

Correspondence

B.C.G. Vaccination

SIR.—Discussions in print on controversial subjects seldom advance truth and occasionally breed ill feeling. I have stated my views on B.C.G.,¹ and Prof. Wallgren² and Dr. Malmros³ have now stated theirs. I prefer to trust to experiment and experience to reveal the truth. The importance of the subject, however, is so great that I feel it only right to remove the misconceptions about my article under which Prof. Wallgren, at least, seems to be labouring.

In the paper that I was asked to read before the International Conference of Physicians last September I did not attempt to assess the value of B.C.G. vaccination. I made it clear at the beginning that I was deliberately marshalling the evidence against it, and at the end that the conclusions I drew were solely in relation to its probable effect on tuberculosis in this country. I wanted to show that no entirely satisfactory investigations on the efficacy of B.C.G. had yet been carried out, and that, because of the differing epidemiology of tuberculosis in different countries, conclusions drawn in one part of the world were not necessarily applicable to another. I pleaded for a properly controlled investigation into the value of B.C.G. vaccination under English conditions, which, as readers of Prof. Wallgren's article will realize, are very different from those in Sweden. I asked, in fact, for no more than the International Conference in Washington has asked to do in the United States.⁴

If we do not obtain this information now, we shall probably never obtain it, and we shall be forced by public opinion to adopt the use of B.C.G. indiscriminately. If, on the other hand, we do succeed in carrying out an investigation under "scientifically unimpeachable conditions"—and there seems to be some hope of this being realized—I am ready to assure Prof. Wallgren that I shall abide by the results.—I am, etc.,

London, S.W.1.

G. S. WILSON.

REFERENCES

- ¹ *British Medical Journal*, 1947, 2, 855.
- ² *Ibid.*, 1948, 1, 1126.
- ³ *Ibid.*, 1948, 1, 1129.
- ⁴ *Publ. Hlth Rep., Wash.*, 1947, 62, 346; *ibid.*, 1948, 63, 593.

Some Dust Diseases of the Lungs

SIR.—In your leading article, "Some Dust Diseases of the Lung" (June 5, p. 1087), in which you comment on my Goulstonian Lectures, there are some misquotations which may give rise to misunderstanding. You say, "In 1947 men were certified in South Wales at the rate of 100 a week, and certification has risen in all the other coalfields of Great Britain, the provisional figures for 1947 showing a rise of about 1,000 in that year." In fact, it was in 1945 that this high rate of certification was reached in South Wales and in 1947 it had fallen to about 60 a week. In the rest of Great Britain provisional figures show a rise to about 1,000 not of about 1,000 in 1947. Later on you say that "high dust concentration is due in the anthracite mines mainly to poor ventilation." This should read "high dust concentration was due in the anthracite mines mainly to poor ventilation." In fact, improvement of dust conditions in the anthracite mines has been one of the outstanding achievements in dust suppression in recent years, and ventilation is in general very good in these mines to-day.—I am, etc.,

Medical Research Council,
Pneumoconiosis Research Unit (S. Wales).

C. M. FLETCHER.

Survival in Pulmonary Tuberculosis

SIR.—Your annotation (June 12, p. 1143) attributes to me the work of Bardswell¹ on mortality after sanatorium treatment. Later the annotation mentions my own work² on artificial pneumothorax, but does not give the reference which I append herewith. Again, for females treated conservatively the correct figure for survival after five years was only 0.25 and not 0.32 as stated. However, my object in writing is only partially to

correct these minor errors, and mainly to plead for more research into this important subject.

My attempt to assess the value of pneumothorax therapy based on the fate of nearly 700 cases compared with that in more than 3,000 cases treated by conservative methods only was not universally popular, as the results were less favourable to collapse therapy than many anticipated and than some felt was borne out by their own experience. Possibly it was insufficiently realized that the cases investigated were treated during the decade 1921–30 and, so far as collapse therapy was concerned, naturally included in those early years a high proportion of cases that to-day would be either abandoned early as unsuitable for pneumothorax or reinforced or replaced by other collapse measures. Nevertheless, a glance again at my conclusions in 1936 shows that even now they are not altogether out of accord with clinical experience. May I quote the following:

"Incomplete collapse obtained by means of artificial-pneumothorax therapy may not only be unavailing in producing quiescence, it may actually prove an added source of danger to the patient if continued."

"Incomplete-collapse cases and those with pockets only yield indifferent results and, forming 60% of the whole, they drag down the general level of the results."

"Complete-collapse cases on the other hand, accounting for 40% of the total, show more encouraging results. Out of 208 such cases investigated in detail 65.9% of the patients were alive after three years, while when the disease was strictly unilateral the percentage survival rose to 77.4." Furthermore it was recorded that in the complete-collapse cases who were sputum-negative on discharge from residential treatment no less than 90% of the subjects were alive after three years."

Clearly what is needed to-day is for a more recently treated group of patients to be followed up and analysed in a similar fashion. Accurate statistical follow-up work in tuberculosis is of immense importance. No one realized this more than myself when I was largely responsible for drastically curtailing the L.C.C. tuberculosis statistical work in September, 1939. It is very easy to stop work of this sort. How difficult it is to get it going again—but how necessary! Possibly it has not been reasonable to expect the L.C.C. to start this invaluable and meticulous work again in view of the impending dissolution of its hospital arrangements. May I appeal to Regional Boards to have regard to the work of their predecessors and, while cutting out ruthlessly any dead wood that may be found, to carry on and enlarge all those arrangements and schemes that were so well founded?

A large Region may find that it is spending anything up to one million pounds annually on its arrangements for tuberculosis. Is it too much to ask that a small proportion of this sum, say 1%, should be devoted to an attempt to ascertain what are the results of this vast expenditure?—I am, etc.,

London, N.W.11.

F. J. BENTLEY.

REFERENCES

- ¹ *Spec. Rep. Ser. med. Res. Coun., Lond.*, 1919, No. 33.
- ² *Ibid.*, 1936, No. 215.

Coronary Artery Disease and Hypercholesteraemic Xanthomatosis

SIR.—Drs. E. Sherwood Jones and P. W. Robertson (June 12, p. 1137) draw attention to the question of the relationship of hypercholesteraemic xanthomatosis to arterial atheroma. Such a relationship was especially discussed by the Russian pathologist, S. S. Chalataw, in his German monograph, *Die anisotrope Verfettung*, Jena, 1922; and in my booklet, *Cutaneous Xanthoma and Xanthomatosis* (London, 1924, p. 14), I illustrated the question by the history of a London surgeon with whom I was acquainted. In 1902, at the age of 41 years, he had tuberous xanthoma of both elbows. About seven years later he began to suffer from intermittent claudication of both lower limbs, at first slight but gradually increasing in severity. This intermittent claudication continued till his death in 1913, and post-mortem examination proved that it was due to severe atheroma (atherosclerosis) of the abdominal aorta and iliac arteries.

A striking illustration of the association of xanthomatosis of tendons and tendon sheaths with cardiovascular disease (angina pectoris) in a woman aged 60 years, with blood-serum