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BEHAVIOR THERAPY AS A SCIENTIFIC DISCIPLINE

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This paper discusses some arguments purporting to show that behavior therapy is an inherently limited, partial approach to mental disorder in human beings. It is argued that (*a*) the oversimplified, partial nature of present theories is the inevitable price to be paid for attempting to approach this field scientifically, rather than in a literary, humanistic fashion; (*b*) this approach could not be judged by philosophical a priori arguments, but only in terms of its own aims and successes or failures; and (*c*) data available to date gave tentative support to the general direction of thought and approach of the behavior therapist. Obvious limitations to scientific theories are outlined, and it is concluded that whatever the weaknesses of the approach under consideration, it presents the only hope for a proper understanding of mental dysfunction and for effective treatment.

Portes' (1971) article discusses behavior therapy in a sociological context; as such, much of what he says is true, but unfortunately not relevant to the position of behavior therapists. Where aims differ, sensible discussion is impossible; all that can be done is to outline the major differences and point out the irrelevance of one party's criticisms to the aims of the other (Kuhn, 1962). I have tried on two occassions to answer criticisms in some ways similar to those of Portes (Eysenck, 1970a; Eysenck & Beech, 1971), and will not go again in detail into points already dealt with elsewhere. Nevertheless, the very fact that many different critics have concentrated on certain points suggests that the misunderstandings underlying these criticimss are widespread and require an answer. The first misconception arises from the distinction between the science-oriented position of behavior therapy and the patient-oriented position of its critics; this gives rise to the accusation of "oversimplification," omission of complications in the individual case, and assumption of greater validity of current theories that can be objectively justified.

To the behavior therapist (or at least to the writer—obviously no single person has the right to generalize to a large and heterogeneous group; this qualification should be understood to apply in what follows whenever "behavior therapists" are mentioned), scientific formulations of learning and conditioning theories are fundamental; the patient's abnormal behavior requires to be explained in terms of these principles, and a cure (behavior modification to those who prefer not to think of a medical model) to be planned on the basis of such knowledge as is available in modern learning theory. It may be said (with truth) that the patient is more than his "symptoms" (put in quotes because in the writer's theory the "symptoms" are the disease, rather than being symptomatic of anything); the only question that remains is whether whatever "more" there is, is relevant to the scientific problem set by the patient and his "symptoms." This is an empirical problem. Portes tries to beg the answer by assuming that this must be so, but he fails to give any evidence, or even quote any studies which would persuade the sceptic. Such an attitude as that of the behavior therapist who wishes to confine his problem to its essentials smacks of "oversimplification," and of course there is always the danger that in doing so he may be leaving out some important variable. But essentially the scientific approach is one of simplification, and often "oversimplification." Genetics is more than the study of smooth and wrinkled peas, but genetics would never have emerged into a scientific discipline if a beginning had not been made with the numerical study of smooth and wrinkled peas. Mendel's precursors and contemporaries attempted to study "the whole plant," or "the whole animal," noting general similarities, but getting nowhere because of their failure to concentrate on specific, manageable portions of the total universe of characteristics they were concerned with. Behavior therapists knowingly restrict themselves to what they conceive to be important aspects of a much wider problem; the failure of more all-embracing theorists to get anywhere, either by gaining universal acceptance of their theories or by improving the effectiveness of therapy

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beyond the spontaneous remission rate, suggests that such restriction may be the wisest course (Rachman, 1971).

There is no implication in anything said so far that learning theory can give us certain, or even universally agreed, knowledge about precisely what should be done in any particular case, or that any suggestions or deductions made will actually work, or that even if success should attend such efforts, this would prove both theories and deductions to have been correct. All that is being asserted is that the whole history of science suggests that we should attack a very difficult and complex problem in this manner, and that in the past such a mode of attack has often been successful. In due course, we will see whether in fact the analogy between science and psychology is a valid one. It might also be asserted that there might be some interesting and important interplay between theory and practice; I have suggested that learning theory might learn much by attempting to apply its principles to such everyday problems as those presented by neurosis and crime (Eysenck, 1967, 1970b). One outcome of making attempts in this direction has been the interesting discovery that unreinforced presentations of the CS do not always produce extinction—they can also produce marked enhancement of the CR (Eysenck, 1968.) The theory of incubation suggested to take care of this finding may be equally important to experimental studies of conditioning as to applied efforts to cure neurotic patients.

