

CHRONIC ENDOCERVICITIS AND ITS TREATMENT.

To the Editor of THE LANCET.

SIR,—It is good news indeed to see the strong interest at last aroused by the pioneer work of the late Dr. Sloan. As long ago as 1910 I used to send patients to see him in Glasgow, or catch him as he passed through London, in order to have his unique opinion on the value of ionisation in individual cases. I had the advantage therefore of having his personal instruction in his methods. Erosions he always considered curable. It is difficult to see how they fail to respond to the means which cure the endocervicitis. It is reassuring to know that other gynaecologists are following his work at length; for so many years now I had become resigned to the discouraging fact that it aroused no interest outside the circle of electro-therapeutists. For those who have not had experience in general practice or special gynaecology it is difficult to master the dexterity required in certain cases; hence one so welcomes the interest of the gynaecologists.

I note that one correspondent refers to the fact that a zinc electrode becomes adherent to the mucous membrane. I think this must be a slip; that he had intended to write copper.

I am, Sir, yours faithfully,

AGNES SAVILL, M.D.

Devonshire-place, W., Oct. 27th, 1922.

To the Editor of THE LANCET.

SIR,—I would like to thank Dr. Johnston Abraham for his complimentary remarks upon my paper on the above subject contained in his letter in THE LANCET of Oct. 28th. Dr. Abraham raises the question of the diagnosis of gonorrhoeal endocervicitis—a question which is quite outside the scope of the paper.

The point which I was anxious to focus attention upon was that chronic endocervicitis, non-gonorrhoeal in origin, was also an infective process and as such ought to receive scientific treatment. For this purpose the more elaborate technique one usually adopts in order to isolate the gonococcus was not necessary. The method I adopted produced 92 per cent. of positive cultures, thus proving my point.

With regard to Dr. Abraham's second point, "the incomplete description of the ionic technique," I cannot agree that the zinc rod "becomes firmly held by the mucous membrane and often cannot be dislodged without causing bleeding." I have not experienced this in any of my cases, and invariably remove the rod without reversing the current. This is one of the great advantages of using zinc over copper, as the latter usually sticks.

I am, Sir, yours faithfully,

JOHN WILLIAM BURNS.

Rodney-street, Liverpool, Oct. 30th, 1922.

THE LATER EFFECTS OF GAS POISONING.

To the Editor of THE LANCET.

SIR,—It is stated by Lieut.-Colonel T. E. Sandall, in your issue of Oct. 21st under the above heading, that "there is a popular impression that pulmonary tuberculosis is a frequent sequela of gas poisoning, but I do not think this is borne out by the facts in this area at any rate." Colonel Sandall also states that "In only one case of the series, also, were there any physical signs suggestive of pulmonary tuberculosis, and in this case the evidence was not conclusive." In this industrial area, population 291,100, the Local War Pensions Committee referred all the gassed cases with any suspicion of lung disease to the tuberculosis officer as one of their medical referees, and in at least 15 of them tuberculosis of the lung developed. None of these cases was discharged with tuberculosis. Of these 15 cases of pulmonary tuberculosis detected, 8 had tubercle bacilli in the sputum and 7 had not. Of the 7 T.B. negative cases, 4 had a definite history of

hæmoptysis (all cases giving a history of streaky sputum being ignored), and in the other 3 the physical signs were definite, the diagnosis in one case being confirmed by radiographic examination. Of the T.B. positive cases, tubercle bacilli were found at various intervals after gassing. These periods are as follows:—

T.B. + Cases, after Gassing.—Within 1 year, 1; 2 years, 3; 3 years, 1; 4 years, 1; 5 years, 2. In all these cases the sputum was repeatedly examined.

Of the hæmoptysis positive cases, the hæmoptysis was discovered at various intervals after gassing. These periods are as follows:—

Hæmoptysis + Cases, after Gassing.—Within 1 year, 2; 2 years, 2.

It will be seen from these results that tuberculosis of the lungs is not an uncommon sequela of gassing.

I am, Sir, yours faithfully,

HAROLD VALLOW,

Oct. 25th, 1922. Tuberculosis Officer, City of Bradford.

PRIMARY PHLEGMONOUS ENTERITIS.

To the Editor of THE LANCET.

SIR,—Dr. Edmund Cautley's suggestion that the case described in your issue of Oct. 21st under the above title should be regarded as one of Henoch's purpura and that the patient would have recovered without operation is one which I am unable to accept. In making this suggestion Dr. Cautley evidently relies more on the clinical phenomena than on the pathological findings. But even on clinical grounds the diagnosis of Henoch's purpura does not meet the case. There was no history of a previous attack, no clinical evidence of any form of hæmorrhagic extravasation, the rash was an erythema—not a purpura—and there was no suggestion of intestinal obstruction. On the contrary, the pyrexia suggested an acute inflammatory lesion and the joint symptoms were regarded as a manifestation of an alimentary toxæmia. The histological evidence, however, is of the greatest importance, and one can only conclude from the presence of a small-celled infiltration—widespread throughout the wall of the affected intestine—that the condition was, as described, a phlegmonous inflammation of the small intestine. In my opinion, if the operation had not been performed, the chances of the patient's recovery would have been nil.

I am, Sir, yours faithfully,

Manchester, Oct. 27th, 1922.

E. E. HUGHES.

THE PREPARATION OF VACCINES.

To the Editor of THE LANCET.

SIR,—In a paper entitled the Action of Vaccines, which was published under my name in your issue of Oct. 7th, brief reference was made to new methods which have been introduced for the preparation of vaccines with the object of reducing what is described as the toxicity of vaccines. In your issue of Oct. 14th is published a letter from Dr. David Thomson in defence of his "detoxication process" which he claims "does not destroy the antigenic nature of the vaccine" (the italics are mine). I should like to point out that the quotation from my paper given verbatim in Dr. Thomson's letter definitely gives my opinion that "the actual effect of this procedure is to weaken enormously the antigenic value of the vaccine." These two sentences disclose an entirely different standpoint.

As I have not access at the moment of writing to Dr. Thomson's papers I trust that my memory serves me correctly in stating that Dr. Thomson's conclusions in this matter are based chiefly on observations on complement deviation and agglutinin. I do not recall any reference to carefully controlled comparative quantitative tests with a vaccine before and after treatment by his process. More important still is the question of comparative protection tests on animals with vaccine before and after treatment by his process. Under these circumstances Dr. Thomson cannot fairly complain if his claim to have provided proof of his contentions is not accepted.