CLINICAL PROBLEMS AND EXPERIMENTAL RESEARCHES: A REPLY

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In his address from the chair to the medical section of the British Psychological Society, Dr Russell Davis (1958) took as his jumping off point my recent monograph: *The Dynamics of Anxiety and Hysteria* (Eysenck, 1957). As the conclusions which he draws are rather different from mine, it would seem worthwhile to examine very briefly the reasons for this disagreement. If these reasons are of a factual nature, it may be possible to reconcile our differences; if they are not, it may be possible to get a little clearer the precise underlying assumptions from which our respective conclusions derive.

Davis points out correctly that what I am trying to do is to derive the treatment of neurotic disorders from a general and consistent psychological theory based on laboratory investigations. His main objection to this appears to be that: 'Although the problems to be resolved are clinical, the hypotheses, the ideas, which govern the researches are formed in the laboratory. In consequence, the laboratory theory decides to which symptoms attention is devoted, and these symptoms are in general not those to which clinicians attach major importance.'

There are two points to be made in reply. In the first place the objection was anticipated and answered on pp. 271 *et seq.* of my book, where I say: 'It might be argued that the papers referred to deal with monosymptomatic disorders quite unlike the usual run of anxious and depressed patients who seek help from the psychiatrist. How would a theory of the type here presented fare when applied to patients of this kind?' I then go on to quote in detail the work of Wolpe (1954) which deals precisely with the types of symptoms 'to which clinicians attach major importance'. Wolpe has demonstrated in the papers I quote, and even more so in his recent book (Wolpe, 1958), that learning theory does in fact generate hypotheses relating to treatment which apply to the major neurotic disorders and permit deductions regarding treatment to be made with considerable success. Davis's statement, therefore, is factually incorrect. Theories originating in the laboratory have in a large number of cases been applied with considerable success to patients suffering from the classical neurotic disorders more usually treated by psychotherapy.

My second point is this. We have in some of our work dealt with disorders such as tics, enuresis, writers' cramp, etc. for the very simple reason that the symptom can easily be quantified and thus treated as a dependent variable in an experimental design in which the treatment constitutes the independent variable. Our interest in these cases was not so much in curing the patient (which was incidental to the experiment), but in showing that the symptom responded in a very precise manner which could be predicted in terms of our theory, to variations in the treatment. In other words, we were interested in showing that psychoneurotic symptoms can be dealt with in the accepted experimental fashion and as laboratory phenomena; it was only natural that our choice was governed largely by practical considerations of this kind.

But surely the antithesis which Davis is making between clinical problems and laboratory hypotheses is an entirely artificial one. A physicist may be interested in the causes of lightning, the movement of the heavenly bodies or the meandering of rivers; these correspond to the 'clinical problems' of the psychiatrist. History has shown that success in understanding and controlling these phenomena does not usually come from their direct study, but rather from laboratory investigations into the nature of electricity, the rate of

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falling of different bodies or the behaviour of small-scale models in the laboratory. It is only when a reasonable theory has been elaborated on the basis of such laboratory experiments, a theory which can then be extended to the natural phenomena under consideration, that we can begin to obtain a proper understanding of these phenomena. And in checking the accuracy of his hypotheses, the physicist, no less than the psychologist, will in the first instance test those deductions which can most easily be quantified and measured. This is not to say that other phenomena must not also be dealt with in the same manner; I am merely pointing out what has always been the strategy of scientists in coming to grips with a complex and difficult subject.

Davis takes me to task for not having proved that the methods I advocate are generally more effective than are other methods. This again is not quite true. To go back again to Wolpe, whom I quote on this point: he has shown on the basis of a statistical comparison that methods deduced from learning theory are significantly more efficacious in the treatment of the major neurotic disorders than are psychoanalytic methods. It may be possible to take issue with him on various grounds such as selection of subjects, criteria of improvement, etc., but this is not what Davis does in fact do. He denies the very existence of such data, and here again, therefore, we must conclude that his discussion is based on a factual error.

