ON MENTALISM, METHODOLOGICAL BEHAVIORISM, AND RADICAL BEHAVIORISM

Jay Moore
University of Wisconsin-Milwaukee

I. INTRODUCTION

The present paper is verbal behavior concerned with other verbal behavior. More technically, it can be said that the present paper is verbal behavior that has been occasioned in large measure by the verbal behavior of other scientists as well as by the variables that have occasioned such verbal behavior. The paper is divided into four sections. The first section of the paper considers the topic of operationism in psychology. The objective in this section is to draw some broad distinctions between the way most psychologists interpret operationism and the way that radical behaviorists interpret operationism. The topic of mentalism is considered in the second section. The objective in this section is to examine what radical behaviorists mean when they talk about mentalism in psychology. The third section of the paper considers the collection of experimental practices called methodological behaviorism. The objective in this section is to examine the nature of these practices, the variables responsible for them, and the relation between mentalism and methodological behaviorism. The fourth section of the paper considers scientific verbal behavior. The objective in this section is to examine certain classes of scientific statements in an effort to assess the nature of the controlling relations for those statements.

In the sense that the present paper is verbal behavior critically concerned with other verbal behavior, the use of language is understandably very important. It is potentially inconsistent, of course, to criticize some explanation of behavior as mentalistic, e.g., by indicating that it appeals to inner causes, and then say that the person offering this kind of explanation does so *because of* certain mentalistic assumptions regarding the causes of the behavior in question (see also Day, 1976, p. 90; Skinner, 1969, pp. 103-4). If "assumptions" are taken to be mental, logical, or subjective entities that are possessed and that have the ability to cause certain explanatory practices, i.e., if they are taken as inner causes themselves, then there is little question that the criticism itself is mentalistic and that the critic is being inconsistent. On the other hand, if the term "assumptions" means a particular kind of stimulus control exerted by a particular kind of stimulus, such as that exerted by a particular verbal rule in cases of rule governed behavior, then there is presumably no mentalism or inconsistency. The central issue in the analysis of verbal behavior is the source of control over that verbal behavior, rather than its form. What appear to be inconsistencies in

the present paper may therefore be simply nontechnical constructions, although presumably such language is not employed at the expense of the behavioristic argument presented in more technical ways in other portions of the paper.

II. OPERATIONISM

A. Conventional Operationism

An analysis of verbal behavior is traditionally subsumed under the heading of operationism. The way that radical behaviorism interprets operationism differs appreciably, however, from the way that the rest of psychological science interprets operationism (Moore, 1975, 1980; see also Day, 1969b; Kantor, 1938; and Skinner, 1945). The differences between Skinnerian and conventional operationism are important enough to be considered here.

Operationism, of course, was conceived as a well-intentioned check against the arbitrary invocation of specious forces in the newly emerging theories of the physical sciences during the first quarter of the twentieth century. By stressing a certain continuity among experimental operations, the data from those operations, and the special kind of verbal statements called theoretical, operationism soon became the conventional criterion for establishing scientific meaning in general, and supported by logical postivism, an extremely powerful intellectual force within the scientific community.

Psychological science, which was attempting to put its own house in order at about the same time as were the physical sciences, quickly implemented the operational point of view in its methods. The way that it was implemented, however, profoundly influenced the development of the field. In a desire to banish the ambiguities of structuralism as well as to terminate the incessant debates concerning the contents of consciousness, experimental psychologists such as Stevens, Boring, and Spence, together with philosophers such as Bergmann, endorsed the new emphasis on "objectivity" and committed themselves to a "publicly observable" data base. Objectivity was seen as the antidote to the methodological ills caused by the prevailing subjectivity, and the emphasis on a publicly observable data base promoted an epistemology of "truth by agreement." The new movement was held to be technically and philosophically more sophisticated, and therefore preferable, to Watsonianism, which had of course earlier challenged the practices of subjective psychology.

The conventional operationism that developed during the 1930s and 1940s was quite different from Watsonianism, however. Certainly by 1930 Watson had come to reject any mental phenomenon as unsuitable for a science of behavior. He repeatedly argued that psychologists should study behavior – real movements in space and time – instead of fictitious mental phenomena. Two such mental phenomena, consciousness and instincts, received particular attention. Earlier, of course, there was some ambiguity in Watson's position. For example, Herrnstein (1972, p. 28) has argued that Watson seemed to endorse the notion of instinct in 1914, and Spence (1948) quotes the following passage from Watson (1913):

> It [psychology] can dispense with consciousness in a psychological sense. The separate observation of "states of consciousness" is, on this assumption, no more a part of the task of the psychologist than of the physicist. We might call this the return to a non-reflective and naive use of consciousness. In this sense consciousness may be said to be the instrument or tool with which all scientists work. (p. 176)

Whether Watson meant by instinct anything more than a special class of reflexes, or by the above use of consciousness anything more than interactive experience in the Machian, positivistic sense, is not abundantly clear. In any case, the later years certainly witnessed the vehement denunciation of consciousness in an ontological sense; Heidbreder (1933) reports it was frequently labelled as "the result of old wives' tales and monks' lore, of the teachings of medicinemen and priests" (p. 235).

Conventional operationism during the 1930s and 1940s differed from Watsonianism in that operationism did not deny mental states *per se*. Indeed, it was taken as obviously true that such phenomena of the mental dimension as sensations, images, and ideas were necessarily involved in explanations of behavior. A conspicuous example of this orientation is the following passage from Spence (1948):

> Fortunately, the relationship of immediate experience (consciousness) to the data and constructs of science has been considerably clarified in recent years by the writings of several different groups of thinkers. The philosophers of science, particularly the logical positivist (1, 5, 6, 7), philosophically minded scientists such as Bridgman (3) and, within psychology, such writers as Boring (2), Pratt (15), and Stevens (18) have succeeded, I believe, in making the point that the data of all sciences have the same origin – namely, the immediate experience of an observing person, the scientist himself. That is to say, immediate experience, the initial matrix out of which all sciences develop, is no longer considered a matter of concern for the scientist qua scientist. He simply takes it for granted and then proceeds to his task of describing the events occurring in it and discovering and formulating the nature of the relationships holding among them. (p. 68)

What operationism did was not to ask whether there were mental events that were distinct from physical events, but rather it asked how to deal scientifically with mental events. Thus, questions concerning sensations were typically reduced to questions concerning how a sensation was measured in terms of some physical operation, such as by estimating the magnitude of a stimulus or by a discrimination procedure. Accordingly, one finds Stevens saying, "Like positivism, behaviorism erred in denying too much. Operationism does not deny images, for example, but asks: What is the operational definition of the term 'image?' " (Stevens, 1939, p. 231). By continually asking for operational definitions in this sense, conventional psychologists commonly thought they were circumventing any problems inherent in making a science out of dualistic entities, i.e., entities

that were not publicly observable and not intersubjectively verifiable.

