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BEHAVIOR THERAPY AND ITS CRITICS

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Summary—The many criticisms of various aspects of behavior therapy may be grouped under three main headings:
1. Modern learning theory is not sufficiently advanced and specific to make application to the treatment of mental disorders possible.
2. Theories such as those of Wolpe, Skinner and others do not provide a way of applying learning theory to these problems.
3. The results of behavior therapy have not been shown to be superior to those achieved with more orthodox methods.

These criticisms are discussed in some detail and it is concluded that they are either untrue, irrelevant, or based on misconceptions.

BEHAVIOR therapy has become widely accepted by psychiatrists and clinical psychologists since the term was first used to denote all methods of behavior modification based on modern learning theory and laboratory practice (Eysenck, 1958; 1959). Skinner and Lindsley (1953) had independently used the term previously, but in a much more restricted sense, confining its meaning to operant conditioning; and Lazarus (1958) had applied it to Wolpe's reciprocal inhibition methods. The publication of Wolpe's *Psychotherapy by Reciprocal Inhibition* (1958) and of Eysenck's *Behaviour Therapy and the Neuroses* (1960) effectively drew attention to the potentialities of what was essentially an alternative theory of disorder and method of treatment to the prevalent psychoanalytic or dynamic theories and methods; Eysenck and Rachman's *Causes and Cures of Neurosis* (1965) became the first text-book of this new movement, attempting to draw together the many and variegated efforts to apply psychological science to mental disorder. Inevitably this new orientation attracted much criticism (e.g. Breger and McGaugh, 1965; Weitzman, 1967; Freeman, 1968; Gelder, 1968; Lazarus, 1967, 1968; Kubie, 1968; and many others), and it will be the purpose of this paper to see to what extent these criticisms are in fact justified. In science, criticism can play an important and salutary role—provided it is based on knowledge and proper understanding; it can also be misleading and non-productive—if it simply reiterates preconceptions and misunderstandings. Examples of both kinds are to be found in plenty in the published literature (Eysenck and Beech, 1970).

Critics have concentrated on three main points, which correspond to the main contentions of behavior therapists. The latter claim (1) that modern learning theory is sufficiently advanced and specific in its laws and recommendations to make application to mental disorders and the behavior patterns characterising them possible; (2) that theories such as those of Wolpe, Skinner and others provide a way of applying learning theory to these problems; and (3) that when so applied, results are superior, both with respect to percentage of cures achieved and length of time required, to orthodox methods. Critics deny all three points. They argue that modern learning theory is so hopelessly split into different schools that no undisputed laws emerge from the writings of the leading protagonists; that even if there were such laws, they would not be of a kind which could be applied directly to the treatment of mental disorders; and that where attempts have in fact been made to use such alleged laws, success has been doubtful and the evidence for improvement unclear. Each of these sets of criticisms can be broken down into subsets, making more precise the disagreement. An attempt is made in this article to formulate the most widely repeated and meaningful subsets, and to determine to what degree they are in fact applicable to modern behavior therapy.

FIRST SET OF CRITICISMS

Learning theory does not provide a suitable basis for any type of therapy because it is not sufficiently advanced in its conceptions, or agreed in its formulations

(a) There is in fact no single 'learning theory' which could be used to generate deductions for use in therapy; there are many 'learning theories' which disagree on fundamental points, and it becomes an arbitrary act of choice as to which of these theories is in fact adopted. As Eysenck and Beech (1970) have shown, there are at present at least four models which can be used to account for the facts of 'desensitization therapy', all making use of fundamental theoretical concepts.

(b) Learning theories were formulated to deal with the problems of simple learning situations in rats; they cannot account for the complexities of human neuroses. Even in the animal field, theories are becoming more 'cognitive', making the theories used by behavior therapists seem old-fashioned and redundant. Experiments on 'constancy' and 'transposition' strongly suggest the importance of 'mediational' precesses.

(c) Human behavior, and in particular neurotic and psychotic illnesses, deal with 'internal' events—value judgments, thinking processes, attitudinal phenomena and the like; learning theory deals largely with 'external', observable behavior. The dependence and meaning of symptoms are traceable to complex internal processes which can only be forced into a behaviorist framework by purely verbal tricks, such as calling imagined and other cognitive events 'responses'.

