Correspondence.

"Audi alteram partem."

TYPHOID FEVER.

To the Editor of THE LANCET.

SIR,—With reference to the occurrence of cases presenting all the phenomena of typhoid (enteric) fever in other countries, I have received the accompanying communication from Dr. Grabham, of Madeira. I was not previously aware of the occurrence of disease in this form in that island. Possibly other medical officers practising abroad in tropical or sub-tropical climates may be able to confirm the opinion that fevers of climatic origin do occasionally present all the symptoms that characterise typhoid in this country.

I am, Sir, yours truly,

Dec. 4th, 1879.

J. FAYRER.

Madeira, Nov. 16th, 1879.

DEAR SIR JOSEPH FAYRER,—I have read with much pleasure a report of your recent remarks on the etiology of typhoid fever.

In Madeira, where I have had much experience during the last eighteen years, there are present from time to time cases of typhoid whose origin is precisely that which you point out as referable to general and climatic causes. I have long ago convinced myself that it is vain to seek to trace many well-marked cases of pure typhoid to sources of filth contamination; though these latter abound in Funchal, our chief town, and are frequently associated with their own proper instances of fever.

The cases which I speak of as having the more vague etiology which you mention are those which happen almost every year to some extent, and chiefly in seasons unusually bright and sunny, in the persons of young, healthy *new comers*, who roam about to see all our mountain scenery, and succumb to attacks of genuine typhoid fever quite identical with the typhoid fever of filth, except, as you say, in origin. I have again and again seen such cases arise in situations many hundreds of feet above Funchal, far away from all dwellings, and in regions where the cold drinking-water springs from beneath basaltic columns of rock. It is quite certain, that, however they acquire it, nevertheless such patients, equally with those who have imbibed their disease from polluted water, are able to spread their infection to others.

I hope you may think this interesting enough to condone the intrusion.—Yours faithfully,

MICHAEL GRABHAM, M.D., F.R.C.P.

EXCISION OF THE HIP.

To the Editor of THE LANCET.

SIR,—Will you do me the favour of inserting in the forthcoming number of THE LANCET the following brief reply to the annotation on "Excision of the Hip," which appeared in last week's issue?

My cases are forty-five in number; seventeen of these are absolutely "cured," and living at the present time. Twelve of them have promised to appear at the meeting of the Clinical Society on Friday evening next, December 12th; and I confidently hope to be able to show them there.

Six patients died in consequence of, or notwithstanding, the operation. Of this number three died of pyæmia, one of septicæmia, one of exhaustion and thrombosis (the operation coming too late), one of amputation at the hip for suppuration of the knee, following erysipelas of the thigh and knee, ten months and more after operation.

One child died of diphtheria of the air-passages. That death cannot be laid to the charge of excision.

Nine other cases ended fatally, *not* in consequence of the operation, but at the end of some length of time afterwards, varying from two months and a half to four years. Two of these died of tubercular disease of distant parts without waxy changes in the abdominal organs, and seven from waxy changes combined with tubercular disease.

The twelve remaining cases are under observation or treatment at the seaside, at their own homes, or in the

hospital. The most recent case was operated on less than three weeks ago.

This statement shows that not more than six deaths in forty-five cases of excision of the hip are to be attributed to sequelæ of the operation. Experience in selecting cases, and the antiseptic mode of treatment during and after operations, will, I feel confident, enable us to still further diminish the proportion of unsuccessful results.

I hope to enter more fully into the whole subject at the meeting of the Clinical Society on Friday evening.

Nine of those who died from diseases not attributable to operation were at least relieved.

I am, Sir, your obedient servant,

JOHN CROFT.

Brook-street, Grosvenor-square, Dec. 9th, 1879.

NERVE-STRETCHING IN TETANUS.

To the Editor of THE LANCET.

SIR,—Will you allow me to call the attention of the surgical world to an error which, if left alone, may prejudice scientific men against the only procedure that promises much in the management of tetanus. This mistake was made by Mr. Henry Morris in a clinical lecture at the Middlesex Hospital, and has been promulgated through the columns of one of your contemporaries.

A mere statement of the case is all that is necessary to show the false basis on which he founded the "conviction that nerve-stretching in tetanus would not prove a favourable mode of treatment." The lecture was based on four operations. The one which he had done was on a patient who had a "black slough below and in front of each malleolus," for the relief of which he stretched *only* the great sciatic. Two others were done by Dr. Eben. Watson, and, as reported by Mr. Morris, were for wounds of the upper extremity, one of which was a crushed hand, but he does not state its exact limits; for this he stretched the ulnar, median, and musculo-spiral. In the other, the position of which he does not state, he stretched the cords of the brachial plexus. The one by M. Verneuil, resulting in recovery, makes the fourth.

Any tyro in anatomy could see that he could not expect to get complete isolation in his own case when he left untouched the long saphenous branch of the anterior crural, which, as we all know, supplies the inner side of the ankle and foot as far as the great toe. It seems that Dr. Watson has been equally forgetful of the internal cutaneous; for, unless his injuries lay entirely to the anterior side of the hand and arm, either directly or indirectly through its inosculations with the ulnar and radial, some of its filaments would be included in the wound; and its origin is so high up in the axilla, and is so deeply covered by the contents of this space, that, to say the least of it, in one case it is certain, and in the other highly probable, that he caught the inner cord below this point, and by so doing missed some of the filaments that led from the irritating focus.

To all who will think a moment, the way to have prevented these errors is plain. For the first, as I have recently done in a burn below and in front of the knee, beside the great sciatic, the anterior crural should have been stretched; and in the other two, the internal cutaneous, in addition to what was done, or, better still, the two primary trunks above the clavicle.

In gathering up the statistics of this operation to go with the report of my case to the Central Kentucky Medical Association, I found that, including these three cases, there have been, so far, fifteen operations, but as neither of Morris's nor Watson's cases met the indications, they should be excluded, which leaves five recoveries to twelve operations. The other seven were cases in which the operation had been delayed until the vital forces had begun to give way, but in all except those of Morris and Watson there was a considerable intermission of the symptoms. In these, however, the spasms returned as soon as the anæsthetic was removed.

I have not tried it, and of course cannot recommend it, but from my study of nerve-stretching, not only for tetanus, but for neuralgia, &c., it seems to me that it will be found on trial that a reopening of the wounds and a second stretching in a case that had obtained a remission from the first operation, would produce the same effect in the second as third. For it has been demonstrated that the paralysis produced by this operation rarely lasts longer than a few