A further effort was made to circumvent the problems inherent in the fundamentally mentalistic orientation by developing a logical-theoretical or a reference theory of meaning. According to such a theory, language is to be construed as an array of symbolic propositions, descriptive of the contents of consciousness. The elements of this array are ultimately to be identified with physical entities that are publicly observable and that may be agreed upon. For conventional operationists, the public observation that could be agreed upon was of course the operation by which a given phenomenon was measured. Thus, the logical-theoretical referent that established the meaning of a term was the set of operations involved in measuring it, operations that could be agreed upon by more than one person (e.g., Stevens, 1939, pp. 227-228).

Unfortunately, operationists of the 1930s and 1940s, as well as other reference theorists, failed to consider the full implication of such a position. In effect, their position conceded the possibility of a private language. That is, by assuming the language was a symbolic activity, they assumed that there were such entities as private subjective meanings that possessed an independent existence. What people did when they used language, according to conventional operationists, was to operate upon their own immediate sensory experience, construct logical entities therefrom, and then speak accordingly. Yet, this position implies that there exists some nonbehavioral system that in effect is responsible for language. It implies that persons are automatically able to describe elements of their own private experience – the contents of their own consciousness – and that language is essentially descriptive of logical manipulations of these elements. When followed to its extreme, this position entails a mischievous dualism and a controlling inner agent, endowed with precisely the powers necessary to explain what needs to be explained. Ultimately, of course, both Kantor and Skinner pointed out these dualistic implications:

> According to Stevens, everything reduces to differential reactions. All operations at bottom come to be discriminations which consume the discriminand. Red, and presumably the earth also, are never independent phenomena. They are created in the discrimination process. . . . In general, for Stevens, there are no crude data; everything is construct. Thus, all experience reduces to the sum-total of the discriminating reactions of human beings. . . . [T]here is no ultimate matrix in which organisms interbehave with other natural phenomena. A genuine operationism on the contrary presupposes a continuum in which constructs, as well as knowledge, are members of a series which include prior contacts that can in no sense be called constructs but rather elementary adjustments. . . .
>
> Obviously we have here such a truncation of the operational conception as to convert it into a thoroughgoing subjectivism. In fact, this is nothing more than a modified Berkeleyanism. Despite the verbal insistence upon discrimination as physical and the inevitable acceptance of psychological phenomena as interbehavior when actual experiments are

> described, Stevens' adoption of the operational principle comes to nothing more than a mentalistic psychologist's surface concession to objectivity. (Kantor, 1938, p. 15)
>
> A concession is made in accepting the claim that the data of psychology must be behavioral rather than mental if psychology is to be a member of the United Sciences, but the position taken is merely that of "methodological" behaviorism. According to this doctrine, the world is divided into public and private events, and psychology, in order to meet the requirements of a science, must confine itself to the former. This was never good behaviorism, but it was easy position to expound and defend and was often resorted to by behaviorists themselves. It is least objectionable to the subjectivist because it permits him to retain "experience" for purposes of self-enjoyment and "non-physicalistic" self-knowledge.
>
> The position is not genuinely operational because it shows an unwillingness to abandon fictions. . . . What is lacking is the bold and exciting behavioristic hypothesis that what one observes and talks about is always the real or "physical" world (or at least the one world) and that "experience" is a derived construct to be understood only through an analysis of verbal (not, of course, merely vocal) processes. (Skinner, 1945, pp. 292-293)

In particular, Skinner's contribution to the Symposium on Operationism in 1945 shows just how dramatically he differed from the conventional approach to operationism. He was perfectly willing to talk about the relation between private phenomena and language, because this issue had to be addressed in order to provide a complete account of behavior. He was, however, unwilling to grant the dualistic premise that language, including scientific language, is essentially descriptive of private entities or logical manipulations of private entities. A complete science of behavior does need to accommodate (a) the process by which a vocabulary describing internal bodily states and conditions is acquired and maintained, and (b) the process by which covert behavior comes to exert discriminative control over overt behavior, but these two processes are the ones that need to be analyzed in connection with the relation between private phenomena and language (Moore, 1980). The whole business of language as logical symbols describing the contents of immediate experience was simply the wrong approach to the issue of operationism. As Moore (1980) has recently argued, the conventional approach entails virtually every aspect of the dualistic position. Moreover, although intersubjective agreement on the meaning of certain terms is a very fine thing to achieve, it does not guarantee that the terms so defined are even useful in the analysis of behavior. As Skinner (1945) said, "The ultimate criterion for the goodness of a concept is not whether two people are brought into agreement but whether the scientist who uses the concept can operate successfully upon his material – all by himself if need be. What matters to Robinson Crusoe is not whether he is agreeing with himself but whether he is getting anywhere with his control over nature" (p. 293). Thus, agreement is not the key to workability and

truth. If anything, the reverse is the case.

B. Radical Behaviorism and Operationism

In contrast, for the radical behaviorist, operationism involves the functional analysis of the term in question, that is, an assessment of the discriminative stimuli that occasion the use of the term and of the consequences that maintain it. Thus, Skinner (1945) has said in his contribution to the Symposium on Operationism: "A considerable advantage is gained from dealing with terms, concepts, constructs, and so on, quite frankly in the form in which they are observed – namely, as verbal responses. There is then no danger of including in the concept that aspect or part of nature which it singles out" (p. 271). The conventional interpretation of operationism is designed, of course, primarily to single out the reference in nature for a scientific term or concept. For radical behaviorism, meaning is to be found in the analysis of the independent variables that determine the response, rather than in the analysis of the properties of the dependent variable. Now, it may very well be that in the process of measuring and operating upon things, the scientist produces discriminative stimuli that occasion the use of some term. A functional analysis will then determine whether a scientist has produced stimuli of this sort. But, this is not to say that a term is *synonymous* with a measurement or an operation. The measurements or the operations yield discriminative stimuli, which become aspects of the environment. These stimuli set the occasion for the use of some term, and the term is then a response on the part of the behaving scientist under complex stimulus control. Thus, Skinner (1945) states, "Operationism may be defined as the practice of talking about (1) one's observations, (2) the manipulative and calculational procedures involved in making them, (3) the logical and mathematical steps which intervene between earlier and later statements, and (4) nothing else" (p. 270). As Skinner (1945, p. 294) states, to establish an operational definition is not logic but psychology.

The radical behaviorist views science as essentially "the behavior of sceintists or the artifacts of such activity" (Day, 1969a, pp. 318-319). Thus, to the extent that science implies the behavior of scientists, an analysis of science implies an analysis of scientific behavior. For radical behaviorists, such an analysis is carried out in the framework of the familiar three-term contingency of reinforcement:

$$\text{Discriminative stimulus: response} \rightarrow \text{reinforcing consequence}$$

At issue in such an analysis is the nature of the discriminative stimuli that set the occasion for scientific behavior and the reinforcing consequences that maintain it.