Portes complains that our approach neglects *meaning*, bypasses individual characteristics, and blurs the distinction between control of behavior and therapy. In the light of what I have said, one can only comment that this is all true, but is it in any way a criticism of behavior therapy? If I am faced with a child addicted to headbanging and threatening to become blind because his retinae are getting detached, should I continue to keep him tied up day in, day out, as was the old medical practice? Should I hug him and love him whenever he indulges in this self-destructive habit, thus reinforcing this particular form of $_SH_R$ and make it worse? Or should I neglect meaning, bypass individual characteristics, and blur the distinction between control of behavior and therapy and cure him of his habit by isolating him in a room for 15 minutes every time he bangs his head—thus negatively reinforcing his $_SH_R$? There can be little doubt about the effectiveness of this procedure, or its congruence with theoretical prediction (Bandura, 1969). Portes, dealing entirely with generalities of a semiphilosophical nature, does not seem to face the real problem which confronts the clinician. Take a group of heroin addicts, knowing that if they cannot be cured, they will be dead within five years. Knowing that nothing else is likely to work, should we refrain from administering aversion therapy, which seems to work, because we do not know all the complexities of human misery (Eysenck & Beech, 1971)? I find this complaint so difficult to understand that I cannot even anticipate what kind of an answer Portes would make.

Nor do I have sympathy with his next point, where he suggests that while psychoanalysis "has been dynamic enough to preserve the goal of helping men to achieve their own unique integration even against societal standards," behavior therapy "is one of the therapeutic approaches most inclined to confuse mental health with conformity to social standards [p. 305]." Apart from the obvious question of how we know what this "unique integration" might be which we wish to help our patients to achieve (these may just be words to slur over the absence of any factual meaning), I doubt if there is any evidence to suggest the existence of any such "confusion" as Portes pretends to discern. Laing (1969) and Cooper (1967) have levied this same accusation, not at behavior therapy but at psychoanalysis and orthodox psychiatry in general; clearly there is much mud to be slung along these lines without any precise target being discernible. Therapists of any persuasion cannot obviously abandon all concern with social standards, or help being influenced by them; similarly, few therapists will act against what they (and their patients) consider to be in the best interests of these patients. To accuse behavior therapists of being less concerned with these interests than others seems to me to require some documentation; it certainly is untrue in my own experience. There are certain problems in this area, but they concern the whole of psychiatry and clinical psychology; behavior therapy does what is done more effectively than any other technique, but is not otherwise different from other techniques (Eysenck, 1970c). There seems no need to burden behavior therapists with undue concern about social approval and social standards; these are individual matters not related in any way to those theoretical aspects which distinguish learning theorists from psychoanalysts.

Portes refers to the different definitions of "symptom" given by Wolpe (1969), on the one hand, and by Krasner and Ullmann, (1965), on the other. Eysenck and Rachman (1965) emphasize maladaptive functioning of both kinds, that is, that which is maladaptive for the individual and that which is maladaptive from the point of view of society; the latter often leads to the former through obvious repercussions which an individual's behavior has on the treatment he receives from others. We would never try to treat a homosexual who was happy and contented with his sexual orientation simply because society dislikes homosexuality; but we would treat him if he willingly came and asked for treatment, even though this wish for treatment might have been due to social pressure which he was unable to resist. Psychoanalysts are faced with precisely the same problem, and ultimately, like behavior therapists, solve it on an individual, ethical basis which is not essentially connected with the particular therapeutic beliefs emphasized. Ethical problems like this are not the province of psychology, and neither I nor Portes can claim special competence in answering them (Eysenck, 1970c).

A second misconception which Portes indulges in relates to certain terms and phrases which are widely used in psychology; he attacks this usage on what seem often to be merely verbal grounds. He complains that behaviorism lacks a theory of mind and a theory of self; presumably all the other criticisms follow from these two major gaps in our theories. The appropriate answer, from some points of view, would seem to be the one given by Heinrich Heine when urged to acknowledge the existence of God: "C'est une hypothèse dont je n'ai pas besoin." If our theories can (within obvious limits) cope with a certain set of phenomena, then concepts like meaning and self would seem supererogatory; it would require definite proof that treatments involving these concepts, or predicated upon them, were more effective than those not so predicated, to make their adoption mandatory or even desirable. No such proof seems to be forthcoming; we can hardly take Portes' word for it. His point acquires a spurious air of meaningfulness from the fact that it fits in with our commonsense experience, where such concepts as "meaning" and "self" seem to have an obvious and self-evident existence—as self-evident as the geocentric view of the world, for instance! What is self-evident, unfortunately, is not always heuristically useful or scientifically true and valid; what is required is proof, evidence, and systematic support for any such suggestions before they can be accorded any sort of welcome.

