for big subtertian infections with brain complications. In children and infants suffering from malarial convulsions quinine per rectum has been found satisfactory. Such is the treatment of the malarial fevers in Perak. A drug, however, is urgently needed to stop the relapses: galyl + quinine is now on trial, but too short a time has elapsed or too few cases treated for any opinion to be yet formed.

We all out here look up to the intramuscular injection of quinine as our sheet-anchor in all obstinate and chronic forms of malarial fever, and generally find we have not placed our trust in vain. With careful asepsis no bad results have generally been observed. Occasionally an abscess has developed at the site of injection, but this has been due to faulty technique and sometimes to the low vitality of the tissues. In a small hospital in Taiping with 60 beds for Europeans, Eurasians, and the better-class Asiatics, 672 intramuscular injections of quinine were given during the last 12 months; abscess formation 4, tetanus nil. The quinine is injected into the gluteal region about 1 inch below the crest of the ilium, and is always given by a European sister or by the medical officer himself. The patient's name is entered into a special book, and the operator signs his or her name and is made responsible for any bad results. Figures for the other hospitals have not been obtained. How the intramuscular injection of quinine acts I am not prepared to state; perhaps it is due to a slow, continuous absorption of the salt or to an attack in the rear, the quinine getting into the blood current of the spleen and bone marrow easier from this direction. But in whatever way the results are obtained, until some other better method or drug is suggested or discovered the intramuscular treatment of malaria by the rank and file of the profession in this State will certainly be continued.

The treatment of malarial fever by 30 grains daily for three weeks, as suggested by Captain David Thomson, is no doubt effective in a non malarious country; but whereas in the tropics reinfection is the rule, such happy results as he obtained in Liverpool cannot, I fear, be our lot. The obvious reply to this is to get rid of your mosquitoes—but that is another story.—I am, Sir, yours faithfully,

STEPHEN C. G. FOX, M R.C S, L R.C.P. Lond.,
Senior Medical Officer, Perak, F.M.S. Government
Oct. 11th, 1917. Medical Service.

THE RETRACTION OF BLOOD CLOTS.

To the Editor of THE LANCET.

SIR,—When reading Captain W. d'Este Emery's interesting article on the Retraction of Blood Clots in your issue of Oct 20th, I was surprised at the small percentage of serum yielded in many of his experiments, varying from 0 per cent. in large-diameter tubes to nearly 60 per cent. in 1 mm. tubes, his best—the 9 mm. tubing giving only 6 3 per cent. to 21 5 per cent. of serum. Later, however, with this same tubing and more suitable temperature conditions, he obtained 50 per cent. and 56 6 per cent. Captain Emery deduces that the narrow tubings of 1 mm. and 3 mm give far better results than larger diameter tubes—and that conditions of temperature are also of great importance. With the latter conclusion I agree, but my own experience leads me to doubt the former.

In the course of the last two years I have prepared comparatively large quantitives of agglutinating serum for meningococcus from rabbits and have used several hundreds of these animals in making the sera in batches of about 150 c.c. at a time. The animals are bled to death through the carotid into large sterile test-tubes of 75-100 c.c. capacity and 38 mm. diameter, or more than double the diameter of Captain Emery's largest tubes. The blood is then incubated at 37° C. for one hour, the clot separated from the tube for about three-fourths of its circumference by a sterile wire, and after remaining at room temperature for 24 hours the serum is poured off. Previously to reading his paper I had not troubled to ascertain the proportion of serum to clot which I obtained, but I felt sure when reading it that I got larger quantities by my method than his deductions would suggest were possible. Major A S. G. Bell, R.A.M.C. (late Fleet-Surgeon, R.N), has recently assisted me in preparing this serum, and, in fact, at present is doing the major portion of this work; at the first opportunity, therefore, we weighed the clot and serum of a rabbit bled by this method and found that we had obtained about 76 per cent. serum and 24 per cent. clot. This result was sufficiently surprising, and we therefore measured a series of other bleedings with the following average results.

Twenty-two rabbits produced 672 c.c. of serum and 414 grammes of clot, or about 64·2 per cent. of serum and 35·8 per cent. clot by volume. It should be noted that these bloods were collected in the agar-lined tubes recommended by Dr. Gardner, and which tend, as Captain Emery shows, to prevent a maximum output of serum; nevertheless, the average percentage of serum from over a litre, in the aggregate, of blood is higher than the best figure Captain Emery gives. The 76 per cent. serum from the first rabbit was got from a plain tube without agar, and four rabbits were therefore bled into plain tubes, with the result that these five rabbits gave 188 c.c serum and 104 grammes clot, or 66·7 per cent. and 33 3 per cent. respectively by volume. The total for 27 rabbits, therefore, is 64·7 per cent. serum and 35·3 per cent. clot.

