for the reception of curable cases, and even those that are suitable in themselves do not always provide the essentials for curing early cases; the existence of such institutions naturally brings discredit on the whole treatment.

No one I suppose would recommend commencing to isolate scarlet fever at the end of the second week, yet it is just as foolish to imagine that isolating what are better called end cases will produce any diminution in the incidence of the disease. Pulmonary tuberculosis is a disease which usually lasts for months or years, and is often more infectious in the early stage than later. If a case is really an advanced one the expense can only be small if the patient lives for a short time; isolating at this stage will only prevent infection for a few weeks, and therefore will produce no result. If, on the other hand, by advanced case is meant a chronic who may live for months and who placed in an institution may live a great deal longer without any prospect of permanent improvement, then the expense becomes prohibitive. Furthermore, every penny spent at the wrong end of the scheme takes money from the treatment at the right end, and in addition as all sorts of spurious promises are held out to these hopeless cases in order to persuade them to go into institutions discredit is thrown upon the whole treatment. The most feasible way of combating the disease would seem to be to leave alone, except in so far as nursing is possible, all advanced cases, and commence with the early case; if, for instance, the first case from a family be taken at once and placed in a sanatorium, not only would the patient receive the maximum benefit, but the remainder of the family would be saved from becoming infected. It seems recognised by all that this is a very difficult disease to cure, the reason being that no effort is made to treat it effectually until it is hopeless. That this feature of the case has not been overlooked by all may be gathered from a quotation which I have already made in THE LANCET of April 3rd, 1909, in a paper on Graduated Rest, from a lecture delivered by Professor Niemeyer, of Tübingen, at least as long ago as 1866. He makes the following observations in the course of other remarks on treatment:—

In the first case strict measures are as a rule considered useless and their enforcement unnecessary. The patient is allowed to follow his employment until the rising pyrexia and the increasing debility force him to remain at home or until hæmoptysis, pleuritic pains, the appearance of dullness over the chest urgently demand that the supposed "secondary" pneumonia and pleuritic processes be submitted at least to a symptomatic treatment. It will not be said that I exaggerate—let us be sincere! Is not this *laissez aller* practice founded on reasoning of everyday occurrence with regard to the affection under discussion? It is seldom that a medical man reproaches himself for having by his carelessness been guilty of the development of a phthisis florida. It is true even in case the physician should have fully recognised the nature of the disease, he will not be able to cut it short at once, but still he has every reason to give the strictest orders, he must do everything in his power to protect the patient from injurious influences; and to place him under such conditions as may be expected to put a stop to the further extension of the pneumonic process; and prevent its termination in cheesy infiltration and disintegration of the lung tissue. Let any one make the experiment of having patients with the supposed signs of commencing pulmonary tuberculosis strictly confined to their beds for a time, forbidding them all unnecessary talking and coughing. and he will soon satisfy himself that many a patient gets well who would formerly have been assumed to be the subject of tuberculosis.

With the exception that fresh air is not mentioned this is a very good account of what sanatorium treatment should really be. On meeting with a chronic or with an advanced case, inquiries should always be made as to whether this treatment had been tried at the proper time, and if not, why?—for at least as great discredit should fall on the medical man for omitting this treatment, if the indications were clear, as for neglecting to have an acute appendix operated upon. Of course, we know now, though many seldom act upon the knowledge, that the pre-tuberculous stage is really always tubercle.

If early cases are to be obtained it is highly necessary that sanatorium accommodation, however small in amount, should be suitable. A sanatorium should fulfil the following requirements: (1) It should be a place where patients not manifestly ill can be sent; (2) the patients should be kept in bed so long as they have not a normal rectal temperature (97.7° F. in the morning), this period will usually be short; (3) bad cases should not be mixed up with good, especially on the plea that it is impossible otherwise to get them to go to an institution; and especially (4) it should be clearly understood that on leaving the patients are not expected to require after-care, and that they will be able to return to their previous vocations. It is doubtful if it is worth while attempting to cure a disease if, after treatment, the patient is unable to live a normal life and cannot look after himself. Anyone who has his mind full of after-care must also have his mind full of a very unsatisfactory type of case. The object of treating these patients thus is threefold: (1) to prevent the infection of families and the public, early cases with a small amount of sputum may be very infectious; (2) to instruct the patient how to treat himself in case of relapse, and how to avoid infecting his family should he remain or become infectious; (3) to prevent the formation of chronics who are the chief carriers of the disease.

It is not to be assumed that a chronic will necessarily die from consumption; indeed, the danger of infection would seem to be proportionate to the length of the duration of the disease. Finally, I should look very critically upon any scheme for attempting to place these patients in industrial colonies, a proposition which in itself admits incapacity to cure.

I am, Sir, yours faithfully,

EDWARD E. PREST.

Ayrshire Sanatorium, New Cumnock, Feb. 26th, 1916.

THE BACTERIOLOGY OF CEREBRO-SPINAL FEVER.

To the Editor of THE LANCET.

SIR,—The report of the Special Advisory Committee upon bacteriological studies of cerebro-spinal fever during the epidemic of 1915¹ is one of the most inspiring and important pronouncements that have been published on the subject, and will be read with the greatest interest by everyone who has worked on the subject. I take it that by the publication of this report the matter has been thrown open for discussion, and one is therefore justified in meeting as far as possible one or two of the criticisms made therein.

