

## RADICAL BEHAVIORISM IN RECONCILIATION WITH PHENOMENOLOGY<sup>1</sup>

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Much of the material which follows is under partial audience-control of the symposium on behaviorism and phenomenology held in the spring of 1963 at Rice University. The papers presented at this symposium, together with the discussion among the speakers and members of the audience, have been published in a recent book edited by Wann (1964) and entitled *Behaviorism and Phenomenology: Contrasting Bases for Modern Psychology*. Of the six speakers at the symposium two, Sigmund Koch and B. F. Skinner, are acknowledged authorities on the practice of behaviorism; two, R. B. MacLeod and Carl R. Rogers, are widely held to be advocates of phenomenology in psychology; and two, Norman Malcolm and Michael Scriven, are prominent professional philosophers, both known for their interest in the philosophical implications of contemporary psychological thought. The significance of the Rice symposium should not be underestimated by psychologists. The papers presented are instances of professional comment of unusually high quality, and they cast an especially instructive light on the current state of affairs of psychology as a science.

In overview, two significant conclusions appear to have emerged from the Rice symposium. The first of these is that behaviorism, in the sense in which the term is widely used among psychologists, is essentially an unproductive and unrealistic framework within which to pursue psychological research. This is to put the matter as mildly as possible, for the point was pursued with much force and clarity by Koch in his paper, and it was strongly seconded by Scriven, as part of his

broader appraisal of the long-range potentialities of professional psychology. In a particularly choice statement, Koch expressed what appears to be to some extent his reaction to the symposium as a whole: "I would be happy to say that what we have been hearing could be characterized as the death rattle of behaviorism, but this would be a rather more dignified statement than I should like to sponsor, because death is, at least, a dignified process" (p. 162).<sup>2</sup> The following is representative of what Scriven had to say on the same topic.

So I would conclude by saying that Professor Koch's criticisms of behaviorism effectively destroy a specter which was, indeed, haunting and which has continued to haunt the subject. I think of behaviorism, as I know he does, as something which will leave its mark on a generation of graduate students now arising and will thus be with us for thirty or fifty years. I spend my life going around campuses and finding in each new psychology department a new burst of colossal enthusiasm; the leading lights of the graduate student body turn out to be enthusiastic, tough-minded positivists circa 1920. And they are now in their twenty-second year of age and, unfortunately, are likely to live a very long time. Some of them will presumably retain this approach to the subject. This is one reason why I believe that philosophy has an enormous influence on psychology though it has often been very bad. But that is, of course, because it was the wrong philosophy! (pp. 181-182)

Were anyone to search the symposium for disagreement with the conclusion that if con-

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<sup>2</sup>Where no date is given, references apply to page numbers in Wann (1964).

ventional behaviorism is not now dead we should certainly all be better off if it were, he would have to content himself, rather remarkably, with certain statements of Carl Rogers. At one point in the discussion Rogers remarks that it had not been his purpose to point out "any theoretical flaw in behaviorism. There is a lot about behaviorism that I accept. I was simply trying to go beyond it" (p. 157). To be sure, Rogers calls attention to what he considers certain weaknesses of behaviorism. "Valuable as have been the contributions of behaviorism, I believe that time will indicate the unfortunate effects of the bounds it has tended to impose. To limit oneself to consideration of externally observable behavior, to rule out consideration of the whole universe of inner meanings, of purposes, of the inner flow of experiencing, seems to me to be closing our eyes to great areas which confront us when we look at the human world" (p. 119). However, the spirit of his paper is essentially conciliatory. "There are without doubt some individuals in this current of thought who maintain the hope that this new point of view will supplant the behaviorist trend, but to me this is both highly undesirable and highly unlikely. Rather it will mean, I believe, that psychology will preserve the advances and contributions that have come from the behaviorist development but will go beyond this" (p. 118). Rogers proceeds to argue for the measurement of new phenomenological variables, employing "thoroughly objective measures, whose results are publicly replicable" (p. 120), "using methods which are strictly operational" (p. 121), *etc.* Such procedures would presumably yield knowledge stemming from only one of the "three ways of knowing" that he differentiates in his paper.

The reader need not look to the paper by B. F. Skinner for a defense of conventional behaviorism. Skinner's radical behaviorism stands sharply in contrast to the more popular varieties of behaviorism criticized so effectively by Koch and defended in part by Rogers. The theme of Skinner's antagonism to conventional behaviorism runs through much of his later work. His objections to the inroads upon behaviorism of logical positivism and of what he calls "the operationism of Boring and Stevens" can be seen explicitly in as early a work as his revolutionary paper on operationism (1945). It is clear that Koch did not intend to exempt

Skinner from the force of the attack developed in his paper. Yet it is equally clear that Skinner did not feel that the major points of Koch's argument applied to him. In the discussion following Koch's paper Skinner insisted that he does not "subscribe to any of the stratagems of science" Koch described (p. 42). A member of the audience remarked that what Professor Koch presented did not seem "truly representative of what Skinner has to say" (p. 42). Even Scriven, in his formal critique of Koch's paper, emphasized the need for keeping clearly in mind the differences between conventional behaviorism and Skinner's views: "We notice that it is characteristic of Skinner's behaviorism that he does not saddle himself with this apparatus of the logic of science, which Koch so rightly criticizes. I would, therefore, put in a plea here for making a very careful distinction between the standard forms of behaviorism, and Skinner's, which really meets only two of Koch's five criteria for behaviorism, and these, in a rather special way" (p. 181). However, Koch was not unaware of the fact that certain differences exist between conventional behaviorism and Skinner's point of view. He seemed, rather, to feel that the Skinnerian position is in some fundamental way internally inconsistent. He referred to the position as "strange and equivocal", "systematically ambiguous", and "Pickwickian" (p. 43). Other more specific objections are mentioned. Although Koch persisted throughout the symposium in his view that Skinner's radical behaviorism is little more than a rather unusual variety of neo-neobehaviorism, in one of the discussions he made the following comment concerning the need to clarify the differences between conventional and radical behaviorism: "[Skinner's formulation in the paper presented was so] extraordinarily libertarian . . . that one begins to wonder what the actual defining characteristics of the behaviorist thesis or the behaviorist method might be in his particular case" (p. 98).