The radical behaviorist sees much of behavior, including scientific behavior, as multiply determined. That is, the radical behaviorist sees science as the product of an exceedingly complex combination of many sources of control. Two such sources are particularly important for the present argument. One is what may be called operational influences upon the scientist, i.e., influences from operations and contacts with data. Discriminative stimuli from this class lead in turn to the reinforcing consequence of effective prediction, manipulation, and

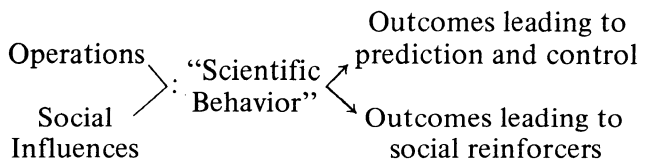
control of environmental events. Thus, one system that determines scientific behavior may be described as follows:

Operations and Contacts with Data	:	Scientific Behavior	→	Outcomes leading to Prediction and Control

The second class of influences upon the scientist may be called social or cultural, in the sense that they are derived from the prevailing traditions of the lay community in which the scientific work is embedded. This class is not so much the class that determines the priorities of scientific work or the ethical considerations concerning its conduct. Those influences exert an effect also but an analysis of those influences is somewhat beyond the scope of the present paper. Rather, what is meant here is the set of traditions and preconceptions already existing in the community that pertain to the issue under investigation. What is meant here is the adherence to what the society in general holds to be an acceptable orientation to the issue. Scientists typically live the first 25 years of their lives, and 12 to 16 hours per day thereafter, in the lay community where they are exposed to such popular preconceptions. The process of acculturization is in large measure the process of coming under the influence of such preconceptions. Discriminative stimuli from this class lead not so much to prediction and control of events, but rather to acceptance for following the established rules of the culture. That is, the reinforcers here are essentially social and cultural. This system may be described as follows:

Social and Cultural Stimuli	:	Behavior	→	Outcomes leading to social and cultural reinforcers

Of course, these two systems do not exist in isolation; rather, both scientific and lay influences of the kind outlined above interact conjointly to determine the behavior of the scientist. Thus, for radical behaviorism, the resulting behavioral system may be described as follows:



The behavior of the scientist may therefore be understood as a product of the conjoint action of scientific and lay discriminative stimuli and scientific and lay reinforcers. As Kantor (1938, p. 29) has perceptively inquired, the central issue in such an analysis is how much the scientist is influenced by his contacts with operations and data relative to his contacts with the lay community. To the extent that the meaning of a term is a function of the discriminative stimuli and reinforcers that control its use, the scientific meaning of a given term derives from the extent to which the term is under the control of the discriminative

stimuli arising from operations and the reinforcers arising from manipulation, prediction, and control. A term with substantial scientific meaning is thus a term with regard to which the contribution of scientific discriminative stimuli and reinforcers is substantial, relative to the contribution of lay discriminative stimuli and reinforcers. Conversely, a term with minimal scientific meaning is a term with regard to which the contribution of lay discriminative stimuli and reinforcers is substantial, relative to the influence of scientific descriminative stimuli and reinforcers. A functional analysis assesses the complex of discriminative and reinforcing stimuli that control the verbal behavior of the scientist and an operational definition, in the sense that Skinner (1945, p. 270) has used the term, focuses upon the influence of scientific discriminative stimuli.

In this regard, Skinner is often attacked for his mere "translations," or for his attempts that serve only to "prune the surplus out of mentalistic language" (Wann, 1964, p. 106). An analysis consistent with the present considerations suggests, however, that Skinner's verbal behavior is not simply a translation, in the sense that translations are members of the same verbal response class that differ only topographically. Rather, Skinner's writing is verbal behavior that is appreciably free from the lay influence, an influence that attenuates the extent to which one's language comes under the control of observations, operations, and contacts with the data. Thus, much of Skinner's writing is under the substantial control of the kind of influences called operational above, rather than of the lay influences. Indeed, Skinner's ability to bring his own verbal behavior under the control of such influences is what makes his writing at once difficult for the beginner to comprehend, because the beginner is sensitive to verbal behavior conspicuously under the control of the lay influence, yet incredibly stimulating for those who have studied behaviorism in some detail, because they recognize what Skinner is up to. We must now specifically assess the nature of the lay influence in psychology.

III. MENTALISM

For radical behaviorists the lay influence in psychology is called mentalism. More specifically, mentalism may be considered as a particular orientation to the explanation of behavior, involving the following implicit or explicit features: (a) the bifurcation of human experience into a behavioral dimension and a pre-behavioral dimension, (b) the use of psychological terms to refer to organocentric entities from the pre-behavioral dimension, and (c) the use of the organocentric entities as causally effective antecedents in explaining the behavior.

This definition specifies that mentalism is a particular approach to the explanation of behavior. The approach involves much more than simple anthropomorphism, in the sense of attributing allegedly uniquely human qualities to infra-human organisms. First, a pre-behavioral dimension of an organism's activity is asserted. This dimension is typically the mental dimension or the non-physical half of a traditional metaphysical dualism. Then, psychological terms are taken as referring to, or as indices of, entities within this dimension, entities

which are understood as causally effective antecedents of behavior. The entities within this dimension are neural, psychic, or conceptual in status, and are acts, states, mechanisms, or processes in kind.

The specific terms used here (e.g., neural, psychic, conceptual) are taken from Skinner's writings (Skinner, 1953, Chapter 3). Neural is used in the sense of physiological, especially having to do with the nervous system, and more especially with the brain. Psychic is used in the sense of nonphysical. Conceptual is used in the sense that it is a convenient analogy or a symbolic expression for an observation, but nevertheless involves the appeal to another level or other terms. Much of cognitive psychology would be included here, if not under psychic. Acts are inner functionings that yield certain outputs; states are inner conditions or feelings; mechanisms are representations of inner hooks, eyes, pulleys, and gears, which are then connected to overt behavioral manifestations; processes are inner activities that provide a basis for the action. A brief review of virtually any text in psychology will reveal common examples of mentalistic usages. It is especially critical to note that mentalism is a way of engaging in explanatory practices. Use of any of the above sorts of terms *per se* does not necessarily constitute mentalism. Rather, it is using them as explanations that constitute mentalism. A simple table, presenting representative terms, will illustrate the distinction involved.

	Act	State	Mechanism	Process
Neural.....	Triggering	Excitation	Gate	Filtering
Psychic	Reasoning	Feelings	Perception	Discrimination
Conceptual	Retrieval	Hunger	Hydraulic Models	Increment in Habit Strength

The origins and maintenance of the mentalistic practices outlined above lie, of course, in the generally prevailing dualistic practices of our culture. As Kantor (1963) has documented, a long tradition, starting at least with the influence of the early Christian church during the Hellenistic era, has mandated the appeal to verbally inferred inner entities. Alternatively stated, a long tradition of primary social reinforcement from the nonscientific community has strengthened particular kinds of explanations, such that these explanations are now very tightly rule governed. The rules may be found in virtually any textbook on research methodology, and the rules concern not only the behavior of the organism in question, but also the behavior of the organism who is offering the explanation. Skinner (1971, Chapter 1) has addressed similar issues in a discussion of autonomous man. The important consideration is that, for both Kantor and Skinner, the effect is about the same: (a) the full power of a scientific analysis is not brought to bear upon human behavior, (b) the analysis of important controlling variables, primarily outside the skin but also within the skin, is neglected in favor of the alleged analysis of exclusively inner causal variables, and (c) a kind of false image of the human being is perpetuated, an image that in turn prompts potentially counterproductive cultural practices. The influence of a mentalistic orientation upon the science of behavior may be called methodological behaviorism, to

which we now turn.

