

RESPONSES AND REJOINER

Behavior Therapy Is Behavioristic

HANS J. EYSENCK¹

University of London

This article tries to answer an objection sometimes raised by critics of behavior therapy, namely, that it is not "behavioristic." The objection is shown to be ungrounded and dependent on an outmoded use of the term "behaviorism."

"Is "behavior therapy" behavioristic?" asked Locke (1971), and concluded that the answer is in the negative. One feels inclined to quote in reply Sir Francis Bacon (Dicks, 1955, p. 182), who wrote that it is "The first distemper of learning, when men study words and not matter." Locke brought into juxtaposition two terms, "behavior therapy" and "behaviorism"; in order to form a judgment of their compatibility, one must first arrive at a definition of both which would not be arbitrary, but widely agreed. By using his own arbitrary definitions Locke was able to make a case; I shall try to show just how arbitrary the definitions are.

According to Locke, the term "behavior therapy" was coined by Skinner and Lindsley (1954), and "has since been widely promulgated by Eysenck (1960, 1964)." This is incorrect; the term was "coined" independently by at least three people or groups, namely, the two mentioned above, and by Lazarus (1958). Its use by Skinner and Lindsley was confined to a Status Report to the Office of Naval Research, and never published; it was not familiar to either of the other two authors. Furthermore, it referred entirely to operant methods of treatment, a restriction which makes it clear that its use was quite different from that intended by Eysenck (1959), who explicitly established its present usage as referring to all methods of therapy which are based on modern learning theory—thus including both the Skinnerian operant methods and Wolpe's desensitization method, as well as many others (Eysenck, 1960). Lazarus

¹ Request for Reprints are to be sent to H. J. Institute of Psychiatry Maudsley Hospital Denmark Hill, London S.E. 5.

(1958) only used the term in passing, but from the context it is clear that he, like Skinner and Lindsley, first used it restrictively, referring entirely to desensitization methods. It is claimed, therefore, that (a) the only author to have suggested the use of the term "behavior therapy" in its present sense was Eysenck (1959), and (b) that the same writer was the first to discuss in detail the definition of the term, its scientific meaning, and its application to a set of procedures opposed to the group of "psychotherapies." Having cleared up this confusion, I may perhaps add that Locke, having attributed the coining of the term to Skinner and Lindsley, continued his argument by dealing entirely with Wolpe and his method of desensitization—which would not have qualified at all as "behavior therapy" in the sense of the word intended by Skinner and Lindsley!

A careful look at my writings will disclose that I have not called behavior therapy "behavioristic," and neither has Wolpe, as far as I can recall (it is impossible to check all his numerous writings to establish this point beyond cavil). We have both stressed that methods of therapy should be derived from modern learning theory, and be based on well-established experimental laboratory paradigms; use of the term "behavioristic" may be characteristic of Skinner and his co-workers, although even there I doubt if the term is much used. Locke tried to worm it in surreptitiously by saying that "Wolpe's main theoretical concepts, for instance, 'conditioned reflex,' 'conditioned inhibition,' 'drive reduction,' 'reinforcement,' are all drawn from well-known behaviorists such as Pavlov, Watson, Hull, and Skinner." These are all learning theorists, and are quoted as such; whether their being behaviorists has anything to do with the matter is quite another question. Thus, it would seem that Locke's question is a pseudoquestion; it is not implicit in the definition of behavior therapy that it should or should not be "behavioristic." My own reason for not using this term was simply that it has too many divergent meanings to be understood in any common way; I suspect that Wolpe was similarly motivated. Perhaps a brief look at the usage of "behaviorism" may be appropriate.

A useful classification of behaviorist views was made by Mace (1948). He distinguished three versions of behaviorism, which he called "metaphysical," "methodological," and "analytical." A metaphysical behaviorist is in essence a naive realist (using the term "naive" in its philosophical, not its pejorative sense!); he asserted that such things as "mental events" or "minds" do not exist. A methodological behaviorist does not deny or affirm the existence of such events or entities; he simply asserts that for methodological reasons they cannot be made the objects of proper scientific study. And an analytic behaviorist asserts that such sentences which

would appear to be about "minds" or "mental events" turn out on examination to be sentences about behavior. A good discussion of the application of these notions to psychotherapy and psychoanalysis has been given by Miles (1966), in a book which has not received the attention it should have had. Miles's demonstration that even psychoanalysis can be subsumed under the umbrella of "behaviorism" might have given Locke pause before accusing behavior therapy of not being capable of so being subsumed.