Criticisms such as these are difficult to refute, partly because they contain an element of truth. It is always true to say that scientific theories (particularly in rapidly advancing areas) are not agreed upon by all experts in the field; that many phenomena can be explained along divergent lines; that any particular view is 'oversimplified' and does not do justice to the multiplicity of phenomena. Most psychologists seem to take as their example of scientific theory Newton's conception of universal gravitation; but this was a very exceptional case, quite unlike the more usual and mundane type of theory which the physicist or the chemist might encounter in his researches. Even in Newton's own time there were so many discrepancies between theory and fact that he pleaded 'divine intervention'—i.e. the notion that angels were pushing the planets out of their appointed courses. He failed to account for the motions of the moon, in spite of prolonged work and cogitation. Even his theoretical and mathematical contributions were (rightly) criticized by the French school as "lacking in rigor"—a rigor finally introduced over 100 years later by Cauchy in his *Cours d'Analyse*. One of the problems that defeated Newton was the precession of Mercury's perihelion; Mercury's orbit moved round the Sun some 43 sec of arc per century faster than Newton's laws allowed for. Did Einstein do better? His equations gave a figure of 43·03 sec per century for the discrepancy, which made this one of the three main supports of his general theory of relativity (the other two being the reddening of the light emitted from a source with a strong gravitational field, and the bending of light rays by a gravitational field). But recent work by Dicke has shown that the predictions are in error because they assume the Sun to be perfectly circular; in fact it is flattened by 5 parts in 100,000, a degree of distortion which would cause nearly 4 sec of arc out of the unaccounted-for 43 sec of arc of Mercury's precession round the Sun. We thus have a number of theoretical explanations of the observed phenomenon. Newtonians suggest the effects of the solar corona, of the matter that causes the zodiacal light, and of an invisible interior planet. Einstein's prediction depended on the speed of rotation of the planet. Dicke himself, who had produced a theory combining Einstein's curved space with the idea of a Newtonian force field, used this scalar-tensor theory in preference to either. The only theory to give an accurate quantitative account of the phenomenon is the one least widely accepted by physicists!

This example, brief though it is, is quoted to show how in one of the oldest problems in physics even some of the most elementary facts are still without explanation, and that the absence of an agreed theory has not caused astronauts and other 'users' to wait for the arrival of an agreed theory which could 'explain' *all* relevant phenomena. The position is much more chaotic still in fields like modern sub-atomic theory, where recently (1957) the principle of parity was overthrown, to be followed soon (1964) by the principle of PC symmetry, and where even now the famous PCT theorem is in danger. Yet this revolutionary upset at the very foundations of modern physics has left the applications of modern theory quite undisturbed; fission and fusion bombs still seem to work as well as ever (unfortunately!). Clearly the notion that one single, universally agreed, all-explanatory theory is needed in a scientific field before application becomes possible is wide of the mark; to use it in order to discredit behavior therapy shows nothing more than a certain amount of ignorance of the way theory is used in the 'hard sciences'. Those who argue in this way arbitrarily set up an impossible state of perfection as the necessary requirement before any application of theory becomes possible; naturally they then argue that behavior therapy is impossible!

It is not my purpose to argue that modern learning theory is more perfect than it is; no-one could be more aware of its imperfections. But however poor, it is all we have; if we cannot make any deductions from this body of knowledge, then we cannot make any deductions at all. I do not believe that anyone aware of the vast amount of factual material available in this field could seriously maintain such a position; granted that something useful, valuable and worthwhile is contained in such books as Hilgard's *Theories of Learning*, or Kimble's *Learning and Conditioning*, then it becomes the behavior therapist to dig out the parts which are relevant to his work, and try to incorporate the principles in question in his experimental and clinical work. It is difficult to see why he should be exhorted to use in this way the vague speculations of the Freudians, which completely lack experimental support of any kind, and forswear the well-supported facts of the experimentalists. As Pasteur said: "Without theory, practice is but routine born of habit. Theory alone can bring forth and develop the spirit of invention." Behavior therapists choose to employ the theories of academic psychology, based on laboratory evidence, in furthering this spirit of invention. *A priori* arguments of the kind listed above are not likely to prevent them from doing so.