Portes complains about other psychological, behavioristic concepts; thus he accuses the concept of reinforcement of circularity. This is hardly an original accusation (Postman, 1947). The law of effect is as circular as Newton's law of universal gravitation, neither more nor less; we explain the falling of unsupported bodies in terms of gravitation, and we derive the law of gravitation from the falling of unsupported bodies. This semicircular process is universal in science; why should the unfortunate law of reinforcement be singled out for reproof? In due course, of course, the law of universal gravitation was supplanted by Einstein's more complex formulations of curved space; in due course, equally, the law of effect will become more quantitative, only to be supplanted by some more inclusive, possibly more neurological, law which we cannot even anticipate now. But what has all this decline and fall of scientific empires to do with behavior therapy? If Portes has something better to suggest than the law of effect, behavior therapists are willing to listen; we are fully aware of the weaknesses of our concepts, laws, and theories, and need no rehearsal of their inadequacies. What we need are better laws, improved concepts, superior theories; Portes is not making any suggestions that would lead to any such desirable consequences.

Portes uses this general line to throw doubt on the "objectivity" of behavioristic concepts and procedures. He unfortunately uses a philosophical argument in support of his thesis which would land us squarely in solipsism if we really pursued it to its logical conclusion; he fails to mark the essential difference between subjective and objective which is of concern to behaviorists. As Medawar (1967) has pointed out, there is a difference between saying "The dog is afraid" and "The dog is howling"; he traces acceptance of this distinction to the teachings of behaviorists. This is the essential difference between subjective and objective as it emerges from the contrast between the Oedipus complex and aversive conditioning, between psychoanalysis and learning theory, between psychotherapy and behavior therapy. The difference is not a philosophical one, but its practical importance is immense; it marks the difference between speculation and science.

Concerning the notion of the self, Portes doubts whether the notion of generalization could have any meaning in the absence of "man's self-image." So much has been written about possible mediating mechanisms in humans and animals (Osgood, 1953; Staats & Staats, 1963, pp. 98–101) that the assertion strikes one as lacking in detailed knowledge of the behaviorist position; it is difficult to see just what force it can possibly have other than that of *obiter dicta* in general. A similar objection attaches to the complaint that social reinforcements require the individual to be aware of himself and value his own image. Surely there are many accounts of social reinforcements in animals who presumably are not "aware" of themselves in this sense?

Portes finally turns to the effectiveness of therapy, but again his presentation is marred by his failure to cite any evidence for what are certainly not self-evident pronouncements. Thus he comments on "the well-known fact that the essential, or one of the essential, causes of therapist's effectiveness lies in the depth and accuracy of his perceptions and the appropriateness of his commonsense approach to other humans [p. 308]." These "facts" may be well known, but I have hunted in vain for any reference to the research on which they must surely be based to carry any conviction; Truax and Carkhuff's (1967) work suggests some properties of the "successful" therapist, but they would require a lot of interpretation to be recognizable in the description given by Portes. Even more curious is Portes' statement that basically behavior therapy is "no different in interpretations and goals from other therapeutic systems [p. 308]." I would have thought that "interpretations" play no part in behavior therapy at all, unlike psychoanalysis, and the suggested identity of goals has certainly been vigorously disputed by psychoanalysts. Nor is it true that "the action-dependent approach of behavior therapies has limited most of its uses to simple behaviors and to noncomplex patients, such as children, retardates, psychotics, and autistic individuals [p. 304]." It is certainly interesting to hear that psychotics are "noncomplex patients"; for the rest this statement is simply not true (Eysenck & Beech, 1971). Nor is it true that "as the generality and perceptual-cognitive character of the disease increases and as the cognitive complexity of the patient grows, behavior therapies seem to decrease in usefulness [p. 304]." It is true that *for experimental studies* behavior therapists have preferred simple cases, such as monosymptomatic phobias; here one can have a clear-cut, quantitative criterion of degree of success, similar in nature to the CRs used in the laboratory. But *clinically* there is no evidence to show that such a relation as suggested by Portes actually exists; agoraphobias, for instance, are apparently much more resistant to behavior therapy than much more complex cases such as those treated by Wolpe (1969). These and other misconceptions are presented without any attempt at documentation (my own assertions will be found fully documented in Eysenck & Beech, 1971), and consequently may be regarded as authoritative by readers not *au fait* with the literature; Portes' case would be more easily acceptable if it were based on a correct and factual interpretation of the present position of behavior therapy.

