Correspondence.

"Audi alteram partem."

ERYTHROPHLOEINE.

To the Editors of THE LANCET.

SIRS,—In last week's issue of THE LANCET (Jan. 28th, p. 190) an account is given of erythrophloëine, which is stated, on the testimony of Dr. Lewin of Berlin, to be a new anæsthetic. Will you allow me to state that during the last fortnight I have made many trials with solutions of the hydrochlorate of this drug on the human eye, but have failed to observe any decided anæsthetic effect. The solutions were obtained from Mr. Martindale of New Cavendish-street, and may therefore be assumed to be pure and genuine. My first trials were made with a 0.5 per cent. solution; but this produced very severe pain and irritation, accompanied by ciliary injection lasting from six to forty-eight hours. A 0.25 per cent. solution produced only a degree less pain and irritation, without discoverable anæsthesia. A still weaker solution of 0.125 per cent., or 1 in 800, was also negative as regards anæsthesia. After many trials with a solution of this strength I have been unable to obtain any appreciable anæsthetic effect. The immediate effect is severe smarting, lasting from ten to thirty minutes; and at any time from ten minutes to twelve hours after the application a minute drop of a solution of nitrate of silver or of carbolic acid, applied to the conjunctiva, produced as much smarting in the eye to which the erythrophloëine had been applied as in the other eye. I would even say that in my own case the smarting lasted longer in the eye to which erythrophloëine had been applied. In other respects the effect of the application is very disagreeable; the sight is very much blurred for many hours, so that everything appears as if enveloped in a dense pea-soup fog; and around any artificial light there is a pronounced circular halo, with the red rays outwards. The halo is not associated with increased tension, but is probably due to disturbance of the corneal epithelium, which is obviously dulled. I would add that so far from erythrophloëine producing contraction of the pupil, as alleged in your notice, in all cases I watched there was a wide though not complete dilatation.

In the face of these opposite results, it would perhaps be well to assume that erythrophloëine has not yet been obtained in a simple and definite form, and that there is more than one active principle present in the substance now in the market. Be this as it may, it is certain that, if even a very weak solution be used as a substitute for cocaine for eye operations, disappointment and perhaps something worse may follow.

In conclusion, I may say that Mr. E. T. Collins, the senior house surgeon at the Moorfields Ophthalmic Hospital, has also made independent observations at my suggestion, and has obtained results substantially the same as mine.

I am, Sirs, yours truly,

Harley-street, W., Jan. 31st, 1888.

JOHN TWEEDY.

THE RECENT DISCUSSION AT THE PATHOLOGICAL SOCIETY.

To the Editors of THE LANCET.

SIRS,—I had hoped that the Hendon cow discussion would have been allowed to rest for a while. I must, however, ask you to kindly permit me to reply to Dr. Klein's letter in your issue of Jan. 28th.

Dr. Klein writes, "Dr. Crookshank expressed great indignation at my having exhibited to that meeting 'without his permission' 'his' calf. . . . Now, the fact of the matter is that this calf was not Dr. Crookshank's property. It belonged to Professor Brown, and had been sent along with similar calves by Professor Brown to the animal vaccine station," &c.

The protest which I made was a firm but not an indignant one, and is expressed in your issue of Jan. 21st, p. 122, as follows: "Without being consulted, some of his results had been published, and one of his calves brought down to this meeting, by Dr. Klein. He thought that an observer being allowed to give his version of another investigator's work before it was completed and published, and being allowed to show one of his animals, was an event unparalleled in the

history of the Pathological Society." It was not a question of the actual ownership of the calves. As a matter of fact, they are the property of the Agricultural Department of the Privy Council. But they were entrusted to me for my experiments, and I could therefore surely speak of them for the time being as "mine." It was I myself who, on the morning of the special meeting, arranged that the calf which was to be exhibited should go straight on to the Animal Vaccine Station, and I (not Professor Brown) further arranged for two other calves to follow afterwards from the Royal Veterinary College. I did this under the impression that the experiments to be done were to be jointly done, and I distinctly understood at the time that Mr. Murphy would co-operate with me. Mr. Murphy gave me to understand that he was most anxious to work with me at the cow-pox aspect of the question, and he accepted my invitation to visit the Wiltshire farm and cross-examine the milkers. He returned "greatly impressed" with what he had seen and heard, and even went so far as to say that if it was not vaccinia he did not know what it could be. Yet his co-operation fell through. Why? I attended very regularly at the Animal Vaccine Station, and the first inoculation I made jointly with Dr. Cory; the second inoculation I made jointly with Mr. G. W. Collins, Dr. Cory being away; the third and fourth inoculations were made by Dr. Cory in some forty places for each animal, and in the presence of Professor Brown, Dr. Klein, and myself.

Now, my protest was directed to the fact that although Professor Brown and I met Dr. Klein, and we together inspected the calf on the very morning of the meeting at the Pathological Society, Dr. Klein gave us no intimation of his intention to exhibit it at the meeting. Had this proposal been made to me I should willingly have approved of it, provided that I had shown also the other calf (the first remove from the boy's thumb) which had resisted the test of revaccination. To keep this calf back, and to show only the second remove or pustular calf, which had proved not to be protected, will hardly commend itself to anyone as fair criticism or impartial co-operation for the advancement of science.

The second part of my protest was directed to the fact that Dr. Klein endeavoured to explain away certain experiments of mine which are not yet published, and at which he was not even an onlooker. From some rough notes of these experiments, which I sketched out for the benefit of those interested in looking on at the experiments, Dr. Klein presented to the Society a pedigree with certain comments, which was printed in leaflet form and distributed by an attendant to the members of the Pathological Society. I am surely justified in taking exception to this when I have promised that all these experiments will in due time be published in full.

In conclusion, the subject of cow-pox has been pressed to the front, but it must be remembered that the main question which is at present before the profession is whether a certain vesicular disease of the teats of cows at Hendon, a disease communicable from animal to animal by the hand of the milker, was the very disease which we in man call scarlatina. In other words, was the Hendon cow disease a disease new to science, which might be called the Hendon or "scarlatinal" pox? The results of my investigation have forced me to very different conclusions, and these it was my duty to lay before the profession.

I am, Sirs, yours sincerely,

Jan. 30th, 1888.

EDGAR M. CROOKSHANK.

IMMEDIATE CLOSURE OF TRACHEAL WOUNDS.

To the Editors of THE LANCET.

SIRS,—In support of your editorial remark on the advantages of immediate closure by suture of tracheal wounds, I would refer to a case published in THE LANCET, Jan. 2nd, 1886, where the blade of a broken tooth forceps was extracted from the right bronchus. The incision in the trachea was an inch and a half long, and this I carefully closed by suture. The external wound was also closed after drainage tubes had been introduced. I am convinced this proceeding materially contributed to the successful issue of the operation, by shutting out the cold air from the lungs and preventing wound secretions entering the trachea. It will not be always desirable to close the tracheal wound, but in those instances where an operation is performed for the removal of foreign bodies from the air passages, in some