DIAGNOSIS AND TREATMENT OF PULMONARY TUBERCULOSIS.

SIR,—The comments on my address published in the *Journal* of March 1st (p. 376) have raised some points of general interest.

I should like to thank Dr. Rose Jordan for her letter (March 8th, p. 466), and to say that I can endorse the two practical points there emphasized—the importance of firm pressure by the pleximeter finger, and the help it may be to have the patient breathe with the mouth open.

If Dr. McNeil (March 15th, p. 515) considers that my quotation from his article on the von Pirquet test in any way misrepresented his views I am extremely sorry. The burden of my argument was that the Pirquet reaction may be negative in an actually tuberculous condition. This Dr. McNeil admits. In spite of that admission, he concludes his letter thus: "The principal value of the test is when, having been performed by a correct technique, it is negative; in such cases tuberculous disease can be excluded." There is here no qualifying phrase; no suggestion that "although rarely," a serious error of diagnosis may result. It is a definite assertion which, on Dr. McNeil's own showing, is fallacious. That is the crucial point.

Dr. Weatherhead (March 15th, p. 516) quotes Clive Riviere on tuberculin tests in general, but does not get down to the bed-rock difficulty in the Pirquet reaction—the presence of a negative test in a tuberculous subject. He is all out for "the only tuberculin test which is now commonly held to have any real diagnostic value—namely, the subcutaneous test." By the time the practitioner has kept a careful three- or four-hourly record of the temperature for some days and finds that the mouth temperature does not rise above 99.2 F., and then understands from Dr. Weatherhead that "even this is running the risk of a dangerous focal reaction," he will be exercising only common-sense precaution if he refuses to touch this test at all.

I admit that to pass an opinion on the value or otherwise of ultra-violet light in pulmonary tuberculosis is liable at the present time to induce in some quarters rather strong reactions, and I have no doubt that Dr. Weinbren (March 15th, p. 516) can show reasons for the faith that is in him. All I can say is that the high priest in this particular temple is, I fancy, Dr. C. H. Würtzen, consulting physician to the Finsen Light Institute, Copenhagen. I have sat at his feet and done what in me lay to evaluate his results. This particular factor in treatment is obviously difficult to isolate and assess, but I do not think that the judgement I expressed, that its value is "very problematical," erred on the side of strictness.—I am, etc.,

Gloucester, March 17th. W. ARNOTT DICKSON.

DISADVANTAGES OF SANATORIUM TREATMENT IN WINTER.

SIR,—Dr. Arnott Dickson (*Journal*, March 1st, p. 376) draws from a deep well of experience in writing of tuberculosis. I wish he had given us one more bucketful and told us of the futility, sometimes even cruelty, of submitting advanced but chronic patients to the winter conditions of a sanatorium as usually arranged. Three factors cause the evil to continue: (1) Lack of other accommodation of a more suitable type. (2) The urgent need to "do something" for patients found in overcrowded homes, or with inadequate care available. (3) The pathetic trust of the average patient that the "doctors know best" and that his part is to "stick it." He realizes also that the assets of good food, good care, and good fellowship go far to counteract the discomfort.

Two solutions of the problem are possible. Central heating should be installed, in a part at least, of every sanatorium. (The whole of the institution which Dr. Dickson controls has been heated.) Some of the Poor Law infirmaries that will be available shortly for local authorities should be utilized as tuberculosis "hospitals" for chronic cases. To achieve reform, however, there must be a more general realization that the present system is not desirable.—I am, etc.,

Peppard Common, Oxon., March 11th. ESTHER CARLING.

OPERATIVE DEATH RATES AND PROFESSIONAL OPINION.

SIR,—A perusal of Sir John Thomson-Walker's admirable third Lettsomian Lecture, in which he shows that the general operative mortality of prostatectomy is just on 20 per cent., has set me wondering how it is that a death rate as high as this should be accepted by the profession with relative equanimity when a similar death rate in the case of Wertheim's operation for cancer of the cervix evoked much adverse criticism. It is the more puzzling because prostatectomy is carried out for a disease not necessarily fatal to life if unoperated upon, whereas Wertheim's operation is performed for a disease the mortality of which, untreated, is cent. per cent. It is true that death rates much lower than 20 per cent. have been recorded for both operations—for instance, that of St. Peter's Hospital, London, of 9.9 per cent. for prostatectomy, and my recent figure of 8.1 per cent. for Wertheim's operation—but these do not affect the question I raise.

It may be argued that prostatectomy cures those whom it does not kill, or a large proportion of them, whereas Wertheim's operation only cures (meaning by that five years' survival) 50 per cent. of those recovering from the operation, but I think the real explanation of the different attitude that obtains towards these two operations lies in the fact that one is carried out on men and the other on women, and that sentiment therefore weights one scale. I am very far from complaining of this, but it is interesting to speculate whether the scales would be weighted differently if women instead of men made up the bulk of the profession.—I am, etc.,

London, W.1, March 15th. VICTOR BONNEY.

RETROGRADE NEEDLING.

SIR,—Dr. Pridie in his article on March 1st (p. 380) indicates several points in radium technique which are not sufficiently appreciated. The ease with which radium needles can be inserted into the tongue through the floor of the mouth makes one wonder that this method of approach, especially for cancers at the base of the tongue, is not more commonly employed. In the *Medical Press and Circular* of December 8th, 1915, I described "direct needling" of the tongue by this route. The length of my platinum needles is varied by screwing extension pieces into the radium-containing barrel; different areas are radiated by pulling out the elongated needles, which are marked in centimetres, and shortening them as required. I bury threaded short radium or radon tubes and needles with the aid of a trocar, cannula, and ramrod. Radiating several areas by one insertion of a needle, besides being economical, is less severe on the patient.—I am, etc.,

Dublin, March 10th. WALTER C. STEVENSON.

OPERATION FOR VISCEROPTOSIS.

SIR,—Mr. W. H. Carson's paper on indigestion from the surgeon's standpoint (March 8th, p. 429) was interesting, but I would like to ask him a question with regard to his statement that "visceroptosis is not a disorder likely to benefit from surgery." Has Mr. Carson personally performed the "anchoring operations" which he condemns? If so, has he followed up his cases over a considerable period? If he has, it would be interesting to see his figures; if he has not, one cannot think that he is justified in his statement.

There is a prejudice against anchoring operations, no doubt because many types of operation are not successful; but my experience, and that of others, is that Waugh's operation performed conscientiously gives good results. Carslaw's article in the *British Journal of Surgery* of April, 1928, gives results over a period of seven years which are eminently satisfactory. The statement that these operations are of no use seems founded more on tradition than on scientific investigation of the facts.

Mr. Carson also takes the common course of blaming the patient for undergoing a series of operations. Surely the people who perform the operations are more to be blamed! Either the patient is a genuine neurotic and should never have been operated upon—a rare type, I