The second general conclusion to emerge from the Rice symposium was that in spite of the patent difficulties of a conservative and conventional behaviorism, there are marked indications of an increasing *rapprochement* between the interests of behaviorism and phenomenology. This conclusion was apparently not expected by those who organized the symposium. Wann appeared surprised to have to

conclude that the major trend in the symposium was a "blunting rather than a sharpening of the contrasts between behaviorism and phenomenology", and that "Professors MacLeod, Malcolm, Rogers, and Scriven, in one way or another, suggest the possibility of coexistence" (p. vii). Even Koch was aware of this trend, although he found it deplorable (p. 162). There was little uniformity of opinion concerning the nature of the implied compatibility. MacLeod suggested, with some diffidence, that the phenomenological approach in psychology might lead in part into some kind of "sophisticated behaviorism" (p. 55), and Scriven spoke specifically of the reconciliation of what he called *defensible* forms of phenomenology and behaviorism (p. 180). Malcolm was led to the conclusion "that Skinner had stated here an absolutely decisive objection to introspectionism", and he devoted considerable attention to giving "an account of the hard core of logical truth contained in behaviorism" (p. 149). Malcolm qualified his endorsement by stating that the Achilles' heel of behaviorism lay in its treatment of psychological sentences in the first-person-present tense. Skinner, for his part, thanked Malcolm for his support and suggested that perhaps agreement could be reached, even with respect to first-person statements (p. 155).

However, Wann is not so sure that Skinner's views as presented in the symposium are compatible with the interests of phenomenology. In his introduction, Wann leaves it for the reader to decide whether Skinner joins the majority of the symposium in seeing the possibilities of a reconciliation (p. viii). On the inside flap of the book's jacket it is stated that "only Koch and Skinner dissent from the view that coexistence is possible." Certainly this hesitancy to regard Skinner's position as in any way compatible with phenomenology is characteristic of the attitude maintained by a majority of contemporary psychologists. The purpose of this paper is to show that Skinner's radical behaviorism is indeed capable of encompassing a productive phenomenology. In the material that follows there is first a description of what I shall call the basic dimensions of radical behaviorism. This is partly in response to Koch's request for a statement of the "defining characteristics" of Skinner's position. Next, I shall attempt to illustrate the way in which radical behaviorism might profitably

proceed to interact with problems that are often considered phenomenological in nature. Finally, I shall close with a brief discussion of some of the major problems that are faced in bringing about an effective reconciliation of radical behaviorism and phenomenology.

#### THE BASIC DIMENSIONS OF RADICAL BEHAVIORISM

In this section I shall try to describe what I consider to be the basic dimensions of Skinner's radical behaviorism. I feel that almost all of what I have to say is rigidly under the control of a careful study of Skinner's work in breadth and depth. Nevertheless, I am prepared to accept responsibility if some defense should be necessary of the presentation that I make. I have chosen this course of action largely for reasons of simplicity and convenience. Skinner's work is difficult to understand, and a detailed textual explication of the relevant material scattered throughout his writing would involve a tiresome analysis of specific contexts.

As an example of the problem faced in detailed explication, consider the following two statements by Skinner which bear directly upon the difference between radical and conventional behaviorism. First, in the early symposium on operationism Skinner discusses the difficulties which arise in making a distinction between things that are public and those that are private. He criticizes "the arid philosophy of 'truth by agreement'", a perspective often adopted by conventional behaviorists who claim that scientific knowledge must somehow be essentially public in nature. Skinner proceeds to state that "the distinction between public and private is by no means the same as that between physical and mental. That is why methodological [or conventional] behaviorism (which adopts the first) is very different from radical behaviorism (which lops off the latter term in the second)" (1945, p. 294). Secondly, in response to a question raised by Scriven in the Rice symposium concerning how he can justify calling himself a radical behaviorist, Skinner replies that, "I am a radical behaviorist simply in the sense that I find no place in the formulation for anything which is mental" (p. 106). These comments by Skinner are likely to appear somewhat cryptic to someone who has not studied Skinner's

published work intensively. It is clear that Skinner is objecting here not to things that are private but to things that are mental. It is true that the distinction between radical and conventional behaviorism hinges in a number of ways on the issue of mentalism. It is also true that one of Skinner's most persistent objections to conventional behaviorism is directed at a fundamental mentalism which he sees as all too thinly disguised. Yet actually, the issues involved in what Skinner means by "mentalism" are quite complex. The careful clarification of what Skinner is getting at in such statements as these requires a more detailed analysis than can be given here.

#### *A Focal Interest in the Control of Behavior*

Perhaps the most conspicuous characteristic of radical behaviorism is its focal interest in the control of behavior. Radical behaviorists view themselves as essentially engaged in a search for what they call controlling variables, even though the term *variable* is often used in a sense only distantly related to its etymology. Events are considered controlling variables when they are seen, or perceived, to be related to behavior in some way. However, many times the identification of controlling variables does not follow from anything so simple as an observation of the temporal contiguity of phenomenal events. The identification is often likely to be emitted more after the fashion of what might be called a guess, a hunch, or an insight. Verbal behavior describing a relation between behavior and controlling variables is called the statement of a functional relationship, and a more or less systematic attempt to describe functional relationships is called a functional analysis of behavior. In the statement of a functional relationship, the controlling variable is called a stimulus, and that aspect of behavior seen in relationship to the controlling variable is called a response.

In using such words as *seen*, *perceived*, *observation*, *guess*, *hunch*, and *insight*, as in the preceding paragraph, the radical behaviorist does not feel that he is specifying with very much precision what many psychologists would call either behavioral or mental processes. He is simply talking as best he can—actually, in this case he is not talking as carefully as he might—and he is responding to discriminable events which have not been very consistently differentiated by whatever

factors govern the way in which we learn to talk as we do. He calls a stimulus a stimulus for reasons that are presumably similar to those which make people speak of cows as cows, and he can be led to attempt to define a stimulus under circumstances (and with characteristic difficulties) that are much the same as what one might expect to find in being called upon to define a cow. Yet why struggle to define a cow when any child sufficiently exposed to the ordinary verbal community can identify one on sight? Similarly, the capacity to identify a stimulus as such presumably depends largely upon the reinforcing practices of some scientific verbal community.