Captain Emery does not say whether the percentage is by weight or volume, but I show it by volume, taking the specific gravity of clot as 1100. I noticed some time ago that the yields of serum from different rabbits varied considerably, and this I attributed to the known disproportion which exists between corpuscular and fluid contents of the same rabbit's blood at different periods—i.e., a variati n in fluid percentage. We generally find a better yield of blood shortly after the morning meal, and accordingly try to kill our animals in the forenoon. Captain Emery used human blood which does not show this variation, and this may possibly account for the disparity in our experiences. One thing, at least, is certain, that to obtain large quantities of serum by using small tubes is impracticable, for in order to procure 100 c.c. of serum, using Captain Emery's figures, some 230 yards of 1 mm. tubing, or about 50 yards of 3 mm., would be required. Some other means of obtaining maximum yields must therefore be found, and the method we are now using seems to give satisfactory quantities.

I am, Sir, yours faithfully,

T. G. M. HINE,
Major, R.A.M.C.
Central C.S.F. Laboratory, Caxton-street, S.W. Nov. 23rd, 1917.

THE VALUE AND LIMITATIONS OF SANATORIUM TREATMENT.

To the Editor of THE LANCET.

SIR,—Of the many fallacies which beset the observer of tuberculosis statistics, not the least is the prevailing but inaccurate notion that sanatorium treatment is of itself a complete treatment of pulmonary tuberculosis and that ultimate mortality rates are an index to its value. It is true that the duration of life after treatment might afford a fair criterion of the efficacy of such part treatment, but mortality rates prove nothing about the results of residence in a sanatorium except that a short stay has not achieved the impossible. After all one does not estimate the value of a "preparatory" school training by the numbers of old boys who, say, 15 years later receive an honorary LL.D.

The reason of this fallacy is clear: most of the sanatorium results on which current tuberculosis traditions were based were originally obtained from isolated institutions, either private or endowed, in connexion with which preliminary treatment was rare and after-treatment unheard of. The cases might or might not be chosen from artisan populations, but at all events they were chosen without any definite relation to the numbers of consumptives in any one district, county, or country. Now what possible light can these figures shed on the mortality rates or duration of life rates for the consumptive working population as a whole? Speaking as a past member of the staff at Midhurst, I cannot see that the Midhurst results prove anything about tuberculosis except that in variously selected cases the results vary. The patients were chosen from different sections of society and from different parts of the country, the bacillary cases were more often advanced than early, and from one-third to one-half of the non-bacillary cases were at any rate arrested on admission if not negative. Yet figures from these and similar institutions are freely used to form the basis of deductions as to the value of the sanatorium for consumptive workers, of whose treatment it only forms a small part. Is it not time that such statements should give place to the reports of tuberculosis officers, whose schemes, either rural or urban, give a comprehensive idea of the incidence, treatment, and after-results for any one county or city area ?

Perhaps, therefore, I may be pardoned the rôle of showman in again drawing the attention of your readers to the results obtained at the Birmingham Tuberculosis Centre. An illustration :—

A consumptive in Birmingham is no sooner notified than he is called up for examination, classified, and if found positive is sent to one of the many sanatoria and hospitals set aside for early, moderately advanced, or late cases respectively. During his absence various preventive measures are taken and all suspects or contacts dealt with. On his return he may continue treatment at the centre for as long as 18 months to two years; later he may return to a sanatorium for shorter spells of treatment if neecssary, in fact, from the time of his notification he is shepherded until his recovery or death. Yet although patients may be kept on the books for three, five, seven, or ten years, there is no lack of sanatorium beds in Birmingham, no long waiting list, and the results for *all* cases, both immediate and afterwards, are far more gratifying than those of any sanatoria or semi-detached schemes, whether for private or public patients, so far noted in Britain. To take one point only—i.e., "bacillary loss"—in the Birmingham Sanatorium set apart for early cases, 57 per cent. lost their bacilli during 1916, whilst during the same year of 555 *completing* treatment at the centre after from six months to two years, 70 per cent. had lost their bacilli (as tested by sedimentation methods) and this in spite of a prolonged return to ordinary working and social life.

The fact is that with a certain type of bacillary case in artisan populations a relatively short stay in a sanatorium, with after-treatment at a dispensary, is all that is necessary to secure complete arrest. It is not suggested that this scheme is perfect; there is ample scope for modification and improvement, but the point I should like to insist on is that before dogmatising on the duration and results of treatment, before comparing the results in wealthy and artisan communities, and, above all, before considering whether sanatorium treatment "has utterly failed," or "partly failed," or succeeded, it would be well to ascertain whether, in fact, every county or large town has got a passable scheme, and, if not, to insist on the establishing of one. Five years afterwards the discussion on results might be reopened with profit.

I am, Sir, yours faithfully,

EDWARD G. GLOVER, M.D. Glasg.

Birmingham Municipal Sanatorium, Cheltenham, Nov. 23rd, 1917.

SIR RICHARD CROFT AND THE CASE OF THE PRINCESS CHARLOTTE.

To the Editor of THE LANCET.

