apparent slight hyperaesthesia proximal to the line of demarcation, which might be explained by the band of hyperaemia as described by Gelfand. Knee-jerks were indeterminate. Wassermann examination and blood counts could not be performed. The urine was free of sugar and albumin.

The patient refused operation and would not enter hospital, so his further progress is not known.

Williams (1939), discussing the results of 894 post-mortem examinations in Uganda, found 86 cases showing syphilitic arterial disease and considers that this is common in East Africa. Coombs (1932) points out that lesions most commonly follow one, two, or more decades after infection. This would fit in with the age groups of both Gelfand's and my cases, particularly when it is remembered that the African often acquires his syphilis relatively early in life.

Prior to Gelfand's paper I had considered my case to be definitely one of syphilitic origin. Now the question requires reconsideration. It may be that the part played by syphilis is merely an initial damage to the artery (Gordon *et al.*, 1942), on which further processes later bring about an ischaemia sufficiently suddenly to cause gangrene. On the other hand in cases where gangrene has followed syphilis the onset has usually been much less sudden than in these seven cases. Choussat (1938), for instance, describes a case of bilateral dry gangrene of the feet, of slow onset, in a child of 10. Bilateral gangrenous areas on the arms of a white woman of 61, in which the lesion on the left preceded that on the right, is cited by Goodman (1939). Similar cases will be found in papers by Fusco and Kell (1943) and Leone Bloise (1936), though I have personally not yet been able to trace them.

When the high rate of syphilis in certain native populations is taken into account, figures ranging from 23% (Kark and Le Riche, 1944) to 40% (Rauch and Saayman, 1938) being substantiated by various authors, the fact that three out of these seven cases of bilateral gangrene of the feet had syphilis may not be of significance but merely a coincidental finding.—I am, etc.,

London, W.C.1.

JOHN G. SALTER.

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Peptic Ulcers and Radiology

SIR,—It was with considerable interest that I read Dr. A. Landau's contribution under "Medical Memoranda" (June 28, p. 928) in which he describes a case of peptic ulcer responding rapidly to a line of treatment advocated by himself. Without wishing to comment on the efficacy of the treatment described by Dr. Landau I would like to point out that it is not an uncommon experience among radiologists to find an apparently large gastric ulcer become almost undemonstrable radiologically after a few weeks' medical treatment on the usual accepted lines. This is due mainly to the fact that the apparent depth and size of the ulcer, as demonstrated on the x-ray film, is much greater than the actual depth. Most of the apparent depth is made up of the swollen oedematous edges of the ulcer projecting into the lumen of the stomach, while the actual ulcer crater is often quite shallow, as has been demonstrated at operation.

On conservative medical treatment the first thing to subside is the oedema, thus producing apparently rapid healing radiologically, but the actual shallow ulcer crater can often be demonstrated for some time after the initial rapid reduction in size. The first x-ray photograph illustrating Dr. Landau's case (allowing for loss of detail in reproduction) would appear to show one of these typical apparently large craters with some narrowing of the neck and projection inwards of the oedematous edges. If the normal line of the lesser curve is followed, it will

be found to include only a small proportion of the ulcer base, which is probably the true depth. In view of the above points I am of the opinion that to describe this ulcer as "the size of a cherry" is misleading in the assessment of its ultimate progress with regard to the treatment described.—I am, etc.,

Orpington, Kent.

RONALD L. MANSI.

The Thalamic Syndrome

SIR,—Any new approach to the treatment of psychoses is welcome in view of the limited value of present methods. I do not think, however, that the dogmatic statements with which Dr. G. Tayleur Stockings (June 28, p. 918) prefaces his account of work on a drug resembling Indian hemp are very helpful. He blames the thalamus for the symptoms associated with reactive depression, the list of which reads like a proprietary medicine advertisement. The logic of this claim is, however, not apparent. To pick out two of the symptoms at random, it is not clear on what evidence tremors and obsessional thoughts are attributed to dysfunction of the thalamus. True, Stockings goes so far as to admit that the hypothalamus may also be involved in "the thalamic syndrome," but why seek to exclude any part of the brain from involvement in this pantechnicon syndrome? At one point the thalamus is referred to as a "higher centre." It would appear to have been promoted recently. In fact the majority of the symptoms which he describes as being produced by the drug under investigation, which Stockings refers to as having a specific effect on the thalamus, appear to be at least equally attributable to cortical changes. Thus photopsias, simple coloured patterns, visual illusions, increase in the power of phantasy, and vividness of visual imagery are surely not characteristic of thalamic activity.

