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Is Behaviour Therapy on Course?*

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When I was asked by Professor Aubrey Lewis (later Sir Aubrey Lewis) to undertake the establishment of the first school of clinical psychology in this country, and to establish a profession of clinical psychology, I went to the United States of America in order to study how this profession was organised (Eysenck, 1980a). The very hospitable reception I received did not lead me to advocate the establishment of a similar system in this country. It seemed to me that there were three major faults inherent in the American system.

(1) The psychologist was strictly subordinate to the psychiatrist, depending on him for employment, direction, and even terminology, theoretical models, and methodology.

(2) In general, the methods of treatment were based on psycho-analytic ideas and the methods of assessment (e.g. Rorschach and T.A.T.) were similarly based. Thus Freudian psychotherapy and projective testing seemed to be the be-all and end-all of clinical psychology.

(3) Clinical psychologists had little or no training in academic psychology, and were pathetically ignorant of the possible contribution that experimental psychology (both animal and human) could make to the understanding of their problems, and their solution.

On my return I determined to try and orient teaching and practice in this country in quite a different direction. Analysis of the literature, as well as experience, had shown me that psychoanalytic methods of psychotherapy did not "cure" patients more rapidly or more frequently than did the varied environmental effects which we summarise under the title of "spontaneous remission"; hence it seemed to me that to teach psychotherapy of this kind to students was little better than a confidence trick, and quite incompatible with the scientific status to which psychologists were aspiring. Consequently I determined that the teaching of psychotherapy should have no part in any programmes for which I was responsible. Similarly, a review of the literature on projective tests showed quite clearly that these tests were both unreliable and invalid; they violated the most elementary criteria of statistical construction, and there was just no evidence to show that they contributed to the

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successful treatment of patients. As a consequence I determined here too that the teaching of such tests should play no part in the instruction of the new profession.

Finally, I decided after a careful consideration of the literature on the application of psychological methods of treatment, from Watson and Mary Cover Jones to Guthrie and Mowrer, that the time was ripe for the development of a theory of neurosis and treatment based on modern learning theory, particularly Pavlovian conditioning and extinction. This new "behaviour therapy" together with proper use of nomothetic tests of intelligence and personality, was to constitute the major part of the teaching of future clinical psychologists.

This is not the place to enter into the obvious difficulties that arose from many different facts, such as the absence of any trained personnel to do the teaching, or the almost incredible hostility of most psychiatrists to the undertaking by psychologists of any kind of therapeutic functions. I think it is safe to say that behaviour therapy has been firmly established by now, both in psychology and in psychiatry; that the use of projective techniques has waned considerably, and that there is now at least lip-service to the application of a scientific approach to the problems of clinical psychology. Special journals like *Behaviour Research & Therapy* have been established, as have associations such as the E.A.B.T. These are all results that must count on the positive side, but the picture is by no means as favourable as it might appear at first sight. There are still many weaknesses in the training and the methods of working of clinical psychologists, and these threaten to undermine all the progress that has been made in the past few years.

Thus one might have hoped that the profession would not be subject to the rapid and uncritical growth of fads and fancies, such as the so-called cognitive therapies which have become so popular in the United States of America, and threaten to have the same success in the United Kingdom and in Europe. It is difficult to account for the enthusiasm with which cognitive therapies have been received, for two reasons. In the first place, as pointed out so well by Allport (1975) in his examination of the field, there is in fact no such thing as a "cognitive theory", i.e. a theory which has firm roots in the experimental laboratory, is expressed in testable form, and has positive achievements to its credit. As he points out, the movement is characterized by:

> An uncritical, or selective or frankly cavalier attitude to experimental data; a pervasive atmosphere of special pleading; a curious parochialism in acknowledging even the existence of other workers, and other approaches, to the phenomena under discussion; interpretations of data relying on multiple, arbitrary choice points, and underlying all else a near vacuum of theoretical structure within which to interrelate different sets of experimental results, or direct the search for significant new phenomena.

Theories of this type are not likely to help us in gaining a scientific understanding of the phenomena of neurosis and treatment!

Equally, the practical application of these methods to treatment has proceeded along the same lines as had earlier on psychodynamic types of psychotherapy. There is no attempt to test the new therapies against established behavioural methods, in the typical clinical kind of experiment. It would indeed be interesting to see how these new "cognitive" methods would stand up, say, to a comparison with the flooding and response prevention methods of treating obsessive–compulsive neurotics demonstrated by Rachman and Hodgson (1980). No such comparisons have come to my notice, and until they are made, and shown to favour significantly the new methods, one can only say that they have no scientific basis, have not demonstrated their efficacy, and hence have no scientific standing. Belief in them only indicates the biases of the person making judgement; there are no objective facts on which to base such a choice.

