

in America with the liver preparation I described last year (*Lancet*, 1931, ii, 791); he does not, however, allude to our interest in it.—I am, etc.,

Manchester, Feb. 17th.

JOHN F. WILKINSON.

SIR,—In the article on the intravenous liver treatment of pernicious anaemia in the *Journal* of February 13th (p. 273), a previous communication of mine to the *Lancet* (1931, ii, 1022) is referred to.

By the omission of the word "later" from the quotation "and a little later, hog's stomach extract," some doubt is engendered as to the adequacy of the pre-injection treatment. I should like to say that liver in full doses (3½ lb. per week) was used, and 40 grams daily of ventriculin, and that oral treatment failed in this case owing to the absence of response and the utter inability of the patient to face further liver by mouth, on account of nausea, as stated in my original communication.

As to the permanency of the treatment, this patient has recently suffered a relapse, not from the point of view of his symptoms (the patient feeling quite well), but from the point of view of his blood count, weekly injections of 5 c.cm. of hepatex P.A.F. intramuscularly having proved insufficient to keep his blood to a good standard. The red cells, which were 3,000,000 per c.mm., with haemoglobin 55 per cent., have, since his grading-up, come down to 1,750,000 per c.mm., with haemoglobin 30 per cent. (February 7th). He is now on another intensive course of injections.—I am, etc.,

Cardiff, Feb. 15th.

W. CAMPBELL ANDERSON.

SIR,—May I draw your attention to a misstatement which occurs in the *Journal* of February 13th (p. 269), in Professor Davidson's paper. He states:

"Credit for having first shown the remarkable difference between the effectiveness of the active principle when administered orally and parenterally belongs to Professor Gänsölen of Tübingen."

Professor Gänsölen's work was published in 1930. In February, 1928, two and a half years before Gänsölen's publication, I demonstrated the superiority of the parenteral administration of liver over the oral method in my monograph on *Acute Aplastic Anaemia*. I estimated then that the effectiveness was possibly two thousand to three thousand times greater when given intramuscularly. Pages 4 and 64, and the serial blood counts and graphs in my publication, support this claim.—I am, etc.,

Bradford, Feb. 15th.

A. HAYES SMITH.

FOREIGN BODIES IN THE AIR PASSAGES

SIR,—In his interesting post-graduate lecture on foreign bodies in the oesophagus, which appears in the *Journal* of February 13th, Mr. Holt Diggle refers to the comparative rarity of foreign bodies in the lower air passages—in his own series, two cases of foreign body in the air passages as against sixty-seven cases of foreign body in the oesophagus, during a period of ten years. As cases of foreign body in the air passages appear to be more frequent in America, Mr. Diggle suggests that it would be interesting to have the experience of British endoscopists as to the relative frequency of foreign bodies in the food and air passages.

I have looked up my records (hospital and private), which extend over a period of twenty-five years, with the following result. Foreign bodies in the oesophagus—dentures, 12; meat and fish bones, 17; coins and other objects, 75; total, 104. Foreign bodies in the lower air passages—larynx, 7; trachea and bronchi, 20; total, 27. This shows a relatively much higher frequency of air-passage cases than appears in Mr. Holt Diggle's records. The discrepancy may perhaps be due in part to the fact

that some of the cases of foreign body in the oesophagus coming to hospital have been dealt with by my assistants, and so are not included in my personal records. Even so, twenty-seven lower-air-passage cases in twenty-five years is a small number, and it remains true that few endoscopists can expect to acquire a very large experience of these cases.—I am, etc.,

Liverpool, Feb. 16th.

THOMAS GUTHRIE.

RADIUM

SIR,—Radium is the most potent element yet discovered for the production of discontent in the medical profession. The recent letter, signed by consultants, appearing in the *Times* reminds me of one addressed to the same newspaper on the subject of radium therapy by some distinguished surgeons in the spring of 1914.—I am, etc.,

London, W. Feb. 17th.

CLIFFORD MORSON.

THE SPAHLINGER VACCINES

SIR,—May I briefly comment on one point only in your leading article of February 13th—namely, your remarks on B.C.G. I quite agree that at first sight it may appear that Calmette's method and mine are so different that, if the one can confer immunity the other cannot. Mine is based on specificity, and Professor Calmette's on attenuation. One may wonder how two methods, so widely apart, could both be effective. Yet closer consideration shows that it is quite possible for both to confer immunity.

My vaccine contains no living micro-organisms. Each dose must therefore include, *ready made*, all the antigenic elements required to create immunity; the antigens are elaborated *in vitro*, hence the need for specificity. B.C.G., on the other hand, is an emulsion of living, attenuated bacilli. The inoculation of a vaccine containing *living* micro-organisms may introduce into the body to be vaccinated a source of *antigen-production in vivo*. Therefore, although the antigens required to create immunity may be in insufficient quantities in the vaccine itself, or even completely lacking, immunity may nevertheless be produced, provided the attenuated micro-organisms have retained enough of their original characteristics to allow them to elaborate *in vivo* sufficient disease-specific antigens to vaccinate, but insufficient to create lesions of a progressive type. This degree of attenuation is considered by many as an adequate safeguard, justifying the use of living, attenuated cultures. Personally, however, I decided to confine my researches in tuberculosis to the preparation of a vaccine derived from *dead micro-organisms*; but that is not the point at issue here. The question you raised was that of the efficacy of the two methods.

To go more fully into this subject would involve exploring the complex problems of immunology at more length than it would be considerate to ask of you now, especially as I intend shortly to publish a series of communications, in the course of which the mechanism of antibody production will be analysed in the light of my own researches.—I am, etc.,

London, Feb. 16th.

H. SPAHLINGER.

TYPES OF THE TUBERCLE BACILLUS

SIR,—During the past eight years I have isolated and further investigated more than three hundred strains of the tubercle bacillus from various sources, human and animal, and have, in addition, studied more than a hundred strains received in primary culture by me from Dr. W. T. Munro, medical superintendent, Glenlond Sanatorium. About 10 per cent. of these strains have been of the bovine type. During that time I have read numerous articles concerning the bacteriology of the