The practice of looking for functional relationships is obviously similar in certain respects to the effort to find relations between cause and effect. Yet in attempting to discover functional relationships the radical behaviorist does not accept any *a priori* logical assumption of a universe that is orderly in a mechanical sense upon which he feels he must base his scientific work. To be sure, he can easily be led, by appropriate verbal manipulation, to state that he "[assumes] that nature is orderly rather than capricious" (Skinner, 1950, p. 193). However in doing so, nothing of the least systematic significance is asserted. A rancher can undoubtedly be led to state that he assumes cattle are on a particular range where he expects to find them, yet it is absurd to look to a cowboy for profound philosophy.

The interest of the radical behaviorist in the concept of control reflects his conviction that if knowledge is to be trusted it is often likely to lead ultimately to effective action. He is most confident in his statement of a functional relationship if it plays some part in guiding him eventually to the successful manipulation, *i.e.*, control, of specific behavior. Furthermore, a focal interest in the control of behavior does not prejudice the case for the importance in human functioning of genetic or constitutional factors, nor does it lead to any such grandiose hypothesis as that all behavior is controlled by reinforcement.

#### *The Focal Awareness That Any Scientist Is Himself a Behaving Organism*

A second basic dimension of radical behaviorism is its insistence that scientists are themselves no more than behaving organisms. Science is at heart either the behavior of

scientists or the artifacts of such activity, and scientific behavior is in turn presumably controlled by much the same kind of variables as those which govern any other aspect of complex human behavior.

However, as commonplace as this notion may appear to most psychologists, it leads in the mind of radical behaviorists to conclusions that are likely to seem strange to many persons. The radical behaviorist faces the fact that the ultimate achievement of his scientific activities is for the most part either further verbal behavior on his own part or a new set of acquired behaviors which hopefully enable him to control nature more effectively. Yet in viewing his own verbal and intellectual behavior as significantly controlled in a number of ways, he is led in a sense not to trust it at face value. He is aware, for example, that much of what he says in offering systematic psychology is likely to reflect psychological distinctions that are modeled after linguistic practices uncritically acquired simply in learning to speak the lay vocabulary. He is particularly conscious of the fact that much psychological talk reflects stereotyped conceptions both of the nature of the knowing process and of the relation between our knowledge of things and the structure of whatever it is that is taken to be the object of psychological investigation. For example, he is suspicious of primitive animism, which embodies nature with man-like powers, strengths, and forces, as well as of a facile determinism, which views the aim of research as isolating the fundamental elements of nature which are thought of as existing in some kind of mechanical interrelationship.

His resistance to such hidden epistemology leads at times to an obstinate refusal to think in terms of a particular common-sense theory of what it is to have knowledge about one or another subject matter. This is the notion that whenever we have significant knowledge, this knowledge consists of an at least partial identification of the inherent nature of what it is that is known about. The notion is, in other words, that in knowing about something the expression of our knowledge consists in a comment on the *nature* of the object of knowledge or of a statement of what the object of knowledge *is*. It is as if in verbalizing our knowledge of things we have always to express an identification of one or another aspect of the permanent structure of nature. Yet the

radical behaviorist is aware that we may attribute thing-ness to events largely because we are accustomed to speak of the world about us as composed of objects which are felt to possess an inherent constancy or stability. He is reluctant to take for granted that all useful knowledge must be conceptualized in terms of verbal patterns of thought derived simply from our experience with material objects. Consequently, he is led to a position which is peculiarly anti-ontological.

In particular, he objects to speaking of the events associated in a functional relationship as if they were things and objects having a more or less permanent identity as real elements of nature. He does not believe that the functional relations he describes constitute an identification of anything which might be called "true laws of nature", in the sense that the systematic collection of such functional relations can ultimately be expected to fit together into a completed picture of an account of human interaction with the environment. Rather, he is content for the most part simply to describe whatever natural consistency he can actually see, and to hope that the report he makes of his observations will in turn generate ultimately more productive behavior in the control of human affairs. He adopts this course of action out of an interest in increased efficiency and a conviction that only the analysis of behavior will lead some day to a more trustworthy set of guidelines for the acquisition of knowledge.

Consider several illustrations of this point of view. The statement made above that science is the behavior of scientists is not viewed by the radical behaviorist as a reductionist treatment of what might be viewed as an ontological assertion. It is regarded instead as an highly abstract description of what we are probably looking at when we identify events as constituting science. In the Rice symposium Skinner bluntly states that he is not interested in the nature of reinforcement. He comments: "I do not know why [food is reinforcing to a hungry animal] and I do not care" (p. 104). Skinner is also well known for his repudiation of reference theories of meaning (*e.g.*, 1957, p. 7f., p. 114f.). Such theories generally assume that words are objects which are somehow attached to other objects or entities which are called meanings. In what Skinner calls mentalism, inner psychological processes are given ho-

muncular power to cause other more behavioral events to come about. It is not so widely recognized that it is possible to "mentalize" environmental events, as where reinforcers are endowed, often in the thinking of avowed Skinnerians, with some sort of demoniacal power to forge the chains of a reified conception of conditioning.

The reader is likely to resist strenuously such an anti-ontological outlook. Consider the following remarks by Koch: "More generally, I think there is something frightening about the way neo-neobehaviorism is treating the newly reclaimed subject-matter. . . . Scientific knowledge is, of course, 'selective'—but when ontology is distorted, denied, or evaded past a certain point, one is no longer in the context of serious scholarship" (p. 32). Similarly, MacLeod has the following to say: "To build a science of psychology one must begin with the phenomenal world, but then one must transcend it. . . . Every scientist is a metaphysician, whether or not he likes to admit it, at least to the extent that he asserts the existence of something which he does not fully understand but which he is determined to investigate" (p. 54).