Firstly, in regard to the rapid agglutination test which I put forward for the recognition of the meningococcus. It is, I believe, a reliable test, but

¹ See THE LANCET, Feb. 26th, 1916, p. 471.

it has been criticised by the committee under a misapprehension. This I desire to remove in order that the method may not suffer from what is an undeserved stigma. They say:

That this method is rapid cannot be doubted, but its reliability is another matter even apart from the difficulties introduced by racial agglutinability. A method must be judged by its results, and in Dr. Crowe's hands it seems to have led to a considerable percentage of error, if we may judge from Major Gordon's criticism of the inordinate number of positive contacts segregated at Devonport, a question to which we shall return later.

The method was evolved after the contacts had been discharged, and was not in use when testing the organisms derived from these contacts. The research laboratory fitted up by the War Office was not in working order until after the camp was broken up, hence the "judgment by results" will, I trust, be suspended until the work of the present season can be taken as the criterion.

The second criticism to which I wish to allude is contained in the same sentence quoted above. Again, I would request that the present season's results may be brought under review before my medium is condemned on account of the high proportion of positives found by me. In their statistics the Special Advisory Committee felt bound to exclude these as doubtful in view of the criticism by Major Gordon. I would not for a moment object to this criticism were it put forward by the Special Advisory Committee as their own. It is, however, founded on that of the Millbank observers; and evidence from still other observers, as I will shortly show from the report itself, tends to justify my disputed findings.

In the first place considering the conditions under which these contacts were segregated I do not think it surprising that large numbers of them were positive. That some were held who had organisms in their throats resembling meningococci, but which were not meningococci, is extremely likely, and the reason for this will shortly be clear, yet I do not think that the proportion was a large one. Contacts were segregated in crowded hutments and, owing to the bad weather, frequently in close confinement, and this segregation was enforced before and not after the men had been swabbed to exclude the non-infected. Now the report itself shows that under circumstances of overcrowding as many as 66 per cent. may be found to be positive. Again, very intense methods were used to try to cure the carrier condition. In the report of Dr. Colebrook's work it is plain that such methods result in an extremely obstinate carrier condition. There is thus no inherent improbability in my findings, for carriers would rapidly infect their neighbours, as was shown by me to occur (*vide* report) owing to close segregation, and would remain positive for a longer time by reason of excessive treatment. The surprising thing is that after several weeks of such surroundings any of my contacts were found to be negative.

On studying the report one finds that the Local Government Board obtained from 10 to 13 per cent. positive among non-contacts, whilst among military contacts the total percentage worked out at only 6.5 per cent. We learn also that the greater amount of these were obtained by the Millbank method. This percentage (6.5) excludes mine at the Devonport camp, and, of course, quite properly so. On comparing the Local Government Board figures with those of the military, one is forced to conclude either that the Millbank method is inadequate in finding positives, or that the Local Government

Board workers, as is imputed to me, find meningococci where none exist. Or we are left with the paradox that contacts give a lower percentage of carriers than non-contacts, from which it would appear that the way to avoid becoming a carrier is to mix as far as possible with cerebro-spinal fever cases, a *reductio ad absurdum*. I cannot be certain as regards my positives because, as the Advisory Committee justly say, all confirmatory tests were abandoned. This was, however, through force of circumstances, owing to an only partially trained laboratory staff and press of all sorts of other work. But neither in the Millbank method could I perceive that any confirmatory tests were made use of, if we exclude the 23° C. test, shown since to be unreliable.

To put the matter succinctly, if I may say so, the Special Advisory Committee, of course quite unintentionally, have given full weight to the Millbank criticism that my medium errs in finding too many positive contacts, but without laying equal stress on the comments of other workers in regard to the medium sent out from the central laboratory.

I am, Sir, yours faithfully,

Military Hospital, Devonport, Feb. 24th. H. WARREN CROWE.

THE SIGNIFICANCE OF CERVICAL CAPILLARY MARKINGS.

To the Editor of THE LANCET.

SIR,—In the recent correspondence on the "New Sign in Pulmonary Tuberculosis" I attempted to direct attention to certain red surface-markings in the neighbourhood of the upper dorsal vertebrae in cases of pulmonary tuberculosis. As none of the other correspondents wrote about them, and as I am unable to find any accurate reference to them in any work on pulmonary tuberculosis, I take the liberty, with your permission, of again directing attention to them.

They may be seen as bright or dark red markings. Sometimes they are isolated straight lines, few in number. At other times they have a Y-shaped appearance set close together or scattered. Some cases show a distinct irregular circular form, almost like the spokes of a wheel radiating from a centre to a periphery. They suggest capillary obstruction or congestion, and as such have no definite form. They are situated most frequently on one or both sides of the seventh cervical and first dorsal vertebrae. They may occur on one side only. They may extend unilaterally or bilaterally as far as the third or fourth dorsal vertebrae. Sometimes the circular form is found on one side only, or embracing both sides of the vertebrae, or a few lines may be found about the seventh cervical, and there may be a circle below.

These markings are an abnormality, and though I have only seen them in association with pulmonary tuberculosis it is quite possible they may occur in association with other diseases, but the fact has yet to be recorded. They are to be noticed at every age from 2 years to 50, and perhaps at a further age. Statistics which I have collated show that 80 per cent. of the supposedly tuberculous outpatient children under 5 years of age have the markings. Of children between 5 years and 15 years of age 70 per cent. showed the markings. These include outpatients and tuberculosis dispensary patients, the 100 per cent. being notified cases. Of the tuberculosis dispensary patients over 15 years of age, 56 per cent. of them have the markings and 44 per cent. have no markings. Of these dispensary patients, 15 have other lesions in addition to their pulmonary lesion. Seven show markings and eight