TABES:

ITS EARLY RECOGNITION AND TREATMENT.

To the Editor of THE LANCET.

SIR,—Dr. E. F. Buzzard, in a valuable article with this title, published in THE LANCET of Jan. 8th, states:—

"We are all asked 'How long must I go on with treatment before I am cured?' For many years my answer has invariably been, 'For the rest of your life.' I am never consulted about a primary chancre, but if I were my advice would be the same."

The statement is a pessimism not calculated to encourage prompt diagnosis and zealous early treatment, and should not pass unchallenged. On the contrary, I venture the dogmatic assertion that if a patient first comes under treatment while the blood is still negative (it is here the Wassermann test is of prime value) and receives the usual course of 2·6 g. of "606" and 8 gr. of mercury, the disease will be "knocked out." I shall be contradicted, but if I am not telling the whole truth, it is near enough for propaganda purposes. Personally I know of no exception and in the army I discharged such cases with much assurance. Unfortunately all trace of these men is lost, but surely by now there is sufficient material available to settle this important point and to define the rare exceptions which others may have to relate, where a full course has been given early and yet failed to eradicate the disease. The pathology of both gonorrhœa and syphilis teaches us that nothing can or ever will compensate for damning delay in diagnosis, but with early diagnosis one may be altogether optimistic concerning the ultimate result.

I am, Sir, yours faithfully,

Leicester, Jan. 9th, 1921.

JOHN DONALD.

ORAL SEPSIS,

WITH SPECIAL REFERENCE TO CHRONIC DENTAL APICAL INFECTION.

To the Editor of THE LANCET.

SIR,—I find myself in agreement with much that Dr. Stanley Colyer says in his letter in your issue of Jan. 8th. It was not my object, however, to give an account of the history or literature dealing with oral sepsis or dental radiography. Had this been so I should have quoted the words of Rush, an American doctor, who, in 1801, only a quarter of a century after the Declaration of Independence, wrote: "I have been made happy by discovering that I have only added to the observations of other physicians in pointing out a connection between the extraction of decayed and diseased teeth and the cure of general diseases." Dr. Colyer will have noted that in my paper I referred to Dr. William Hunter as the pioneer in this field of research in England. Dr. Colyer appears to miss the purpose for which the paper was written, which was, as I stated, "to draw particular attention to one special part of the problem, that of chronic focal apical infection." For many years most of us have been preaching "the gospel of the clean mouth," but what I wished to emphasise was that the diagnosis of a clean mouth is impossible either by the doctor or the dentist without the aid of the radiographer. I submit that this fact is only beginning to be generally recognised in this country, and that a dental radiographic investigation does not form a part of the routine examination of the patient here as it does now in America. Further, the utilisation of radiography by the dentist for the many purposes for which it is helpful is in England the exception, in America the rule.

My experience is "small"—not with reference to oral sepsis in general, but merely in those cases with apparently clean and healthy mouths, revealing after radiographic and bacteriological investigation active foci of infection in the alveolus. Both doctors and dentists have been slow in grasping the importance of chronic dental apical infection and have not availed themselves of this method of diagnosis to the extent that they should. In my paper I asked the question, "Is simple extraction adequate?" I am sure your readers, like myself, would welcome Dr. Colyer's opinion on this point.—I am, Sir, yours faithfully,

Harrogate, Jan. 8th, 1921.

W. BERTRAM WATSON.

DILUTE SULPHURIC ACID IN
FURUNCULOSIS.

To the Editor of THE LANCET.

SIR,—I have read with much interest the "Dissertation upon Carbuncles" by Dr. Sidney Phillips in your issue of Jan. 8th, and agree with much that is contained therein; but when I read that among other internal remedies for this complaint he has found "dilute sulphuric acid" of *no good to him* I can only suppose that either he has not given it a sufficient trial, or that he has not used *dilute sulphuric acid* of the right strength. The proper strength is 10 per cent. of the strong acid, and the dose is 25 to 30 minims of this mixed with 2 ounces of water every four hours. I have now been in practice over 50 years, and have used this remedy with 100 per cent. successes for the last 40 years. If Dr. Phillips cares to know what this remedy can do I would refer him to a communication to THE LANCET of March 15th, 1913, also to the *British Medical Journal* of August 15th, 1908. Letters received from all parts of this country, and also reports from India, confirm my statements regarding this treatment. Lastly, the ordinary B.P. acid sulph. dil. is too weak to be of any use, unless given in 40 *minim* doses.

