The Extent of Neurosis

SIR,—This vital and important subject has elicited an interesting correspondence. I venture to say that it can be studied only in relation to fear, its nature, its distribution, and its source. Up to the close of the Victorian era life for the ordinary citizen was a matter of competition. The worker had to find a job and keep it, or face suffering for himself and his family (though not actual starvation). This fear was real and well-nigh universal, but the results of this kind of fear produced the finest kind of work in quality and quantity; and there was little neurosis.

There followed the Socialist outlook which has developed all through the present century. Fear—economic and social—has been gradually eliminated till it matters little to the worker whether he is in work or in benefit. Taking a superficial view this should reduce the extent of neurosis, but on the contrary it has increased enormously. In other words, the elimination of the fear motif has also eliminated the response of effort. Thus we get a situation in which individual effort becomes useless or even detrimental, and economic advancement depends upon an identification of the individual with the group. In other words, a higher standard of living is no longer achieved by an effortful reaction to economic fear and anxiety. Now it is to be obtained by identification with the herd and an attitude of passive solidarity with the group. In other words, the effect of economic stringency is to talk of strikes rather than to concentrate on effort. And no one can blame the individual worker for accepting the change, which has come so gradually and come from his political leaders.

But we are entitled to look ahead and to ask ourselves, "What will be the next phase?" For the sake of argument let us accept the Russian claim that in the U.S.S.R. there is no neurosis. That means that economic anxiety is geared to effort, and if rumour is to be believed it means that the worker gives of his best lest he should find himself in Siberia. Certainly any passive attitude to the group solidarity is not encouraged. There are many other aspects of this question which cannot be discussed within the limits of a letter, but the more we study it the more we are brought back to the essential relationship between fear and effort.—I am, etc.,

Harrow, Middlesex.

H. CRICHTON-MILLER.

SIR,—The provocative has its place among the artifices of discussion, but surely it loses its tang when it is employed too bluntly and without nuance. Dr. Arthur Dearlove (Oct. 11, p. 590), describing the inadequacy of the treatment which the neurotics he has referred to psychiatrists have received, declares: "The results are, roughly [sic]: cured, nil; improved, nil."

It is evidently not the intransigence of the neurotics of Dorset he is referring to, as he reflects upon the general impotence of psychiatrists later in his letter. Dr. Dearlove has no doubt acquainted himself with at least some of the numerous follow-up studies which have been published. Is it to be assumed that he meets them not merely with the "retort courteous" but with the "lie circumstantial and the lie direct"?—I am, etc.,

London, W.1.

FREDERICK DILLON.

The G.M.C. and Medical Education

SIR,—Mr. A. Wilfrid Adams's letter (Oct. 4, p. 545) was both useful and interesting, but it lands us in a quandary: a student while preparing for his licence to practise must first learn those things that he will want to know when he begins to learn how to conduct general practice; then and only then he has to begin to learn the art of practice. The suggestion of a year with a reputable general practitioner is accompanied by a liability to misuse or indifferent use. The old apprenticeship unfortunately began at the wrong end.

Would it not be more effective to attach a newly passed medico to a health centre, when available? Here he would have the advantage of taking an active part in the general medical practice of his seniors in all the varied services—early diagnosis, observation of conditions not yet labelled, practice in the consulting-room and in the patient's home; here observe the probabilities of treatment being successfully carried out, the need for proper sanitation or prevention; not only these sides of practice, but also work in the hospital to which the centre

is attached. In this way he would learn to associate his previous hospital experience with actual practice among patients under conditions new to him. Here, without prejudice, he could observe the results of treatment, immediate and remote.—I am, etc.,

Midford, nr. Bath.

CHAS. E. S. FLEMMING.

Mercurial Diuretic

SIR,—I believe "neptal" (mercuric salicyl amino-acetate) is considered the safest and least toxic diuretic of the large group of mercurial diuretics, while it is simple in use since its efficacy does not depend on urinary pH. Since, unfortunately, it seems liable to cause prolonged aching pain, however well spread throughout skeletal tissues, probably many employ it—rightly or wrongly—by the intravenous route.

Dr. A. G. Oetlé (Oct. 4, p. 530) sounds a warning note in this connexion. His article interested me very much, since in 1945 I had an unpleasant experience with this drug, which was very probably an acute poisoning of the conducting tissue of the heart and was at all events a near-fatality. I was continuing a series of injections which a middle-aged cardio-renal female patient had received (2 ml. tri-weekly, bi-weekly, and finally weekly) from another practitioner and from which she had derived much benefit. Two injections were wholly uneventful; my third (her 28th or 29th) was almost disastrous, and would certainly have been so if I had not paused to fill the syringe by reflux after 1 ml. had very slowly entered the vein. She complained of such severe faintness and precordial constriction that I stopped the injection. The attack was much as Dr. Oetlé describes, dyspnoea and syncope being followed after a few minutes by an epileptiform convulsion.

