The Sanitary Board persisted in carrying out their intention, with a trifling modification. On Friday, March 20th, a high tide in the Thames blocked up the mouth of the sewer, and the compressed gases forced an opening through the ventilator. The rooms of the school facing the road were filled with foul-smelling sewer-air. Next morning a boy sleeping in one of these rooms was taken seriously ill with pneumonia; on the evening of the same day two other boys and two servants became similarly affected. One of the servants ultimately died. Mr. Waterfield at once broke up his school for the time being.

It will thus be seen that Sir W. Jenner "forecast" the outbreak of pneumonia some days before it actually occurred.

Apologising for the length of this letter,

I remain Sir, your obedient servant,

JOHN WILLIAM MOORE, M.D.

Dublin, September 9th, 1878.

PIEDRA.

To the Editor of THE LANCET.

SIR,—I think that your correspondents who are interested in the subject of "Piedra" would do well to turn to the following references before putting pen to paper—viz.: Beigel on the Hair (Renshaw). Beigel published and figured an account of the disease, or something like it, different from that on the chignon fungus, in 1855, I think. (I am away from home, and cannot give the exact reference.) Kaposi. *Hebra*, vol. iii., under the term *Trichorexis nodosa*. Wilson (Lectures on Dermatology, 1871, p. 125), who calls it *Trichoclasis* or *Trichochlasia*. Duhring's work on Skin Diseases, under the head of *Fragilitas crinium*. Devergie's article on *Tricoptilosis*, in the *Annales de Dermat. et Syph.* for 1871, I think. Luigi Billi (of Florence), article in the *Giornale Italiano delle Malattie Venerice delle Malattie della Pelle*, Aug. 1872. And the second edition of my own work referred to by Dr. Hoggan in THE LANCET of Sept. 7th.

The exact nature of the disease, however, is, without doubt, a fair subject for inquiry, and cannot be said to have been satisfactorily established.

Yours faithfully,

TILBURY FOX.

THE RHEUMATIC ORIGIN OF CHOREA.

To the Editor of THE LANCET.

SIR,—In answer to Dr. Goodhart, I may say that in my remarks in THE LANCET of the 31st August upon some cases of chorea, the personal histories only are given, because the numbers dealt with were not large enough, as I thought, to furnish any reliable conclusion as to the frequency of rheumatism amongst the relatives of the patients. We have no knowledge as yet, or at least no common agreement, as to the number of rheumatic relations which may be reckoned to each individual as his fair share. Perfect health should properly have no such connexion, but perfect health is only an expression. Amongst thirty-two choreic children Dr. Goodhart found rheumatic relatives for more than half. He admits, however, that "the relationship was not always so near as he could wish." It is obvious, indeed, that unless the degree of kindred be very rigidly defined, we may find such relatives in almost every instance. Again, the family records of hospital patients are very imperfect and unequal, and their descriptions almost always inexact. The rich and the poor, the educated and the ignorant, must all be taken separately. Moreover, it is so difficult to recognise rheumatism afterwards from mere description, that a dangerous choice always remains with the observer to accept the evidence or not, according to his bias. I do not say that these difficulties are insuperable; they are certainly too much for my present material.

But supposing it made out for certain that choreic children are hardly more rheumatic than others (and Dr. Goodhart bears me out upon this point), I venture to think that a strong presumption is raised against any direct association between the two affections. For, surely, amongst the liabilities which children incur from being of rheumatic stock, a predisposition to rheumatism itself must be reckoned? The "rheumatic diathesis" (or any other),

however multiform in its mode of descent, requires at least as much as this in proof of the reality of the transmission. My friend, Dr. Barlow, has suggested to me that, for reasons he mentions, the rheumatism of childhood may, he believes, be often overlooked. This, however, is not Dr. Goodhart's contention. He denies that rheumatism is a direct and immediate cause of chorea, and in so doing, as it seems to me, puts in jeopardy the whole doctrine.

But, however this may be, a grand point is gained if it can be shown that choreic patients themselves are not and do not become the subjects of acute rheumatism in much larger proportion than other children. Two theories of two schools would be seriously threatened: the hypothesis which ascribes chorea to a mysterious poison which is partly spent upon the joints, yet still lingers in the blood, ready for mischief, and capable of this amongst other manifestations; and the hypothesis which asserts a purely mechanical origin for chorea in the conveyance of material from the heart to the brain.

Your obedient servant,

Wimpole-street, 13th Sept.

OCTAVIUS STURGES.

THE VIOLET POWDER POISONING CASES.

To the Editor of THE LANCET.

