CLINICAL RESEARCH IN PSYCHIATRY

IT is not so long ago that there was a lively discussion in this country on the scope for clinical research in general medicine. There can be few branches of clinical medicine in which there is more scope remaining for pure clinical research than in psychiatry. It is a curious reflection that few discoveries of fundamental importance in psychiatry have come from the laboratory, apart from the work that led to the delimitation of the structural diseases of the brain—especially of general paralysis. But neither in manic-depressive psychoses nor in the large miscellaneous groups that we call the schizophrenic, nor in the paranoiac conditions, and still less in the psycho-neuroses, can anyone point to anything fundamental in our understanding of these conditions that has been due to the technique of the laboratory, and that is at the same time widely accepted. Even in epilepsy clinical observation has yielded so far the most important results. Only in mental defect and the miscellaneous group of mental illnesses which are secondary to gross bodily disease are we indebted to other than clinical methods for our deepest understanding of the conditions involved.

This is a state of affairs which can hardly fail to excite comment, but judging from the relative proportion of papers devoted to clinical and laboratory work on psychiatry in this country one would imagine that the circumstance had passed unobserved. This is not to suggest that laboratory work should be in any way relaxed. It may be that with the better methods now increasingly at the disposal of the biochemists the disciples of what is after all comparatively a young science may at no distant date find it possible to come to some discoveries worthy to be regarded as fundamental. But in the meantime the lessons of the past point in the direction of the benefit to be obtained from a more intensive pursuit of purely clinical methods, pari passu with laboratory studies. This is the more to be welcomed as, after all, the training of most of the men who take up psychiatry is predominantly clinical. Doctors in general are not specially trained for research on
physicochemical lines. In so far as their training has been mainly clinical they have, in 99 cases out of 100, received a training which qualifies them better to carry out clinical investigations than to do any other kind.

A young graduate taking up research, if he wishes to work in a laboratory, must in these days master a technique and must usually for a considerable time confine himself, if he hopes to make a contribution, to a narrow field in which that technique can be used once he has acquired sufficient skill in it. It is exceptional for a man to have had the additional qualification of a long training in some branch of pure science. It would consequently appear that his best work is likely to be done in that domain for which his training fits him more than any other, namely, clinical work.

It will be asked in what directions clinical investigation remains to be done. One obvious line of inquiry is the psychopathology of the psychoses, which would repay systematic analysis not only for the sake of the psychoses themselves, but for the sake of confirming or modifying current psychopathological theories. In the psychoses it is commonly held that much that is usually unconscious comes to frank conscious expression. What would take weeks or months of effort to obtain, if it were obtainable at all by free association in other circumstances, is here to be observed in return for the trouble of patient selection of the data which present themselves to direct observation. This should be of value for the understanding of the type of psychosis in question, independent of any causal claim of psychological factors in them. Even if it cannot be shown that such factors are pathogenic, the value of elucidating them for the dynamogenetic understanding of the personality and in demonstrating along what lines the personality can disintegrate should be very considerable. In other words, the principle which Hughlings Jackson applied to the study of delirium might be fruitfully applied to the study of the psychoses, especially now that there exists, as there did not in his time, a body of psychopathological theory with which the results of clinical observation may be compared.

It will be admitted that there is still much scope for the introduction of accurate methods into clinical psychiatry. The possibility of supplying statistical analysis to the delimitation of various syndromes suggests itself. For example, in what we call manic-depressive psychoses on the one hand and schizophrenic psychoses on the other, what is the intercorrelation of the various symptoms? What percentage of depressions are
retarded? How does the retardation correlate with hallucinatory mental experiences or with delusions of sin, or more generally and less superficially with indications of feelings of guilt? Or what features of depressions in general correlate specially highly with retardation? This is a sample of the kind of question that might be answered by systematic clinical enquiry along statistical lines.

The most obvious field for statistical investigation however is that of prognosis. There are few collections of cases large enough to give statistical validity to statements about outcome and still less ample are the data for correlating particular symptoms with prognosis. For example, there is little statistical basis for the clinical impression psychiatrists often have of the gravity or otherwise of any particular symptom. Do unreality feelings in depression really betoken a prolonged attack? Is a single symptom like the fear of insanity expressed in the course of an agitated depression ominous of a prolonged course? Then as for recurrences, what is the proportion of manic-depressive patients having a third attack? There has been a study of this last topic by Pollock in the New York State Hospitals, but we know of no similar study in this country.

Closely related to the study of prognosis is the problem of the advisability of what are usually called 'follow-up' studies. These have not often been done hitherto and it is sometimes objected that they are necessarily unreliable, and even that they are best left undone. This last contention is too extreme to be considered, but it is based on the undoubted fact that there are many fallacies in a 'follow-up.' No fallacy however is greater than that of supposing that because a patient leaves hospital symptom-free he is necessarily cured. Nowhere else is this more likely to be misleading than in mental diseases, which are often by definition the reaction of an individual to a continuing environment to which he must return. A cure 'in vitro' is not, so to speak, necessarily a cure 'in vivo.' It is held that the following up of a patient who has suffered from some form or other of mental illness does not give a true account of his condition. His criteria of recovery may not be those of his doctor or his wife. While a follow-up by correspondence may spell difficulties of this kind, they are largely overcome by a personal enquiry carried out by a trained person. It is not to be expected that even by these means the intimate data can be obtained which will give a complete picture of the patient's mind. Science and the interests of the patient are not always
at one in this matter. But it will always ensure that social recovery is assessed with reasonable accuracy.

Controls are much needed, especially when claims are made for forms of treatment. For example, what proportion of psychoneuroses recover spontaneously? How do those treated compare with those untreated in the degree and nature of their recovery?

It will be seen that what is advocated is not a descriptive study of symptoms, which has been done often enough, but clinical observation with a view always to finding out what the symptoms mean.