I am, Sir, yours faithfully,

J. REYNOLDS, M.D. Brux., &c.

Brixton, S.W., Jan. 10th, 1921.

THE CULTURAL DIAGNOSIS OF ENTERIC IN
INOCULATED INDIVIDUALS.

To the Editor of THE LANCET.

SIR,—Several recent numbers of THE LANCET have contained references to my article of Oct. 30th, 1920, on the Myth of "Atypical" Enteric Fever. It would be impossible to deal adequately with all of these in the form of a letter, and there is only one to which I wish to reply now. Dr. J. C. G. Ledingham, in THE LANCET of Jan. 8th, very justly questions my statement that "the difficulty of recovering enteric bacilli from the blood of inoculated soldiers is well known, even in the early stages of illnesses which clinically resemble enteric." This statement (especially having regard to the context) conveys the impression that I believe that in inoculated subjects enteric fever may exist without bacillæmia, or with a bacillæmia reduced in degree or duration or both, and Dr. Ledingham says that he is not aware of any published data in support of this view. Neither am I, and I thank Dr. Ledingham for drawing attention to the point. It is another of the myths arising out of a desire of some individuals to prove that agglutination provides the most reliable means of diagnosing enteric amongst inoculated soldiers. Dr. Ledingham's figures go far to dispel it.

It is a matter of great satisfaction to me to find that so distinguished an authority should be in sympathy with the substance of my argument. The question is, as Dr. Ledingham remarks, of high theoretical and practical importance. If the myth of "atypical" enteric fever prevails anti-enteric inoculation is not an unmixed blessing. Only when the myth is finally exploded and the dust has blown away can we get a clear conception of what a wonderful scientific achievement was the protection of the British armies from enteric.

I am, Sir, yours faithfully,

Jan. 8th, 1921.

R. P. GARROW.

RELIEF ON INCOME-TAX IN RESPECT OF
SERVICE INCOMES.

To the Editor of THE LANCET.

SIR,—Towards the end of 1919 or early in 1920 the Board of Inland Revenue issued a Memorandum to the effect that civil practitioners doing work in military hospitals should be granted the same relief on their service incomes as army medical officers had received—this concession to be retrospective as far as the year 1916–17. To those whom it concerned this appeared to be a plain act of justice and it was welcomed accordingly. The question then arose, How would the

Memorandum be carried into effect? To many of us the answer seemed simple and to be the necessary consequence of the following considerations. The civil practitioner was to receive the same treatment as the army medical officer; the latter had already received a definite and easily calculated financial relief on his service income; this same relief, now to become available for the civil practitioner, was as readily calculable—then why not regard the problem as a case of excess payment of income-tax and refund the excess? The issue of the Memorandum, it must be urged, was tantamount to an admission that from the year 1916-17 the service income of the civil practitioner had been overtaxed.

On approaching the income-tax authorities on this subject we learned, to our surprise, that the matter could not be dealt with in this plain and direct manner, for the reason that, whereas the army medical officer was assessed year by year (at the special rates) on the actual amount of remuneration for the year of assessment, in the case of the civil practitioner who had included his Service income in the total income from all sources (as he was bound to do), the Service income had been subjected to the "three years' average method of calculation," and that this made all the difference. Following upon this pronouncement, and having submitted the figures for the Service incomes of the years in question, we next received a document which, as it stands, is literally a cloud of figures as appalling and confusing as a November fog. Speaking personally, I do not feel that I have the acumen, even if I had the patience and the necessary time, to subject these ranks of figures to a critical examination, and I think it likely that others will feel with me in this matter. What is to be done? For to accept results without being able to follow the steps of the argument commends itself as little, as to agree to the postponement of the full settlement of the claim for relief until the year 1922-23—i.e., seven years after the relief first came into operation; yet this is what is asked of us.