SIR,—Having been to some extent concerned in the above I should like to say a few words about them. I had hoped that Mr. Lewis, of Loughton, and Dr. Fowler, of Epping, who saw most of the cases, and bore the brunt of the preliminary inquiry, would have reported their experience.

Dr. Tidy, in his article in THE LANCET of August 24th, says that the credit of showing that the violet powder was the cause of the mischief was due to Mr. Power, of the Local Government Board. Mr. Power made, I believe, every investigation into the cause of the supposed epidemic in Loughton and neighbourhood, but it was through Mr. Lewis, and a private patient of his (Mr. Deacon), that the powder was first forwarded to Dr. Jones (Evans and Jones), analyst, of Leadenhall-street, and it was on Dr. Jones's report that the powder contained arsenic that further proceedings were taken.

Two cases in Loughton came under my notice. In the first, a child a few weeks old, I found deep sloughing in the perineum, scrotum, and folds of the neck; sickness, diarrhoea, and retention of urine for many hours. Until I saw the sloughs in the neck, I thought that extravasation of urine was the cause of the mischief, but after seeing these I passed a probe a short distance down the urethra, and this was followed by a copious flow of urine. In the second case there was sloughing in the groin, perineum, about the sacrum, under the armpits, in the folds of the neck, and behind the ears; an erysipelatous redness over the abdomen, as high as the umbilicus, with great tenderness; vomiting and purging. The sloughing in this case was again puzzling, as without it I should have put the symptoms down to infantile cholera. In both these children the sloughs were first treated with poultices, followed on their separation by carbolic oil, and both recovered, probably owing to the fact that they were of more mature age when the powder was first applied than were the victims.

In the late case of *Regina v. King*, only one indictment was preferred, and that a recent one, so that very little medical evidence was forthcoming. Dr. Tidy was asked by the counsel for the defence "whether arsenic was a preservative, and, if so, how he could account for the great decomposition of the body in the case in question?" His reply was to the same effect as part of his article in your columns. May there not be some explanation for decomposition occurring rapidly in this case, notwithstanding the presence of arsenic? Sloughing I take to be a solution of continuity in the same sense as a wound, and of the post-mortem appearance of wounds Dr. Taylor says: "Putrefaction always commences sooner in parts which are wounded than in those which are uninjured." Dr. Guy says: "Putrefaction commences soon, and runs a rapid course, in *inflamed parts*, in bruises, and at the edges of wounds." Now, I should like to ask our leading pathologists whether they find on the post-mortem table that those who have died from extensive sloughing, phlegmonous erysipelas, or gangrene, are more decomposed generally, when placed on the table, than others, and whether in such cases they would expect the ante-mortem presence of arsenic in the system to retard putrefaction?

Mr. Henry Browne, in his evidence at the Central Criminal Court stated that the liver was enlarged in the case referred to. I, and other medical men present, understood him to state this as an evidence of death from arsenic. I am not aware that this is the case, and were it so it would be interesting; but I think that in an infant, where poisoning commenced immediately after birth, the liver would probably remain in nearly a foetal condition from arrest of development.

I regret that the vague term "exhaustion" should have been stated as the cause of death, when "arsenical poisoning" would have been much more to the point. The counsel for the defence pounced on the word "exhaustion," and cross-examined upon it as much as he would have done had he been told the child died from "want of breath."

One of our most eminent analysts (not a medical man), engaged in the case, but not called, told me that when the elimination of arsenic was taking place he should expect to find the last traces in the urine, and equally, in the dead body, in the kidney, and not in the liver. Is this in accordance with the opinion of others?

The importance of elucidating any new points in forensic medicine must be my apology for troubling you at such length.

I am, Sir, yours faithfully,

A. M. ROBERTS, L.K. & Q.C.P.I., &c.

Buckhurst-hill, Essex, Sept. 10th, 1878.

WESTPHAL'S TEST FOR LOCOMOTOR ATAXIA.

To the Editor of THE LANCET.

SIR,—With reference to Dr. Buzzard's answer to my letter of August 17th, in which I opposed the idea of the presence of the normal patellar reflex being considered incompatible with a true diagnosis of posterior sclerosis, as was stated by him in his lecture in THE LANCET, July 27th, I must decline to accept his refutation of my case being one of locomotor ataxia on the grounds he states—viz., absence of ophthalmic symptoms and pain. These, I think, I can prove to be insufficient data on which to disagree with me in my diagnosis. First, then, as regards ophthalmic symptoms, I cannot find an author who states that they are *always* present, and, indeed, Trousseau, in his "Clinical Medicine" (vol. i., p. 156), states that he has several times noticed these affections to be absent; and, secondly, so far as the presence of pain is concerned, Dr. Buzzard, on referring once more to my letter, will see that the patient *had had pain*, though he was free from it at the date my letter was published; but even granting that he *never* had had any pain, either rheumatic, lightning, or otherwise, it is not a universal condition by any means, as *vide* Trousseau, vol. i., p. 147.