IV. METHODOLOGICAL BEHAVIORISM

For the purposes of the present paper, methodological behaviorism may be regarded as a set of experimental practices, together with the variables responsible for such practices – commonly called "underlying assumptions" – pertaining to how a psychologist should observe the scientific method while doing psychology. Day (1976, 1977) has written extensively on the characteristics of methodological behaviorism, and what follows is taken largely from those references. In brief, this set of practices and assumptions involves the following characteristics:

(a) That scientific knowledge is different from, and is intrinsically superior to, common sense knowledge.

(b) That scientific knowledge is to be gained by conducting carefully controlled experiments that test predictions from hypotheses and evaluate results by using impartial tests of statistical inference. Replication, reliability, and generalizability are the central issues in interpreting the meaningfulness of the results.

(c) That scientific knowledge involves constructing logical domains, within which the logical properties of symbolic entities and mathematical formulae are to be established. Hypotheses derived from manipulation of these symbolic entities evolve into theories, theories evolve into laws, and deductions from the laws may be taken as explanations of the event under consideration.

(d) That in order for the features of the scientific endeavor to be admissible into the body of science, the features must be publicly observable and tightly specified according to the procedures entailed in their measurement.

(e) That causal processes are to be accommodated according to the model of antecedent, linear causation, where causal efficacy with respect to some dependent variable is fully vested in one or more preceding independent variables.

In summary, methodological behaviorism is the effect of a mentalistic epistemology upon a science of behavior. Thus, if one is a methodological behaviorist, one is by definition mentalistic. Conversely, if one is mentalistic, one is usually also a methodological behaviorist, but one need not always be so. Examples of mentalism that do not necessarily involve methodological behaviorism are certain kinds of Gestalt psychology, phenomenology, and humanistic psychology, which hold that human phenomena are not amenable to conventional scientific analysis, but rather only to a different kind, such as rational inquiry or introspection.

Perhaps the most common misunderstanding regarding mentalism and methodological behaviorism concerns the role of "conceptual" causes of behavior. Skinner (1964, p. 106) has explicitly stated that he is a radical behaviorist simply in the sense that there is nothing in his formulation for anything that is mental. Presumably, what this statement means is that just as there is no room for a conceptual cause of the observed organism's behavior, *neither is there room for*

a conceptual cause of the observing organism's behavior. Very often the argument that is offered by the methodological behaviorist runs something like the following: although it is mentalistic to explain behavior by appealing to causal inner entities, it is not mentalistic if the explanation does not advocate the existential reality of the causal entity. For example, it is not mentalistic if the term is asserted to be simply a logical device that mediates accurate description. In the common version of such an argument, the organism is said to act "as if" it possessed such an entity, or "as if" it is "obeying" some equation. Whether the organism really does is unknown, but postulating that it does enables the scientist to predict or describe a behavioral event accurately. The more abstract version of the argument is presented as characteristic (c) above.

The radical behaviorist considers this argument mentalistic because of the way that it accommodates the behavior of the scientist who is doing the explaining. The argument postulates that there are conceptual entities that have a "logical-theoretical" existence, and that these entities in turn cause the scientist to predict and describe correctly. Thus, the position is mentalistic not because it postulates a mentalistic cause of the behavior that is being described, but rather because it postulates a mentalistic cause of the scientist's behavior involved in predicting an explaining. In other words, scientists become mentalistic about the causes of their own behavior when they make such an argument.

The analysis that the radical behaviorist makes of such cases must be presented clearly. The radical behaviorist does not consider the creation of verbal stimuli *per se* mentalistic. Indeed, the creation of verbal stimuli that occasion effective prediction is one of the hallmarks of authentic science. The issue is one of stimulus control. Why does the verbal stimulus occasion effective prediction? Not just any verbal stimulus will so function – what aspect of the situation does the stimulus reflect? What is it about the created stimulus that makes it work in the way that it does? What discriminations are involved? What facts are metaphorically represented? For the radical behaviorist, a given verbal stimulus occasions effective predictions and descriptions presumably because the stimulus is derived from some factor in the observed situation, not because of its subjective or logical-theoretical status. Thus, the radical behaviorist wants to establish the nature of the stimulus control over any verbal device that facilitates effective prediction. Radical behaviorists are never satisfied with just the fact *that* something works: they also want to know *why* it works. The radical behaviorist argues that without specifying the nature of the stimulus control, a scientist is condemned to know nature at best in a fortuitous sense, with little or no possibility of systematically specifying the uniformities that can be derived. Scientists who don't deal with the above questions regarding the nature of stimulus control over their verbal behavior – and to assert that all that is important is the logical application of the term and not where it comes from is surely to ignore the sources of control (see Moore, 1975, pp. 131-132) – will waste resources and mislead generations into mentalistic blind alleys.

Methodological behaviorism is therefore mentalistic many times because it implicitly appeals to mentalistic causes of the behavior of the experimenter, not necessarily because it postulates mentalistic causes of the behavior of the subject.

This mentalism has the same origins, of course, as does the mentalism associated with the causes of the subject's behavior: the prevailing dualistic practices of our culture. The difference between the two forms of mentalism is in effect the number of organisms involved, i.e., whether the mentalistic causes are held to apply to only one organism – the experimenter in a solipsistic sense, or at least two organisms – at least the experimenter and the subject. In any case, the methodological behaviorists assert that they are behaving consistently with respect to certain underlying conceptions regarding the inherent nature of scientific knowledge. These conceptions, of course, are ultimately traceable to the cultural traditions of dualism and autonomous man. The radical behaviorist, in contrast, feels that the epistemological assumptions regarding autonomous man are nothing more than diversions, i.e., culturally instigated rules and verbal stimuli that infringe upon investigative inquiry and thereby occasion the search for the wrong kind, and the accompanying neglect of the appropriate kind, of factors in a behavioral event.

