heart; and on the other hand the general impressions of an intelligent man, of which doctors hold no monopoly. Indeed, there is remarkably little evidence that doctors can select their own staffs with any more wisdom than is to be found in any other profession or trade. It is still usual, even among psychiatrists, to select men on the basis of their records and behaviour during interview, and one hopes this will continue to be the case and that employment in future will not be restricted to those who can pass unscathed through a searching psychiatric interrogation. In this particular case, selection from among the medically fit should be undertaken by the Civil Service Commissioners themselves or their lay agents in the traditional way; the responsibility should not be accepted by medical men and the choice invested with a spurious authority that will not stand the test of time.

The second point, and separate from the question of our capacity to select, is the rightness of providing such a report for the perusal of a lay body. If the candidate knew exactly how full the report was to be and to whom it was to be transmitted, no question would arise, since communication would be with his consent; but clearly such a report could have little value, for few candidates would be so unwise as to disclose information which would prejudice their chances. It seems improbable, too, that the Commissioners would desire the candidate to be in possession of such information. If he were not, then to me it seems clear that communicating such a report would certainly be wrong, and possibly unlawful, though perhaps there is no judicial decision on this point. Certainly the matter should be clarified, and since your annotation (Aug. 16, p. 260) suggests a doubt as to who would make such a ruling, I would submit with respect that there are at least two possibilities. One is that the Royal College of Physicians should pass a resolution similar to that passed to guide the practitioner faced with a case of criminal abortion. This resolution, even if it has not the force of law, carries a weight of authority that has even earned judicial recognition and acceptance. Secondly, I think it is true that the General Medical Council has jurisdiction conferred upon it by the Medical Act of 1858 which covers certificates or reports which are untrue, misleading, or improper. It is difficult to imagine anything more improper than this.

It is perhaps unnecessary to dwell on the fundamental unpleasantness of the whole business—the searching investigation, the furtive “confidential” report, the secret filing, and possibly the interruption of a career by misinterpretation or overweighting of technical terms by those not equipped to understand them. Perhaps the words of Jeremy Bentham, used in a slightly different context, are relevant:

“In the darkness of secrecy interest and evil in every shape have full swing. Only in proportion as publicity has place can any of the checks applicable to judicial injustice operate. Where there is no publicity there is no justice. The security of securities is publicity.”

It is to be hoped that in this matter the lead which Dr. Curran has given will be widely followed, and that what Lord Hewart several years ago spoke of as departmental aggression and bureaucratic pretension will not cause us to forget that we have professional responsibilities as well as privileges.—I am, etc.,

Bristol.

GERALD GARMANY.

SIR,—All will agree with Dr. Desmond Curran (Aug. 2, p. 187) and Dr. Edward Glover (Aug. 16, p. 269) in their objection to submitting full psychiatric reports to laymen. Dr. Glover, however, objects to the application of wartime selection methods to civilian life, and implies that betrayal of confidence is a necessary part of the method. In my experience this is not so.

Having been busily concerned with the application of these methods in Army Selection Centres during the last three years of the war I missed Dr. Glover's earlier warnings, so am perhaps at a disadvantage. However, I found nothing to offend the professional code. Psychiatric reports were confidential documents addressed to the senior medical officer, as were the reports of medical, orthopaedic, or any other specialists. Advice to the laymen concerned with the soldier's employment was limited to a brief positive or negative recommendation.

There seems no reason why some such method, applied without compulsion, should not be used to make the most of the nation's manpower during the present crisis. Most of those who have had experience of the method and have been able to study the results objectively are convinced of their value. Many who have benefited from its application in their own cases are of the same opinion.—I am, etc.,

St. Albans, Herts.

C. GUY MILLMAN.

The B.M.A. and World Organizations

SIR,—In his letter (Aug. 9, p. 228) Dr. W. H. Spoor asks several questions about the World Medical Association and its relationship with the B.M.A. He is afraid that the W.M.A. will establish a dictatorship to which the B.M.A. will be subordinate. This is a travesty of the facts. The W.M.A. will be a means for the various national medical associations to exchange information, to confer, and to endeavour to bring the opinion of doctors all over the world into a focus and to present that opinion to the World Health Organization and to Unesco, which are organs of U.N.O. on the Government level.

The W.M.A. will have no powers whatever over its constituent bodies other than those of persuasion. Its power in the world will be commensurate with the degree of unity it can influence amongst the doctors of the world, and here its possibilities are great. In view of the long-established ethical standards of our noble, learned, and liberal profession, that power, if attained, should work wholly for the good of humanity.—I am, etc.,

Weymouth.

J. A. PRIDHAM.

Leprosy and its Problems

SIR,—The leading article under the above title (June 7, p. 813), in which my name (misspelt as Moisier) was mentioned in connexion with a new theory that cockroaches carry lepra bacilli, suggests that an exposition of this theory may be of use not only to the medical profession but also to the public through professional influence.