Still, the force of radical behaviorism presses for the formulation of a radically new epistemology. The most conspicuous characteristic of this new epistemology will be that it will have been obtained by the psychological analysis of the behavior, both public and private, of scientists, scholars, and whatever other persons can reasonably be said to know things. It will involve, more specifically, the analysis of the variables controlling the verbal behavior of whosoever uses the word *knowledge* and related terms in an interesting and significant way. Skinner has called at great length and over a period of many years for the formulation of such an epistemology (*e.g.*, 1945, p. 277; 1957, Ch. 18; 1961, p. 392; 1964, p. 104), but as yet no serious attempt at the requisite behavioral analysis has been undertaken.

Even so, in the last analysis the radical behaviorist is committed to an exceedingly liberal position with respect to the verbal behavior of his professional colleagues. Admittedly, the reliance upon a speculative epistemology is deplorable, especially when unrecognized or unintended, but objection is ultimately to be raised only on pragmatic grounds.

Anyone is basically free to speak as he does. A man says what he can say; he says what he *does* say, and all this is in principle acceptable to the radical behaviorist, since whatever is said is as such a manifestation of complex human functioning and is consequently the legitimate object of behavioral investigation. In responding to professional language, the radical behaviorist has his own new course to follow: he must attempt to discover the variables controlling what has been said. Even the most mentalistic language is understandable and valuable in this sense. The meaningfulness of psychological and mental terms provides no insuperable problem, provided the verbal practices of both speaker and hearer have been shaped by overlapping verbal communities. The meaning of such terms can be clarified by an attempt to assess the observable (not necessarily publicly observable) events that act as discriminative stimuli in control of emission of the term. This kind of analysis is what Skinner has in mind when he speaks of "operational definition" (1945, p. 271).

#### *The Focal Interest in Verbal Behavior Controlled by Directly Observed Events*

The radical behaviorist is further characterized by the heavy value he places on the consequences of direct observation. In his view, the more he can bring his own verbal behavior under the control of what he has actually observed, the more productive and useful it is likely to be. The control exercised by the observed event may be relatively direct, as in simple description, or rather more complex, as in the identification of controlling variables or in the behavior of deciding which of a variety of potential variables next to manipulate. In the early stages of research, or when an overabundance of theoretical speculation has become involved, there is generally a preference for simple description. The power of simple description as a method for generating knowledge appears to have been grossly underestimated. Of course, nothing in the Skinner system requires that the observer restrict his talk simply to the emission of descriptive statements. Once the observation of behavior has taken place, the observer should be encouraged to talk interpretatively about what he has seen, not necessarily restricting himself to the identification of controlling variables. To be sure,

the radical behaviorist recognizes that the particular interpretation that he makes will be a function of his own special history, and clearly interpretation guided by extensive observation of relevant behavior is to be preferred to speculation by the novice. It is, of course, only under very special circumstances that the interpretations that an observer makes of what he has seen should be identified as a contribution to psychological "theory".

This outlook is viewed as markedly in contrast with most popular approaches to psychological research. The standard machinery of experimental method in psychology is seen as yielding results that are much too distantly related to anything directly observable. To be sure, in most psychological studies subjects are required at some point or other to do at least something which is capable of being seen. But how often does the psychologist actually watch his subjects in action, hoping simply that what he sees will lead him to talk more informatively about what he is investigating? All too frequently, the principal investigator of a research project merely surveys an orderly collection of numbers, usually purported to be composite "measures" of something or other. These numbers have in turn been written down by some more fortunate graduate student who presumably had at least the opportunity to observe the relevant behavior as it actually took place. Even in circumstances where the behavior of immediate interest is preserved intact, as in the verbal protocols used in content analysis, how frequently is the experimenter himself in a position to observe the specific stimulating conditions under which the behavior has been emitted? Without the most skillful practices of observation on the part of the experimenter himself, why should one expect a relation between stimulus and response ever to be perceived? It is not that conventional experimental method is incapable of generating the observed functional relationships so much of interest to the radical behaviorist, especially when the method has been purged of statistical over-refinement. The point is only that most psychologists rarely take active advantage of the opportunity to inspect both behavior and its controlling stimulation as closely as they might. Cumulative records are valued by Skinner precisely because he feels they make certain interesting changes in behavior conspicuously *visible*.

However, the interest of the radical behaviorist in the effects of observation is neither complex nor profound. He merely hopes that what he sees will come to exert an increasing influence on what he says. In this he is not unlike the unpedantic clinical psychologist who simply behaves in the therapeutic situation in a way that he regards as the natural outcome of his past experience in treating patients. Usage here of the term *observation* does not imply any special mental or behavioral process. The verbal community teaches us all to distinguish observation from reflection, speculation, wool-gathering, thinking, and other psychological activities, although the extent to which the differential reinforcement involved is consistent has not as yet been described. Neither does the focal interest in observation commit the radical behaviorist to the notion that observation must somehow be essentially public. In fact, most of the time it is easiest to view observation as something private, in the sense that no more than one individual participates in the behavioral event we identify as a single act of observation. Similarly, there is no restriction of interest to events which are considered to be observable "in principle" by someone else. The radical behaviorist feels as free to observe or otherwise respond to his own reactions to a Beethoven sonata as he is to observe those of someone else.

#### *The Focal Awareness of the Importance of Environmental Variables*

The radical behaviorist is interested in the environment for a variety of reasons. First of all, it is simply obvious that a great deal of behavior is to some extent under environmental control. However, it is not so obvious that the grain of the environmental control of behavior is much finer than is commonly appreciated; the slightest difference in stimulating conditions (which the experimenter is often not prepared to appreciate) may lead to very gross differences in behavior. For example, in an experimenter's attempt to get a human subject to press a button over and over again in a standard rate of response study, the instruction, "Following your first press, if you wish to continue to press the button, you may do so", is observed to lead some subjects simply to keep the button depressed for as long as 15 minutes, with no environmental change

taking place. Similarly, in teaching machine programming, it generally requires very subtle environmental engineering to make it highly likely that the experimental student will emit the desired response.

