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