In these circumstances I propose that we examine, not the figures themselves, but the premisses on which they are based, for if it should appear that these premisses contain a fallacy the whole calculation will of necessity be fallacious. In like manner it is proposed to examine the results of the calculation, for if among these there should be found any one to which the *reductio ad absurdum* argument would be applicable, again the calculation would be invalidated. Now at the very outset the authorities contend that, as a result of the three years' average system of assessment, the amount received by the State in any given year (for income-tax purposes) is one-third of that year's income and no more, and that in consequence two more years must elapse, in order that this particular income may figure twice again in the averaging, before the State will have had the use of the whole sum. This contention is, in my opinion, a complete fallacy, for the three years' average pools the incomes of three whole years and three whole years divided by three yield one whole year's income, not one-third of a year's income. And even if we accept the ingenious way of putting it, adopted by the income-tax authorities—viz., that the figure obtained by the averaging consists of one-third of the given year and one-third each of the two preceding years (which is quite true)—we must point out that the year which *pays* for the sum of these three-thirds is the given year, and three-thirds make one whole. Therefore in any given year the State receives a whole year's income, averaged on three years. Indeed, if the year in question happens to be a lean one as compared with the two preceding years, then the taxpayer, so far from paying on one-third of his income, will pay on more than his whole year's income. The accompanying figures will show this: (1) £600, (2) £500, (3) £400 = £500 average for the three years—i.e., for year, No. 3, £100 in excess of the actual income. Of course, if the third year is a fat year the effect will be reversed. On reflection it must be apparent that the method of averaging does no more than distribute inequalities, an obvious convenience for Budget purposes. If the above reasoning be sound, the calculation is

based upon a fallacy and its outcome must be fallacious, including the contention that the tax-payer must wait three years for the settlement of his claim.

Lastly, to examine one other of the actual results of the calculation. I find that for the year 1919-20 (in my own case) it is stated there is due to me in relief for the whole period since 1916-17—i.e., for four years—the sum of £20 18s. 6d., but that, per contra, I am in debt to the Revenue to the extent of £89 8s. 5d., leaving a balance against me of £68 9s. 11d. It is true that subsequent years are to make good and that by 1922-23 there is to be an effective ultimate relief of £97 2s. 1d., though as far as I can gather it will not materialise in the form of a cheque; that, however, is not the point on which I would lay stress, but this—that in the year 1919-20, having paid in full all my income-tax demands, without any deduction whatsoever on account of my included Service incomes, this calculation finds me in debt to the Revenue to the amount of £68 9s. 11d., and this is the result of the granting of a relief! Is not this a *reductio ad absurdum*?

The Trojans were bidden to fear the Greeks "*et dona ferentes*"; is this to become our attitude of mind towards the income-tax authorities?

I am, Sir, yours faithfully,

HARRINGTON SAINSBURY.

Wimpole-street, W., Jan. 10th, 1921.

SCALDED MILK.

To the Editor of THE LANCET.

SIR,—As there seems to be some confusion as to the wisdom of heating milk for infant feeding by boiling, pasteurising, so-called sterilising, or scalding, I venture to suggest, as the result of the following experiments, that scalding the milk is quite sufficient for all ordinary purposes. One of the principal objects in heating milk is the destruction of tubercle bacilli, and this is effectually accomplished in pasteurising, boiling, or "sterilising," but if the milk is allowed to remain at 100°C. for a considerable period, the activity of the antiscorbutic accessory factor is also all but destroyed. The question therefore to which an answer was required resolved itself into the following: "Is there a period in the heating of milk at which the destruction of tubercle bacilli is assured without serious interference with the activity of the antiscorbutic vitamine?"

To settle this question I was fortunate in seeing at the Lister Institute a monkey in an advanced stage of artificially induced scurvy. So bad was the animal that Miss Harriette Chick had decided to give it antiscorbutic treatment to save its life. She kindly agreed to try the effect of scalded milk. The method of scalding was as follows. The milk was warmed in the gas flame, without stirring, till it frothed up, when it was at once removed and allowed to cool. The milk took 5 minutes to heat and 20 to cool. It was at a temperature of 90° to 100°C. for about 1½ minutes. The monkey was allowed daily 200 c.cm. of this scalded milk and in a short time completely recovered.¹ Scalded milk did not, therefore, destroy the activity of the antiscorbutic vitamine.

The next point was to discover whether scalding was sufficient to destroy the tubercle bacilli. I therefore asked Dr. F. H. Teale's assistance, and he very kindly fell in with my wish. He inoculated milk with an active strain of tubercle bacillus, scalded it as above, and injected three guinea-pigs. In due time the animals were destroyed and were found to be quite free of infection of tubercle.

These simple experiments prove two points: (1) scalded milk retains at least a considerable portion of its vitamine activity; and (2) tubercle bacilli are destroyed by such scalding.

I am, Sir, yours faithfully,

E. A. BARTON,

Medical Officer to Infant Welfare Department,
University College Hospital.

Jan. 12th, 1921.

¹ Biochemical Journal, vol. iii., p. 317.

Col. Sir W. R. Smith, V.D., T.D., and Dr. G. S. Brock, M.B.E., have received from the King of Italy the decoration of the Cross of Commander of the Order of the Crown of Italy.