Leprosy is indeed a mysterious disease, but all are agreed that an acid-fast bacillus (*Mycobacterium leprae*, or Hansen's bacillus) is the causal agent, though it does not comply with a single one of Koch's postulates. May the mystery not be due to the fact that we have taken for granted that the disease is directly infective from man to man, most probably by contagion? I submit that this is mere supposition. There has never been any proof of the contagion theory. All along opinion has been divided between the contagionists and the non-contagionists—and there is no doubt that at present the former are in possession of the ball. But even they have to admit that the actual mode of transmission is still unknown, so that their theory is not founded on proved facts.

First, the rarity of conjugal infections in leprosy must have struck every worker, and seems to negative the transmission theory. Secondly, if the disease is directly contagious, and is caused by Hansen's bacillus, how can we account for the failure of almost all inoculation experiments? Surely this suggests that the bacillus is not infective when passed from man to man, but that an intermediate host is necessary, in which development must take place in order to render the bacilli infective. Thirdly, if the disease is contagious, how is it that so few doctors, nurses, and orderlies in hospitals have become infected? Few precautions are as a rule taken. Fourthly, how can we explain that so few patients can give a history of contact? In my own experience, no less than 60% of native patients come into this category.

I have become more and more convinced that the mode of transmission must be sought elsewhere—namely, by insects. Cockroaches seemed the most likely, as they exist everywhere and are of ancient origin. My investigations apply only to Southern Rhodesia, Kenya, and Uganda, but I think the results should be widely known, and others induced to carry out their own investigations in other parts of the world, so that we may be enabled to take the correct precautions against spread of the disease, and even to eliminate it altogether. Isolation of the patient, whether voluntary or compulsory, has not hitherto been effective, and cannot be claimed to have eliminated the disease from any part of the world at any time.

But suppose we can pin the blame to the cockroach. That would explain the failure, for we have left the infection behind in the

house of the patient, and leprosy is so obviously a house disease, especially in poor squalid overcrowded huts, where in warm climates cockroaches abound. From one African hut no fewer than 2,500 cockroaches were caught in one night! It is not generally known that cockroaches bite man, and in doing so remove a piece of skin, which they swallow, leaving a small bleeding wound which later becomes a characteristic whitish depressed scar lasting for many months. Neither my African assistants nor I have ever actually seen a cockroach in the act of biting, but there is not the slightest doubt about it. The natives have reason to know the bites well.

In my leprosy hospital these wounds and scars have repeatedly been found to contain Hansen's bacilli, even when the organism could not be demonstrated elsewhere. Children are much more commonly bitten than adults, which probably explains why the disease generally develops in them.

Within the hospital about 23% of cockroaches taken from huts occupied by lepromatous patients are found to contain masses of Hansen's bacilli. Outside the hospital it is not usual to find acid-fast bacilli in cockroaches, though they have been found at times. The bacilli occur in large numbers in the faeces of cockroaches, and can pass from roach to roach by this means, since they feed on anything, from paper, clothing, and blacking to flesh and the dead bodies of other cockroaches. The feeding propensities would explain why, in one kraal from which nine patients had been admitted at various times, the last one five years earlier, acid-fast bacilli indistinguishable from Hansen's were found in a roach caught in a hut. The infection must have remained in that kraal in the shape of infected cockroaches and faeces. Hansen's bacilli have been demonstrated in faeces kept in a stoppered tube for 29 months, and were found to stain as deeply as when fresh.

Now, experimental inoculations of laboratory animals have failed to be of value, as have attempts at cultivation, so that man remains as the only object of experimentation. African natives do not fear the disease, but volunteers should be sought in other parts of the world, and should be inoculated (1) by allowing infected cockroaches to bite them, and (2) with infected faeces. One such experiment, has produced suggestive though not conclusive results.

—I am, etc.,

Kenya.

BERNARD MOISER.

Prognosis of Pleural Effusion

SIR,—May I at this late date reply to Dr. J. D. L. Reinhold's letter (June 7, p. 824) relating to my paper on this subject published in an earlier issue. The 190 patients concerned were, almost without exception, treated initially with prolonged bed-rest and a convalescent or sanatorium regime, and there was no evidence that differences in treatment of the original pleurisy affected the subsequent incidence of pulmonary tuberculosis. Although I continue to keep such cases in bed at least until their blood sedimentation rate becomes normal, I have the uneasy feeling that in this, as in other manifestations of the evolution of a primary tuberculous infection, its fate depends on factors probably outside of medical control. The essential thing is close observation during the ensuing 12 to 18 months, when most of the phthisis (i.e., tuberculosis that we can treat) occurs.

Dr. Reinhold and anyone else who is interested will find a much fuller account of this work, with details of the intra-thoracic lesions encountered before and after the onset of pleurisy with effusion, in the *American Review of Tuberculosis* of October, 1946.—I am, etc.,

Dunedin Hospital,
Dunedin, New Zealand.

