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 Venerie 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?