V. SCIENTIFIC VERBAL BEHAVIOR

A. Tacts and Abstractions

The way that radical behaviorism views science and scientific verbal behavior is, of course, quite different. Skinner (1969, Chapter 6, esp. pp. 141-143) has said that much of science is an analysis of the reinforcement contingencies found in nature. That is, much of science aims to devise rules that specify what persons must do to derive reinforcers from nature. The scientific law is valuable because it becomes part of the stimulus complex that occasions effective behavior with respect to nature. So specified, science aims to bring verbal behavior under the discriminative control of the variables and relations from an observed event. Scientific verbal behavior, then, is at heart linked to description of such contingencies. Consider the scientific statement, $s = \frac{1}{2}gt^2$. The knowledgeable reader will recognize that according to Skinner's (1957) classification, this statement is presumably an instance of the manipulation of tacts, i.e., of autoclitic activity. Tacts are verbal responses of a particular form that are under the control of some property of an object, event, or situation. Autoclitics, perhaps the most sophisticated of Skinner's notions regarding verbal behavior, are instances of verbal behavior that depend on other verbal behavior, and serve to identify such features as the strength of the response and the relation to a part to the whole. When scientists formulate a scientific statement, therefore, they tact certain features of what they have observed, but they also manipulate those tacts according to any one of several autoclitic processes (Skinner, 1957, Chapters 12 and 18). The exact details of such processes are complex and await systematic analysis. For the expositional purpose of the present paper, much scientific verbal behavior will be designated simply as manipulated tacts, with the recognition that this specification does not begin to do justice to the richness of Skinner's explanatory system. In any case, with respect to the scientific statement $s = \frac{1}{2}gt^2$, the important consideration is that someone who heeds this equation may interact meaningfully with falling bodies in nature. The equation is in an important sense

a rule that governs the behavior of people. The equation does not govern the behavior of falling objects.

Thus, for the behaviorist, much scientific verbal behavior involves manipulation and comment upon other verbal behavior generically of the sort called the tact. Moreover, the tact may involve abstracting. Abstracting is a kind of tacting that is under the special control of a singular feature of an event or situation, even though other features may also be involved. The notion of the abstract tact in the analysis of scientific verbal behavior is exceedingly important. For example, consider Skinner's article "Current Trends in Experimental Psychology" (as reprinted in Skinner, 1972). Here a 3-stage progression of science is described: (1) to identify the basic data (p. 305), (2) to express orderly relations among the data (p. 307), and (3) to derive higher order concepts from the orderly relations (p. 307). Of particular interest for present purposes is the following passage:

> This step – at the third stage in theory building – can be exemplified by a simple example from the science of mechanics. Galileo, with the help of his predecessors, began by restricting himself to a limited set of data. He proposed to deal with the positions of bodies at given times, rather than with their color or a hardness or size. This decision, characteristic of the first stage of building a theory, was not so easy as it seems to us today. Galileo then proceeded to demonstrate a relation between position and time – the position of a ball on an inclined plane and the time which had elapsed since its release. Something else then emerged – namely, the concept of acceleration. Later, as other facts were added, other concepts appeared – mass, force, and so on. Third-stage concepts of this sort are something more than the second-stage laws from which they are derived. They are peculiarly the product of theory-making in the best sense, and they cannot be arrived at through any other process.
>
> There are few, if any, clear-cut examples of comparable third-stage concepts in psychology, and the crystal ball grows cloudy. But the importance of the stage is indicated by the fact that terms like wants, faculties, attitudes, drives, ideas, interests, and capacities properly belong there. When it is possible to complete a theoretical analysis at this stage, concepts of this sort will be put in good scientific order. This will have the effect of establishing them in their own right. At present they need external support. Some of them, like wants and attitudes, come to us trailing clouds of psychic glory, and a wisp or two of the psychic can usually be detected when they are used. Other concepts, like drives and motives, borrow physiological support in certain favorable cases. Still others, like abilities and traits, have been made respectable through correlational analyses, which give them the status of "individual differences." Although most psychologists think of an ability as something which has meaning in the behavior of a single individual, current techniques of measurement find it necessary to make use of the position of the individual in a population. Magnitudes are assigned to the abilities

> and traits of the individual in terms of his relation to the group rather than through direct measurement. A proper theory at this stage would characterize the behavior of an individual in such a way that measurements would be feasible if he were the only individual on earth. This would be done by determining the values of certain constants in equations describing his behavior, clearly a third-stage enterprise. (pp. 307-308)

What does Skinner mean here? It is hardly the case that Skinner is advocating the use of mentalistic terms in an analysis of behavior. Rather, he is pointing out that *the level and kind of generality* reflected in many mentalistic terms is desirable, and that it is appropriate for scientific terms to be general in this sense. The concept of acceleration in physics may be understood, then, as an abstract tact with a high level of generality. The term acceleration may be used in connection with many different circumstances regardless of the particular object that is accelerating or regardless of the reason why it is increasing in velocity. The same is true for mass and force, in the sense that they are terms under the highly refined control of important features of a set of observed situations. Moreover, they are under the control of these features even though the object has such other features as size, weight, location, etc. Just as one can respond to the color of an object regardless of its hardness or its size, so can one respond to the acceleration of an object regardless of its other physical properties.

Upon analysis, the abstraction of relations within an event may be seen to play a vital role in the development of many scientific accounts. For example, when physics deals with frictionless surfaces or objects falling in a vacuum, truly frictionless surfaces or complete vacuums weren't presumably involved in devising the equations. However, by responding to certain features of an event, such as the acceleration or the mass of a body – even though other features such as wind resistance may also have been involved in the event, physicists found they could develop important formulations that could not have been developed if they had tried to incorporate the other factors. In a sense, they simplified the situation in order to deal with it, and this process of simplifying may be understood as involving abstraction. The scientific statement $s = \frac{1}{2}gt^2$, for example, may be understood as the product of autoclitic activity in which abstractions concerning variables influencing the speed of falling bodies are manipulated. The social sciences are quite naturally following the same pattern as other sciences, although most social scientists do not recognize that that is what they are doing. The economist who deals with a "guns and butter" economy is abstracting; so also is the psychologist who examines behavior generated by some schedule of reinforcement in an operant experimental chamber.

Now, Staddon (1973) has recently proposed a conception of scientific behavior that may be somewhat related to the present one. In brief, Staddon holds that there are two kinds of scientific accounts: (a) the cause-and-effect account, and (b) the "mechanism" account. A cause and effect account is simpler and deals with the straightforward, unidimensional relation between an

independent and dependent variable. Such an account is propadeutic to the development of a "mechanism," which may be understood as a multidimensional account that involves contextual features. The mechanism is the higher order account, and as such is the account toward which a science should strive. However, it is unclear whether Staddon's distinction means that there are two qualitatively different and discontinuous kinds of scientific accounts. If so, Staddon's conception differs from the present one. The present conception, focusing upon the analysis of verbal processes, suggests that scientific accounts are continuous, although their continuity may be differentiated by a greater or lesser degree of generality. Thus, to use Staddon's classifications, a cause and effect account would be an account with a low or intermediate degree of generality, and a mechanism would be an account with a high degree of generality. Staddon's example of a "mechanism" is the gas laws, specifying the interrelations among pressure, temperature, and volume of a body of gas. The present conception agrees that the gas laws are indeed a useful example of a scientific account, but it views them as abstractions, rather than as an autonomous category of a scientific account. The gas laws may be construed as manipulations of abstract tacts in the following way. The statements are under the control of the cause-effect relation between whatever change in energy occurs (the cause) and whatever change in pressure/volume of the gas is observed (the effect), irrespective of whether heat or mechanical energy is the cause of the change. Thus, the gas laws simplify and combine across at least two distinct causal processes, and specify the same effect in terms of the other measured properties.

