

Correspondence.

"Audi alteram partem."

GENERAL MEDICAL COUNCIL ELECTIONS: THE ELECTION OF DIRECT REPRESENTA- TIVES FOR ENGLAND.

To the Editor of THE LANCET.

SIR,—Like Dr. John Brown of Blackpool, I also have been nominated as a candidate for election as a Direct Representative for England on the General Medical Council, my nominators being the National Medical Union, with the approval of other associations having a non-panel membership. I have, however, too high an opinion of the independence of the individual professional man to believe that any organisation can deprive him of his liberty of thought and action. I have myself for long been, am, and propose remaining, a member of the British Medical Association, though regretting some of its actions in the past in relation to State Medicine, and it would certainly never occur to me to do otherwise than scrutinise carefully, and in the matter of medical politics very carefully, any suggestion emanating from its directorate for the time being. I have no fear, therefore, that with all the advantages which Dr. Brown claims for its specially nominated candidates, the profession at large, or even its own membership, will unreflecting do its bidding. In a medical election heads are counted, not noses. I think, therefore, Dr. Brown and other candidates need not fear that their just claims will be overlooked merely because the rulers of any given organisation endeavour to influence the electorate in favour of certain nominees. I have too high a regard for the electorate now in question to believe that it will without careful consideration yield to any organised attempt to manipulate its intelligence.

I am, Sir, yours faithfully,

ALEXANDER BLACKHALL-MORISON, M.D., F.R.C.P.
London, Oct. 25th, 1919.

POST-MORTEM ACTIVITY OF THE LIVER AFTER DIABETIC COMA.

To the Editor of THE LANCET.

SIR,—About 20 years ago I was called out early in the morning by the relations of a patient who had died the previous night, about 10 o'clock, from diabetic coma. I arrived about 8.30 A.M. and found that the reason for summoning me was that the body was so warm that they could not believe that he was dead. The weather was very cold and the body had lain all night in an unheated room. I was able to convince them that the patient was really dead, though the upper abdomen and lower thorax were still quite warm to the touch.

The incident remained in my memory as a curious one, but I did not attach any particular import to it at the time. Not long ago, however, I was in the post-mortem room and noticed steam arising from the abdomen in a case of the same nature. On placing a thermometer in the substance of the liver the temperature was found to be 91° F. It was then 3 P.M., the temperature of the room was 40° F., and the body had been there certainly over 10 hours, the patient having died before 2 A.M. that morning. The warmth was confined to the liver and neighbouring viscera.

It is clear, of course, that nothing but prolonged physiological activity of the liver could account for the maintenance of such a temperature under such conditions, and it would be very interesting to know whether this activity is peculiar to diabetic coma. It may be presumed that the condition was the same in the first case as in the second, though not proved. I am not aware of any previous observations on the subject, and I therefore send this note in the hope of stimulating further inquiry. It is possible that an investigation of the chemical condition of the liver under these circumstances might throw some light on some of the vexed problems of diabetes. In any case the question of the post-mortem activity of organs, the delay of cellular death, and the conditions under which it occurs is one of considerable interest and importance.

I am, Sir, yours faithfully,

Brighton, Oct. 23rd, 1919.

E. HOBHOUSE, M.D.

CONJUGAL TUBERCULOSIS.

To the Editor of THE LANCET.

SIR,—It is difficult to follow Dr. C. Muthu's argument in his letter which you publish in your issue of Oct. 18th. We know that consumption is caused by tubercle bacilli, provided the dosage is sufficient, and we know that contact cases (especially husband and wife) are exposed to strong infection. Why, therefore, must one explain any slightly greater prevalence of the disease amongst married people than others by supposing that the stress and strain of married life are factors in rousing into activity some assumed latent lesion? What good evidence is there that re-infection does not occur, and that disease which manifests itself in later life is only the result of infection in childhood? Even if one holds this view one must admit the necessity of guarding against infection throughout life, for the child will remain susceptible until infected.

Again, Dr. Muthu suggests that most people who nurse consumptives do not contract the disease. But surely the same applies to scarlet fever. Such negative evidence can have very little value. Is it not equally reasonable to believe that we are all exposed to infection, but that the individual possesses some resistance which is usually sufficient to prevent the infection from developing into a serious disease? Repeated small infections may add to this resistance, but there is a limit, and if the dosage is too great the patient develops tuberculosis, whether or not he has been previously infected. For a long time I have held the strong opinion that the danger of contact has been much exaggerated, but I would never allow a consumptive patient to sleep with his wife, and I think it would be a bad day if medical opinion convinced the public that consumption is not a contagious disease.

I am, Sir, yours faithfully,

Wimbledon, S.W., Oct. 19th, 1919.

L. S. BURRELL.

CÆLIAC DISEASE.

To the Editor of THE LANCET.

SIR,—The subject of celiac disease is one so comparatively seldom raised that I hope Dr. David Forsyth's paper in THE LANCET of Oct. 25th may serve as a pretext for some discussion on this condition. Could an agreement be reached on some points in connexion with the disorder, even though mainly negative ones, it would be useful. Firstly, and before considering Dr. Forsyth's suggestion, there is the question of the morbid anatomy. While I do not forget such changes as have been reported post mortem, yet it seems true that no causative organic lesions are constantly found. Most commonly, as in a recent case of my own, no important lesions are found, even microscopically, in the pancreas and liver and their ducts, nor in the intestines. Secondly, if this be agreed to, the causative theories advanced must be viewed in this light. Must we not seek a cause which can operate over many years without being associated with any organic changes traceable (or as yet traced) post mortem?

Dr. Forsyth's suggestion of boric acid poisoning, however applicable to the acute case he reports, is difficult of acceptance in the true chronic celiac case. Such points as the following occur to my mind. The withdrawal of milk does not cure, only improves, the condition. I have had good results in mild recrudescences by giving the usual milk well skimmed instead of with its cream. In one patient of mine, travelling all over England in the last three years, attacks have occurred anywhere, town or country. In recovering cases the stools will vary in colour and consistency enormously without change in diet. Finally, it is difficult to account for the continuance of the symptoms unless the boric acid set up a chronic enteritis, which neither the character of the stools nor post-mortem findings suggest.

The theory of abnormal intestinal flora is attractive, particularly in view of the abrupt onset of some cases. But in agreement with other observers, in my search for abnormal organisms I have found none or any. In one instance under me for the past five years an organism of the paratyphoid-Gaertner group was originally reported several times. This disappeared in a course of soured-milk treatment and has never reappeared, but no definite good seems to have accrued. Must we not, however unwillingly, set this theory aside as insufficient?