In the past there has been a tendency to neglect the thalamus as a factor in the production of neuroses and psychoses. This fault will not be remedied, however, by swinging violently to the other extreme. What is needed now is an effort to see the brain as a functioning unit with the different centres balanced harmoniously, one against the other. This dialectical concept would avoid the clumsy attempt to divide off "neurotic depression" artificially from psychotic depression, and to divorce it on the other hand from environmental factors, and to pin it for the sake of formal neatness on to one particular region of the brain.

Incidentally, Stockings states that the "true addiction syndrome" is peculiar to morphine and cocaine. I had always been under the impression that alcohol sometimes caused addiction, but possibly this drug is now in such short supply that it can be neglected.—I am, etc.,

St. Mary Cray, Kent

BRIAN H. KIRMAN.

Vaccination against Variola

SIR,—The outbreaks of variola in this country in recent years represent a danger which should not be regarded as negligible in a population whose standard of vaccination is constantly declining. The number of contacts who develop the disease although vaccinated—or to the greater part revaccinated—at a time when successful vaccination, due to its shorter incubation period, should protect against the outbreak of variola is not very small. What is the reason?

I think that it will often be very difficult even for an experienced vaccinator to decide whether a small local reaction can be regarded as evidence of successful revaccination. How much more so in the case of many practitioners who had little opportunity to acquire the necessary experience with revaccination. It is, therefore, very likely that those contacts who developed variola under the above-mentioned conditions have been revaccinated unsuccessfully.

I should like to mention a point in my own personal experience. During my own medical career I have been in contact with variola patients on a number of occasions, and I have been revaccinated about ten times. Only once, many years ago, a very slight papular reaction made it rather likely that vaccination was successful. On all the other occasions the evidence was at least very doubtful. The last time this happened was in the third year of the last war. About two years later I inoculated myself from pure curiosity with the contents of a vesicle of a primarily vaccinated child, and I developed a large and very typical vesicle which would have given an excellent

textbook illustration for the effect of primary vaccination. Also the clinical course was only slightly shorter than that of primary vaccination.

This proves again the well-known fact that, especially in the case of revaccination, much better results can be expected from the use of human lymph as compared with calf lymph. The use of human lymph has practically been abandoned because of the danger of transferring syphilis, erysipelas, and other infections. But nowadays syphilis can be easily excluded by serological examination, and penicillin and the sulphonamides have greatly diminished the danger of septic infections.

Under these circumstances might it not be quite reasonable to use human lymph, if available, instead of calf lymph in such persons who as contacts are in immediate danger of developing variola and becoming themselves again a focus of further spread?—I am, etc.,

London, N.W.1.

G. FRIEDLAENDER.

Physical Therapy of Mental Disorder

SIR,—The numerous letters you have published recently and in 1946 on physical therapy in mental disease indicate the interest the profession takes in the subject, and many of the general public show considerable interest also. In my letter in the *Journal of March 23, 1946* (p. 447), I suggested proof of the efficacy of this treatment should be the percentage of recoveries on admissions in the report of the Board of Control. The report for 1945 was issued some time ago, and the recovery rate for that year was 33.2%—males 28.1% and females 36.7%—of the direct admissions, or slightly less than in 1939. The recovery rate for many years has been practically stationary.

The percentage of cases discharged as relieved has, however, risen considerably, the combined number of recovered and relieved being 63%, compared with 48% before the Mental Treatment Act of 1930. In their comments the Board state, "It is difficult to say whether the increased discharge rate is due to improved treatment or to the large number of voluntary admissions." It will be noted it is an increased discharge, not recovery, rate that reference is made to, and at least one popular paper seems to have confused the two. In 1945 the voluntary, temporary, and certified admissions were 50.7, 4.1, and 45.2% respectively. In 1934 the voluntary admissions were 29%. In 1945 the number of voluntary patients discharged as relieved was 5,872, as compared with 2,672 certified, the recoveries being 4,900 and 4,544 respectively. Possibly shortage of staff and beds may have contributed to the relieved discharges.

The stationary recovery discharge rate does not necessarily imply that E.C.T., leptazol, etc., are not of some use. Owing to the advanced average age of the community more senile admissions may have taken place; also there are fewer admissions of recent alcoholic psychoses, the latter a favourable type as regards prognosis. A good many Service patients may have been treated with or without convulsion therapy, and recovered, in places outside the jurisdiction of the Board of Control, and possibly the same applies to a number of civilians. Nevertheless the stationary rate in the mental hospitals does tend to show that the treatment may not be so efficacious as many consider.