An interest in the environment also follows from the inherently practical orientation of the radical behaviorist. In so far as he is interested in manipulation and control, he becomes committed to a basic concern with the environment. Whatever is done by way of any manipulation inevitably consists of some change in the environment of the person whose behavior is to be affected, and one has little reason to expect a manipulation to be successful unless it reflects some functional relation between behavior and the relevant environmental change. To precisely the same extent as one is interested in manipulation he becomes concerned with ways in which the environment is related to behavior. However, the radical behaviorist is interested in manipulation not only for its immediate effect upon the behavior he is attempting to control, but also because he wants the manipulation to have some effect upon his own behavior as a scientist. The extent to which he is able to manipulate behavior successfully is perhaps the most important variable that acts to shape his own research activities. He is likely to feel that the most effective means of acquiring knowledge about some aspect of behavior is to attempt to learn how to shape up that very behavior in which he is interested. In speaking of the need for an empirical epistemology, Skinner states that "it is possible that we shall fully understand the nature of knowledge only after having solved the practical problems of imparting it" (1961, p. 392).

The radical behaviorist is interested in the environment for a still more basic reason. He holds the view that *all* verbal behavior, no matter how private its subject matter may appear to be, is to some significant extent controlled by the environment. Although he recognizes that the range of phenomena related to human verbal functioning varies from the most intimately personal to the most spectacularly social, he sees that all meaningful language is shaped into effective form by the action of an environmental verbal community. It is this contact of language with the environment that enables us to respond effectively to it. We know, in other words, what language

means because some common environmental contingency controls both our own behavior and that of the speaker whose talk is of interest to us. To be sure, it is only rarely possible for us to perceive directly the relevant environmental variables as they operate to shape the verbal behavior with which we are concerned. Yet the problem here is no different in kind from that faced in attempting to infer the contingencies controlling any aspect of a person's previous history. The verbal community has taught us a variety of practices by which we guess at relevant factors, some more useful than others.

The case is not prejudiced for an interest in what someone has to say about what he considers his own private experience. Verbal behavior constitutes by far the most convenient avenue of access to anything that might be considered a significant aspect of human knowledge, including one's own knowledge of himself. If we want to find out more about what a man is experiencing in a certain situation, one of the simplest things to do is to try to get him to talk. Of course, whether or not we happen to trust the speaker depends upon the nature of the environmental control exercised by him over our own behavior. Yet the radical behaviorist is not basically concerned with whether or not a speaker is telling the truth. What he wants to know is what makes him say the things that he does. This leads him inevitably to a concern, at least in part, with the environmental events that have acted to teach him to talk. It leads also to an interest in possible events in the present and recent environment of the speaker that bear some similarity to the stimulation available to the verbal community in providing initial differential reinforcement. In searching for such influences he will be himself for the most part responding in some way to the environment. It is the belief of the radical behaviorist that by tracing the environmental chain of command over verbal behavior as far as possible, he can extend the range of his effective action as a scientist most profitably. Suppose, for example, that a student begins to suspect that he senses some order of a particular kind in human functioning. What must he do? He must not fail to proceed directly to an explicit verbal description of *what he has seen* that appears to make him think he has found something. This first step involves, of course, an

analysis of the environmental control of his own behavior.

There is yet a fourth way in which the radical behaviorist is interested in the environment. He tends to regard explanations as simply incomplete if they do not involve tracing the observable antecedents of behavior back as far as possible into the environment. Many current psychological explanations are thus seen as incomplete, since they often do little more than specify some inner process as the cause of a particular aspect of behavior. Issues of ontology are again involved here, since explanatory inner processes are generally regarded as having a kind of power metaphorically related to primitive animism. However, if the ontological pattern of language is insisted upon, it is only reasonable to ask what makes the inner process work as it does. Since an answer to this question is usually not provided, the radical behaviorist regards such explanations as incomplete.

The case is different if the explanation involves no more than the description of a relation between behavior and some observed private event. Such a relation constitutes a legitimate functional relationship in precisely the same way as does the statement of a relation between behavior and the environment. Here the radical behaviorist asks only that the situation be more closely examined to see if the private event in turn cannot be seen to bear some relation to the environment. If it can, then the functional relation as stated is clearly incomplete as an explanation of the behavior. In those cases where the private event is conspicuously related to the environment, then reference to the private event is likely to be considered irrelevant or unnecessary for purposes of manipulation and control. In those absurd situations where private events are said to control behavior even though they are not themselves directly observable even to a single observer, as in much of Freud and in certain uses of the term *self*, then the explanation is of interest only as a sample of very complexly constructed verbal behavior.

However, the preference for environmental explanation does not mean that it is the only meaningful form of explanation. The radical behaviorist makes no prior epistemological assumption that an explanation is complete only when environmental controlling variables have been identified. Many patterns of verbal

behavior pass as successful explanation to many people, and indeed common practices of explanation provide an interesting area for empirical investigation. The complex verbal material that composes psychological theory is not to be abhorred on principle. The radical behaviorist simply calls attention, again, to the fact that psychological theory is after all directly observed as verbal behavior on the part of the theorist, and it seems good advice to suggest that the theorist at least attempt to understand the factors that operate to make him generate his theory in the way that he does. Even a fairly casual inspection of most of the verbal material that is considered by many to be psychological theory can be seen to manifest conspicuous control by ordinary language habits, extensive chains of familiar intraverbals, and one or another preconception about the inherent nature of scientific explanation.

