, which I may fairly call a distinct (though limited,) epidemic, commencing on the 25th of August, 1846, and ending on the 17th of January, 1847; lasting five months, arising without any traceable cause, and communicated from individual to individual, in eighteen certain and clear, and three other more doubtful, instances. It was confined to eight families, of whom it attacked in the case of—

Garrad	1	out of a family of 4 who recovered
Pegg	8	" " 11 of whom all recovered
Threadgill	2	" " 3 of whom one died
Roper	6	" " 9 of whom all recovered
Sparrow	1	" " 4 who recovered
Steggall	1	" " 5 who recovered
Davy	1	" " 7 who recovered
Gardener	1	" " 5 who recovered

There are several points in the history of the epidemic which are extremely interesting:—

First. The effect of contagion,—i. e., of a morbid poison, produced in, and exhaled from the human body, causing the same disease in others by direct communication, is distinctly clear.

Secondly. The fact observed in all fevers, and well-established in regard to many other diseases,—viz., that a certain state of the system, at present unknown and undefined, is essential to the reception of this contagious principle, was well shown in the epidemic I have described. In eight families, the members of which were in daily, and some of them in hourly communication with the party infected with this highly contagious fever, twenty-seven out of forty-eight escaped.

Thirdly.—The interesting fact of the poison of fever remaining dormant in the system for a fortnight (Case 11,) one month (Roper and Sparrow,) or even two months, (Gardener,) causing no alteration in the health, until the state of the system was favourable to the development of fever, is clearly and unanswerably demonstrated in the facts I have related.

Fourthly.—The probability of contagion being effected through the medium of a third party, who himself never has fever, is strongly indicated in Case 13.

I could extend these observations, but I have already occupied space enough, and I am anxious to avoid detracting from any value which may attach itself to the facts I have related, by being tempted into the fascination of theoretical speculation.

One word more, however, upon the all-important subject of treatment. Several (I think about nine,) years ago, I had nearly the whole management of an epidemic similar to that which I have related, and occurring in the same parish. In this case the symptoms commenced with a type which appeared sufficiently inflammatory to warrant the abstraction of blood, which in three or four instances was done by myself, and one or two by other practitioners; but every case in which blood was taken, either generally or locally, died. In fifty-four cases there were ten deaths, or about one in five. In the epidemic I have just related, in twenty-one cases there was only one death—one in twenty-one—a difference very great. Of late years I have used the lancet very sparingly indeed in the treatment of all diseases, except those purely and acutely inflammatory; but I have never taken a drop of blood from a patient labouring under

continued fever, and I have had no reason to regret the omission.

In the instances above related, I kept two indications before me:—

1st. To obviate *the effects* of local congestion, of what I believe to be in fever *altered blood*; *which effects*, again, I believe we see in or *about* the capillary system, or ulceration of Peyer's glands, &c.; and,

2nd. To prevent my patients from dying by asthenia; to keep them, in fact, alive.

The first indication I attempted to fulfil by the use of the Pulvis Sodæ Comp. of Guy's Hospital, a most useful medicine in these cases. It is composed of carbonate of soda, compound chalk powder, and calomel; sixteen grains contain one grain of calomel. Of this I gave from five to twelve grains every four hours, with or without a solution of the carbonates of soda and ammonia.

The second indication I endeavoured to fulfil by the administration of port-wine, with or without brandy, in large quantities.

In the cases I have detailed I had frequently abdominal and thoracic complications; in one case (11,) there was decided pneumonia, but I did not on this account omit the wine, though I pushed on the mercury. I was equally regardless of delirium, which is, probably, always an effect of innervation in these cases, or of dry tongue. Whenever I found unequivocal indications of debility, as evidenced by a quick, thready, irritable pulse; *trembling*; sordes about the mouth and teeth, &c., I invariably gave wine. The result of the cases I have related fully bears out the propriety of the practice. Had I been able to get more wine down the throat of poor Ann Threadgill, (No. 10,) I believe that I should have had no fatal case to record.

The doctrine of contagion in fever is all-important in these days of sanitary reform; every addition to our knowledge, therefore, upon the subject, is valuable. Dr. Watson,* in his usual masterly manner, has brought much evidence and reasoning to bear upon the subject. He believes that malaria or miasmata produce a disease which is not contagious, and that continued fever, and the exanthemata are never caused by malaria, but are invariably produced by contagion,—*i.e.*, by an animal poison arising in the individual, and propagated from one person to another, either by actual contact, or by exhalations from the lungs or surface of the body. But this argument, it is clear, must in itself extend to animal excretions, and what is undoubtedly true of the body, may thus, *mutatis mutandis*, be equally applicable to the dunghill or the neglected privy. Dr. Watson says, having established the proof of contagion, it is difficult to imagine any other way of propagation. Granting that the difficulty is great as a mere mental effort, with respect to

the malaria of ague producing continued fever, it vanishes altogether when brought to bear upon putrid animal excreta, and this perhaps is the most natural theory we can devise to account for those cases in which no contagion can be traced.

Stowmarket, November 24, 1847.

SHORT NOTES OF THE OPINIONS AND PRACTICE OF THE LATE JOHN PEARSON, ESQ., F.R.S., ON SYPHILITIC DISEASE.

By W. S. OKE, M.D., Physician to the Royal South Hants Infirmary.

(Continued from page 654.)

When chancre is associated with phymosis, it sometimes ulcerates backwards as far as the arch of the pubes; at others it penetrates laterally into the canal of the urethra, causing the urine to communicate with the sore, and producing excruciating pain.

So long as the urine is imperfectly discharged, the ulceration of the integuments will continue to extend, and the pus and urine pass out together through one or more openings.

Sometimes in *complete* phymosis, ulceration takes place through the prepuce at the point where the corpora cavernosa join the glans, and the urine, having no other outlet, will be forced upwards at a right angle with the urethra, producing the greatest possible annoyance to the patient. If the chancre, having perforated the urethra, has formed a sinus, making its way towards the scrotum, the urine will escape into the cellular membrane, and produce great pain and inflammation, and in a few instances affect the testicle. When the sinus is narrow, and the aperture small, the inflammation will extend, and form a small hard tumour near the ossa pubis, the superincumbent integuments not always being involved. In these cases the urine will be expelled by squirts; and if the tumour be neglected, the sinus will proceed downwards in the course of the urethra, and produce fistula in perineo. In another instance, when the glans has been destroyed, the ulceration will extend along the corpus spongiosum, and cause the greater part of it to slough away, whilst the integuments may remain unaffected; but the sound skin will so contract upon the space left by the sloughing of the urethra, that the urine will be expelled in a stream not larger than a hair, and a bulbous enlargement will be found behind the point of contraction, by every effort to discharge it. There is generally at the same time thickening and induration of the prepuce, with a contraction of its aperture. Should the ulceration include the common integuments, these, as well as the corpus spongiosum, will be destroyed, and the urine will be discharged through a small aperture near the pubes, at the anterior part of the scrotum, whilst the corpora cavernosa might probably remain entire.

* Lectures, Vol. I., "Malaria;" and Vol. II., "Contagious fever."