Taking the subject as a whole, upon what symptoms are we to base our diagnosis of this disease? This seems a matter difficult of solution if we have to follow the various whims and fancies of our recognised teachers and authors. Now, on what minimum aggregate group of symptoms are we to be allowed to style a case as one of "locomotor ataxia"? And this question, I think, is an important one in a disease devoid of physical signs, and in which the symptomatology is our only guide. Surely there must be some symptoms, however few, which indicate unequivocally the presence of this affection, notwithstanding the scientific hair-splitting and passing fancies of our lecturers, else we can logically come to no other conclusion than that there does exist a nervous lesion of common occurrence without any appreciable expression of symptoms at all.

W. BEATTIE SMITH, L.R.C.P., L.R.C.S.

Stockton-on-Tees, September 16th, 1878.

PHYSICIANS' FEES.

To the Editor of THE LANCET.

SIR,—The general practitioners can do much to help forward the movement for improved remuneration for professional services by selecting for consultation those *only* who take the higher fee of £2 2s. for seeing a patient, and £3 3s. for consultation. When compared with the fees received by the leaders of the legal profession, and the profits earned by tradespeople, the amount seems reasonable.

On the other hand, the heads of the profession can greatly assist the estimation and remuneration of the general practitioners by insisting on payment for hospital work. Why the wealthy public should pay only for the food, lodging, and nursing of their sick poor and not for their medical attendance I fail to see.

The great abuse of indiscriminate advice at our hospitals and dispensaries does much harm by educating the public to undervalue medical attendance, and cultivating the feeling that it is less disgraceful to receive *medical* charity than to accept *money* charity, when not absolutely necessary.

The general practitioner ought to exclude from medical clubs all who do not require to insure a payment (not exceeding £1 a week) in case of sickness, and when the better off join a benefit sick society more for the sake of medical attendance than for the money payment, the practitioner ought to be very stringent in making him adhere to club rules.

With age and experience most of our profession could safely and beneficially increase their fees more than they customarily do; their services are absolutely more valuable, and should be paid for at a higher rate, and the public generally value an article according to the price put on it.

Yours faithfully,

GEO. E. JEAFFRESON.

Sept. 16th, 1878.

To the Editor of THE LANCET.

SIR,—It ought to be known that the subject of physicians' fees, which has lately been before the public and commented on by you, has reference only to the question of payment at the doctor's house; it does not apply to the visiting fee, which appears to me the only one requiring reform. At the present time, those physicians who are demanding their one or two guineas at home, according to circumstances, have no hesitation in taking one guinea for an out-door visit. This is very remarkable, seeing that they have only to follow the sound principle determined in matters of business to rectify the anomaly. The new arrangement of co-operative stores has its origin in the fact that the price of a commodity is much enhanced when the tradesman has to wait upon the purchaser; and in the same way there can be no doubt that the time occupied in going to and from a patient's house, and maintaining a carriage for the purpose, should bring its equivalent remuneration. Yet at the present time rich or titled persons who are too proud or indolent to take their turn in the doctor's waiting-room, have only to send for the doctor to see them at their own convenience, and hand him a guinea (or, rather, a sovereign), and the latter is content. It has caused much annoyance to many younger men in the profession, when, under these circumstances, they have naturally asked for two guineas, to be informed that the leaders in the profession have been content with one guinea.

I say that the question which has been under discussion in reference to the fee at the doctor's house is unimportant in comparison with this evidently just increase of two guineas for out-door visits. If physicians would never take less than two guineas for a visit to a patient's house, the most important reform in the matter of fees would be established.

I am, Sir, yours, &c.,

September 16th, 1878.

X.

A TUMOUR OF OBSCURE NATURE.

To the Editor of THE LANCET.

SIR,—Cases like the following are exceedingly rare, and therefore I will thank you to find room for it in your journal:—

W. B—, a bricklayer, aged sixty-one, two years since began to suffer from pain in the epigastrium after taking food, and for the last nine months from frequent vomiting, though not every day. Solid food always gave him great pain. He had also had double inguinal hernia for several years, but easily reduced. I had attended him several times for these symptoms, but he had not received much benefit from any treatment, and had looked thin, pale, and anæmic during the latter part of his illness.

On December 20th, 1877, he felt sudden pain in the left side (probably in left groin) after defecation, and observed a swelling above the left testicle, which he thought had been produced by straining. The swelling was thought to