

REPLY TO A "CRITIQUE AND REFORMULATION" OF BEHAVIOR THERAPY

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It is argued that Breger and McGaugh's (1965) criticisms are misguided and that they fail to mention numerous studies and arguments which support the view that behavior therapy is an encouraging development and has already achieved some therapeutic success. Attention is drawn to various "laws of learning" which have been employed in constructing treatment techniques and for generating and assessing specific hypotheses. Several doubtful assertions made by Breger and McGaugh are discussed and factual errors are corrected. Their suggested reformulation of behavior therapy is rejected as being fragmentary, vague, and unconstructive.

This reply to the recent paper by Breger and McGaugh (1965) will confine itself to a small number of crucial points; we will not discuss in detail, among others, two main contentions put forward by those authors. One of these is their "reformulation," according to which learning conceptions of neurosis should make use of the "acquisition of strategies." The suggestions made under this heading are so fragmentary, programmatic and elusive that we fail to see either their theoretical usefulness or any practical consequences which might follow from them; when Breger and McGaugh have some actual applications to report, or have at least succeeded in showing how the major facts of neurotic behavior can be accounted for in terms of their scheme, then may be the appropriate time to take issue with their "reformulation." The other contention relates to their preference for an "Expectancy \times Value" type of theory, as compared to a "Drive \times Habit" type of theory, to use Atkinson's (1964) phrase. They are, of course, free to make any preference choice they like, even without repeating at some length arguments presented many times before; here too, however, one would require some more direct evidence indicating that Expectancy \times Value theories give rise to different and more efficient methods of treatment than Drive \times Habit theories before entering into any formal argument. As this point is crucial to certain other assertions made by Breger and McGaugh, however, it will be referred to obliquely again below.

The first criticism made by Breger and

McGaugh is labelled "science issue"; they feel that there is no such thing as "modern learning theory," that there is no agreement on sufficient points to make testable predictions and applications to the treatment of neurotics, and that behavior therapists are wrong in claiming that their procedures are based on scientific theories. Evaluation of this point may be aided by consideration of a quotation from Sir George Thomson, F.R.S. and Nobel-Laureate in physics. He points out that

if differences of opinion . . . are still possible about space, time, and gravitation, this is an example of something common in physics. Very different points of view may lead to identical or nearly identical conclusions when translated into what can be observed. It is the observations that are closest to reality. The more one abstracts from them the more exciting indeed are the conclusions one draws and the more suggestive for further advances, but the less can one be certain that some widely different viewpoint would not do as well [1961, p. 15].

Much the same is true in psychology. MacCorquodale and Meehl (1954), Atkinson (1964), and many others have pointed out that Expectancy \times Value and Drive \times Habit theories overlap in many ways, and give rise to similar predictions, although experimentalists may show a preference for one or the other of two ways of talking about phenomena. But both are agreed about most of these phenomena, and it is these which "are closest to reality," and which form the factual, scientific basis of behavior therapy. No learning theorist of any persuasion would deny statements of behavioral laws of this kind:

"Reinforced pairings of CS and UCS under appropriate conditions produce conditioning"; "Intermittent reinforcement slows down extinction"; "Nonreinforcement produces extinction"; "Different schedules of reinforcement produce predictably different response rates." It is laws of this type that are made use of by behavior therapists, who may choose to talk about them in the language of Hull, Tolman, Skinner, or any other major learning theorist. As an example, consider the work of Lovibond (1962) who made detailed predictions on the basis of the known facts of learning theory for the behavior of enuretic patients, and showed how in doing so he could (a) accelerate recovery and (b) reduce relapses; Young and Turner (1965) may furnish another example in the same disorder. Many others are given in Eysenck (1959, 1964), Ullmann and Krasner (1965), Krasner and Ullmann (1965), Eysenck and Rachman (1965), Rachman (1965a), and others. The application of scientific principles to any area must be specific, and must be discussed in terms of specific results; Breger and McGaugh's failure to do so makes their ex cathedra condemnation meaningless.

This lack of specificity, unfortunately, runs throughout their paper.

On the critical side, their argument primarily consists of doubtful assertions presented as if they were self-evident truths. They often contradict themselves and also distort the nature of behavior therapy.

