

SPECIAL REVIEW

BEHAVIOR THERAPY AND THE PHILOSOPHERS

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This short article owes its existence to a recent book by Erwin (1978), a professional philosopher whose book on 'Behavior Therapy' has the subtitle 'Scientific, philosophical, and moral foundations'. Many scientists, and this must include many behaviour therapists, have little faith in the usefulness of philosophers, and feel that outsiders can make little contribution to their subject matter. This is not a view I share; the outsider often sees more of the action than the insider, and the logical training of the philosopher may make him aware of deductive errors and other faults which might escape the uninitiated. In addition, many of the beliefs proclaimed by behaviourists, such as Watson and Skinner, are essentially philosophical, e.g. those relating to the body-mind problem, and someone steeped in the age-old controversies surrounding such topics may make a genuine contribution by clarifying the discussion. For all these reasons I found the book interesting, and I believe that most behaviour therapists could benefit from reading it, and becoming a little more modest and less contradictory in their claims.

For all that, I believe that Erwin is essentially wrong in the two major claims he makes, namely that there is no proper definition of behaviour therapy which identifies it in a reasonably exclusive fashion, and that insofar as behaviouristic theories exclude cognitive concepts, they are demonstrably wrong. These two points are of course connected, and it must be admitted that Erwin makes out a strong case for his assertions. However, in doing so he picks out rather weak adversaries, and avoids confrontation with what I believe would be a much more formidable version of the argument. Even so, it has to be admitted that many behaviouristic claims, often echoed by behaviour therapists, are much too grandiose to be defensible. It is not necessary for behaviourists to claim that conduct is 100% determined; belief in complete determinism is a philosophical belief, not a scientifically demonstrable reality, and Heisenberg's principle should make us rather cautious about claiming for psychology what has been rejected by physics!

Similarly, I think we might with advantage be more careful about claiming that certain behaviouristic principles (such as the law of effect) have universal validity, or that the laws of learning explain behaviour in general. (Mackenzie, 1977, has given a good critique of behaviourism along these lines.) As behaviour therapists, we are concerned with a special area of behaviour, and should not make claims going well beyond that area. We are in business, as it were, to explain the origin of neurotic disorders in terms of general principles; to deduce from these principles methods of treatment, if possible; to demonstrate that these methods work in a lawful manner; and finally to explain, if we can, why other methods (spontaneous remission and whatever is responsible for that, or psychotherapy, if it does work) also have some sort of effect. To assert, as I would be willing to do, that all this can be accomplished by reference to Pavlovian learning theory does not commit us to asserting that *all* behaviour can be so explained. Erwin has an easy time showing up universal claims as unfounded; behaviour therapists have no business making the task of the critic so easy. I have set out a miniature theory which accounts for the origins of neurosis (Eysenck, 1976) and for the success of all methods of therapy (behaviour therapy, psychotherapy, psychoanalysis, spontaneous remission) which seem to effect changes in neurotic disorders (Eysenck, in press); this theory is entirely behaviouristic, as I understand the term, and requires no cognitive additions, as Erwin understands the term. This theory, however, is definitely restricted

to neurotic behaviour (and allied sub-neurotic forms of behaviour similar in a definable manner to it). It would never pretend to pose as a general theory of human conduct.

Erwin quotes a minimum set of learning theory laws which Rachman and Eysenck (1966) suggested as typical of the 'laws' which behaviourists would use in accounting for the causes and cures of neurosis; he comments (1) that the principles apply mainly to autonomic conditioning, and are "too narrow to explain much deviant behaviour". But of course they were cited to apply to *neurotic* behaviour, not to all types of deviant behaviour; such restriction would seem a virtue in not claiming too much. Other principles, e.g. those of operant conditioning, would no doubt be applicable to other types of deviant behaviour. Erwin also criticizes us because there are some experiments which suggest that 'awareness' is involved in Pavlovian conditioning, and 'awareness' is a cognitive variable, thus making the theory cognitive.