The role of hypothetical constructs and intervening variables in scientific accounts is an often discussed issue in the analysis of scientific behavior. For the radical behaviorist, such terms must be analyzed as *verbal behavior*. The radical behaviorist does not deny that in some cases, such terms may have proved useful. However, it is not the fact that they are logical entities *per se* that makes them useful. Rather, the radical behaviorist argues that whatever utility they have arises from the extent to which they are abstract tacts. They may not always be abstract tacts, of course, and an operational analysis would so reveal. On some occasions, they may be influenced more by extraneous social and cultural forces, than by operations and contacts with data. It is another legacy of mentalism to hold, as Tolman did, that the job of the scientist is to invent these devices and to establish their logical attributes. Skinner (1945) comments in this regard:

> Psychology, alone among the biological and social sciences, passed through a revolution comparable in many respects with that which was taking place at the same time in physics. This was, of course, behaviorism. The first step, like that in physics, was a reexamination of the observational bases of certain important concepts . . . Most of the early behaviorists, as well as those of us just coming along who claimed some systematic continuity, had begun to see that psychology did not require the redefinition of subjective concepts. The reinterpretation of an

> established set of explanatory fictions was not the way to secure the tools then needed for a scientific description of behavior. Historical prestige was beside the point. There was no more reason to make a permanent place for "consciousness," "will," "feeling," and so on, than for "phlogiston" or "vis anima." On the contrary, redefined concepts proved to be awkward and inappropriate, and Watsonianism was, in fact, practically wrecked in the attempt to make them work.
>
> Thus it came about while the behaviorists might have applied Bridgman's principle to representative terms from a mentalistic psychology (and were most competent to do so), they had lost all interest in the matter. They might as well have spent their time in showing that an eighteenth century chemist was talking about when he said that the Metallic Substances consisted of a vitrifiable earth united with phlogiston. There was no doubt that such a statement could be analyzed operationally or translated into modern terms, or that subjective terms could be operationally defined. But such matters were of historical interest only. What was wanted was a fresh set of concepts derived from a direct analysis of newly emphasized data . . . (p. 292)

The view that certain important analytical terms are abstract tacts clarifies a number of issues that are occasionally regarded as troublesome in the analysis of behavior. For example, consider the operant-respondent distinction. The terms operant and respondent may be fairly considered as abstractions, in the sense that they are tacts of controlling relations. Very often this distinction is criticized because no one can find a key peck engendered by some schedule of reinforcement that might not be influenced by keylight/food respondent relations, or salivation engendered by tone-food powder pairings that might not be influenced by reduction in the aversiveness of dry food. Such criticisms appear to reflect certain mentalistic assumptions regarding scientific language, rather than a weakness in the utility of the distinction. In particular, the criticisms assume that words are logical entities, possessing certain properties by virtue of their meaning. The criticisms further presume that there must be some entity in the objective world that corresponds to the term. A search is then conducted for the corresponding entity, and if it is not found, the term is simply designated as meaningless or nonsensical. For the radical behaviorist, many scientific terms are responsive to subtle features of observed events, even though other features are simultaneously present. To label a given key peck on a variable-interval schedule as an operant key peck does not mean that part of the control may also be exerted by respondent relations, that the peck may have been originated by respondent relations using an autoshaping procedure, or any number of other considerations. It means that with respect to the issue of control, the preponderance of control derives from the consequences of the behavior, rather than from simple eliciting relations. It may very well be that what formerly was operant behavior may be brought under respondent control, but the distinction always involves controlling relations, not the topography of the response, its latency, or

the nervous system that is thought to be the physiological mediator of the response.

Consider also the ways that the term reinforcement is used. A common statement is that reinforcement increases the probability of a response. What appears to be commented upon in such cases is the extent to which the occurrence of a given response is a function of the kind of consequence called reinforcing. More specifically, when a scientist makes such a statement, what the scientist is saying is that the presence of a contingency makes it more likely to observe the response in question. The scientist concedes that he/she does not know the totality of stimuli that impinge upon an organism on any given occasion. Thus, even though a given contingency may be in effect, it may well be that the organism is doing something other than interacting with the stimuli of the contingency. Maintaining the contingency over a period of time has shown in the past, however, that it becomes increasingly likely that the organism will interact with the contingency. Thus, the scientist notes that in the past, the organism has interacted with the contingency in a particular way, and to the extent that the present situation is similar to the past situation, the organism may be expected to interact with the contingency in a similar fashion. The term "probability" is occasioned by the circumstances in which the scientist observes the organism. The term concerns the conditions under which the scientist states why the response occurs. The term does not describe a characteristic of operant behavior *per se*.

Similarly, it is often charged that the definition of reinforcement is circular. What is the case is that persons label a given stimulus as a reinforcer because of its effect on behavior. More specifically, a stimulus is labeled as a reinforcing consequence when three conditions exist: (a) the response produces the consequence; (b) the response occurs more often when it produces the consequence than when it does not; and (c) the increased responding occurs because the response has that consequence (Catania, 1979, p. 74). The term is not used when these conditions are not satisfied. There appears to be nothing circular about such a distinction. As Moore (1980, p. 44) has more extensively discussed, the question of why a given stimulus, when presented contingently upon a response, has the effect we call reinforcing is in part a genetic or physiological question, the answer to which is to be found in the animal's genetic constitution. That we say such a stimulus has the effect it does is attributable to the careful observation of behavior. The matter of circularity arises only when scientific terms, such as reinforcement, are viewed as logical entities that must map onto logical categories in nature. Indeed, an acid test for detecting mentalism is to ask whether the definition of reinforcement is logically circular. Those who answer "yes" and indicate that the problem is that there is no independent criterion for specifying whether a given stimulus is a reinforcer may safely be considered mentalistic.

B. A Critique of Current Experimental Practices: Choice and Matching

In discussing quantitative treatments of behavior, Skinner (1950) commented almost thirty years ago that:

> Beyond the collection of uniform relationships lies the need for a formal

> representation of the data reduced to a minimal number of terms. A theoretical construction may yield greater generality than any assemblage of facts. But such a construction will not refer to another dimensional system, and will not, therefore, fall within our present definition. It will not stand in the way of our search for functional relations because it will arise only after relevant variables have been found and studied. Though it may be difficult to understand, it will not be easily misunderstood, and it will have none of the objectionable effects of the theories here considered.
>
> We do not seem to be ready for theory in this sense. At the moment we make little effective use of empirical, let alone rational, equations. A few of the present curves could have been fairly closely fitted. But the most elementary preliminary research shows that there are many relevant variables, and until their importance has been experimentally determined, an equation which allows for them will have so many arbitrary constants that a good fit will be a matter of course and cause for very little satisfaction. (p. 216)

Like much of what Skinner has said, these comments seem especially relevant with respect to current experimental practices.