#### RADICAL BEHAVIORISM AND PHENOMENOLOGICAL RESEARCH

It is probably already clear that there is no inherent incompatibility between a robust interest in phenomenology and the basic dimensions of radical behaviorism. Koch himself has stated the terms that must be met before a harmonious coexistence between behaviorism and phenomenology is possible. He argues that psychology has to be a "conceptually heterogeneous" science. He would "no longer have any objection to neo-neobehaviorists" if "even the most libertarian" among them would "permit" such conceptual heterogeneity (p. 186). Clearly these terms are met by radical behaviorism. Any kind of professional language, no matter how esoteric, is of interest to the radical behaviorist as a sample of verbal behavior. What he wants to know is what sort of factors have been involved in leading the speaker to say what he does. Still, it seems natural to protest that when professional psychological talk is viewed simply as a sample of verbal behavior, what the psychologist is really trying to say has not been taken very seriously. Yet such a reaction reflects a misunderstanding of how earnestly the radical behaviorist is prepared to try to understand whatever factors control the emission of any interesting psychological talk. To know thoroughly what has caused a man to say

something is to understand the significance of what he has said in its very deepest sense.

It is true that Skinner has not rushed to embrace with star-eyed enthusiasm the sentimental, emotional, common-sensical, or obscure outpourings that often pass as pleas for phenomenology in psychology. Why should he? Skinner has his own row to hoe in attempting to advance an explicit interest in the analysis of behavioral control, which is what many psychologists are basically interested in anyway. He does this in the face of a frankly embarrassing professional shallowness in the interpretation of his work. The popular "Skinnerian" myth that any concern with private experience must be "mentalistic" and is hence unbecoming to the radical behaviorist simply flies in the face of some of Skinner's best thought. Consider the following quotation from Skinner's contribution to the Symposium on Operationism.

"Science does not consider private data," says Boring. (Just where this leaves my contribution to the present symposium, I do not like to reflect.) But I contend that my toothache is just as physical as my typewriter, though not public, and I see no reason why an objective and operational science cannot consider the processes through which a vocabulary descriptive of a toothache is acquired and maintained. The irony of it is that, while Boring must confine himself to an account of my external behavior, I am still interested in what might be called Boring-from-within. (1945, p. 294)

In the Rice symposium only Scriven seems really to appreciate what Skinner is basically up to, and Scriven is a philosopher. Both Rogers (p. 140) and Koch (p. 186) consider Skinner's verbal behavior as in some way inherently intolerant. But a specialization of interest does not imply intolerance, nor is intolerance implied by a decreasing interest in verbal behavior as its control by observable events becomes more hopelessly obscure.

The radical behaviorist understandably reacts slowly to phenomenological talk that is to some extent too distantly removed from the direct observations that have made the speaker excited to begin with. Even MacLeod, a phenomenologist to whom the radical be-

haviorist can look squarely with respect, remarks, "To be quite frank, I must confess that I find Heidegger as deadly as Hegel and that many existentialist plays leave me simply uncomfortable" (p. 51). It is easiest, for example, simply to try to swim along with Rogers through the first page of his paper for the Rice symposium, where the composition is heavily controlled by a torpid oceanic metaphor. "Like the flotsam and jetsam which float on each ocean current, certain words and phrases identify, even though they do not define, these separate flowing trends" (p. 109). Or, "Toward what shores, what islands, what vastnesses of the deep is its compelling current carrying us" (p. 100)? With respect to the second of these quotations, it could in fact be helpful, in trying to get an idea of what Rogers must have seen in his relevant therapeutic experience, to know to what extent and in what way the composition of this particular statement of his was under partial control of "The Tyger" by Blake.

However, it is more the purpose of this paper to clarify the possibilities of active reconciliation, rather than mere peaceful co-existence. There are numerous ways in which a flourishing phenomenology and radical behaviorism need each other. Consider first the case of radical behaviorism, whose current situation presses for the more explicit study of phenomenological functional relationships. Skinner's analyses of obviously phenomenological subject matter, as his chapter on "Private Events in a Natural Science" in *Science and Human Behavior* (1953, Ch. 17), or the paper on operationism (1945), or his contribution to the Rice symposium (1964), are clearly under the control of considerable self-observation on the part of Skinner himself. It is not that what Skinner has to say in this material needs "experimental test". What is needed is extensive descriptive analysis of verbal behavior controlled by observable events that are likely to be identified by the speaker as his own conscious experience, his inner subjective feelings, or his private hopes, fears, and aspirations. Without such a behavioral analysis, coverage of the obviously interesting aspects of human functioning will remain incomplete. Perhaps one need not expect to find in the analysis of phenomenological verbal behavior important causes of the social and personal-adjustmental behavior so much of current in-

terest. However, the careful description of such functional relations can be expected to have an ameliorative influence upon the extent to which inner mental processes are called upon in the explanation of behavior. A sound phenomenology is the best defense against a facile mentalism.

Similarly, radical behaviorism needs to attend to other, more complex aspects of behavior which are coming to be identified as phenomenological in a looser usage of the terms. There is great current interest in questions pertaining to the value, meaningfulness, and significance of a person's experience, and this interest is increasing. Except for want of time, there is no reason for the radical behaviorist to neglect to analyze the complex behavioral situations that are taken to be the signs of these broader phenomenological concerns. In making sense out of this behavior, he has no other recourse than to put himself in a position to make the same kind of observation—often clinical, literary, social, religious, or aesthetic in nature—that gives rise to such phenomenological talk. Extensive observation of the extraordinarily wide range of human functioning, from the contemplative behavior of the mystic through the puzzling behavior of the leather-jacketed cyclist, is urgently needed. How can we expect a viable psychology, when we find that so many psychologists are themselves simply rather narrowly experienced people? At the present time, radical behaviorism rapidly advances to the study of higher intellectual functioning in education, the rehabilitation of juvenile delinquents and criminals, the appreciation of music and art, and the behavior of psychotics and people who seek psychotherapy. In all this, the simple observation and description of the relevant behavior, conspicuously including what the persons involved have to say themselves, becomes of increasing importance. The current work of Murray Sidman (see, *e.g.*, Sidman and Stoddard, 1966) on the rehabilitation of mentally defective children is exemplary of the way in which a radical behaviorist can profitably take advantage many times of analyses which might easily be called phenomenological in nature.

