and wounded soldiers of its own army. From the experience we have had during this war of the various voluntary aid societies, we are not disposed to rank their labours as very efficient. Partly from lack of experience and organisation, partly from the jealousy they excited among military medical men, partly from the abuses to which the "red cross" was subjected, from being assumed by all sorts of people, under all sorts of circumstances, there has been a terrible waste of money and energy. It is a lamentable fact—but fact we fear it is—that all attempts at softening the horrors of wars by neutral powers are liable to misconception, and possible mischief. The duty of taking care of the sick must devolve upon the Government employing the troops who have become disabled in its service. The most that can be done is to supplement State aid in various ways, and under circumstances where it would be impossible for the Government to do so, or vain to expect any Government to afford the requisite amount.

Correspondence.

"Audi alteram partem."

SANITATION IN INDIA.

To the Editor of THE LANCET.

SIR,—Miss Nightingale has been so good as to send me the enclosed letter, written, you will observe, for publication in your journal, in reply to what Miss Nightingale is good enough to call my "kind and gentle criticism."

I am glad to find that this very influential writer had no intention or desire to throw cold water on the inquiry in defence of which I wrote. I wish, however, to state that I was not singular in thinking such was likely to be the effect of Miss Nightingale's observations.

In conclusion, I wish your readers to understand that the inquiry in defence of which I took up my pen was not the one instituted by the Sanitary Committee of the War Office, of which Miss Nightingale "was one of the first and strongest advocates," but that more special inquiry suggested by the Senate of the Army Medical School, and now being carried on by Drs. Cunningham and Lewis.

I am, Sir, your obedient servant,

Royal Victoria Hospital, Netley.

W. C. MACLEAN.

To the Editor of THE LANCET.

SIR,—In your number of Oct. 29th you inserted a kind and gentle criticism from Professor Maclean on certain parts of my "few words" appended to the last annual India Office Report on sanitary measures.

Since the outbreak of this most terrible of all earth's wars, I have had a hard time of it in defending others, and I have not had the least little moment to defend myself. Still, Sir, if you will allow me, I should wish to say that Professor Maclean seems to have read from a point of view opposite to that which was in my mind when writing these "few words." And I am glad that he has given me an opportunity, with your permission, to explain them.

My object was, as I need scarcely say, purely practical. It was to deprecate a tendency complained of by all of late years (this very complaint came to me from India)—viz., the tendency to base sanitary proceedings on theory. Dr. Maclean appears to think that I question the propriety of the cholera inquiry now proceeding in India. I was one of its first and strongest advocates. I strenuously urged the granting of necessary funds to carry it out; and I consider it one of the most important public Indian inquiries which has been ever undertaken on our subject. The very importance of it lies in this: if a fact is proved, it ceases to be theory. The inquiry in question is to ascertain, as far as may be, what is fact. This was the meaning of the passage which Dr. Maclean interprets as implying "ridicule" on my part.

What I said about the present state of the fungoid theory I learnt from the published report of Drs. Cunningham and Lewis; about the ground water, from Dr. Town-

send's Report on Cholera in the Central Provinces. These statements are not theories, but facts. If they are facts, they cease to be theories. The theories remain just where they were. Of course, if the theories were found on longer inquiry to be true, they too would be no longer theories, but facts; and as such would afford good ground for expending public money in applying the facts to save life.

The case of Jenner, cited by Dr. Maclean, is in reality my case. Jenner first started a theory; but the Vaccination Acts, with the costs and penalties, were not enacted until Jenner's theory had become a fact by long experience.

We all have the same object in view—viz., saving human life. This cannot be done without expenditure. And as theories are many and uncertain, all we ask is, that the public should know what we are spending their money for. This, the said cholera inquiry will, perhaps more than anything else, help to tell us.

Also, in what I said about cholera excreta, I simply dealt again with the facts. Dr. Bryden has shown in his Report on Cholera, p. 214, that the dry-earth system, which, in its application, would prevent the dangers of putrid cholera excreta, had not stayed the ravages of cholera. Hence, Dr. Bryden himself calls in question the theory. And I have done no more.

At p. 59, para. 170, of the Report for 1869 by the "Sanitary Commissioner with the Government of India," just received, the results of dealing with cholera excreta are stated as follows:—"With regard to the effect of the careful disinfection and safe disposal of evacuations which seem to have been generally practised, there is no evidence to show that any results can be professedly attributed to them."

Since Professor Maclean's letter appeared in THE LANCET of Oct. 29th, I have received Dr. Lewis's able and most interesting report, the first instalment of the scientific inquiry into cholera which Dr. Maclean fears my remarks may injure. It confirms the former joint reports of Drs. Lewis and Cunningham by a host of microscopic examinations and drawings, from which the following conclusion (No. 3, p. 164, Sanitary Commissioner's Report) is deduced by Dr. Lewis:—"3. That no special fungus has been developed in cholera stools, the fungus described by Hallier being certainly not confined to such stools." But these are the theories which have hitherto occupied the attention of the observers.

Even this information, though most important, we shall not, however, receive as final. We must inquire into objections, and ask further questions. The real inquiry is only about to begin. Mere controversy is here useless.

With many apologies for writing so long a letter, which, had I had more time, I could have made shorter,

Pray believe me, Sir, your faithful servant,

London, Nov. 14th, 1870.

FLORENCE NIGHTINGALE.

*** We have been compelled to omit the publication of Professor Maclean's letter (in type), replying to Dr. Morehead's communication of last week, in order to find the necessary space for the insertion of the above.—ED. L.

TORSION OF ARTERIES.

To the Editor of THE LANCET.

SIR,—In the paper by Mr. J. D. Hill, on "Torsion of Arteries as a hæmostatic method" in your impression of the 5th inst., I remark that, in the description he gives of the operation as performed by Amussat, he makes no mention (nor indeed does either Mr. Bryant or Prof. Humphry in their several papers referred to by Mr. Hill) of the special pair of forceps by which the vessel was fixed before being twisted. This instrument, called by Sédillot "pince à rafolement," and by Malgaigne "pince à baguettes," is figured in the medium operation of the former author. It consisted essentially of a forceps, with branches terminating in smooth cylinders, with which the vessel was seized transversely to its long axis, after being drawn out and freed from the surrounding textures; compression of the arterial coats by its means then led to the division of the two internal ones, whilst their separation and rolling back was facilitated by slight traction on the free end of the artery.

In the modification employed by Mr. Hill, one blade of the forceps is placed within, and the other without, the