In your issue of April 6, 1946 (p. 548), you published a letter from Dr. Ian Skottowe in which he stated that in 1945 the "useful discharge" rate for the Bucks Mental Hospital was 63.4%, or, excluding those over 60, 87.8%, and that E.C.T. was used in not more than half a dozen cases. He did not state the separate percentages of recovered and relieved, but his results seem to compare favourably with those obtained in hospitals where E.C.T. is extensively used, and may be regarded as a useful control experiment. In any form of therapy in mental disease it may be said—it is more or less a platitude—that the better the original make-up of the patient the better the results. The best results with E.C.T. are with the affective psychoses, especially involution melancholia—i.e., where there is no dementia, and where the nerve cells, unlike those in dementia praecox, are durable. If those involution cases associated with arteriosclerosis, etc., are excluded a certain amount of depression at the involution age might be regarded as normal, and some cases of melancholia are merely exaggerations of this. Further, a person having no mental attack until that age must have had a better brain than one breaking down at 20, except perhaps in some cases that have had no severe environmental stress.

Half a century ago Bevan Lewis and Clouston in their textbook gave a 50 to 60% recovery rate in climacteric insanity. We know, however, that a number of involution cases go on to severe agitation,

with distressing delusions, and if in fact E.C.T. prevents this it is indeed a boon. One would like, however, to see statistical proof.

In 1937 two medical commissioners of the Board of Control were instructed to visit Vienna, Budapest, and Frankfurt, also some English hospitals, to study treatment by leptazol and insulin, and their interesting report was published in 1938. I suggest that the time has now come when the Board of Control, the R.M.P.A., or the B.M.A. should appoint a body to make an exhaustive investigation into modern methods of treatment of the insane. So much E.C.T., etc., has been given in British hospitals that there would be no need to visit the Continent, where no doubt the ravages of war have seriously hampered treatment, but a visit to America might be of much use. Even if a satisfactory explanation of the mechanisms of these treatments cannot yet be given—and I understand anoxaemia is held by many to be the explanation in convulsive therapy—a comprehensive report would be of great value.—I am, etc.,

Hastings.

HARVEY BAIRD.

** This correspondence is now closed.—ED., B.M.J.

Sulphone Treatment of Leprosy

SIR,—A considerable amount of attention is being given to the question of the treatment of leprosy by means of the derivatives of diaminodiphenyl sulphone, and once again there is a tendency for treatment to receive precedence over prevention. In view of the fact that I started leprosy work some twenty-three years ago at a time when the enthusiasm for the hydnocarpus (*chaulmoogra*) derivatives was reaching its height, and when responsible authorities stated that leprosy should be eliminated from the Empire within thirty years, I think it might be well if I sounded a warning note. It would be a tragedy if errors of the past were repeated and a belief encouraged that the leprosy problem could be solved by curative measures alone.

Leprosy has its roots in the dim ages and has a disconcerting habit of confounding those who would be over-optimistic. The past twenty-five years are strewn with the results of alleged cures. It therefore would be wise for us to remember this and not publicize too widely a new hope. If we do, administrative authorities may once again find themselves thwarted when they had high expectations that the problem had become easy of solution. Further, we tend to forget the psychological effect on the patients, for those who have severe lepromatous leprosy clutch at a new remedy as a drowning man would grasp at a straw.

We have been using these new preparations in India for the past year or eighteen months, and the following is a brief summary of our preliminary findings:

"Promin."—This substance is too toxic for ordinary use, and patients, in India at any rate, tend to show intolerance after some months.

"Diasone."—In the dosage recommended it is very liable to set up lepra reactions, frequently extremely severe. Fourteen out of twenty-two cases showed such reactions, and five of these fourteen asked that diasone should be discontinued. The reactions could not be attributed to the usual course of the disease, for regular records of the number of reactions and the interval between the reactions before the administration of diasone had been kept. We found, however, that adequate blood levels (5 mg. %) were impossible to maintain.

"Sulphetrone."—This remedy is not likely to be on the market for a considerable time. It is the least toxic of the sulphone preparations and has up to now caused no serious reactions. Further, the levels of the drug in the blood were satisfactory. Sulphetrone, in our opinion, has not been used over a long enough period for definite conclusions to be drawn. All that can be said at present is that workers are encouraged by the clinical improvement in lepromatous leprosy.

In conclusion I should like to emphasize the following points:

(1) The present methods of administration are not practicable of application on an extensive scale, except in institutions where patients are well regimented or sufficiently keen to be prepared to swallow these drugs in varying amounts, from three capsules of diasone to twelve tablets of sulphetrone per day, for long periods (2–4 years).

(2) These drugs should not be used without adequate blood level estimations. Regular blood counts and haemoglobin