II. RESEARCH IN MEDICAL PSYCHOLOGY: A COMMENT

By H. J. EYSENCK*

In his Presidential Address to the Medical Section of the British Psychological Society, printed in this *Journal*, Dr Sutherland (1952) presents an argument which strikes at so many convictions firmly held by many psychiatrists and psychologists that one may safely regard it as 'controversial'. It is in the hope of stimulating further discussion of the many important points raised in connexion with this extremely intricate problem of the direction which further research in the psychology of personality should take that I venture to take up Dr Sutherland's argument.

It would be difficult, I think, to take issue with the main points put forward by Dr Sutherland in the first part of his paper. I shall briefly quote what appear to be the essential steps in the argument. (1) Psychological illness presents a grave problem to society, as well as to the individual. 'A conservative estimate of the national bill for psychological disorders must be £100m. per annum. It would therefore appear that the need to take therapeutic and preventive action is urgent.' (2) 'Our first priority is an acceptable approach to aetiology; we need some scientifically established principles and knowledge on which effective therapeutic and preventive action can be based.' (3) 'The radical approach of dynamic psychology..., a body of knowledge... which has been mainly derived from the work of the psycho-analysts, has not been placed upon a sufficiently scientific basis for it to be widely accepted and used as a scientific contribution.' (4) Most approaches to the problems involved are partial; 'one result of these partial approaches is that there are impenetrable barriers between the rival schools, most of which perpetuate their own inadequacies.'

With these points and with many of the criticisms which Dr Sutherland makes of certain current approaches, it is difficult not to agree. The conclusion one might be tempted to draw from these points would surely be equally acceptable, namely that better provision for research, greater facilitation of team work, and a better understanding of the different points of view of different workers in the field by their colleagues, are all desirable and indeed necessary. It might also seem to follow that editors of journals such as the *British Journal of Psychology, Medical Section*, should take great care to open the pages of their journals impartially to all serious students of the problem, in order to aid this process of imbrication. The conclusions drawn by Dr Sutherland, however, are quite different, and are indeed so startlingly unrelated to his premisses that most logicians would regard them as a clear case of *non sequitur*.

'It is my contention', he says, 'that we have enough evidence already on which to found a single integrated science of human behaviour, the basic science for psychiatry. It is no longer inherent in the situation that completely incompatible schools should be perpetuated.' It is not difficult to see which of the many schools Dr Sutherland has in mind when thus stating his belief: 'An evaluation of all the known facts... shows that the only therapy of mental disorder which would appear likely, selectively and radically, to remove pathological aspects of personality without harming the rest is a psychodynamic psychotherapy.' He admits that 'our hypotheses are not stated in a form which can be tested in a scientifically valid way', but believes that many psychiatrists will feel 'in their bones' the truth of what he has been saying about non-dynamic theories. It would follow, he maintains, that 'a large amount of current psychiatric and psychological research would fall into (the) category

* Department of Psychology, The Institute of Psychiatry, Maudsley Hospital, London.

of...a luxury', and that research funds and resources should therefore be concentrated on 'likely lines of approach'. And on the practical side, he believes that 'the psychiatric clinic must nourish and equip parents, teachers, managers, social workers, doctors, the clergy and so on with more effective knowledge'.