Equally sad is the continued failure of many clinical psychologists to heed even the most elementary statistical and experimental criteria in evaluating research on the efficacy of therapy. A recent book on *The Benefits of Psychotherapy* by Smith, Glass & Miller (1980) has received much acclaim from clinical psychologists, such as Dr D. Shapiro, and its thesis, namely that psychotherapy certainly works, and that there is little to choose between different types of psychotherapy, has been ardently embraced by many others. Yet the book is little more than a plethora of faulty designs, faulty arguments, and faulty statistics (Eysenck, 1983). To take but one or two examples, we find, for instance, that in comparing the many different types of psychotherapy with no-treatment control groups, placebo treatment is included, not among the controls, but among the treatments! The fact that placebo treatment does not begin to square with the definition of psychotherapy adopted by the authors themselves, and is always included as a control, rather than a treatment variable, is conveniently forgotten by the authors. When we look at a comparison between psychodynamic therapy and placebo treatment, as analysed by them on the basis of 108 studies of the former, and 200 of the latter, we find a tiny difference, just over one tenth of one standard deviation, in favour of psychodynamic therapy! We also find that any effects of treatment rapidly disappear over time, so that the only thing that could be claimed is a very small and evanescent effect of psychotherapy.

However, psychotherapy as understood by Smith *et al.* is clearly something entirely different from psychotherapy as understood by practising therapists. Thus they find that the length of training and experience of the therapist is completely uncorrelated with the effectiveness of his treatment! Nor is there any correlation between the duration of the treatment and its effectiveness, so that very short treatments of a few minutes or hours are equally effective as treatments continued over many years! If we were to take Smith *et al.* seriously, we would have to advocate a minimum degree of training of psychotherapists, and advise the trainees to carry out their therapy for only the shortest possible period of time! This certainly does not provide support for the views expressed by professional psychotherapists whether Freudian or otherwise, but rather flies in the face of all their theories and advocacies.

Much else could be said about this book, but the essential point I want to emphasize is that it has been taken seriously by people allegedly trained in scientific objectivity and statistical methodology, knowledgeable about experimental designs and hopefully able to tell the difference between gold and dross. That the conclusions of this book should have been so widely accepted by behaviour therapists is a sad commentary on their training and judgement.

Equally sad is the lack of interest among many behaviour therapists in the development of a proper theory concerning the origin of neurosis, and its treatment. Usually there is not only an absence of interest, but even an absence of knowledge of quite elementary points which are crucial to an understanding of, say, the conditioning theory of neurosis. I have suggested that it is crucial to an understanding of the events taking place in the development and extinction of a neurosis that we are dealing with Pavlovian type B conditioning, not Pavlovian type A conditioning (Eysenck, 1980b); yet in the many discussions I have had with behaviour therapists and other clinical psychologists in this country and elsewhere, I have found very few who knew of this distinction, or could apply it to the case in question. How many behaviour therapists could honestly say that they had pondered the application of the new findings in autoshaping to our conception of neurosis as a conditioned response? How many clinical psychologists habitually read the *Journal of Experimental Psychology*, or the *Journal of Comparative and Physiological Psychology*, in order to keep up to date with the experimental literature, much of which is relevant to their work? It is only necessary to ask the question to know what the answer is. This certainly is not the position I hoped to see when I set out to follow the mandate given me by Sir Aubrey Lewis. I had hoped to establish a profession of clinical psychologists well read in the experimental literature, capable of assessing and evaluating the literature, and capable of applying new ideas and methods firmly based on academic principles. The few examples I have given will suffice to show that reality is far removed from this hope. The undoubted successes of behaviour therapy have served to throw a veil around the less appetising reality that lies behind the image. Undoubtedly behaviour therapists are more successful than psychotherapists in helping their patients (a fact that even Smith *et al.* cannot disguise in their curious meta-analyses), but this undoubted success should be compared with what could be done if a more scientific approach were to be adopted. All sciences and all professions have to go through an ordeal by quackery before they reach the Promised Land of recognition and scientific respectability. Our task is by no means completed; it has only just begun!

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