Correspondence

Medicine in the United States

SIR,—Being a very young doctor, it is with some diffidence that I criticize our old-established customs and institutions, but the letter of Dr. S. L. Simpson (June 28, p. 949) shows up in sharp contrast to many of the criticisms I have heard from my friends and seniors about medicine in the U.S.A.

I spent 2½ years at medical school in Boston, with a three-month break to visit Johns Hopkins, and feel justified in making a few comments on the contrast between these schools and our own London and university teaching systems. Without doubt the most inspiring teaching that I have ever had was at Harvard Medical School; and yet, taken as individual teachers, some of those from whom I have been privileged to learn in this country are without equal anywhere. It was not the system of teaching at Harvard that impressed me, with its frequent examinations throughout the course, which came first as a rude shock and then as a rather unpleasant stimulant to one used to the more haphazard and individualistic Cambridge ideas, but the atmosphere. It was the continual thirst after new knowledge of our teachers and the way in which they accepted all criticism and questioning, whether from students or colleagues, as genuine attempts at furthering knowledge rather than sly traps by rivals. Everyone was kept very much alive, and a spirit of friendship and co-operation pervaded.

Dr. Simpson mentions the "collaborative sessions" at the Peter Bent Brigham Hospital. One of these was a weekly event called the "Clinico-Pathological Conference" and locally known as the C.P.C. (The Massachusetts General Hospital also had one weekly.) These were so good and so helpful to us as students that I feel justified in describing them in the hopes that a similar session may be introduced into some of our hospitals. They were organized by the pathology department, which would collect the notes from an old case that had been treated in the hospital and extract them on to a mimeographed sheet containing: (1) the patient's complaint as described by himself; (2) the past history of the patient up to admission; (3) the clinical findings as recorded on entry and the relevant laboratory findings; (4) a very brief outline of the patient's course in hospital, finishing with either "an operation was performed" or "the patient died." These sheets were distributed to all those attending, and a guest speaker, usually from another hospital but certainly a doctor who had no previous knowledge of the case, was invited to discuss the diagnosis and therapy. X-ray films were provided, and he could, if he wished, ask for and expect advice on any points outside his own specialty. The case was always chosen taking into consideration the speaker's special subjects, so that we could listen to an expert analysing a case in his own field. The speaker would go through the case point by point giving his differential diagnoses and stating how he thought the recorded course of the patient fitted these or ruled them out. At the end, if the case was surgical, the surgeon who operated would tell of his findings, or, if medical, the pathologist would give the necropsy findings, illustrated with photographs and sections on the screen. The speaker was often wrong in his diagnosis, because the cases were difficult and had often caught out the staff of the hospital, but no one was offended and everyone felt that they had gained by the experience.

Later the Brigham went further, and final-year students, usually about eight at a time, were occasionally briefed a week beforehand for the discussion, being given the case history to think about. This gave them a chance to look up the relevant literature. (I believe that most of the speakers were given some warning.) On the appointed day the students took the front seats and one was drawn by lot to speak. This victim then had to discuss the case before his teachers. These conferences were voluntary but were very well attended, being one of the most popular features at the medical school.

There is just one other point that I should like to make emphasizing a statement in Dr. Simpson's letter, because I hear so much about the American laboratory diagnostician. I had more teaching in psychosomatic medicine at Harvard than I have seen or heard of in England. Also my instructor in medicine once said to me, "If the laboratory data do not agree with your clinical findings, have the tests repeated. If they still do not agree, neglect them." The laboratory work at Harvard is not a short cut to diagnosis but an adjunct to research and sometimes a gauge of the patient's progress under treatment. My criticism of medicine in Boston is that it aims too much at research, if that is possible.—I am, etc.,

Ely, Cambs.

N. K. CONNOLLY.

World Medical Association

SIR,—In the Supplementary Report of Council (*Supplement*, June 21) I was very glad to see that the Council had prepared a statement to be submitted to the General Assembly of the World Medical Association in September, 1947. As I have been appointed by the Medical Women's International Association in my capacity as their honorary treasurer to act as an observer at the meeting of the Assembly, I should like to take the opportunity of saying how strongly I agree with the Council's view that the medical profession as a whole have a grave responsibility towards their fellow men, not only as doctors but as leaders of public opinion in the field of moral and ethical values.

During the terrible years of occupation by a brutal enemy the large majority of doctors of most of the occupied countries maintained their moral integrity, their unswerving loyalty to their patients, and their spiritual and professional freedom, even at the risk of torture and death. They thereby set a great example and vindicated the honour of their profession.

Now that the war is over we are faced with a curious and dangerous shift of values. On the one hand men and women in so many countries are refusing to submit in any particular to the power of individual employers, while on the other hand they appear willing and even eager to denude themselves of every vestige of personal and political freedom and to surrender all their liberties to the State. This must lead insidiously but surely, as it did in Germany, to a growing disregard for the value of the life and human rights of the individual. As the Council so truly says in its statement, "The doctors who took part in these deeds (i.e., medical war crimes) did not become criminals in a moment. Their amoral methods were the result of training and conditioning to regard science as an instrument in the hands of the State to be applied in any way desired by its rulers." The Council might have added that the doctors were trained and conditioned to regard human beings, including their patients, as mere robots or sub-human statistics entirely unimportant in themselves, and therefore subordinate to the will and interests of the State and its rulers.

This horrible distortion of values is one which doctors have an especial duty and, if we will only use it, a considerable measure of power to resist and overthrow wherever we find it developing in our midst. The principles which are included in Appendix 2 to form a part of a Charter of Medicine are fundamental, but I would suggest that they should be extended to include a statement that the medical profession affirm their belief in the sanctity of the rights of the individual as a human being, and pledge themselves to safeguard these rights in all their dealings both with their patients and in relation to any function which they may be called upon to exercise in their capacity as medical men and women. Let us never forget that "the price of freedom is eternal vigilance."—I am, etc.,

London, W.1.

DORIS M. ODLUM.

Symmetrical Gangrene in the African

SIR,—The condition described by Dr. Michael Gelfand (June 14, p. 847) appears to be of sufficient rarity to justify my quoting an almost identical case which fulfils his six diagnostic criteria. The patient was seen in the province of Sidamo, Ethiopia, in 1945.

CASE REPORT

An adult male Sidamo aged between 30 and 35 was brought by his friends from a village some long distance away. He gave a history of being in good health and symptomless until some 3–4 months previously, when quite suddenly he had an attack of "rheumatism" in his legs and feet, which became swollen and painful. A little later the toes and soles of his feet appeared "as though dead." There was no history of recent attacks of malaria, typhus, or other disease. He had had syphilis at least ten years before, for which he had not received any treatment. His general nutritional state was good, and his diet appeared satisfactory.

On examination a condition rather similar to that shown in Gelfand's first photograph was seen, though more advanced. Both feet were blackened, dry, and wizened, particularly on the underside, and flies were crawling in and out of the various cavities in the mummified gangrenous area. The line of demarcation was not as pronounced as is shown in Gelfand's second photograph, but ran from about 1½ in. (3.81 cm.) proximal to the base of the toes to just below the level of the malleoli. No pulsation could be felt in the dorsalis pedis or in the femoral artery of either side. There was

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