It is almost inevitable that such 'inventions' should be castigated as 'oversimplifications' by those who favor the old-fashioned 'dynamic' type of view. Science by definition does not aim to encompass all the phenomena in a given field; Newton's law did not deal with the color of the Earth, nor did it concern itself with the religious beliefs of its inhabitants. Science lives on abstraction; it abstracts those features which are important and relevant to its purpose from those which are not. This is absolutely essential for scientific progress, however much it may displease those who have a sentimental attachment to the features which the scientist rejects. 'Internal', 'cognitive' and other complex processes may be considered useful and even essential by some theorists; learning theorists may prefer to make do with fractional antedating responses and r_g–s_g mechanisms. There is no implication of absolute rightness and truth in this preference, only a recognition of what kind of theory is more likely to be useful and satisfactory in the long run. It is open to other scientists to advance other, more complex and cognitive theories; if these work better, then they will no doubt supersede the less complex and inadequate ones. But such proof is needed *before* rejecting the behaviorist approach; it has to be *demonstrated* that other theories can do better before preferring them and critising their rivals. Such demonstrations are conspicuously lacking in the therapeutic field.

The main point made here is that simplification in science is not a fault, but a virtue; not an indulgence, but a necessity. At an early stage of development, simplification easily appears overdone, incapable of doing justice to the vast complexity of nature. But there is no alternative; if we attempt from the beginning to do justice to the complexity of things, then we simply get lost, and emerge with theories such as the Freudian, which explain everything and predict nothing. If we begin with a vastly oversimplified theory, then we can gradually work out which parts seem to fit and which not; we can then go on to improve the theory by dropping out some parts, and trying out others. Eventually, we will emerge with a rather more complex theory, but one which is experimentally supported in all its aspects. Mendel was laughed out of court when he tried to reduce genetics to the simple ratios of wrinkled to smooth peas in the offspring of his parental generation, but he was right; all those who tried to deal with the complexity of many different characters simultaneously came to grief, while his oversimplified scheme laid the foundations of a new science. I conclude that the first objection to behavior therapy is true but irrelevant; learning theory is not as well developed as we would like, but it is better than nothing. In the absence of anything better, we have no choice but to use whatever parts of it we can incorporate in our schemes; in this way we will be able to improve our therapy, and we shall also be able to discover the weaknesses in learning theory which can only be brought to light by its rigorous application to life situations. In this way, both learning theory and behavior therapy stand to gain by their close interaction.

SECOND SET OF CRITICISMS

Behaviour therapy does not in fact derive from learning theory, but merely expresses its preferred methodology in the language of learning theory

(a) Behavior therapists do not in fact make any use of the concepts of learning theory; they merely appeal to this theory in order to demonstrate their scientific ability.

(b) In behavior therapy situations the very terms of 'stimulus' and 'response' become 'remotely allegorical' (Breger and McGaugh, 1965), and do not carry the same meaning as they do in the experimental laboratory.

(c) The notion of a neurosis as a 'habit', and the identification of 'symptoms' with the neurosis, are erroneous; neurosis is in fact a complex structure involving not only 'habits' but also values, attitudes, false beliefs, personal constructs, and many other cognitive concepts.

(d) Behavior therapists often have difficulties in dealing theoretically with the successful outcome of dubious applications of learning theory, e.g. the apparently sometimes successful outcome of the application of 'backward conditioning' in the aversion therapy of alcoholics.

(e) The application of animal-based findings to human beings is misleading; the emergence of language, social facilitation, the importance of verbal instructions, etc. demand a complete restructuring of such theories.

Here too we may with advantage quote Pasteur: "If anyone should say that my conclusions go beyond the established facts, I would agree, in the sense that I have taken my stand unreservedly on an order of ideas which, strictly speaking, cannot be irrefutably demonstrated." Deductions made by behavior therapists from learning theory are not sacrosanct; they may be mistaken, trivial, or even counter to fact. Where they are wrong, experiment will show them to be so. The importance of behavior therapy does not lie in any one, single deduction, but rather in a climate of opinion which elevates empirical proof to be the only judge of the value of a theory, or of a deduction from a theory; which prefers to make simple but testable statements to making complex but untestable ones. Its main value lies in the introduction of this 'order of ideas' which reduces psychotherapy from a *mystique* to a science.

The importance of this transition from the 'clinical' to the 'experimental' approach was fully realized by Pasteur:

"Physicians are inclined to engage in hasty generalizations. Possessing a natural or acquired distinction, endowed with a quick intelligence, an elegant and facile conversation . . . the more eminent they are . . . the less leisure they have for investigative work . . . Eager for knowledge . . . they are apt to accept too readily attractive but inadequately proven theories."

