You state that “the drainage of the whole place is defective.” Now, so far as Hove is concerned, this statement is distinctly challenged as being not only misleading, but positively untrue. May I be permitted to ask, by what means you ascertain that “the drainage of the whole place is defective”? So far as can be ascertained, the drainage of Hove has never been examined by any commissioner on your behalf, and therefore it would be interesting to those having charge of the public drains, to know what foundation there is for such a sweeping assertion. The Hove sewers are laid at such angles, and have placed at each charge of gradient and direction an inspection shaft fitted with a ladder, so that examination is easy and can be made at any time. I should therefore be glad if you would do now what you should have done previous to condemning a system of drains about which you know absolutely nothing—viz., send down a competent and impartial authority to judge whether or not the public drains are “defective.”

With regard to the ventilation of the sewers, it is effected by open gratings placed at frequent intervals in the centre of the road—at present, in my judgment, the best known practical means of effecting this object—for it would be impossible to give reasons in a communication of this length, but they are set forth fully in a paper on the subject read by me before the Association of Municipal Engineers at Liverpool three years ago. Undoubtedly smells arise from open ventilators, which render them disagreeable and prejudice the inhabitants against them. As to the efficacy or otherwise of this mode of ventilating sewers, I would simply remark: (1) that along the route of many miles of sewers so ventilated in Hove, during the past six years there has only been one authenticated case of death from typhoid fever; (2) that in the street where the death from blood-poisoning occurred *the sewer was not ventilated by open ventilators*, being, in fact, a sewer belonging to a private individual. It had not been constructed by, nor was it maintained by, the public authorities. It has since been cleansed and ventilated by large open shafts. I should be happy to offer your representative every facility for personally inspecting the sewers, and in conclusion would draw your attention to the fact that, notwithstanding your statement that “the place is a very hot bed of pestilent sewer gas,” and that “perpetual peril remains,” the death-rate of Hove for the past six years has averaged less than 15.0 per 1000.—I remain, Sir, yours, &c.,

E. B. ELLICE-CLARK, Assoc. M.I.C.E.,
Engineer to the Hove Commissioners.

April 4th, 1882.

ACONITIA.

To the Editor of THE LANCET.

SIR,—It has been so frequently stated during the last week or two that there is only one case of poisoning by aconitia on record, that it may be as well to mention that in THE LANCET of November 13th, 1880, four cases are referred to. Professor Plugge of Gröningen, in his paper read before the International Medical Congress, “Sur l'Activité léthale des différentes Espèces d'Aconitine,” mentions a case recently under the care of Professor Huisinga and himself. In Bouchardat's “Annuaire de Thérapeutique,” 1881, M. Desnos records the case of a gentleman who was nearly killed by the substitution of one kind of aconitine for another. In Gubler's “Leçons de Thérapeutique” an excellent account is given of the clinical uses of aconitia; and Oulmont, in his paper “De l'Aconit, de ses Préparations et de l'Aconitine considérés au point de vue thérapeutique” (an abstract of which appears in the *Progrès Médical* for 1879, p. 968), calls attention to its value for the relief of the pain of paraplegia.—I am, Sir, yours faithfully,

Weymouth-street, W.

WILLIAM MURRELL.

“FOREIGN BODY IN THE ŒSOPHAGUS FOLLOWED BY SURGICAL EMPHYSEMA.”

To the Editor of THE LANCET.

SIR,—In your issue of April 8th is the report of a case under the above heading, in which the explanation of the production of the emphysema, given by Mr. Morgan, seems to me not altogether satisfactory. Mr. Morgan gives undue prominence to the hypothesis that the air found its way into the neck through some tracheal rent, not mentioning any other route by which it might have travelled. Under

pressure a weaker structure gives way before a stronger. Is it not more probable that the breach occurred in some small or terminal bronchus rather than in the trachea itself? If such were the case, the air would track along the connective tissue sheath of the bronchus till it reached the root of the lung, then, diffusing itself through the mediastina, pass up out of the thorax into the neck. The method of its production would be the same as in the emphysema of the neck and trunk, which occasionally takes place in young children suffering from whooping-cough, whose lungs are the seat of interlobular emphysema, only, of course, in this case being that of an adult, the absence of all interlobular tissue excludes alveolar rupture as a cause. This explanation suggests itself to me from what I saw in a patient who died from hydrophobia, in whom, violent expiratory efforts taking place during spasmodic closure of the glottis, extensive emphysema of the neck, face, and upper part of the thorax supervened. In my case there was no apparent rent in the trachea, or in any of the larger bronchi, and the only conclusion arrived at was that some terminal bronchus had given way, the emphysema being caused as above stated. In Mr. Morgan's case the foreign body by its presence would likely enough set up some spasm of the glottis, provoking at the same time powerful expiratory efforts in the form of cough. Again, by its actual bulk it may have compressed to some extent the trachea, especially as the cartilages are imperfect posteriorly, and so lessening the calibre of the windpipe, would increase the intra-pulmonary pressure. On the whole it seems to me much more probable that the thinner bronchus would give way before the stouter trachea, the pressure on both being much the same. I offer this explanation, as Mr. Morgan does not seem quite satisfied with his own, and because to me it appears more reasonable than that already given.—I am, Sir, yours truly,

W. T. WYATT, M.A., M.B. Oxon.

Stamford-hill, April, 1882.

“MODIFIED LISTERISM.”

To the Editor of THE LANCET.

SIR,—Mr. Turton, of Heckmondwike, in his letter in your issue of the 1st inst., recommends the use of carbolic oil, strength 1 in 20, with a covering of carbolic tow as a modification of Listerism, and as an efficient wound dressing.

As an old dresser of the late Mr. Callender, I wish to state that the use of any other dressing in his wards was exceptional. A piece of gutta-percha tissue was usually placed between the oiled lint and the tow or “marine lint.” I desire not so much to point out the claim of the above most eminent surgeon to be the greatest exponent of this system of dressing wounds—as that will readily be allowed by all old Bartholomew's men, at least of the last ten years, as well as by most well-informed surgeons—as to draw attention to the fact, which he was always anxious to impress on his pupils as well by his practice as his words, that carbolic oil becomes all but inert as an antiseptic dressing after being kept a few hours. His method was to have carbolic acid and olive oil, side by side in the ward, to be mixed in their right proportion (in his wards about 1 to 12), a few minutes only before the dressing was applied. His cases almost always did well, as his statistics in the hospital reports will show.

I think the disuse of carbolic oil dressing in hospitals is mainly due to the fact that the mixture is sent up to the wards in large bottles from the dispensary, and kept there till used up, sometimes for weeks or even months. If any person will compare the appearance and smell, especially the latter, of a freshly made sample, with those of one that has been kept for a month or more, he will at once be aware of a great difference; and if he will further make a trial, by treating similar cases with the two sorts, he will be convinced of the deterioration of the mixture in antiseptic qualities after being kept.

Mr. Callender was fully persuaded that a change in composition took place in the carbolic oil in a comparatively short time, though I have heard him state that he was not able to explain it on chemical grounds. I may add, that I have mentioned the facts to more than one noted chemist without being able to obtain any suggestion as to the decomposition or reaction, if any, which takes place. If any of your readers can supply such a one they will, I am sure, confer a benefit on surgery.

I am not aware that this point has before been discussed. If