Portes continues with his own views of the reasons for the success of behavior therapy, which he attributes to its "emphasis on doing rather than on talking." Typically this discussion is vague rather than precise, does not put forward any testable theory, or even suggest any deductions from what is being said, and does not deal with embarrassing facts—if the wrong thing is "done" by the patient, then he may get worse. In other words, the emphasis is not on doing as such, but on finding out what is the right thing to be done by the patient—and this decision requires considerable knowledge of learning theory principles. Portes is not suggesting a meaningful theory; he arbitrarily selects from a number of important curative factors a single one which may or may not be the most relevant one. Not for him the long-continued, earnest search for proof of the importance of different parts of the therapy process (Eysenck & Beech, 1971); *ex cathedra* pronouncements are sufficient grist to his particular philosophical mill. It is idle to deal *in extenso* with mere assertion; where even reference to past research is lacking, science goes out of the window. The same comment applies to Portes' *obiter dicta* on affect, such as that "there is an all-important base of affection underlying the actions of effective parents [p. 309]." Maybe so; we are all against sin and for motherhood. But is there really any evidence that this statement is true? Portes does not give any reference to research on this matter, nor does he define quantitatively "effective parenthood"; it would be interesting to know how reliably this could be rated by psychiatrists and psychologists of different persuasions! Appealing as such notions are to the groundlings, without experimental support they amount to little; they certainly cannot be used as a basis for behavioristic techniques of therapeutic effectiveness. Why do experiments, which are difficult, time-consuming, and energy-sapping, if we can get the answer by this referral to the wisdom of the ancients? The trouble of course is that the ancients do not seem to have done very well with either their child upbringing or their behavior modification.

Portes finally ends up by calling the "literal application of behavioristic theory to complex human disorders" absurd; if attempted, he considers that it will "yield meaningless results"—although he does not explain how results can be meaningless. He goes on to say that "attempting to use empirical research to lend permanent objectivity to intellectual productions is somewhat futile [p. 310]." We have already looked at his somewhat original use of the term "objective"; we may now consider his curious notion that scientists regard theories based on facts as "permanent." One might recommend to him Medawar's interesting little book on the methodology of science (1967); within the narrow confines of this comment I can do no better than quote Claude Bernard (1865), the founder of experimental physiology:

> When propounding a general theory in science, the one thing one can be sure of is that, in the strict sense, such theories are mistaken. They are only partial and provisional truths which are necessary . . . to carry investigation forward; they represent only the current state of our understanding and are bound to be modified by the growth of science . . . [p. 17].

So much for the alleged "permanence" of our experiment-based theories.

To many humanists, the whole attempt to construct a scientific psychology on what Hull called a "natural-science foundation," is immoral, outrageous, and doomed to failure; it presents a mixture of chutzbah and hubris, to be smothered in philosophical a priori argument. To the behaviorist it presents an effort to come to grips with the last frontier of the unknown, man himself; the effort may fail, but even failure would not be ignoble when we consider the magnitude of the task, and its tremendous importance for the whole future and happiness of mankind. The small successes gained in the past, mediated by theories obviously weak and far from adequate, suggest that perhaps we are on the right way; final success may be forever elusive, but partial success may be sufficient reward. It is not much use telling us that our theories are oversimplified, that our assumptions may be faulty, that our interpretations, even of our own successes, may be in doubt; all this is of course readily admitted—indeed, such has always been the state of science at the edge of discovery. Let me conclude with a quotation from Boscovich (1760), whose theory of the atom presaged modern subatomic physics:

> We are enabled to supply the defects of our *data* and to conjecture or divine the path to truth; always ready to abandon our hypothesis, when found to involve consequences inconsistent with fact. . . . Legitimate theories are generally the slow result of disappointed essays, and of errors which have led the way to their own detection [p. 3].

This legitimate process of self-correction, so characteristic of science, is not helped by nonfactual criticisms based on philosophical grounds; it requires detailed examination of countless experimental results, close theoretical argument and deduction from theory, and the empirical verification or rejection of such theories. Only in this fashion will we learn the limitations of our present hypotheses.

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