The whole history of psychiatry is eloquent witness to this statement; Freudian theories achieved wide acceptance in the complete absence of factual support for their consequences. What is important in behavior therapy is not the value of any particular deduction from learning theory, but rather the recognition that (1) such deductions, as rigorous as possible, are desirable in themselves, and (2) that these deductions require empirical, and preferably experimental, proof. It is freely admitted that not all such deductions are realistic or even sensible; not all behavior therapists are budding Newtons, and some make errors. Even when the deductions made seem to be sensible and correct, there is no certainty that they will work in practice; even in the hard sciences deductions sometimes come adrift. All this should not need spelling out; it is taken for granted in other sciences, and in other applications of science to practice. There is no reason to assume that behavior therapy should be in a different position. Its practitioners range from the competent, the knowledgeable, the wise, to the incompetent, the ignorant, the foolish; it should be judged by the acts of the former, rather than the derelictions of the latter.

Two points are sometimes made in this connection. It is said that behavior therapy is but common sense, and that in some form or other it has existed since time immemorial. The other point is that essentially behavior therapy is but a verbal restatement of psycho-analysis, and that this translation into the language of conditioning adds little to the value of psychotherapy. It is certainly true that common sense has here, as elsewhere, anticipated certain statements of modern learning theory; the use of rewards and punishments antedated Skinner and Thorndike. So did the knowledge that bodies left unsupported fall to the ground antedate Newton. Pasteur, too, was anticipated; as he points out, "as early as the first century B.C., Varro and Columella had expressed the idea that disease was caused by invisible living things—*animalia minuta*—taken into the body with food or breathed in with air." Yet we still give some credit to Newton and Pasteur! Plutarch tells the story of Demosthenes who suffered from a shoulder tic; he hung a very sharp sword above his afflicted shoulder, so that every time his tic caused him to move his shoulder upwards he suffered a painful prick. Plutarch reports a complete cure through this first example of aversion therapy! This anticipation is interesting, but it is difficult to see why it should be taken to detract from the importance of the general principle of reinforcement, or the value of the introduction of aversion therapy.

The notion that behavior therapy in some sense is merely a restatement of Freudian therapy in a different language probably derives from the efforts of Dollard and Miller (1950) to do precisely this—to translate Freud into the language of learning theory. They assumed the validity of the Freudian theory, and made no attempt to prove this point; their book is essentially a dictionary. Modern behavior therapists hold views so different from the ones espoused by Dollard and Miller that no identification can be considered possible; I have given a list of differences between psychotherapy and behavior therapy (Eysenck, 1959) which indicates the extent of this difference. In view of statements such as this, something more than a mere assertion of identity would seem to be required; it has not been forthcoming.

Let us consider a single example. Mowrer (1938), Lovibond (1964) and other learning theoriests regard enuresis as a simple habit deficiency, the effects of which may lead to anxiety through the socially undesirable consequences of the act. Psychoanalysts view enuresis as a form of sexual gratification; they discuss it as a substitute for gratification of repressed genital sexuality, as a direct manifestation of deep-seated anxieties and fears, or as a disguised form of hostility towards parents or parent substitutes. Enuresis is thus viewed as a regressive phenomenon, produced by intense anxiety following repression, an anxiety which has its source in tabooed impulses of a sexual or aggresive and hostile character. These notions do not seem to me in any way identical or similar to those which incorporate the view of enuresis as a simple habit deficiency; certainly some argument would seem to be required if the two theories are to be accepted as formally equivalent! Furthermore, the type of therapy suggested in either case appears quite different—bell-and-blanket, or stimulant drug, in the one case, long-continued intensive Freudian psychotherapy in the other. In the absence of any attempt to support this vague and in my opinion untenable view, further discussion would seem useless.

THIRD SET OF CRITICISMS

The evidence for the efficacy of behavior therapy is too weak to regard it as a useful method of treatment

(a) There are no satisfactory studies of behavior therapy using adequate numbers, proper controls, and suitable methods of evaluation.

(b) Comparison with other forms of treatment are largely meaningless, because of the possible lack of comparability of studies in terms of types of patients treated, outcome criteria, different aims of therapists, etc.

(c) Many of the papers published by behavior therapists contain single case histories; these are just as anecdotal as similar papers published be psychoanalysts, and prove nothing, however successful the outcome.

(d) The patient–therapist relationship is a crucial agent in behavior therapy, contrary to behavior therapist's theories, suggesting the importance of psycho-dynamic influences (transference).