Having dealt with errors of fact, we now come to a difference in assumptions. He states that 'It is generally assumed that psychotherapy based broadly on the suppositions enunciated by Breuer and Freud is effective in a wide range of cases...the assumption has been little if at all weakened by Eysenck's (1952) much discussed failure to demonstrate statistically in large pooled samples that "psychotherapy facilitates recovery from neurotic disorder"'. First of all, we may ask ourselves how widely held this assumption in fact is. Dr Weinstock, chairman of the Fact Finding Committee of the American Psychoanalytic Association, stated categorically, in a lecture delivered at the Maudsley Hospital, that *the American Psychoanalytic Association does not make any claims regarding the therapeutic effectiveness of psychoanalytic methods.* Similarly, Glover (1955), in his recent book, has explicitly disowned the assumption of therapeutic effectiveness of psychoanalytic methods. Many former psychoanalysts, such as for instance Albert Ellis, have expressed their lack of faith in precisely the assumption made by Davis, and have tried to elaborate new and better methods to supersede the psychoanalytic mode. If we must work with assumptions, then it would be interesting to know precisely who is making the assumption, and why such expert bodies and people as those mentioned are not willing to make it. Davis in fact is making his task a little too easy by simply disregarding the evidence and pointing to a certain assumed temporal contiguity of treatment and cure. The notion that *post hoc ergo propter hoc* is a valid logical principle has been hard a-dying, and apparently is still not quite defunct.*

* Altogether the logic of the argument somehow escapes me. Davis is concerned with abreaction and the recall of traumatic experiences, but abreactions in many ways similar to those observed by Breuer and Freud were described and deliberately produced a hundred years earlier by Mesmer, who also claimed considerable successes for his methods. He also used the alleged successes as an argument in favour of his theory of celestial magnetism. It would be easy to rephrase Davis's argument to read: 'It is generally assumed that magnetic therapy based broadly on the suppositions enunciated by Mesmer is effective in a wide range of cases....' Would Davis go on to say that the experimental psychologist should take his cue from such a 'clinical' statement and concentrate his energies on the study of magnetism? The assumptions made by Mesmer and his followers were based on the same kinds of observation as those made by Freud and his followers, and both parties adduced many examples of therapeutic successes allegedly due to their methods of treatment based on their particular theories. Why should we accept one and reject the other argument in the absence of those experimental and statistical studies which alone can give us a rational cause for effecting such a choice?

My own position, of course, is a very simple one, namely that in such an important area assumptions, even if they were as widely held as Davis mistakenly believes, are not sufficient proof for the correctness of a given view, and require specific experimental and statistical support. The history of medicine is full of examples where assumptions of this kind were almost universally held, only to be disproved a little later. Even in the more exact sciences we need only recall the almost universal assumption that the earth was flat, or that it was in the centre of the universe, to recognize that assumptions, however firmly held, derive no scientific validity from the firmness of the belief of the person holding them.

Altogether then, Davis appears to me to be suggesting a course of action precisely counter to that which is usual in science. Abnormal psychology is an applied science; clinical work generates problems but must for the solution of these problems depend on the pure science of psychology. The application of psychoogical principles to the explanation of neurotic disorders and their cures is undoubtedly complex and difficult but not in principle impossible; in *The Dynamics of Anxiety and Hysteria*, I have tried to take some steps in this direction and have given some examples of how this could be done. Davis would appear to want to reverse this process. Starting out with an unproven assumption, he wants vague clinical hunches to determine laboratory investigations, thus putting the cart before the horse in the almost literal meaning of that phrase. He does not at any point give any reasons for reversing the traditional scientific procedure, but appears to base his views entirely on the 'assumption' of therapeutic usefulness of psychoanalytic methods. In arguing his case, as pointed out in the first few paragraphs of this note, he has gone counter to fact in a number of statements, and it must be assumed that, in so far as his argument considers these statements relevant, the fact that they were erroneous must lead him to adopt a position contrary to the one advocated in his paper.

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