There are several answers to this criticism. When the UCS is produced by the experimenter, there is an infinite number of possible CS impinging on the subject, including that selected arbitrarily by the experimenter; unless the predetermined CS has certain properties (such as high intensity, novelty, or other collative properties) there is no guarantee that it will be selected; we now know that the doctrine of equipotentiality which was assumed by Pavlov is not in fact possessed by potential CS. 'Awareness' may mean nothing more than that the CS in question has been singled out from the welter of potential CS by some definable property, or action on the experimenter's part; this does not make the experiment a cognitive one. In the case of conditioning anxiety to previously neutral stimuli, 'preparedness' has been shown to be a vital property; yet Erwin never even mentions 'preparedness', or discusses the evidence against equipotentiality.

Another possible answer would take a much more wide-ranging account of the general criticism Erwin makes of behaviouristic theories, and his arguments in favour of cognitive theories. Behaviouristic theories do in fact take into account cognitive factors, so-called, but assume that these factors also obey certain laws of learning, e.g. can be conditioned. Pavlov already emphasized the importance of the second signaling system, and Platonov (1959) devoted a whole book to an experimental account of words as conditioned stimuli and responses. He started out from Pavlov's statement that "A word is as real a conditioned stimulus for man as all the other stimuli in common with animals, but at the same time more all-inclusive than any other stimuli. . . . Owing to the entire preceding life of the human adult, a word is connected with all the external and internal stimuli coming to the cerebral hemispheres, signals all of them, replaces all of them and can, therefore, evoke all the actions and reactions of the organism which these stimuli produce". Staats and others have followed up these original ideas in recent years, demonstrating their power. Yet, apart from quoting a brief summary of statements outlining such a stance from Delaney (1974), Erwin never enters into a proper discussion of these possibilities; he does not even mention Platonov's or Staats' work, and the second signalling system is conspicuous by its absence. This is not an adequate account of how behaviourists would deal with 'cognitive' factors, and hence the conclusion favouring cognitive theories is not properly grounded in factual research or survey.

The second signaling system of course enters powerfully into the definition of stimuli and responses; we have come a long way from the primitive conception of stimuli as pure sensory impressions, and responses as reflex-like muscle twitches. Stimuli are perceived and organized into Gestalten and meaningful entities, and stimulus generalization can, and often does, follow the lines of semantic meaning or simple auditory similarity. Reflexes too are organized into proper responses, bearing a meaningful relation to integrated activities directed toward some goal or other. Yet there is no contradiction between admitting all this and holding a conditioning view of neurosis and behaviour; the organization of stimuli and responses, no less than their connection, can be accounted for theoretically by conditioning processes. It would be going much too far to assert that this had been *proved* to be the correct explanation; I merely assert that

it is a *tenable* one. Cognitive factors, so called, are admissible in so far as they obey more general laws of conditioning and learning; they are not admissible as something *sui generis* having causal properties independent of the laws of learning. Erwin makes much of the fact that behaviour therapists talk and write using mentalistic terms; but this is just a shorthand and does not commit them to a mentalistic, cognitive type of theory. To say "Bloody hell!" when you hit your thumb with a hammer does not commit you to a belief in the existence of the nether regions; we have become conditioned to the use of certain verbal formulations, and continue to use them, particularly when the theoretically correct usage would take many more words, and would be intolerably clumsy. No misunderstandings are usually caused in this manner.

