

imagine, for we have had the influenza here. Commencing down south in the Argentine Republic, where immigration from all parts of the world is going on, it gradually worked its way up the coast to Brazilian ports—Santos, Rio, Bahia, and last of all Pernambuco. One Englishman died in Santos from pneumonia following an attack of influenza. In this place (Pernambuco) I believe I had some of the first cases under my care on board the Western and Brazilian Telegraph Company's steamer *Norseman*. The crew consists partly of Englishmen, and partly of natives of Sierra Leone. The Englishmen were first attacked. Two cases had well marked eruption; there was very little catarrh or coryza, but the pains in the back and head (frontal and orbicular especially) and feeling of soreness all over were general symptoms; in one or two cases there was severe pain across the front of the body about the line of the attachments of the diaphragm. I send a copy of a Pernambuco daily paper (they have no medical organ here) which gives an account of the proceedings of the Medical and Pharmaceutical Association of Pernambuco, in which Dr. Cosme de Sá Pereira had the temerity to read before his colleagues some cases which he considered to be true influenza. I say temerity, for the medical faculty here do not seem to have recognised the disease yet. He remarks on the absence of the catarrhal state of the respiratory tract here in contrast to its frequency in Europe; and thinks that the difference is due to the climate. His colleagues, however, do not agree with him, asserting that there has been no case of influenza here, and that the cases he had brought before them were probably due to meteorological changes or errors of diet, and that the general absence of catarrhal symptoms put influenza out of the question. The only matter I would call attention to is as regards the infectivity of influenza, its commencing in Buenos Ayres, where great numbers of Italians, Russians, Germans, and people of other nationalities land every week; then follow Santos and Rio de Janeiro, which have few except Portuguese immigrants; and, lastly, Pernambuco, where there is no immigration at all. I have reason to believe that the first cases here occurred among Englishmen. Could it have been through letters?

On the whole, whether due to the difference of climate or not, the numbers attacked in Brazil seem to have been so few and the affection so mild as scarcely to be worthy of mention. I am, Sirs, yours faithfully,

Pernambuco, April 26th, 1890. J. TOPPIN, L.R.C.S.I. &c.

URINE TESTING IN PREGNANCY.

To the Editors of THE LANCET.

SIRS,—My attention has been drawn to a letter which appeared in *THE LANCET*, May 27th, in which Dr. Fyffe, in criticising some observations of Dr. Herman's, objects to the inaccuracy of the tests he employed, the methods for which he fixes on me. Had Dr. Fyffe, instead of taking the statement at second hand, applied to me as to its accuracy, which should be the usual course, he would have learned that the method I had suggested to Dr. Herman was not mine, but that originally proposed by Hammarsten for the separation of serum-globulin from serum-albumen, and that I was in no wise concerned for the printer's error which in the report rendered "heated" for "treated." Since Dr. Herman's correction last week I have expected to hear from Dr. Fyffe expressions, at least of regret, for the annoyance such an attack might have given me, but as up to the present moment I have not heard from him, I must ask you to insert this letter. I am, Sirs, yours truly,

Queen Anne-street, W., June 4th, 1890. CHARLES H. RALFE.

DYSENTERY AS A CAUSE OF HEPATIC ABSCESS.

To the Editors of THE LANCET.

SIRS,—I notice in *THE LANCET* of May 31st that Mr. Lowson, while commenting on his interesting case of abscess of the liver, expresses some surprise that I should have omitted referring to dysentery as one of the exciting causes of hepatic suppuration in the paper I read at the Leeds meeting of the British Medical Association. Had my paper been on pathology instead of treatment, I should most

assuredly have referred to dysentery, as I think no one doubts the frequency of liver suppurations following on it. At the same time I should have to mention a number of other things—gall-stones even among the rest—as direct exciters of hepatic abscess. To have done so in a communication on treatment, in which I merely incidentally alluded to the most common, and probably least thought of, predisposing and exciting causes of the disease, would have made my paper far too long. I shall, however, on a future occasion consider the etiology of hepatic suppurations as a whole. In doing so we shall have to take into consideration the position of the liver, acting as a filter for the blood from the alimentary canal, before it enters the general circulation.—I am, Sirs, yours truly,

VAUGHAN HARLEY, M.B., M.R.C.P. Lond.

Physiological Laboratory, Strassburg Universitat, June, 1890.

THE SOCIETY OF APOTHECARIES.

To the Editors of THE LANCET.

SIRS,—I shall be very much obliged if you will kindly publish the enclosed correspondence, and allow me to ask all members of the Society of Apothecaries to communicate with me with a view to united action if necessary.

I am, Sirs, your obedient servant,

ALBERT BENTHALL.

Cobham Lodge, Clarendon-road, Southsea, June 4th, 1890.

Society of Apothecaries, Blackfriars, London, E.C., June 3rd, 1890.

SIR,—The Master has handed to me your letter of the 31st ult. There was no want of courtesy on my part in not acknowledging your letter; but until the regulations are settled under which the limited number of licentiates are admitted by the Court of Assistants, it is impossible to give any information on the subject. A Court of Assistants will meet in the course of the month to settle these regulations, and when settled they will be published in the London daily papers, the papers of the large towns, and in the medical papers. Until then I am unable to give, and in fact have no power of giving, any further information.

Your obedient servant,

A. Benthall, Esq.

JAMES R. UPTON.

(COPY.)

Southsea, June 4th, 1890.

SIR,—I beg to acknowledge your letter of June 3rd, and I am glad to hear that there was no intentional want of courtesy to a member of the Society. I gather from your letter that the Court of Assistants intend to settle and publish a scheme fundamentally altering the constitution of the Society, and deeply affecting the position of the members, without calling a meeting or in any way consulting them.

It may be that the new scheme is in every way desirable; but, at any rate, it should be placed before the members before it is definitely decided upon.

I am, Sir, your obedient servant,

ALBERT BENTHALL, M. & L.S.A.

J. R. Upton, Esq.

ELECTRICITY: ALTERNATING AND CONTINUOUS CURRENTS.

To the Editors of THE LANCET.

SIRS,—I have read the letter of Mr. Newman Lawrence to which Dr. Harris refers, and also the paper under discussion, but I fail to see that Mr. Lawrence has demonstrated the fallacy of Professor Forbes's arguments. It appears to me that the "fallacy" consists in Mr. Lawrence, after quoting the "learned professor's" remarks concerning the maximum current, proceeding to treat it as the mean current, and to express it in milliampères, forgetting that the milliampère is the quantity per second, whilst the maximum occupies but the fraction of a second, that fraction being inversely as the maximum divided by the mean; but Professor Forbes's estimate is, in my opinion, much below the mark in putting the maximum current at 20 to 50 times the effective current; I think 200 to 500 would be nearer, as the volts (on break) of an induction coil may be reckoned by thousands and tens of thousands, and in the case of very large coils by hundreds of thousands. It would be well if Dr. Harris and Mr. Lawrence would repeat their experiments with an alternating dynamo or alternated battery current of moderate tension—say, 20 or 30 volts—as the disruptive and unequal alternation of an induction coil are quite unsuited to the object they had in view.

I am, Sirs, yours faithfully,

FREDERICK GEORGE, M.D.

Uley, Gloucestershire, May 13th, 1890.