Of a particular interest for present purposes are the practices involved in analyzing behavior in choice situations. Many, if not most, current experiments dealing with choice are subsumed under the theoretical formulation of what has been called the "matching law." That is, according to the matching law, an organism's behavior with respect to several concurrently available alternatives is distributed so as to be proportional to, or to match, the reinforcing value of the alternatives. Moreover, the matching law is held to be tautological. Organisms are automatically assumed to match, and departures from matching are assumed to reflect only imperfections in experimental control, for example, from bias or failures to discriminate sufficiently the available alternatives.

Baum (1973) has discussed the theoretical basis for such a position extensively. For example, in examining a molar basis for the law of effect, Baum (1973) concludes that organisms "intergrate all feedback over time" (p. 149). At issue is what controls this statement. If it is derived from a generalized review of experiments, then there is little problem. Presumably, there would not be many experiments showing that organisms don't respond to all feedback integrated over time. Alternatively, if the statement means that by treating organisms "as if" they were feedback machines that have the capability to integrate all reinforcers over time, one can derive an accurate description of their behavior, there is a problem. So interpreted, the statement implies that the experiment's behavior is a product of conceptual causes, as outlined above.

Similarly, Baum (1973) contends that "such integration [of feedback over time] *must* be commonplace in an organism's reactions to its environment" (p. 148, italics added). At issue here is why it is stated that such integrations *must* be commonplace. There is a problem with mentalism if this statement

implies the existence of a logical-theoretical dimension into which an experimenter may tap, in order to uncover the true laws of nature. Day (1976) has elaborated on the pitfalls of such explanatory practices:

> As I have said before, it seems to me that most people who at times function most successfully as contemporary behaviorists, can at other times be led into mentalistic practices. When they do so, of course, they fail to function as behaviorists. An important circumstance in which this often seems to occur is when behaviorists involve themselves in expounding or defending in professional situations what they take to be the behaviorist position. Instead of trying to call attention to whatever contingencies they believe may be operating in the situation at hand, it is easy to fall into practices of explaining the reasons why one should or should not approach a particular problem in a given way. If the reasons are generated self-consciously in knowledge of their likely controlling consequences, then there is little to quibble about. However, serious problems of inherent mentalism can be present for any behaviorist who believes his reasons, or who takes his reasons seriously to himself as legitimate grounds for his professional behavior. If one may not have a very clear notion of what the causes happen to be for his behavior, that is perhaps a particularly appropriate occasion for him to begin the effort to assess what they are. (p. 91)

Thus, the present argument is that mentalism is intrinsically involved in scientific accounts if the experimenters implicitly treat their own behavior, especially their own explanatory behavior, in terms of the classic S-O-R mediation model. If the organism's behavior is accounted for in such terms, then the mentalism is easily detectable, but what is not generally recognized is that the S-O-R mediation model comes into play when an experimenter's own statements regarding an organism's behavior are held to be only metaphorical or descriptive. The implication here is solipsism as it relates to the experimenter. What is being said is that all experimenters can know are their own sensations and the logical manipulations thereof that cause them to make correct descriptions. That is, all one can know are the causes of one's own behavior, and although there may be causes of the organism's behavior, one can't know what they are. All one can do is to try to create a logical-theoretical system that parallels observations.

Choice behavior has, of course, been subjected to sophisticated and potentially productive quantitative analysis. However, a basic issue is the dimensions of the activities so analyzed. For example, Skinner (1950, p. 193) has characterized a theory – or at least the way most people have seemed to view a theory – as any explanation of a fact that appeals to events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions. Similarly, Skinner (1964) noted that a basic consideration in the analysis of behavior is "not the nature of the stuff of which the world is made or whether it is made of one stuff or two but rather the dimensions of the things studied by psychology . . ." (p. 79). Of present interest is the

extent to which current quantitative treatments of choice are responsive to the dimensions of the things they purport to study.

For example, Herrnstein (1964), in discussing a theoretical issue related to choice and the determination of value, argued that a pigeon's "method of averaging tends to weight shorter intervals of the variable interval more than the longer" (p. 181). A potential problem lies with the use of the term "averaging." Is it being stated that the bird first calculates the subjective value of the schedule by averaging certain quantities, and then responds? To so state seems to imply that the activities that cause the pigeon's behavior take place in some other dimensional system. This would be mentalism. On the other hand, if averaging is a purely theoretical construct, then one must ask what is the basis for its explanatory force? If the answer is that it helps to predict or describe the data, then when viewed in connection with the other explanatory practices, it becomes a conceptual cause of the experimenter's behavior.

Similarly, in a recent article about choice that advocates a particular mathematical transformation and equation to deal with certain experimental data, there is the following statement: "The . . . value specified by Equation 1 is a dimensionless number that represents mathematically the observed functional relationship" (Hursh and Fantino, 1973, p. 449). Why is this number labeled as "dimensionless?" Why is there the emphasis on "representing" an observation? If these statements mean that the experimenters are trying to devise a mathematical system that parallels the way that the bird responds, then the experimenters are being mentalistic about their own scientific behavior. The statements mean that the experimenters view themselves as organisms whose explanatory behavior is caused by the logical properties of the mathematical system, rather than by contingencies of reinforcement. Why did the experimenters not assess the processes by which they arrived at the equation?

Viewed in perspective, the explanatory practices involved in many analyses of choice seem to resemble those of S. S. Stevens. Indeed, it is likely that many choice theorists have been influenced by Stevens, either directly through academic training or indirectly through his enormous influence on the field of experimental psychology. The practice is that of circumventing a functional analysis of the behavior of interest by trying to devise different quantitative statements that accord with one aspect or another of the behavior. This explanatory practice is essentially structuralism. Mathematics plays an especially large role in justifying such practices. Again, this influence is traceable to Stevens. For example, in his opening chapter in the *Handbook of Experimental Psychology*, Stevens (1951) states that "the stature of a science is commonly measured by the degree to which it makes use of mathematics" (p. 1). A matter worthy of considerable attention is whether current experimenters in the area of choice have affirmed the consequent. By using mathematics, they have tried to make their science more sophisticated. The more transformations that are involved, the more sophisticated would seem the analysis. All this mathematical activity gives rise to a spurious sense of rigor that is not necessarily reflected in greater control of behavior. For radical behaviorists, the problem with Stevens' view of

science – or Hull's, for that matter – is not that it isn't precise enough or not that one can't make correct predictions. The problem is that it doesn't permit people to assess the variables that facilitate scientific behavior, all because of certain culturally prescribed views regarding the nature of acceptable scientific explanation, and – particularly for Stevens – regarding the nature and causal status of the experimenter's private events.