How, for instance, are they able to conclude that their quotations from the three case histories mentioned on page 353 are representative ("they seem representative of the practices of behavior therapists")? As two of the quotations were in fact taken from cases reported by one of the present writers, we take this opportunity to point out the following facts. The two sentences quoted from the treatment of patient A. G. (Rachman, 1959) describe *one* incident which occurred during the course of 22 interviews. At no time prior to the treatment of that patient, nor in the succeeding 6 years of work in this field, has a similar incident been encountered. Is this representative of behavior therapy as Breger and McGaugh claim, or is it a distortion caused by ignorance of therapeutic practice and of

the literature on the subject? If Breger and McGaugh wish, in the other examples quoted, to indicate that behavior therapists actually speak to their patients and explain the rationale and nature of the treatment to them, then their point is taken even though it does lack novelty. Perhaps they are unaware that during the course of therapy, be it desensitization or any other method, the therapist also attempts to locate any sources of stress which may be provoking or maintaining the neurotic behavior. Where possible, these stresses are eliminated or at least ameliorated. The cases (of psychotic patients in these instances) described by Ayllon (1963) and Ayllon and Michael (1959) illustrate clearly how improvements can be obtained by breaking the links between stimulus and response patterns as they occur in the patient's environment (Eysenck & Rachman, 1965).

Breger and McGaugh's paper is also self-contradictory. Immediately after deploring the emergence of a so-called dogmatic school of Behavior Therapy ("it is unfortunate that the techniques used by the Behavior Therapy group have so quickly become encapsulated in a dogmatic 'school.'") they proceed to distinguish between the "three different positions." They also imply that behavior therapy is oversimplified (e.g. p. 346); in other parts of the paper, it is said to be cumbersome (p. 348). Behavior therapists certainly pursue simplicity both in theory and in practice; this seems to us to be a desirable aim in itself and a welcome contrast to the convolutions of other psychotherapeutic theories. This contrast is neatly, if inaccurately, demonstrated by Breger and McGaugh themselves.

The behaviorist looks at a neurotic and sees specific symptoms and anxiety. The psychodynamicist looks at the same individual and sees a complex intra- and interpersonal mode of functioning which may or may not contain certain observable fears or certain behavioral symptoms such as compulsive motor acts. When the psychodynamicist describes a neurotic, his referent is a cohering component of the individual's functioning . . . etc. [p. 349].

The doubtful assertions contained in the paper by Breger and McGaugh are numerous and cannot be reproduced in full. The following examples could be multiplied without effort. "What is learned," then, is not a me-

chanical sequence of responses but rather, *what needs to be done in order to achieve some final event* [p. 342].” Is all learning really an attempt at achievement? Have neurotic patients presumably also “learned what needs to be done” in order to achieve a neurosis? A conditioned PGR is, likewise, a doubtful achievement. The list is endless, but in any event who decides “what needs to be done,” or what a “final event” is, or when it is achieved? The phrase “some final event” is hardly a model of precise definition.

Another doubtful assertion is the statement that Harlow’s experiments with monkeys provide a “much better animal analogue of human neuroses than those typically cited as experimental neuroses [p. 356].” This cavalier dismissal of the mass of work in the subject of experimental neuroses (see Broadhurst, 1960; Massermann, 1943; Wolpe, 1952; etc.) is neither explained nor justified by Breger and McGaugh. Their attitude to the evidence seems to stem from a belief that “saying so, makes it so.”

Their assertion that the “attribution of behavior change to specific learning techniques is entirely unwarranted” is also misguided and appears to be based on ignorance of the relevant evidence. No mention is made of the experiments of Lazarus (1961), Wolpe (1952), Eysenck (1964), King, Armitage, & Tilton (1960), Lovibond (1962), or of the studies of Ayllon and his co-workers (1959, 1963). They will further be surprised by the accumulation of recent studies which bear on this point and which, with minor exceptions, corroborate the viewpoint of behavior therapists (see Eysenck, 1964; Eysenck & Rachman, 1965; Krasner & Ullmann, 1965; Rachman, 1965; Ullmann & Krasner, 1965, among others). The currently available evidence will, we feel certain, convince all but the most biased workers that the methods of behavior therapy are indeed effective in the modification of neurotic behavior. Not all the methods are successful; nor is it yet possible to treat all types of disturbances successfully. There is an immense amount of developmental work and experimentation which remains to be done, but a degree of optimism is not misplaced.

Breger and McGaugh are surely correct in

drawing attention to the deficiencies of learning theory; most of their criticisms, however, have been stated by others before them. In any event, a detailed consideration of all their comments would be inappropriate here. Their arguments about the problem of perceptual constancy, for example, have been amply analyzed by Taylor and Papert (1956) and Taylor (1962), and the restating of their complex arguments and experiments would be out of place. The concept of reinforcement is of course replete with complexities and seems to us to be best regarded in terms of Mowrer’s two-factor theory (1960). The difficulties which arise from a consideration of central activities such as thinking were discussed in an earlier review by Metzner (1961)—one which they appear to have missed—and again 2 years later (Metzner, 1964).

Certainly, it would be exceedingly foolish to regard “learning theory” as a complete, coherent, and final account of human behavior. This does not mean, however, that people engaged in therapy should ignore the established findings and the best available theories. Quite the contrary. We feel that they are obliged to use these findings and ideas wherever it is feasible to do so. Furthermore, four of the main techniques used in behavior therapy (desensitization, aversion treatment, operant retraining, and the “bell-and-pad” method) were derived solely or very largely from these findings and ideas. It is highly improbable that these methods would have been developed to their present stage and form *sui generis*.