As for the phenomenologist, he is for the most part still grossly unaware of the first lessons to be learned from the experimental analysis of behavior. The ways in which

radical behaviorism impinges upon the domain of phenomenology have been delineated to a great extent by Skinner in the works referred to above. This paper has also attempted to clarify the radical behaviorist point of view so that its relevance to phenomenological interests can be appreciated. However, it should perhaps be stressed again, by way of summary, that insofar as the phenomenologist is in any way in contact with human functioning, he is looking at, and responding to, *behavior*, even though it may not be public in nature. Much of both his own behavior and that in which he is interested is under complex control, a control that is likely to be to a considerable extent environmental in nature. The phenomenologist needs greatly to recognize that a little less metaphor and theory, and a lot more simple description of the things that he has actually observed, would be of much help to others in understanding the problems he faces. For example, it is relatively exciting to learn that Rogers suspects some relationship between a patient's "willingness to discover new feelings and new aspects of himself" and "personality change". Yet it is not so interesting to watch him then go out and get a small gun from the arsenal of statistics and victoriously reject the notion of precisely zero correlation between some measure of self-exploration and another measure of personality change (p. 124). Here the perceived functional relation is directly of interest in itself. It is enough that it control verbal behavior. There is no need to use statistics to bolster one's self-confidence or to justify one's right to talk about behavior. The practical value of statistics lies elsewhere than in providing a simple criterion for deciding whether or not one knows anything about behavior.

The phenomenologist should be especially wary of the ways in which his previous experience acts to influence however he happens to talk, particularly in constructing theories, planning research, and reaching explanatory conclusions. He should, at least to some extent, attempt to assess the variables in his own history which affect his professional performance in any of its significant facets. When he places heavy emphasis on a word taken from the lay vocabulary, he must not fail to examine carefully the observable events which control his usage of the term and guarantee its successful effect upon the behavior of others.

He should satisfy himself, in making use of a psychological test, that he really wants to regard the elicitation of specific samples of behavior as the "measurement" of some inner "thing", supposedly in part an element of the psychic apparatus. If he happens to prefer practices of explanation in which powers or forces are attributed to entities of any kind, he should try to form some notion of how he has come to acquire this particular preference. In the study of complex mental processes he must be sure he had observable evidence to help him decide what is chicken and what is egg, the prior environment or some inner mental entity presumed to influence behavior. When a mental process is purported to exist, he must know clearly in his own mind precisely to what extent the process is observable, *directly* observable, at least to someone.

Finally, the phenomenologist should recognize that he engages blindly in efforts at manipulation unless he is clearly aware of the pertinent relations between the environment (including his own behavior) and the behavioral change he is interested in making. He should keep in mind that an environmentalistic frontal attack on any problem of control may be considerably more profitable than recourse to verbal interpretations involving mental states. The best way to change a mental condition may be to try to change other, more conspicuous aspects of behavior first; the desired changes in covert behavior may occur as a result. For example, it is one thing to get a subject to respond on a paper-and-pencil test and quite something else again to know what the same Caucasian subject will feel when he first learns that his children will soon have to attend a predominantly non-white school. A particular manipulation may well succeed for rather trivial reasons in changing performance on a paper-and-pencil test; yet clearly much more complex problems of control are involved in shaping vigorous support for integration in the public school system. It is entirely possible that the best way to get a person to feel comfortable inside himself about issues of civil liberties is to attempt *first* to control his overt behavior in relevant social situations. One would possibly then find that the observable covert or overt behaviors taken as evidence of his attitudes on the subject have also changed.

#### PRACTICAL PROBLEMS IN RECONCILIATION

In closing, let me mention four problems that seem to me to stand particularly in the way of a healthy interaction between radical behaviorism and phenomenology. The first of these is the superficiality with which the profession at large is familiar with Skinner's work. A couple of survey courses in learning theory, possibly a reading of *The Behavior of Organisms*, a glance at *Science and Human Behavior* or *Walden Two*, the isolated study of such papers as "Are Theories of Learning Necessary?", or primary reliance on such digests as Hilgard and Bower's *Theories of Learning* (1966) will not do. If radical behaviorism is to be understood, Skinner's work must be studied by professionals with precisely the same diligence as that we take for granted from our better graduate students. In particular, the paper on operationism (1945), *Verbal Behavior* (1957), the later papers on programmed instruction (*e.g.*, 1961), and the paper for the Rice symposium (1964) must be *mastered*. By way of help, Verplanck's critique of Skinner's views in *Modern Learning Theory* (1954) can be taken as authoritative, in spite of the fact that it is now very much out of date.

The lack of careful study of Skinner's work has led to professional absurdities too numerous to review in detail. Strange blends of Skinner and conventional behaviorism abound. I would 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 their 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 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.

A second problem is the failure to distinguish sharply enough between radical and conventional behaviorism, a point much emphasized in this paper. Operationism and the logical positivism of the 1930's operate to influence both points of view but in markedly different ways. Skinner came quickly to detest logical positivism. In the Rice symposium, one would have expected Koch to focus his intelligence on radical rather than on conventional behaviorism, since it was Skinner who was obviously intended to champion the most vigorous practice of behaviorism at the time. It is much to be hoped that Koch will not long delay in accepting the challenge of a critique of radical behaviorism. The radical behaviorist cannot look profitably for relevant criticism to the painstaking review of *Verbal Behavior* made by Chomsky (1959). Chomsky writes under the misconception that Skinner's work is more or less another form of conventional behaviorism dominated by logical positivism in the usual way. The same misconception leads Malcolm in the Rice symposium (p. 144f.) to try to clarify the philosophical implications of Skinner's views by extensive quotations from Carnap and Hempel. We can similarly excuse Malcolm's unfortunate remark that, "In his brilliant review . . . of Skinner's *Verbal Behavior*, Noam Chomsky shows conclusively . . . that Skinner fails to make a case for his belief that 'functional analysis' is able to deal with verbal behavior" (p. 154). The philosophical character of Skinner's work is considerably closer to what is often now called "analytic philosophy" or "ordinary language analysis" than to the narrow forms of logical positivism that have influenced psychological thinking to so great an extent. Koch (p. 23) suspects that behaviorism might try to look to analytic philosophy for its defense, and Scriven (p. 179) associates Skinner's analysis of the language of private experience with the central problem of the *Philosophical Investigations* of Ludwig Wittgenstein (1953). Wittgenstein's later work forms the basis of much of the force of analytic philosophy, and a number of similarities between his views and Skinner's are pointed out in a paper now in press (Day, 1969). The student of radical behaviorism is well ad-

vised to read the *Philosophical Investigations* as an antidote to the inroads of logical positivism upon psychology. It would also make the reading of Skinner's work somewhat easier.

