many parts of the lung, but where no efficient pleural space could be opened out.

Dr. Morgan remarks that experience with his apparatus would cause me to remove the objections I have expressed, and I do not deny that some apparent difficulties would probably disappear after prolonged use of the method. But, unfortunately, I am still in the hopeless condition of considering the single manometer an extremely satisfactory instrument, and of differing entirely with Dr. Morgan's condemnation of it. My attitude towards his two manometers reminds Dr. Morgan of the one-time rejection of the single manometer by the pioneer of pneumothorax therapy, but I think a little consideration will soon show him how very superficial is the resemblance between the two cases.

With regard to my fear that the oscillations would be damped down by inertia of the fluid in the reservoirs of his manometer, I am glad to have his assurance that this is groundless, for there seem to me great points of convenience in this arrangement of movable reservoirs. He then remarks: "Curiously enough, the only advantage which Dr. Riviere sees—that is the advantage regarding the range of working pressure—really does not exist." I should like to ask how he reconciles this with his statement in the original paper that "we may record a pressure of 35 cm. by raising, —35 cm. by lowering, the manometer reservoirs." The arrangement of the apparatus certainly seems to bear out the correctness of this, his original assertion.

Finally, Dr. Morgan's preference for oxygen because if it passes into the tissues it is more quickly absorbed and produces less surgical emphysema serves to illustrate very well the objection, if not the danger, of the continuous flow of gas which Dr. Morgan favours. With the established method no such leakage of gas need occur, and I cannot help feeling that its avoidance is a gain on the side of safety and convenience alike.

With apologies for the length of this communication,
I am, Sir, yours faithfully,
Inverness, August 2nd, 1914. CLIVE RIVIERE.

TUBERCULIN "ENTHUSIASTS."

To the Editor of THE LANCET.

SIR,—I think the letter in your issue of July 25th by Dr. Charles Miller a most important one. As he writes the time for discussing this subject is passed. This so-called remedy has received a long and careful trial, and the consensus of opinion formed by un-biassed observers is that no benefit arises from the injection of tuberculin and that even when used with the greatest care harmful results are liable to ensue.

The insurance authorities would do a great public service if they officially condemned this drug, for such action would go far to banish its use from all paths of practice. Surely it is time the true position of tuberculin in the treatment of pulmonary tuberculosis should become a matter of common knowledge.

I am, Sir, yours faithfully,
London, July 28th, 1914. THOS. GLOVER LYON.

To the Editor of THE LANCET.

SIR,—I was glad to find in THE LANCET of August 1st that Dr. J. E. Squire is in agreement with my remarks. Dr. Squire, however, does not see how any blame can be put upon the Insurance Committee, as they do not direct the

form of treatment. In the case which I quoted I do blame the Insurance Committee. If they had not instructed my patient to attend at a certain dispensary she would never have been treated with tuberculin. Official interference between doctor and patient was the initial step towards tuberculin treatment.

With regard to the treatment itself and the method of administration in this case, I prefer not to express my opinion, as I do not think it necessary to do so.—I am, Sir, yours faithfully,

Queen Anne-street, W., July 31st, 1914. CHARLES MILLER.

THE USE OF LIQUID PARAFFIN IN ENTERIC FEVER.

To the Editor of THE LANCET.

SIR,—Professor L. P. Phillips's advocacy in THE LANCET of paraffin in typhoid fever raises a larger question than whether we should administer it for constipation in one massive daily dose or in repeated divided doses for the sake of its lubricant and protective action—namely, what it can do for the septic diarrhoea in the worse type of cases. The "local treatment of the bowel," which must remain our first consideration until the vaccine treatment may have completely suppressed the disease, was not thought of, at any rate not practically attempted, before Sir William Jenner's indictment of milk as a factor in the septic intestinal fermentation of typhoid fever. We owe the clinical proof of the correctness of that view to Prideaux Selby's historical paper¹ on the successful substitution of whey for milk in the dietary. That paper deserves full credit for having led to the wider practical conclusion, subsequently identified under the name of the "empty bowel treatment" or "traitement à vide,"² that from the beginning to the end of our treatment the all-essential indication is the constant sanitation or sanitary toilet of the bowel. For the "evacuant" purpose in that treatment paraffin was used as an adjuvant in frequent fractional doses; and subsequently it was found that, as an additional security, a small daily dose of castor oil was useful and well borne. That treatment also aimed at "disinfection" by the free administration of animal charcoal (originally used in France by Bouchard in typhoid) for the sake of its remarkable absorptive and neutralising power over alkaloids, as, for instance, after massive lethal doses of strychnine or morphine. But the first and indispensable indication was to systematically and completely remove the septic culture from the lower ileum; leaving the upper bowel and the stomach in a much better condition for their all-essential alimentary function of absorbing abundant supplies of varied and nourishing foods (either purely crystalloid, such as sugar and salts; subcrystalloid and diffusible, such as the maltine group, yolk of egg, and whey itself; or leaving practically no residue, such as cream, predigested foods, meat and fruit jellies, &c., and particularly white of egg added in high dilutions to the whey to replace its coagulable casein).

That proposition has, I believe, never been fully discussed. It still faces us, on the foundation, which presumably is not disputed, that the multiple spread of the ulcerations (accounting for the clinical "relapses"), up the ileum and down into the colon, is secondary, and most probably due to

¹ THE LANCET, Nov. 2nd, 1901, p. 1182.

² Brit. Med. Jour., vol. ii., p. 1507, 1905.