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

hours. So why should not this therapeutic agent be used in repeated doses when its good effect is as transient as opium in peritonitis? Had I thought of it in time I would have tried it in my case, and am now impatiently waiting for another on which to test it. Whilst these statistics prove nothing as to the ultimate result of the operation, still they show a better record than any other mode of treatment; and now that we have what *seems* like a clue to this long-sought mystery, let us work it out carefully, operating as early as possible, and being sure in every case to secure *complete* isolation, so that, if the operation "falls to the ground," it may do so by its own demerits, and not by the blunders of its would-be friends.

So striving, I remain, Sir, respectfully yours,

A. W. JOHNSTONE, A.M., M.D.

Danville, Kentucky, U.S.A., Nov. 11th, 1879.

P.S.—The cases that I find reported are: the four already referred to; Dr. Thomas Tours, in *Bulletin de la Société de Chirurgie*, "Archives of Medicine"—one case; Paul Vogt, *Nervendehnung*, "Chirug. Praxis," Leipzig, 1877—three cases, two of which were successful. Dr. Drake, *Canada Medical and Surgical Journal*, Oct., 1876—one case; Joseph Ramshoff, F.R.C.S., *Cincinnati Lancet and Clinic*, vol. i., 1879—one case, successful. Kemroth, *Deutsche Med. Wochenschrift*, No. 44, 1878—one case; Dr. J. J. L. Rattou, of the Medical School of Madras, in a paper on Tetanus, reviewed by *Med. Times and Gazette*—four cases, one recovery; my own making the fifteenth.

SCARLATINA IN EPSOM.

To the Editor of THE LANCET.

SIR,—In reference to the scarlatina at Epsom, I should like to record my opinion that the panic among the parents of the scholars was quite unnecessary. When I visited the school about three weeks since I found every department, especially the dormitories, most scrupulously clean and well ventilated, and every care appeared to be taken for the health and comfort of the boys. At that time, although the panic had commenced, there were but nine cases of the disease, and all were of a very mild character, with the exception of one, which I was assured was not malignant. After this date it appears that five other boys were affected; but I would submit that even fourteen cases of the disease among 250 boys was not a large proportion, and does not, in my opinion, prove defect in the sanitary arrangements of the school, and ought not to have necessitated the removal of a large proportion, and ultimately the removal of the remainder, of the boys to their several homes, more particularly as there was every facility at the school for the removal of even doubtful cases to the infirmary, which is entirely isolated. Supposing, however, that the boys who were removed had any of the infection about them, does it not appear probable that their removal was a likely means of disseminating the disease not only among their families and friends, but also among their fellow-passengers.

I trust, however, now that the scholars are removed, a searching inquiry will be made into all the sanitary arrangements, and, should any defects be discovered, they will be remedied before the reassembling of the scholars; but it must not be expected that such a large number of boys will always be exempt from disease, and should a few cases of disease occur at a future time, unless of a malignant character, it is to be hoped that a similar panic will not again be repeated. I remain, Sir, yours, &c.,

Canterbury, Dec. 4th, 1879.

GEORGE RIGDEN.

THE LAST PART OF THE ZULU WAR.

(From our own Correspondent.)

WE have now all left Zululand, and all seem thankful. By the time this reaches you most of the medical staff, and probably all the civil surgeons, will have arrived at their future destination. It is satisfactory to learn that the Medical Department, on the whole, has performed its arduous part in a creditable manner. The hitches so frequently complained of as having taken place between the advanced field hospitals and the front field hospitals chiefly consisted of

neglect in keeping the front well supplied with drugs and a complete staff of assistants. The base department forwarded what was needed to the advanced field hospitals correctly enough: the base dépôt was under the able management of Lieutenant Johnson, A.H. Corps, at Durban, and he was responsible for the supply of all hospital equipment, medical stores, &c., for the 1st Division.

The percentage of deaths at the base has been about 5 per 1000—a most satisfactory result; but I hear that the water-supply, or rather, that the water used at the military hospital, was bad, and that its defective quality was not discovered for some time after its consumption. I also learn that in consequence of its being stopped an unpleasant feeling cropped up between the mayor of Durban and the army medical authorities at this town, the former saying that the townspeople used the same water and never suffered any ill consequences. However, I am told on the highest authority that diarrhoea and dysentery disappeared from the troops in hospital after its use had been discontinued; and, on the other hand, I learned from good authority that the diarrhoea and dysentery were caused by the sick men eating unripe oranges and other fruits, given out of kindness by the people allowed to visit unrestrictedly, and to distribute these fruits in the military hospitals. The last victim of dysentery was Lieutenant Troy, A.H.C., who was buried with military honours on Wednesday last, the whole of the medical staff and a large body of orderlies attending under Surgeon-Major Wills and Lieutenant Johnson, A.H.C.

It may be interesting to your readers to give a short account of a visit I made to numerous Zulu kraals in the neighbourhood of the Umlalazi plains, by direction of Lieutenant-Colonel Lonsdale Hale, R.E., commanding, this officer taking great interest in the sick and wounded Zulus, as well as in the British forces. I saw in all about 1200 Zulus—men, women, and children. They live in kraals in clusters of from four to thirty each. The men occupy separate kraals from the women—i.e., the married men. The young men and young women frequently live together, sleeping side by side. Only old men or men upwards of forty are allowed to marry. Each man has from one to thirty wives. Wives are purchased with cows, the value being from five to ten or fifteen cows each, so that the larger number indicates wealth. The kraals are tolerably clean and orderly, and sanitary regulations are strictly carried out. They live chiefly upon crushed mealies, cooked. The crushing is done between two stones, and the work is chiefly performed by old women. They eat meat, and usually devour cattle dying from disease, except the diseased parts, bowels and all; indeed, I have seen them eating them uncleaned. It is a common thing to see our own Kaffirs cut out the muscles of the back of an ox that died from disease, cooking and eating the same, the odour at times being frightful. They eat the sweet potato, which grows plentifully everywhere; pumpkins, which grow to an enormous size; and some native fruits. They drink Kaffir beer and sour milk. They frequently suffer from fever (common intermittent). They believe in witchcraft and charms; the latter are tied round the neck or part affected. They take medicine made from herbs and roots of trees and plants; they use the root, stem, and leaves of the common fern, by boiling (first crushing), then drinking the residue, for worms. For diarrhoea and dysentery they use the root of an astringent plant, by crushing and steeping in water; the root is shaped like a carrot, but I could not find out its name. They also practise cupping or scarification. This is done by pinching up a small fold of skin over the part affected, and nicking the same with a rude, sharp piece of iron; into these small cuts gunpowder or powdered roots are rubbed. The mati-man, or doctor, asks that so many cattle be killed as a sacrifice, and the meat is eaten by the doctor's friends. Sometimes the doctor is a woman. Frequently these native doctors pronounce the patient bewitched, and woe be to the man or woman accused—he or she will certainly be killed.

The women are not very prolific: for instance, in the case of Induna Habarna, who had thirty wives, they had only eight children alive amongst them; a few more had been born but died young. He could give me no reason for their dying so young, but on inquiry I gathered that the greatest mortality is amongst young children, from the period of birth to the time that they are able to take care of themselves and look after their own bellies, mind cattle, or become useful—two, three, or four is the ordinary number of children born to each woman; commonly only one. The