It is difficult to see whence this 'more effective knowledge' is to come, or how we are to determine these 'likely lines of approach', or why 'the only therapy of mental disorder...' should be 'a psychodynamic psychotherapy'. It is never made clear just what is meant by 'psychodynamic', a word which may have many and contradictory meanings, but it is admitted by Dr Sutherland that knowledge derived in this way 'has not been placed upon a sufficiently scientific basis', and that the hypotheses in question 'are not stated in a form which can be tested in a scientifically valid way'. If, then, this whole body of theory and practice is not only not based on a scientific basis, but is in fact stated in a form which precludes its being tested in a scientifically valid way, may we not be entitled to inquire why we should throw overboard all other types of research, all other theories and methods, and pin our hopes and our resources to an unproven and apparently unprovable theory? Dr Sutherland seems to rely to an undue extent on the feelings in his bones, and in those of his listeners and too little on the critical requirements of science; the 'bones' criterion is applicable more to religious beliefs than to scientific knowledge. Would it be presumptuous to recommend those who would wish to base action on feeling, rather than on scientific knowledge, to ponder Claude Bernard's dictum: 'In ignorance, abstain'? After all, we have a perfectly reasonable explanation, in 'dynamic' terms, of the origin of the 'feeling in the bones'; it is given by E. Glover (1945), in his paper on the Klein system: 'The transferences and counter-transferences developing during training analysis tend to give rise in the candidate to an emotional conviction of the soundness of the training analyst's theories.'

Even if we were to agree with Dr Sutherland and posit the general superiority of 'dynamic' theories to all others, we would still be confronted with the fact that these theories fail to show any signs of agreement on even points of absolutely fundamental importance. This is not often realized to quite the full extent, and I shall take but one brief example which may bring the point home most forcibly. O. H. Mowrer (1950), an influential 'dynamic' psychotherapist, contrasts his theoretical model with Freud's in the following way (very much abbreviated, but correctly representing his views). According to Freud, neurosis is due to an overstrong super-ego; consequently it becomes the task of the analyst to decrease its strength. According to Mowrer, neurosis is due to an insufficiently strong super-ego; consequently it becomes the task of the analyst to increase its strength. If Mowrer is right, Freudians have therefore tried to do something which would make the neurosis worse, rather than better; if he is wrong, he himself has tried to do something in his clinical work that made the patients' neurosis worse, rather than better. Yet both Freud and Mowrer claim, not only to cure their patients, but also to base their theories on 'clinical observations'. Which of these antithetical systems are our 'parents, teachers, managers, social workers, doctors, the clergy and so on' to be taught? How are we to decide between these rival claims, neither of which is stated in a scientifically testable form? It may be possible to test theories 'on the couch', but what if different couches give different answers?

Only one example has been given of the disagreements in the 'dynamic' camp; many others will come to mind. How, in the face of these facts, can it be maintained that 'we have enough knowledge already on which to found a single integrated science of human behaviour'? How can we accept Dr Sutherland's *non sequitur* and yet argue that 'we must...present to ourselves and to those responsible for the administration of the National Health Service that *compelling logic* that comes from the careful analysis of our

situation and which is the essential precursor of 'effective action'? Surely the only logically compelling deduction to be made from Dr Sutherland's presentation of the case is that we know very little, that 'dynamic' theories are neither verified nor in their present form verifiable, and that any *a priori* decision as to the value of different approaches must rest on subjective feelings, rather than on scientific demonstration. It would seem to follow, as pointed out above, that we should encourage diversity rather than the 'premature crystallization of spurious orthodoxies', and that attempts at integration and imbrication of different approaches and lines of thought are more likely to be rewarding than their arbitrary suppression in favour of one approach. Our need is not for an *integrated* approach to research, as Dr Sutherland thinks, but for an *integrative* approach. Similarly, on the practical side, attempts should be made to ascertain the exact consequences of all types of therapy advocated and used by serious students; indoctrination in favour of just one method appears quite unjustifiable, if only in view of the arousal of 'emotional conviction' so well described by Glover. Knowledge must precede application; 'in ignorance, abstain.'

REFERENCES

GLOVER, E. (1945). An examination of the Klein system of child psychology. *Psychoanal. Study Child*, **1**, 1–43.

MOWRER, O. H. (1950). *Learning Theory and Personality Dynamics*. New York: Ronald Press.

SUTHERLAND, J. (1952). Psychological medicine and the National Health Service: the need for an integrated approach to research. *Brit. J. Med. Psychol.* **25**, 71–85.