BRIAN C. THOMPSON,
Medical Director, Tuberculosis Service.

"The Thalamic Syndrome"

SIR,—I was careful in my previous letter (July 19, p. 109) to limit my criticism of Dr. G. Tayleur Stockings's article to the statements with which he prefaces the account of his work on a euphoriant drug. Work of this type is always valuable if only for the light which it may throw on the mechanism of mental disorder.

My main criticism of Dr. Stockings's article is that he advances his theory that neurotic depression can be equated with thalamic dysfunction as though it were a clearly demonstrable fact. This criticism also applies to his claim that psychotic and neurotic depression can be differentiated by a specific response to "anoxic" therapy. I do not think the evidence so far adduced in support of this claim justifies its being presented to the readers of the *Journal* as though it were a well-recognized fact. Theories are useful where they lead to further research and

enable predictions which can be checked to be made. If, however, they are advanced as assertions they will not serve this purpose but will merely serve to confuse the interested reader who has no specialist knowledge of the subject.

In my simplicity I was unable to see any difference between "thalamic syndrome" and "thalamic dysfunction syndrome." I must confess that even after reading Dr. Stockings's explanation (Aug. 2, p. 187) I would still draw the inference if I saw the name of an organ attached to a syndrome (e.g., pituitary syndrome) that the symptoms which compose the syndrome were being attributed to dysfunction of that organ.

I think that the true role of cortex, basal ganglia, thalamus, and other parts of the brain and the relative importance of constitutional defects and external stimuli in the production of mental disorder can only be established as a result of very extensive research in which chemical agents will no doubt play a very important part. Meantime, in order to assess results accurately I believe it is very necessary to maintain clearly the distinction between experimentally proven fact and theories with little experimental support.—I am, etc.,

St. Mary Cray, Kent.

BRIAN H. KIRMAN.

"Mushroom" Poisoning

SIR,—The advice given by your annotator (Aug. 23, p. 302) that, unless we are experts, we should not eat any fungus which is not obviously a mushroom is sound, even if it deprives us of the edible fungi described by Mr. John Ramsbottom (p. 304). But unfortunately the really deadly fungus, *Amanita phalloides* (the Death Cap), may be easily mistaken for the edible mushroom and passes the popular (fallacious) tests of edibility. I think, therefore, that emphasis should be given to the fact that the gills of the Death Cap are permanently white, while those of the edible mushroom never are, and that the public should be told, "Never eat a 'mushroom' with white gills."

I have met only two cases of Death Cap poisoning and both patients were moribund when seen. Details of possible treatment were hard to find among the "accumulated mass of clotted bosh" which Mr. Ramsbottom rightly condemns. As "mushroom" poisoning may prove to be a very serious emergency, I suggest the following scheme for any future cases:

1.—If symptoms began within five hours of ingestion, wash out the stomach and leave in it 2 fl. oz. (57 ml.) of "white mixture."

2.—If there are blurred vision, sweating, salivation, or other symptoms suggestive of muscarine poisoning, give atropine gr. 1/50 (1.3 mg.) subcutaneously to an adult.

3.—If the onset of symptoms is delayed over five hours, the doctor should ask himself, "Is it Death Cap poisoning?" In deciding this point, have an expert opinion on any uneaten fungi and on the spores in the vomit. Failing this evidence, a delayed onset of symptoms should make the doctor decide in favour of Death Cap poisoning. There are three possible main lines of treatment, which should be given promptly: (a) Obtain and administer anti-phallic serum: Apply to the Director, Central Public Health Laboratory, London, N.W.9. (Telephone Colindale 6041). The dose will be indicated on the amount sent. (b) Commence treatment for failure of liver function (glucose, calcium, vitamin B₁, vitamin K, etc., and possibly blood transfusion). (c) The rabbit stomach-brain treatment of Limousin and Petit. This is based on the fact that, while cats die after eating *Amanita phalloides*, rabbits do not. But the juice of *Amanita phalloides* injected subcutaneously into rabbits is fatal, suggesting that the toxin is destroyed or neutralized in the rabbit's stomach. Cats fed on *Amanita phalloides* plus rabbit's stomach survive several days. If the rabbit's brain is given also, they recover completely. The treatment recommended is to give up to 5 uncooked rabbits' brains and stomachs minced up, daily for several days. Its practicability in a vomiting patient seems questionable, but the emergency is so desperate that it should be tried.—I am, etc.,

Enfield, Middlesex.

C. ALLAN BIRCH.

Tuberculosis and Malnutrition

SIR,—Dr. Agnes C. Clark (Aug. 9, p. 226) raises a very interesting point regarding tuberculosis and malnutrition. The usual teaching is that pulmonary tuberculosis occurs more frequently in an undernourished population and tends to be more severe. In her D.P. camp the exact incidence is not stated, but recovery appears to be very quick with the minimum treatment.