I never doubted it was a Stokes-Adams phenomenon until long afterwards. I knew little of the history beyond that the patient was a cardiac case of long standing who had suffered in adolescence from several mild attacks of rheumatism. Two types of Stokes-Adams syndrome are described—a syncopal and an epileptiform. In this patient both appeared to develop successively. Following the attack a severe degree of heart block was sufficiently obvious from the extremely slow pulse with rapid jugular pulsation. An E.C.G. confirmed this and appeared to clinch the diagnosis of Stokes-Adams attack. I was fortunate in that symptomatic treatment carried out in this firm conviction at least did not prevent recovery, but on reflection I feel sure that it was I who accidentally "did the bundle in" and not a senior and most capable colleague who had treated the patient for some time in ignorance of a pre-existing heart block. Some Stokes-Adams attack—if it were such.

The most surprising feature of the case was the patient's bravery in submitting later to an apparently similar course of treatment, and one greatly favoured in France though not, I think, in England—i.e., intravenous injections of sterile "novocain" (procaine hydrochloride). The recommended dose of 10 ml. of 1% solution can be much exceeded with apparently complete safety, although large doses cause a transient complaint of paraesthesiae in fingers and other extremities. Very bold doses are given by some French physicians, who employ the drug as an antispasmodic agent in treating a diversity of spastic conditions, ranging from migraine through the asthmas and intestinal spasms to passing calculi (renal and biliary). As an antispasmodic agent I do not suppose novocain is ever likely to supplant atropine or pethidine but it has the merit of instantaneous action, and can be most useful alone or as an adjuvant. It is interesting to watch its action on the x-ray screen as regards intestinal spasms, and to employ it to diagnose—e.g., spasmodic dysmenorrhoea from other varieties (it does not, intravenously, appear to act, even briefly, as a general analgesic).

For phorosis, procaine injections repeated at regular intervals are favoured by French physicians, it seems. The head of a hospital department demonstrated several cases for my benefit. Can anyone with experience of novocain confirm or refute the suggestion that it could replace mercurials? It can only be a palliative, but so are the latter; and, apart from this rare intravenous risk, are they not always dangerous to the old and frail, even intramuscularly? The patient is no doubt very often in a parlous condition before these powerful drugs are resorted to, but these drugs often seem to precipitate renal

failure, and, as the Greeks advised the making of a will before purgation with the hellebore, were it not well to advocate this procedure before diuresis with the mercury?—I am, etc.,

Stratford-on-Avon.

PETER PARRY.

Sympathetic Disorders in Poliomyelitis

SIR,—Following the annotation on "Sympathetic Disorders in Poliomyelitis" (Oct. 11, p. 579) might I make a few observations on muscle pain? It is a well-known fact that normal muscle, if exercised to excess, becomes painful and tender—presumably a defence mechanism to prevent a repetition of abuse before recovery has taken place. Everyone knows the "first game of the season" stiffness and tenderness, which may be excruciating and persist for several days. To turn to the abnormal: having slept beneath a ship's blower in the Tropics and awoken with a "stiff neck," one's conclusions are as follows. The reaction to the fall in temperature of the exposed part was that of local shivering or fibrillary twitchings of the muscles in question—in this case the trapezius and sternomastoid of the affected side—summing over a prolonged period to produce the same phenomena as of excessive general contraction—i.e., pain and tenderness.

From the electromyographic findings in poliomyelitis can one postulate an initial pathological stimulation from the diseased anterior horn cells invoking the protective mechanism described above?—I am, etc.,

Twickenham, Middlesex.

D. F. REES.

BAL in the Treatment of Gold Dermatitis

SIR,—In view of the articles which appeared in the *Journal* of Oct. 4 concerning the therapeutic uses of British anti-lewisite, the following case report may be of interest.

CASE REPORT

The patient was a woman of 56 who for six months had complained of increasing pain in both wrists, hands, and fingers. A clinical diagnosis of early rheumatoid arthritis was made and intramuscular injections of "myocrisin" were commenced. The E.S.R. had not been done, but the urine had been carefully examined and was repeatedly checked.

A system of increasing dosage at weekly intervals was employed, and at the end of eight weeks a total of 0.4 g. had been given. The day following the last injection an irritating erythema appeared, which quickly changed to a macular rash, and after a week the typical appearances of an exfoliative dermatitis due to gold were evident. The condition was severe and resisted every method of treatment.

After eight weeks of trial and error injections of BAL were commenced. The preparation used was a 5% solution in 10% benzyl benzoate in peanut oil. Daily injections of 2 ml. were given for five days, with immediate amelioration of the symptoms. The dermatitis, however, did not completely disappear, and after a period of four weeks of comparative comfort for the patient the condition recurred. Further daily injections of BAL 2 ml. were given for seven days, and the dermatitis once more subsided, the improvement this time being maintained. The injections produced two large abscesses in the buttocks which had to be drained. The arthritis, which responded well to the gold therapy, has so far showed no signs of recurrence.

—I am, etc.,

Tenbury Wells, Worcs.