(e) Other factors enter the therapeutic process (reassurance, suggestion, directives) than are envisioned by behavior therapists, and these additional and fortuitous elements play an important part in the cure.

(f) The study of neurosis-analogues, e.g. snake phobias or spider phobias, throws no light on the processes of treatment when real and serious neurotic illnesses are being treated.

(g) Relapses and symptom substitution are not ruled out because of lack of lengthy follow-ups, and may invalidate claims of 'cures'.

(h) Behavior therapy is relevant only to monosymptomatic phobias, not to the great majority of neurotic illnesses.

It is not unreasonable to say that the evidence in favor of behavior therapy is not conclusive; Eysenck and Rachman (1965) and Beech (1968) are among the many behavior therapists who have called for more and better controlled studies. Criticism, however, comes less well from orthodox psychiatrists and adherents of the Freudian school, who have consistently made unsupported claims for psychotherapy and who have equally consistently refused to take the outcome problem seriously, or to perform the required research to put their claims on a proper empirical footing. Even in the short 10-year period of its existence, behavior therapy has been instrumental in generating many times as much research into the outcome problem as has psychoanalysis, and few objective reviewers would contradict the claim that this work has been of a much higher standard than the few, subjective observations vouchsafed posterity by psychoanalytic investigators, in spite of the length of time that psychoanalysts have made intemperate claims for the effectiveness of psychotherapy.

It is in fact unreasonable to ask for an ideal, all-inclusive experiment, which would answer all our questions; in the present poorly developed state of psychiatric knowledge such an experiment is nothing more than a *fata morgana*. What we have to do is make a provisional attempt to form a judgment of the different claims of psychotherapsts and behavior therapists on the basis of existing studies, always with the explicit understanding that new material might change or even reverse our judgment, and that little certainty attaches to the outcome of isolated, early investigations in such a complex and difficult field. In doing so, of course, we must treat both claimants in a similar manner; it is all too clear from the literature (e.g. Porter, 1968) that there is one law for the rich, another for the poor. Psychoanalytic statements without any empirical support whatever are accorded the status of self-evident truths, while the claims of behavior therapists are subjected to a critical examination which goes far beyond anything that is customary in psychology or psychiatry. We have already had occasion to remark on the demands made of learning theory, which go so far as to rule out of court practically all the well-known theories in physics as being equally lacking in all-embracing deductive power, universal agreement and complete rigor; by making arbitrarily severe demands any theory and practice in science and medicine can be shown to be wanting! I do of course support very strongly every attempt to make the study of therapy more scientific, rigorous and objective, but such demands must bear some relation to the present state of knowledge and experience, and must not be made unilaterally of one side to the argument only.

Granted these points, then, there is much reason to claim that controlled trials in behavior therapy do show the method to be worthwhile and usually superior to alternative methods, or spontaneous remission; Marks and Gelder (1968) have provided a good summary of the main studies in the literature. These have mainly dealt with the simple phobias, often in otherwise non-neurotic subjects, but it is quite erroneous to imagine that this indicates, as many authors claim, that desensitization methods *only* work with this type of disorder. Psychologists need a proper experimental paradigm for exact, quantitative work; complex and difficult neurotic disorders do not allow for proper matching, or for the needed precise measurement of change. Hence the widespread use of snake-phobics or spider-phobics; they fill the role of the smooth and wrinkled peas in Mendel's genetic experiments. There is no implication that this restriction on experimental studies imposes a similar restriction on clinical work; Wolpe and his students have reported success with random series of patients unscreened except for psychotic involvement and brain damage.

