

quite open to Mr. Browne to argue in favour of his views from the published cases of Dr. Newman or any other laryngologist, but he does not advance his argument by misquoting the statements of those whom he criticises. We are not called upon on this occasion to express an opinion on one of the most serious and urgent questions of surgery which has been projected into prominence by historical events and controversies of recent years. If such a correspondence as that we have published settles anything it settles this:—that even from an expert's view there is much difficulty in defining the course of professional duty in any given case, and that in any given case much responsibility will rest on the general advisers of the patient. We cannot devote further space to this correspondence.—ED. L.

"SOME COMMON SOURCES OF ERROR IN TESTING FOR SUGAR IN THE URINE."

To the Editors of THE LANCET.

SIRS,—In the communication from Sir George Johnson contained in your last issue it is asserted that no sugar is present in healthy urine. With reference to this assertion I ask to be permitted to submit to Sir George Johnson the following question: How is the production of the osazone crystals obtainable with the greatest facility from the healthy urine of the human subject and from the urine of the lower animals to be accounted for?

I am, Sirs, yours faithfully,
Grosvenor-street, July 10th, 1894. F. W. PAVY.

"TIC DOULOUREUX AND ELECTRICITY."

To the Editors of THE LANCET.

SIRS,—The letter of Dr. Althaus¹ lends support to the contention that in cases of trigeminal neuralgia surgical interference ought, as a rule, to be withheld until electrical treatment has been duly tried. The long-continued application of very weak continuous currents, as advocated by Dr. Althaus, is a proceeding of recognised value, and had the more rapid expedient failed it would doubtless have been proper in the case quoted² to persevere in the use of other and slower methods. By a sublime effort of faith and patience the application of the fractional part of a milli-ampère of continuous current for an hour a day during a period of three successive weeks might even have been carried out. It is, however, to avoid such a necessity that I venture to suggest a trial of this quicker procedure when the current in question is available and the medical man knows how to handle it. In gauging the relative merits of various methods it is not to be forgotten that a course of treatment extending over a period of three weeks allows to come into play so many other possible factors in the process of cure that it becomes proportionately difficult to distinguish the exact relationship of cause and effect. This can scarcely be mistaken in the case referred to. It is cited as an instance in which the results of a single electrification are certainly not less striking, and possibly not less permanent, than those claimed for one of the most difficult operations known to surgery.

I am, Sirs, yours faithfully,
Brighton, July 10th, 1894. W. S. HEDLEY, M.D.

"THE AERIAL CONVECTION OF SMALL-POX."

To the Editors of THE LANCET.

SIRS,—In an annotation upon the aerial convection of small-pox, which appeared in last week's issue of THE LANCET, you commented upon some views which I, as late Medical Superintendent of the Gore Farm Hospital, had expressed upon the subject in the last report of the Statistical Committee of the Metropolitan Asylums Board. In my report I assigned a limit of 1000 feet to the aerial convection of small-pox because scarlet fever convalescents had been treated at this distance from the small-pox hospital

for a period of six months with complete impunity, and I also stated that no outbreak had occurred to my knowledge amongst the inmates of the Darenth Asylum and Schools, whose grounds adjoin those of the small-pox hospital. The writer of the annotation takes exception to this view on the ground that the patients admitted to the fever hospital were limited to those who had been previously vaccinated. I may be allowed to explain that the vaccination referred to in my report was not of recent date, but, almost without exception, had been performed in infancy. The ages of the patients varied from three years upwards, and a considerable number of them, therefore, would not be efficiently protected, but liable on exposure to contract the disease. When, at the instance of the Small-pox Hospitals Committee, it was decided to limit the admission to the fever hospital to such vaccinated patients, it never occurred to my mind to dispense with other precautions against the conveyance of small-pox infection, and every possible precaution was taken except the revaccination of the fever patients, which we were not allowed to do. The patients, therefore, or at least some of them, were liable in my opinion to catch small-pox, though doubtless the disease would have been very much modified by the previous vaccination. The conditions applicable to the inmates of the fever hospital were not on all fours with those applying to a mixed population of vaccinated and unvaccinated persons, but as touching the question of aerial convection of small-pox I felt justified in concluding from its absence amongst these patients, many of whom in my opinion were vulnerable, and amongst the inmates of the adjoining asylum in perhaps a still greater degree, that the diffusion of small-pox did not extend to this distance.

The observation of the Royal Commission on Small-pox Hospitals, quoted by the writer, as to the "absence of danger in the accumulation of convalescent persons," appears to me to require further elucidation before it can be applied with fairness to the case of Gore Farm. It would not be accurate to infer from the term "convalescent" that the patients were not still in a highly infectious condition when they arrived at the hospital. Indeed, owing to the limited accommodation at the hospital ships, many patients were transferred to Gore Farm a few days after their arrival on board in order to make room for fresh cases, and almost all transferred cases would be suffering from infection in the opinion of the medical superintendent of the hospital ships, or otherwise they would have been discharged by him direct. What is it, then, which constitutes the difference between the infection of the acute and convalescent stages, and brings it to pass that "diffusion does not take place at all except when the cases of small-pox concerned are in the acute stage of the disease?" Those who hold this view are no doubt capable of explaining it, but none of the reasons which suggest themselves to my mind seem quite conclusive; and I beg respectfully to ask, What are the grounds upon which the writer of the annotation bases his opinion?

I am, Sirs, your obedient servant,

C. E. MATTHEWS.

Fountain Hospital, Lower Tooting, July 11th, 1894.

"LARYNGEAL MOTOR ANOMALIES."

To the Editors of THE LANCET.

SIRS,—I trust you will allow me to say a few words in answer to Dr. Semon's letter in THE LANCET of June 23rd. It is perhaps discouraging to learn that Dr. Semon cannot endorse my views, but the reasons he gives for his inability to do so seem to me to be very bad ones. I begin to fear that I have written less clearly than I had hoped. I fear this the more after a prolonged and assiduous study of the sentence which stands second in Dr. Semon's letter. It reads like a charge of plagiarism, but that I know is not meant. I do not pretend to have shown the nature of the laryngeal cortical centres. It is one of the facts which Dr. Semon and I alike have to bear in mind in framing a hypothesis. That it fits in with his I would not affront him by doubting. I dissent from Dr. Semon's hypothesis, not because it is inadequate, but because it assumes too much and is improbable. I venture to state the points on which we are agreed and to indicate that from which we begin to differ.

It is agreed that there is in the abductor system, as compared with the adductor, a certain peculiarity—a delicacy or diminished resistance to injury—and that this belongs at once to the abductor muscle, to its nerve, and to the bulbar nucleus which presides over both. To account for this I

¹ THE LANCET, July 7th, 1894.

² THE LANCET, June 23rd, 1894.