Perhaps the most revealing reflection of the attitude of Breger and McGaugh to the entire subject of behavior modification is contained in their curiously unimaginative description of Skinner’s work as “exercises in animal training.” Some notion of the wider significance of the pecking of pigeons can easily be ascertained from the work of Staats and Staats (1964) and Krasner and Ullmann (1965) among others.

Not merely doubtful, but definitely wrong, is the assertion that behavior therapists

have partly avoided this problem [generality] by focusing their attention on those neuroses that can be described in terms of specific symptoms (bed-wetting, if this is a neurosis, tics, specific phobias,

etc.) and have tended to ignore those conditions which do not fit their model, such as neurotic depressions, general unhappiness, obsessional disorders, and the kinds of persistent interpersonal entanglements that characterize so many neurotics [p. 348].

This is wrong factually in two respects. Firstly, a large number of patients with interpersonal anxiety and a moderate number of obsessional patients have in fact been treated (e.g., Lazarus, 1963; Wolpe, 1958). Secondly, Wolpe (1958) and most other therapists did not focus their attention on anything in particular other than the symptoms presented by their patients, *who were not selected or chosen by the therapists*. Others, like Lovibond (1962), Lang and Lazowik (1963), Yates (1958), and the present writers (Eysenck & Rachman, 1965) have indeed experimented with specific symptoms, but not in order to avoid the theoretical problem of generality—the reason was simply that if specific predictions are to be tested, then responses must by preference be accurately measurable. It is possible to count the rate at which tics occur, the number of wet nights per week, or the strength of a snake phobia; therefore, it is possible to experiment with the effect of changing various independent variables on these dependent variables. This choice therefore permits the testing of quite precisely the sort of predictions which according to Breger and McGaugh cannot be made from learning theory principles; it would be interesting to hear their explanation of just how it is that verification has usually followed prediction!

Finally, we turn to criticisms of "claims of success." Breger and McGaugh state that "the most striking thing about this large body of studies is that they are almost all case studies. A careful reading of the original sources reveals that only one study (Lang & Lazowik, 1963) is a controlled experiment [p. 351]." This is simply not an accurate statement of the position as it obtained at the time of writing of the Breger and McGaugh review (June 1964 is the acceptance date). They do not refer to the work of Cooper (1963), Lazarus (1961), Ellis (1964), Anker and Walsh (1961), Lovibond (1962), and others, and their horizon is clearly bounded, as they themselves admit by the fact that theirs "does not purport to be a comprehensive re-

view of the behavior-therapy literature." Rather, it is based on a survey of all the studies reported in the two reviews that have appeared (Bandura, 1961; Grossberg, 1964)." This seems to us an inexcusable defect. Behavior therapy may be said to have begun properly around 1958–59, with the publication of the Wolpe (1958) book and Eysenck's (1959) paper proposing the name "behavior therapy" and stating in some detail its nature and purpose. Given that controlled experiments take several years to execute, write up, and publish, it is clear why summaries of the field published in 1961 or even 1964 would not be adequate substantiation for such a far-reaching condemnation of a whole branch of study. Familiarity with *Behaviour Research and Therapy* (Pergamon Press), a journal concerned entirely with research in behavior therapy and nowhere referred to by Breger and McGaugh, would have served adequately to bring them up to date in this field. (It may be added that several controlled trials of behavior therapy are in progress, to our knowledge; three of them prospective and one retrospective, Marks and Gelder, 1965, in the Maudsley Hospital alone.) Even the Eysenck and Rachman (1965) textbook, which went to press 6 months earlier than the Breger and McGaugh article, is very much more up to date than their account (additional evidence is discussed by Cooke, 1965; Davison, 1965; Paul, 1964; Rachman, 1965a, 1965b).

We must say, indeed, that we feel quite strongly that the burden of Breger and McGaugh's criticism is entirely misplaced. In half a dozen years a relatively small number of behavior therapists, with little official support and often against the most hostile opposition, have succeeded in carrying out more controlled (and better controlled) studies than have hundreds of psychiatrists and psychoanalysts in 60 years, with all the financial resources and the prestige so readily available to them. Even so, we do not consider our studies as in any way beyond criticism, nor do we feel that they go nearly far enough, or are sufficient to establish behavior therapy as superior to other types of therapy in any definitive way. We have concluded in our textbook (Eysenck & Rachman, 1965)

that "the routine use of these methods is undoubtedly not yet feasible; it must await further improvement of techniques and definitive evidence of superiority over other available techniques [p. xii]." This is still our view, and nothing said by Breger and McGaugh would seem to contradict this summary or throw doubt on its accuracy. To call views of this kind "dogmatic" seems a curious misunderstanding of the meaning of the word.

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