It would be true to say that these reports lack a proper control group; this is a serious defect, but even so the detail given by Wolpe and his followers is much greater than is customary in psychoanalytic publications. Behavior therapists must obviously supplement these early claims with more experimental work, but again, criticism ill becomes those who in the past have shown no particular concern to furnish us with evidence of the potency and efficacy of psychotherapeutic procedures. Some such studies are in fact already available. Eysenck (1967) has given some details of Humphrey's (1966) study in which unselected patients in a child guidance department were allocated at random to a psychotherapy and a behavior therapy group; there was also an untreated control group. Success was assessed by independent psychiatrists ignorant of type of treatment received, and a follow-up was undertaken after 10 months. The outcome was entirely favorable to behavior therapy, which took less than half the time of psychotherapy, and showed significantly greater change. A recent study by Gillan (1970) gave similar results for adult patients in a neurosis centre; these were suffering from complex phobic anxieties of the kind declared not suitable for behavior therapy by Marks and Gelder (1968). The patients were allocated by a mixed random-matching method to one of four treatments—psychotherapy, behavior therapy, relaxation alone (i.e. without hierarchies), and desensitization without relaxation. Pre- and post-treatment assessments were made by an independent psychiatrist, uncommitted to either method, and in ignorance of the type of treatment planned or given. Self-assessments were obtained from the patients, behavior avoidance tests were constructed pre- and post-therapy, and physiological recordings taken of patient's reactions. A follow-up was instituted, and the same data obtained again. On all criteria, behavior therapy was greatly superior to the other 3 methods, a superiority which was continued through the follow-up period. Table 1 gives some of the main results to illustrate the findings. One interesting observation, relevant to the evaluation of psychiatric claims for orthodox types of treatment, relates to the judgments of his own success made by the psychotherapist; he apparently tended to over-rate his success when his judgment was compared against outside criteria, the fellow-psychiatrist's judgment, and the patient's own ratings. It is not claimed that these two studies *prove* beyond peradventure the superiority of behavior therapy to psychotherapy; they are quoted to demonstrate two points. (a) Behavior therapy can be shown to be efficacious in disorders other than simple phobics, and (b) properly controlled studies can be carried out in the clinical situation, in spite of the fact that *dynamically* oriented therapists have for so long denied this possibility.

Many of the other criticisms mentioned must be regarded as subjects of research, rather than fit subjects for argument. Suggestion may play a part in behavior therapy, just as it may play a part in psychotherapy; after all, it is possible to conceptualize suggestion as a conditioned response to a verbal stimulus. The notion of 'transference' too, can be rephrased in conditioning terms (Eysenck, 1963); its importance has probably been vastly overrated in any case, as shown by the many controlled studies comparing behavior therapy with alternative methods in which behavior therapy was more efficacious in spite of producing less patient–therapist interaction of the kind supposedly giving rise to 'transference'. Note also Lang's (1969) success with computer-treatment; is it suggested that the patients established 'transference' to the computer? Here as elsewhere critics have taken the easy way out; instead of *showing* that certain criticisms were in fact true, by experimental proof, they simply deduce these supposititious happenings from prior assumptions, equally unproven. This is not very useful, and it is not the way of science.

The objection that behavior therapy produces relapses and sympton substitutions has lost much of its sting since even orthodox psychiatrists (e.g. Marks and Gelder, 1968) on the look-out for these disastrous consequences, failed to find

TABLE 1. (a) PATIENT'S RATINGS: TOTAL PHOBIAS

	Before treatment	After treatment	At follow-up
Behaviour therapy	3·87	2·29	1·98
Hierarchies only	3·58	2·43	2·51
Relaxation with psychotherapy	3·67	3·34	3·04
Psychotherapy	3·77	3·14	3·13
Mean	3·72	2·80	2·66

(b) PATIENT'S RATINGS: MAIN PHOBIA

	Before treatment	After treatment	At follow-up
Behaviour therapy	4·36	2·35	2·13
Hierarchies only	3·91	2·48	2·38
Relaxation with psychotherapy	4·47	3·50	3·38
Psychotherapy	4·35	3·42	3·45
Mean	4·28	2·94	2·48

them; there is little sense in documenting a point which is now probably conceded by most psychiatrists in touch with the literature. But it may not be inopportune to remind readers that relapses and sympton substitutions are not unknown to psychoanalysis; Cremerius (1962) in his long-term follow-up found psychoanalytically treated patients to be subject to both. Again, it is typical that psychoanalysts have criticized behavior therapists (without evidence) on a point on which they assumed (without evidence) that they themselves were in fact innocent; this thread of lack of evidence, and unconcern about evidence, runs through all the writings of the 'dynamic' school.