The role of "value" in current accounts of choice seems to resemble strongly the role of the traditional intervening variable. Miller's (1959) use of thirst is a classic example of the use of an intervening variable, and a strong argument can be made that many experimenters in the area of choice use "value" as a mediational device to explain behavior in a similar fashion. Skinner (1953, especially pages 31 to 35) has commented critically on certain implications of this practice. The thrust of the present argument is that to so view scientific explanation is to view the experimenter's behavior as a product of conceptual causes. This view then inhibits an understanding of what actually does produce useful scientific behavior. The net result is that psychologists will not come under the control of the events they purport to study, but rather will remain under the control of the potent social reinforcers that have led psychologists to misrepresent the state of their knowledge, to use methods – such as hypothetico-deductive methods – that should be abandoned, and to fail to use economic resources to their best advantage.

VI. SUMMARY AND CONCLUSIONS

Behaviorism, according to the Skinnerian point of view, or radical behaviorism, is not simply the scientific study of behavior, but rather an integrated and comprehensive philosophy of science, concerned with the subject matter, methods, and dimensions of psychology. Yet, as Day (1969a) has noted:

> Strange blends of Skinner and conventional behaviorism abound. I'd rather not identify the even relatively prominent Skinnerians who fail to concede that private events have any place in a natural science. Others view *Science and Human Behavior* as somehow beneath empirical dignity; the word is passed around that the sticky parts of the book are to be excused because it is, after all, no more than a sophomore-level text – this in spite of the fact that in a work as crucial as *Verbal Behavior*, Skinner refers the reader back again and again to *Science and Human Behavior* for his most thorough analysis of the issue of private experience (1957, p. 130). Mentalism among Skinnerians is rampant, and they are quickly trapped by the operationism of Boring and Stevens. Unfortunately, only very few people have an accurate idea of what Skinner means by operational definition. I have taken the liberty of speaking here directly to some of those who preach most loudly a supposedly Skinnerian line. One hardly knows where to begin to analyze the grossly uninformed verbal material that is generated concerning Skinner's work by the typical psychologist. (pp. 326-7)

The present paper has attempted to clarify certain of the important characteristics of radical behaviorism, characteristics that past experience has indicated have not been well understood.

Radical behaviorism is attractive to many, of course, including Skinner himself, because of the manner in which it accommodates important epistemological issues, rather than because of what animals can be taught to do. Central to the manner in which it accommodates these epistemological issues is the analysis of verbal behavior, especially the verbal behavior of scientists. For radical behaviorism, then, neither the verbal behavior of the experimenters nor the behavior of the subjects is to be dealt with in mentalistic terms. Implicit appeals to mental entities or to logical-theoretical dimensions in the causal explanation of the experimenter's verbal behavior is as much mentalistic as are such appeals in the explanation of the subject's behavior. The central issue is the source of control over scientific verbal behavior, rather than the percent of the variance that is accounted for. To assess the source of control over scientific verbal behavior is to assess its possibilities as a guide for effective action with respect to nature.

REFERENCES

Baum, W. The correlation based law of effect. *Journal of the Experimental Analysis of Behavior*, 1973, *20*, 137-153.

Day, W. F. Radical behaviorism in reconciliation with phenomenology. *Journal of the Experimental Analysis of Behavior*, 1969, *12*, 315-328. (a)

Day, W. F. On certain similarities between the *Philosophical Investigations* of Ludwig Wittgenstein and the operationism of B. F. Skinner. *Journal of the Experimental Analysis of Behavior*, 1969, *12*, 489-506. (b)

Day, W. F. Contemporary Behaviorism and the Concept of Intention. In W. J. Arnold (Ed.), *Nebraska Symposium on Motivation, 1975*. Lincoln: University of Nebraska Press, 1976. Pp. 65-131.

Day, W. F. On the difference between radical and methodological behaviorism. Paper presented at Midwestern Association of Behavior Analysis, 1977.

Heidbreder, E. *Seven Psychologies*. New York: Century Co., 1933.

Herrnstein, R. J. Aperiodicity as a factor in choice. *Journal of the Experimental Analysis of Behavior*, 1964, *7*, 179-182.

Herrnstein, R. J. Nature as nurture: behaviorism and the instinct doctrine. *Behaviorism*, 1972, *1*, 23-52.

Hursh, S. and Fantino, E. Relative delay of reinforcement and choice. *Journal of the Experimental Analysis of Behavior*, 1973, *19*, 437-450.

Kantor, J. R. The operational principle in the physical and psychological sciences. *Psychological Record*, 1938, *2*, 1-32.

Kantor, J. R. *The Scientific Evolution of Psychology*. Chicago: Principia Press, 1963, Vol. I.

Miller, N. E. Liberalization of basic S-R concepts: Extensions to conflict, behavior, motivation, and social learning. In S. Koch (Ed.), *Psychology: A Study of a Science, Vol. 2*. New York: McGraw-Hill, 1959. Pp. 196-293.

Moore, J. On the principle of operationism in the science of behavior. *Behaviorism*, 1975, *3*, 120-138.

Moore, J. On behaviorism and private events. *Psychological Record*, 1980, *30*, (in press).

Moore, J. *Learning* by A. C. Catania. *The Behavior Analyst*, 1980, *3*, 39-45.

Skinner, B. F. The operational analysis of psychological terms. *Psychological Review*, 1945, *52*, 270-277, 291-294.

Skinner, B. F. Are theories of learning necessary? *Psychological Review*, 1950, *57*, 193-216.

Skinner, B. F. *Science and Human Behavior*. New York: MacMillan, 1953.

Skinner, B. F. Behaviorism at fifty. In T. W. Wann (Ed.), *Behaviorism and Phenomenology*. Chicago: University of Chicago Press, 1964. Pp. 79-106.

Skinner, B. F. *Contingencies of reinforcement: A theoretical analysis*. New York: Appleton-Century-Crofts, 1969.

Skinner, B. F. *Cumulative Record*. New York: Appleton-Century-Crofts, 1972.

Skinner, B. F. *About Behaviorism.* New York: Knopf, 1974.
Spence, K. W. The postulates and methods of "Behaviorism." *Psychological Review,* 1948, *55,* 67-78.
Staddon, J. E. R. On the notion of cause, with applications to behaviorism. *Behaviorism,* 1973, *1,* 25-63.
Stevens, S. S. Psychology and the science of science. *Psychological Bulletin,* 1939, *36,* 221-263.
Stevens, S. S. Methods, measurements, and psychophysics. In S. S. Stevens (Ed.), *Handbook of Experimental Psychology.* New York: Wiley, 1951. Pp. 1-49.
Watson, J. B. Psychology as the behaviorist views it. *Psychological Review,* 1913, *20,* 158-177.

FOOTNOTE

Reprints may be obtained from the author at the following address: Department of Psychology, University of Wisconsin-Milwaukee, Milwaukee, Wisconsin 53201. The author wishes to express his gratitude to A. C. Catania, P. Killeen, and W. F. Day for their helpful comments on earlier versions of the paper.