Locke, unfortunately, did not discuss the large literature which has accrued since Mace's original attempt to get the different meanings of "behaviorism" clarified; he simply referred to a definition of his own which might or might not be acceptable to psychologists who designate themselves (or are designated by others) "behaviorists." According to him, there are three basic premises of behaviorism—determinism, epiphenomenalism, and the rejection of introspection. It seems somewhat foolhardy to introduce determinism in this context; even physics has eliminated this notion as scientifically meaningless since the advent of Heisenberg's principle of uncertainty, and it seems safe to say that if this be the touchstone of a "behaviorist," no one at all sophisticated in modern scientific beliefs will come forward. Epiphenomenalism is relevant to metaphysical behaviorism; again, few behaviorists I know are interested in philosophical doctrines, and those who favor "naive realism" are not usually sophisticated enough to evaluate these notions seriously and defend them against philosophical onslaught. This leaves us with introspection, and here Locke showed some circumspection; "a behaviorist investigator may ask individuals to give 'verbal reports,' but such reports may not be used to make inferences regarding the subject's mental states or processes." (Locke, 1971, page 318). Locke accused Wolpe of making inferences, e.g., by relying on verbal reports of "anxiety" rather than on physiological measures. This is not correct; verbal reports have two advantages over physiological measures which dictate their use in therapeutic situations. (1) They are cheaper and easier to get; ideally one might like to have detailed polygraph recordings from each patient, all through treatment, scored and profiled by on-line computer, but this, unfortunately, is not feasible. (2) As Thayer (1970) has shown, verbal reports integrate measures from various physiological subsystems in a most efficient manner; it would be absurd not to avail oneself of such cheap and demonstrably useful information. There is no implication of "mentalism" in such use of verbal reports. Imagery is another bugbear for Locke, but this too can be monitored and shown to be objectively quantifiable; we have used the penis plethysmograph to check on sexual imagery, and G.S.R. and other measures can be used to check similarly

on anxiety-producing imagery (Eysenck & Beech, 1971). However, expensive checks are mainly used in research; the correlation between verbal report and physiological measure having been established, the former can be used without fear of impugning "behaviorist" objectivity.

What Locke might have been justified in claiming is that the language used by Wolpe is at times mentalistic; this is inevitable if one wants to be understood by psychiatrists and nurses brought up in a tradition different from that of modern behaviorism—although even behaviorists sometimes talk as if they were ordinary, sensible people. In the same way physicists may talk about tables and chairs, when perhaps they should be talking about clouds of electrons and protons; one's verbal habits change depending on whom one is talking to, and for what purpose. All these mentalistic terms can, as shown above, be translated into behavioristic language, but this would make the text difficult to read, and 20 times as long. Restating all the sentences in Wolpe criticized by Locke would thus take 20 times as much space as was given to him; furthermore, it would weary the reader. Let me state simply that such a translation is possible, and that there is nothing in Wolpe's procedure which could not be made to fall under the heading of behaviorism in its methodological or analytical sense. It is the failure of Locke to consider the analytical definition of behaviorism which makes his discussion fail the test of usefulness; having never considered the possibility that proper analysis of sentences about "minds" and "mental events" could, in fact, be about behavior, he never began to discuss the real problem posed in his title. Naive realism is not enough; a little more sophistication in philosophical discourse is needed to discover just what is, and is not, "behavioristic." Using my own definition of "behavior therapy" (Eysenck, 1959), and the methodological and analytic definitions of "behaviorism," I answer Locke's question in the affirmative; I would add that these are the definitions which most behavior therapists would probably adhere to, rather than the very old-fashioned ones used by Locke. But in the wider sense, the whole question seems to be arbitrary and not very useful for the development of behavior therapy; let us study matter, rather than words!

REFERENCES

- DICKS, H. G. (Ed.) *The advancement of learning: Selected writings of Francis Bacon*. New York: Modern Library, 1955.
- EYSENCK, H. J. Learning theory and behaviour therapy. *Journal of Mental Science*, 1959, 105, 61-75.
- EYSENCK, H. J. (Ed.) *Behaviour therapy and the neuroses*. New York: Pergamon Press, 1960.

- EYSENCK, H. J. (Ed.) Experiments in behaviour therapy. New York: Pergamon Press, 1964.
- EYSENCK, H. J., & BEECH, H. R. Counter conditioning and related methods. In A. E. Bergin & S. C. Garfield (Eds.), *Handbook of psychotherapy and behavior change*. New York: Wiley, 1971, Pp. 543-611.
- LAZARUS, A. A. New methods of psychotherapy: a case study. *South African Medical Journal*, 1958, **33**, 660-663.
- LOCKE, E. A. Is "behavior therapy" behavioristic? *Psychological Bulletin*, 1971, **76**, 318-327.
- MACE, C. A. Some implications of analytical behaviourism. *Aristotolian Society Proceedings*, 1948-1949, **49**, 1-16.
- MILES, T. R. Eliminating the unconscious. Oxford: Pergamon Press, 1966.
- SKINNER, B. F., & LINDSLEY, O. R. Studies in behavior therapy. Status Reports II and III, Washington: Office of Naval Research, 1954.
- THAYER, R. Activation states as assessed by verbal report and four psychophysiological variables. *Psychophysiology*, 1970, **7**, 86-94.