Going even further, Erwin assumes that there is something called 'cognitive theory' which is in conflict with behaviour theory, and which can meaningfully be preferred to it. This is not so; there is no cognitive theory worthy of the name. Allport (1975) has recently characterized so-called cognitive theory from the point of view of the experimentalist; the field, he says, 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; interpretation of data relying on multiple, arbitrary choice-points; and underlying all else, the near vacuum of theoretical structure within which to inter-relate different sets of experimental results, or to direct the search for significant new phenomena". This seems an accurate assessment, and although cognitive theories seem fashionable at the moment among some behaviour therapists who should know better (Franks and Wilson, 1978), being fashionable is not the same as being correct, or useful, or in line with the evidence. Erwin's uncritical acceptance of cognitive theories, so-called, is the weakest part of his book, and suggests *a priori* bias rather than careful survey and appreciation. If there are many weaknesses and anomalies in learning theory of the classical sort, requiring changes along the lines of admitting the existence of 'preparedness' of CS, desynchrony of responses, and incubation of anxiety, then one must state that such changes have been made in recent years, although they are not mentioned in Erwin's book. Cognitive theory, *per contra*, does not even exist as a 'theory' that could meaningfully be criticized or tested; it is an aspiration, born of mentalistic preconceptions, in search of hypotheses. Any sensible referee would stop the fight right there and declare it 'no contest'.*

Erwin discusses many other problems, such as the definition of neurosis and psychosis as 'mental illnesses', the medical model, ethical problems raised by behaviour therapy, and so on; all of these are discussed sensibly and with much authority. All in all the book is well worth reading by behaviour therapists who are interested in the wider issues relevant to their craft, and who like to see their assumptions questioned. If only Erwin had adopted as critical a stance *vis-a-vis* cognitive theories as he did *vis-a-vis* behavioural theories, and if he had taken into account behavioural theories concerning language and verbal behaviour, his book would have been outstanding. As it is, he concentrates too much on what are essentially side issues, such as the Chomsky-Skinner debate on the origins of language. This is a side issue simply because behaviourists do not have to take Skinner's part but may well side, as I do, with Chomsky; thus the debate is not vital to behaviourism as such, but only to Skinner's particular theory. Indeed, Chomsky's theory obviously leaves open the precise way in which languages are learned, postulating only the existence of certain 'universals of human language' (Sampson, 1979); such phylogenetic universals and the neurological substructure on which they are presumably based correspond to Seligman's 'preparedness' concept which can without qualm be accepted by behaviourists. In just such a way can behaviourists

* It might be objected that surely such theories as Bandura's concept of 'self-efficacy' are cognitive and deserve the name of theory. I doubt if it is possible to regard this as a theory in the same hypothetico-deductive sense as the conditioning theory of neurosis; there are no firmly stated premises, anchored in well-documented laboratory investigations, and no rigorous deductions leading to testable conclusions (Rachman, 1978). Such 'facts' as there are can be explained much more readily in terms of conditioning than of alleged 'cognitive' factors of uncertain parentage.

accept the genetic determination of personality variables relevant to the growth and decline of anxiety; even Watson and Raynor (1920) postulated, in discussing the case of little Albert, that "such persistence of early conditioned responses will be found only in persons who are *constitutionally inferior*". Similarly, Watson accepted the existence of certain innate fears (e.g. of loud noises, sudden loss of support, etc.); the arch environmentalist agreeing to the existence of both phylogenetic and ontogenetic hereditary determination of conduct makes the whole issue one which is irrelevant to behaviourism as a general theory. Erwin nowhere makes it clear that the Chomsky-Skinner debate has no vital relevance to the problems of behaviour therapy, nor does he seem to see that Skinner is wrong only in what he denies (i.e. the existence of universals of human language); he may be right in what he asserts, namely that language is normally acquired through a process of conditioning. (Skinner too has of course paid at least lip-service to genetic factors, particularly ontogenetic ones, but I would be surprised if he did not also recognize phylogenetic ones—indeed, no one working with different species of animal could possibly deny their existence.)

One possible consequence of the publication of this book may be an increase in interest in theoretical problems of fundamental importance to behaviour therapy. The debate between 'conditioning' and 'cognitive' theories is only one such issue, which unfortunately is often discussed in terms of slogans rather than empirical evidence; there are many others. Such discussions, carried out in factual terms, could be of great importance for the development of the field.

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