A third and related problem faced in the effective reconciliation of radical behaviorism and phenomenology is the lack of training among psychologists in contemporary philosophy. The traces of an Oedipal resentment of philosophy by psychology still persist, much to the detriment of effective self-criticism among psychologists. Unfortunate preconceptions about the nature of science are currently under considerable philosophical attack, and this theme runs throughout much of Koch's paper for the Rice symposium. There is presently a new and vigorous excitement about phenomenology in professional philosophy. The relevance of these philosophical interests to practical problems in psychology is coming increasingly to be appreciated. Witness the birth of the new division of philosophical psychology in the American Psychological Association. Scriven's lengthy paper for the Rice symposium is essentially philosophical in nature, yet it will afford an eye-opening experience for any psychologist who takes the time to give it careful study. In two pages Scriven takes the trouble to list no less than 13 "other philosophical topics that have been deeply involved . . . in the course of the symposium" (p. 178f.). Yet how many psychologists have learned simply to turn a philosopher off when he starts to say something they do not like to hear? In the discussion following his paper, Scriven bluntly comments that "pure research in social psychology is among the most unproductive fields of human endeavor today" (p. 190). In another context he notes that "steaming somebody up to think that the only way to do psychology is via phenomenology and that all behaviorists are wicked may well turn out to be a good way of getting him to do something worthwhile, but it is certainly an unfortunate comment on psychology if psychologists need to do this. Do we *have* to feed ourselves fibs as fuels for our forward movement" (p. 177)? How many of us psychologists are sufficiently prepared to understand Scriven when he talks?

Finally, a fourth problem lies in certain practical difficulties faced in attempting to carry out explicitly descriptive research. These difficulties stem from the complex set of pro-

fessional practices that define what is, and what is not, acceptable as psychological research. A simple interest in behavior no longer suffices to lead a man to try to make the relevant observations. He must first justify, often with hypocrisy, and inevitably with great caution, whatever interest in behavior he may have. The justification of research must walk the razor's edge of sufficient but not too much similarity to the research activities of other scientists. What is the hypothesis tested? What possible outcomes are anticipated? Except in those areas where we already have considerable knowledge and consequently need research the least, how can a man be expected to know what he may find in his research? Conspicuously exploratory research is frowned upon; it is tolerated only when the researcher's competence is buttressed by a formidable list of publications, often in some picked-over area. To remonstrate that the very canons of the respected establishment are under attack is to no avail. Who would suspect that simply by looking carefully at whatever one is interested in, no matter how complex the behavior may be, and by trying to push it around a bit, one can come to know a great deal more about the subject?

The profession greatly needs a lot more writing that consists of little more than careful description of what is actually observed by psychologists. One is understandably anxious in research to go beyond the stage of mere description to the statement of significant conclusions concerning behavioral control. It is understandable also that conclusions, rather than descriptions, find themselves the prized commodity in the market of publication. However, conclusions are specimens of verbal behavior that involve a very complex kind of control. Equally profitable in the control of productive professional behavior are the direct observations that presumably govern the published conclusions. The need for simple descriptions of observed behavior is especially great in precisely those areas of psychology where clear-cut conclusions are difficult to draw. Although someone is able to make only the most tentative conclusions—here, of course, he is conditioned to hesitate even to speak, much less to publish—the professional community still needs the benefit of his experience. Similarly, the man who looks but fails to find at all must nonetheless be encouraged to report what he has seen. Without publication, the

possibility that his observations may have some more fortunate effect on the behavior of someone else is lost. Unless the beginning psychologist can rely on having access to the direct observations of others, he must face the bleak prospect of a career in research that even he himself may view as trivial, or he must undertake the uphill fight of analyzing behavior essentially by himself, alone.

In short, the hearty interaction of radical behaviorism and phenomenology is hindered only by one or another form of narrowness of outlook. With a deeper and wider scholarship on the part of the interested psychologist, with a considerably enlarged familiarity on his part with the rich spectrum of human activities, and with a greater freedom to make the observations upon which a broader understanding of behavior must depend, then the inherent liberalism of radical behaviorism can find successful reconciliation with the libertarian aspirations of phenomenology.

#### REFERENCES

- Chomsky, N. Review of Skinner's *Verbal Behavior*. *Language*, 1959, 35, 26-58.
- 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 (in press).
- Hilgard, E. R. and Bower, G. H. *Theories of learning*. (3rd ed.) New York: Appleton-Century-Crofts, 1956.
- Sidman, M. and Stoddard, L. T. Programming perception and learning for retarded children. *International Review of Research in Mental Retardation*, 1966, 2, 151-208.
- 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. *Verbal behavior*. New York: Appleton-Century-Crofts, 1957.
- Skinner, B. F. Why we need teaching machines. *Harvard Educational Review*, 1961, 31, 377-398.
- Skinner, B. F. Behaviorism at fifty. *Science*, 1963, 140, 951-958, and in T. W. Wann (Ed.), *Behaviorism and phenomenology*. Chicago: University of Chicago Press, 1964. Pp. 79-97.
- Verplanck, W. S. In W. K. Estes, S. Koch, K. MacCorquodale, P. E. Meehl, C. G. Mueller, Jr., W. N. Schoenfeld, and W. S. Verplanck, *Modern learning theory*. New York: Appleton-Century-Crofts, 1954. Pp. 267-316.
- Wann, T. W. (Ed.) *Behaviorism and phenomenology*. Chicago: University of Chicago Press, 1964.
- Wittgenstein, L. *Philosophical investigations*. New York: Macmillan, 1953.