We must now turn to one last criticism, namely that the two types of therapy attempt to do different and incommensurate things; if this were true, then no proper comparison would be possible between their achievements. As Kuhn (1962) has pointed out, this often happens when a revolution occurs in science, and a new paradigm (as he calls it) takes the place of the old. When this happens there is a failure of comprehension; the representatives of the old do not understand what the representatives of the new are trying to do, and continue to evaluate their achievements in terms of inappropriate criteria. Similarly, the advocates of the new paradigm dismiss the old criteria as inappropriate and irrelevant. Clearly something of this kind has taken place in connection with the rejection of psychotherapy and the substitution for it of behavior therapy; there is no meeting of minds, but rather a change of emphasis which carries the protagonists further and further away from each other. Max Planck, in his Autobiography, put the matter very well: "A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it." Psychotherapists often have genuine difficulties in seeing that to the behaviorist not only do 'symptom' and 'illness' coincide, but that to him the elimination of the 'sympton' is all-important. What they seem to be concerned with is not the symptom the patient complains about, but rather some supposititious 'inner state' which they wish to change; their regard for the 'symptom' is minimal. Such differences in outlook may easily lead to differences in criteria for therapeutic outcome (Malan *et al.*, 1968). It is not easy to see how this difference can be overcome, and to the behavior therapist it probably does not seem important; within his framework the concerns of many psychotherapists can find no resting place. As a therapist, concerned with cures, the ultimate state of the soul of his patients is not his concern, nor are unmeasureable and probably unspecifiable 'inner states' which may be part of the patient's personality, but do not form part of the 'symptom' he complains about. There is no obvious empirical way of reconciling these views, or testing their value; society will have to cut the Gordian knot and decide which of these incommensurable criteria it is willing to adopt. There is little reason for behavior therapists to fear this verdict. (There are some psychotherapists, of course, who believe, as Freud did himself, that only by dealing with the 'inner state' first can 'symptoms' be permanently eliminated. This is subject to empirical proof, and seems now an untenable position.)

We have seen that while in principle many of the criticisms advanced against behavior therapy are well taken, they apply in at least equal measure to all types of psychotherapy, and that those which are particular to behavior therapy are not in fact justified. We must last of all take notice of an argument which has become quite popular in recent years, and which claims that some 'broad band' intermediate position, incorporating both behavioristic and 'dynamic' principles, would do better justice to the facts, and would lead to better therapeutic success (Lazarus, 1967). If this last claim could in fact be justified on the empirical level, then this argument could perhaps be taken seriously; so far, there is no evidence of any kind to suggest that this is in fact true. We must therefore look at this point of view from a purely theoretical angle, and here it appears that an eclectic position attempting to reconcile contradictory views has little to offer. It will be remembered how Tycho Brahe tried to reconcile Ptolemy's Earth-centered planetary system and Copernicus's heliocentric system by suggesting that the Sun went round the Earth, and the planets round the Sun—a system which posterity found ingenious but futile. Contradictory theories cannot be reconciled by political compromise or fiat; what is required is empirical, experimental study of those points on which their prediction diverge. It has always been the misfortune of psychiatry that nothing clear-cut and definite was said or done by clinicians, so that all we have is a mish-mash, a hugger-mugger of theories, practices, and outcomes; a gallimaufrey and charivaria of inconsistent and contradictory bits and pieces, uncontrolled and untestable, held together by the thin string of 'clinical insight and experience'. The great contribution of behavior therapy has been from the beginning, not only the substantive improvement in clinical practice, vital though that has been, but even more the insistence on theoretical rigor, experimental proof and clinical check. It would be a betrayal of all that behavior therapy stands for to throw away these hard-won advantages, to abandon the experimentally established clarifications (however small and insignificant), and to throw everything again into the melting pot. 'Broad spectrum' approaches to clinical problems make impossible, in principle and for ever, the clarification of which parts in the procedure work, which are useless, and which are actively harmful. But it is precisely this that we must know in order to improve our practices, and make them more helpful to the patient; such clarification is vital if we are ever to arrive at theories and principles which have a proper scientific status similar to that of theories and principles in other parts of medicine, or even in the 'hard' sciences. To abjure all this in order to obtain some compromise between incompatible theories, and to pacify the advocates of the old order, does not seem a proper or useful exchange.

This position is sometimes called 'arid' and 'doctrinaire'; alternative words would be 'rigorous' and 'theoretically consistent'. Theories such as those put forward by behavior therapists can be experimentally investigated and disproved or confirmed; this has such an important advantage over all previous psychiatric theories that nothing should be done to imperil it. It would be premature to state that all or even a majority of these theories had in fact been confirmed; but this is not important. The vital contribution of behavior therapists has been to make scientific testing of theories possible in this field, where previously there was nothing but subjectivity and personal prejudice. Valid criticisms can be made of many specific theories and hypotheses, but this particular claim is, I would say, indisputable; by itself it justifies the existence of behavior therapy as a new departure in psychiatry, and as an alternative to psychoanalysis.

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