J. A. BURNETT.

The Weil-Felix-Wilson Reaction

SIR,—What is commonly called the Felix-Weil reaction is in reality the Felix-Weil modification (or variation) of the Wilson typhus reaction, for Wilson's (now Prof. W. James Wilson, of the Queen's University, Belfast) discovery of the agglutination of organisms of the *B. coli* and *Proteus* type was made in 1908 and 1909, whereas it was not until the first world war that Felix and Weil carried out their work, publication regarding which was made in 1916. Personally I have every reason to remember the discovery, as I was the first human being on whom it was tried for diagnostic purposes—in March, 1908.

The following is a brief outline of the genesis: From the end of December, 1906, for several years Belfast was visited by a very severe epidemic of cerebrospinal fever, and a considerable amount of work was carried out at Queen's College (now University) on the bacteriology of the meningococcus. While this epidemic was in progress I happened to be R.M.O. in the City Isolation Hospital at Purdysburn, where in my time (about

one year eight months) about 360 cases of the disease were treated (a paper by me describing the outbreak was published in the *Practitioner*, 1915, 94, 831).

Wilson during this period had been making investigations at Queen's regarding the bacteriology of cerebrospinal fever and had been struck by the heterologous agglutination effect of the blood serum of patients suffering from the disease (see *Journal of Hygiene*, 1909, 9, 316). While the epidemic was in progress an outbreak of typhus fever occurred in the city, and almost forty of the cases were admitted to Purdysburn during my residence there, and I suggested to Wilson that if he would care to make investigations about the disease I would try and get him as much material as possible. Wilson then started his investigation, and samples of blood, faeces, urine, etc., were taken by us from patients who, we considered, were suffering from undoubted typhus. Wilson found before long, as in cerebrospinal fever, that there were heterologous agglutinations in this disease also. He found that the serum of typhus agglutinated *B. typhosus* and also bacilli obtained from the faeces and urine of typhus patients. Wilson's first publication on this matter appeared in the *Transactions of the Ulster Medical Society*, session 1908-9, in a joint paper by Wilson and myself (but I must state here clearly that it was Wilson who was entirely responsible for the discovery of the agglutination effect).

Near the end of my residence time in the hospital I developed an acute febrile condition which resembled typhus. There was doubt at first, and my serum was tested. An agglutination reaction was found. This condition began very suddenly, without preliminary catarrhal symptoms, and was accompanied by a rash that was a cross between that of measles and typhus. It ended with a sudden drop of temperature on the twelfth day. Wilson's next paper was published in the *Journal of Hygiene*, 1909, 9, 316. In the *Journal of Hygiene*, 1910, 10, 155, a further paper entitled "The Etiology of Typhus Fever" was published by him. Among his conclusions are the following:

"(4) From the faeces of one case a variant form of *B. coli communis* was cultivated on which the blood serum of 17 typhus fever cases was found to have three to ten times the agglutinating effect of normal serum.

"(5) From the urine of two cases a bacillus resembling *B. coli communis* but having no action on lactose was cultivated. This bacillus was agglutinated in dilutions of 1:50 and 1:100 by the serum of the cases but not by normal serum."

This bacillus from the urine Wilson regarded later as a urinary variety of *B. proteus* ("The Wilson-Weil-Felix Reaction in Typhus Fever," *Journal of Hygiene*, 1920, 19, 115), and pointed out that it is probably similar to the X2 strain of *B. proteus* first used by Weil and Felix. Weil and Felix in "X19" obtained a more sensitive organism for the typhus reaction than X2; Wilson also, in U2, a bacillus of the colon-paratyphoid group, obtained an organism not only more sensitive than his original one but as sensitive as X19 of Weil and Felix in certain cases of typhus fever. Comparisons are given regarding this in Wilson's paper "A Further Contribution to the Serology of Typhus Fever" (*Journal of Hygiene*, 1927, 26, 213). This bacillus U2 is named so on account of its similarity to the original U discovered by Wilson in 1908, which was found to be agglutinated by the serum of typhus fever patients with three to ten times the effect of normal serum ("Bacteriological and Cytological Investigations of the Blood in Typhus Fever Cases," by W. J. Wilson and E. H. M. Milligan, *Transactions of the Ulster Medical Society*, Session 1908-9).

It should be remembered that in addition to the Felix-Weil modification of Wilson's typhus reaction there are other modifications such as those using *B. pyocyaneus* (Kreuschen, 1918; Neukarsch and Kreuschen, 1919; Sampietro, 1920; Wilson 1922), and the *Coccobacillus byzantinus* (Beguet, 1921).

In view of the above observations I think it will be admitted that it is reasonable and right to suggest that in future the Felix-Weil reaction should be called the Felix-Weil modification of the Wilson typhus reaction, or, more shortly, the Weil-Felix-Wilson reaction (W.-F.-W. reaction). Wilson was undoubtedly its discoverer; Felix and Weil have the credit of developing it, expanding it, and getting it more widely used during the first world war.—I am, etc.,

Glossop, Derbyshire.

E. H. M. MILLIGAN.