SIR,—May I trespass a little further upon your space by stating how it was that Sir Richard Croft left Oxford without a degree? The explanation is given in the *Gentleman's Magazine*, vol. lxxxviii. Practising in the University city at that time was a well-known physician, Sir Charles Nourse. This gentleman had given notice of retiring. To succeed him Croft went to Oxford as Nourse's assistant. Soon after Croft's arrival, however, Nourse changed his mind and Croft returned to London.

I am, Sir, yours faithfully,

Holland Park-avenue, W., Dec. 9th, 1917. S. D. CLIPPINGDALE.

SUCCESS AND FAILURE IN THE TREATMENT OF TUBERCULOSIS.

To the Editor of THE LANCET.

SIR,—May I beg a few more lines of your space to answer, as shortly as I can, two questions which Dr. W. M Crofton puts to me in his courteous reply to my letter in your last week's issue. Dr. Crofton asks, first, whether I think it possible by isolation to cut off entirely the sources of tuberculous infection. I do not. Leaving on one side the bovine sources of infection, I could only hope to diminish by partial isolation the number of sources of infection and thus help on the disappearance of tuberculosis, just as, we learn on the authority of Robert Koch [1] and Sir Arthur Newsholme,[2] was done in the case of leprosy in Norway. If tuberculosis continues to decline, as surely it must when happier times return, we may look forward to the isolation of a continually increasing proportion of dangerous cases, and consequently an effect which will grow year by year until it succeeds in stamping out the disease.

Dr. Crofton next asks whether I do not think his "proposal to supplement this measure by an attempt to produce by immunisation a normal resistance in everyone to the tubercle bacillus worthy of consideration and trial." Candidly, I do not. I have already given my reasons at length, and can only add that tuberculosis is not one of those diseases which confer any marked immunity, like so many others (hence its chronic course and bad prognosis when well established). We have received little encouragement from immunisation experiments in animals; and it is necessary that public effort should be concentrated on the one main thing and not be side-tracked by others of doubtful utility.

I am, Sir, yours faithfully,

Cambridge, Dec. 10th, 1917. LOUIS COBBETT.

[1] Nobel Lecture, THE LANCET, 1906, i., 1449.
[2] The Prevention of Tuberculosis, p. 265.

COLLOSOL COCAINE.

To the Editor of THE LANCET.

SIR,—In your issue of Dec. 1st appears an article under the above heading by Dr. G. Barger, Dr. H. H. Dale, and Miss F. M. Durham (from the Department of Biochemistry and Pharmacology, Medical Research Committee), referring to statements published in THE LANCET and the *British Medical Journal*, embodying reports on this preparation by Professor R. Tanner Hewlett, Professor W. J. Simpson, and Dr. John Eyre, who found marked absence of toxicity and duration of anæsthesia in the samples submitted to them by us.

Scientific controversy on the difference of findings between these authorities must be deferred, as space will hardly be available for it together with the serious matters affecting personal honour and confidence which arise from that article, containing as it does, statements of fact and insinuation and indication of procedure which concern not only the persons above-named but also everyone connected with the "makers." Important omissions of material circumstances render it still further misleading. Had the authors of the article—as might be gathered from absence of mention to the contrary—purchased or otherwise acquired in the market, standard samples of collosol cocaine, for examination, their procedure and conclusions (if accurate) might merit justification, but as they omit to disclose the fact that they used various samples obtained from us, under circumstances which I mention later, a false impression is induced. Such impression is cumulatively aggravated to serious import by five separate remarkable comments in the article.

Firstly, there is mention of "a trap in which experienced observers seem to have been caught." True, this is followed by the remark, "We wish to make it clear, however, that we have no reason whatever to doubt the bona fides of the manufacturers"; and Sir William Crookes and the other scientists connected with the Crookes Laboratories will doubtless appreciate this testimony to the integrity of "the manufacturers," even at the possible expense of their intelligence.

The hypothetical criticism appearing in the article as the inspiration of the authors concerning "the other collosol alkaloids" should be tested in the light of the fact that I informed Dr. Dale at the outset that only an infinitesimal portion of the quinine used was found to be recoverable from collosol quinine by laboratory tests, and had he not specifically declined to examine anything other than the cocaine preparation, I would have emphasised the information, already given to members of the medical profession, that collosol quinine has no effect upon the parasite of malaria. The effect produced by publication of the comment without reference to our disclaimer, is obvious.

In the next place, the three authors of the article are responsible for the extraordinary and wholly inaccurate statement that the manufacturers "have withdrawn collosol cocaine from their list on hearing from us of our conclusions." No such withdrawal has been made, nor do the clinical results warrant it, but in fact we had no opportunity of even seeing—let alone considering—those "conclusions," till they appeared in THE LANCET article in which this statement as to withdrawal is made, whilst no data have even yet been furnished us to lead to such a course, nor to refute the detailed tests and reports of Professor Hewlett, Professor Simpson, and Dr. Eyre. Further, this definite statement of withdrawal appears simultaneously with a letter from the secretary of the Medical Research Committee in the *British Medical Journal*, which indicates that he, at any rate, had reason to doubt its accuracy, from perusal of my letter to that journal of the previous week, and he more accurately states that Dr. Dale