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Mentalism, Behavior-Behavior Relations, and a Behavior-Analytic View of the Purposes of Science

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In a behavioral view, the purposes of science are primarily prediction and control. To the extent that a scientist embraces both of these as a unified and generally applicable criterion for science, certain philosophical and theoretical practices are counterproductive, including mentalism in both its metaphysical and metatheoretical forms. It is possible and often worthwhile to recast some mentalistic talk into an issue of behavior-behavior relations. When behavior-behavior relations are approached non-mechanistically, however, analysis cannot stop at the level of the relations themselves. Several analytic concepts common in the behavioral community share some of the dangers of mentalism if not employed properly, including such concepts as self-reinforcement, response-produced stimulation, and self-rules.

Criticism of a behavioral approach to human behavior has been frequent since its inception. Recently, a type of criticism has emerged from knowledgeable critics suggesting that there is a more rapid path to the kinds of scientific knowledge sought by behaviorists (e.g., Keat, 1972; Wessells, 1981, 1982). Even individuals who formerly have been sympathetic to a behavior-analytic position have embraced this line of criticism. For example, Killeen has suggested that we need to "restore the excitement" in our field by admitting mentalism (Killeen, 1984).

Behavorists would enthusiastically embrace this suggestion if the alternatives being proposed (e.g., Killeen's "emergent behaviorism," 1984) advanced the goals of science as seen by behavior analysts. Sadly, that possibility does not appear likely. Instead, the critics seem to be proposing a kind of science that is ill-suited to the scientific ends sought by behavior analysts. Although the criticisms may seem to be about scientific strategies or tactics, in actuality they concern the behavior-analytic view of scientific explanation itself.

The nature of the recent criticism suggests that it may be worthwhile to reexamine some of the philosophical underpinnings of a behavior-analytic view of science. Many of the points we hope to make have been made elsewhere, but the discussions have often been directed toward other specific concerns and have not always been interconnected or given a comprehensive rationale. Keystones of a behavior-analytic position can then appear to be dogmatic or arbitrary, rather than required for the intellectual integrity of the position.

Our starting point will be the primary purposes of science from a behavior-analytic viewpoint—prediction and control. We will attempt to show that an emphasis on prediction and control is not arbitrary in behavior analysis because it is a *necessary* part of successful forms of the philosophy that underlies behavior-analytic theorizing. We will examine mentalism from several vantage points and show that regardless of its form, mentalism is necessarily counterproductive to the purposes of science embraced by behavior analysts. Mentalism can, however, contribute to the purposes of science as seen from other perspectives. We will examine a behavioral translation of some types of mental phenomena in terms of behavior-behavior relations, but warn against uses of the translation that also interfere with the accomplishment of prediction and control. We will then briefly examine several concepts within a behavioral perspective that are on a

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slippery slope to the same problem created by mentalism.

THE PURPOSES OF SCIENCE: PREDICTION AND CONTROL

When differences in goals are made evident, many arguments seem to dissolve. The goals of science from a behavior-analytic viewpoint have been quite explicitly stated: "We undertake to predict and control the behavior of the individual organism. This is our 'dependent variable'—the effect for which we are to find the cause" (Skinner, 1953, p. 35). It seems only fair to evaluate a position with respect to the goals it sets for itself, while recognizing of course that other purposes might be well served by different positions.

The Emphasis on Control

Prediction and control are the primary goals of behavior analysis (the goals of interpretation and explanation are discussed later). The behavioral approach places emphasis on the words "and control" in the phrase "prediction and control." Behavior analysis has sought an explanation of behavior in terms of events that are of a kind that at least potentially allows *both* prediction and control simultaneously. As we discuss below, some kinds of descriptions of events and relations can *in principle* only directly produce successful prediction and not control. Other kinds of descriptions of events and relations can allow both, in principle, though of course for practical reasons control may presently be impossible. Behavior analysis is committed to emphasizing this latter kind of analysis.

The importance of prediction and control as a guide to behavior-analytic theorizing cannot be overemphasized. It is the key to understanding many behavioral positions that might otherwise appear to be arbitrary. For example, behavior-analytic accounts of behavior are always ultimately to be cast in terms of "environmental variables" or "external variables": "Our 'independent variables'—the causes of behavior—are the external conditions of which behavior is a function" (Skinner, 1953, p. 35). This position is not based on an a priori dictum that only what are commonly called "external variables" can possibly influence behavior. In a sense, the flow is in the opposite direction. If an event can in principle directly allow both prediction and control of behavior, then it deserves the name "environmental variable" or "external variable." This relation can be shown by the fact that, in behavior analysis, the words "external" or "environmental" do not always refer to the world outside the skin: rather they refer to the world outside behavior (most of which, of course, is outside the skin). For example, it is sometimes useful ("useful" in terms of prediction and control) to think of an "internal environment" influencing behavior.

An emphasis on "external variables" comes from the goals of science as viewed by behavior analysis. Seeking both prediction and control puts certain constraints on the kinds of statements of relations that are useful for the scientist. Only statements that point to events external to the behavior of the individual organism being studied can *directly* lead to prediction and control. The logic of this claim is as follows. Scientific statements, as Skinner pointed out, are not themselves the causes of the phenomena they encompass. Bodies do not fall, for example, because of the law of gravity. Rather humans can bring their behavior under the control of this verbal statement of a relation (i.e., this rule) and achieve certain ends. Thus, scientific rules help us accomplish particular ends by describing contingencies. They are rules for scientists, not rules for the world. Thus, scientists who seek prediction *and* control must rely on rules that start with the environment, in the sense of the "world outside of the behavior," because scientists are, and can only ever be, in other organisms' environment in this sense. Scientists cannot directly use a rule to control behavior unless it starts where the scientist is—in the potentially manipulable world outside of the behavioral system. If a scientific statement is used by the scientist to control phenomena but

does not itself start from where the scientist resides there must be some unspecified and unanalyzed link between the statement and the scientist's behavior. It may be useful, but it is necessarily incomplete in the sense that the rule itself did not specify whatever was done to control the phenomena of interest.

Why Prediction Must Be Included and Control Emphasized

Much of the confusion over a behavior-analytic perspective on the goals of science would have vanished had control alone been emphasized as the defining property of science. To the extent that control is pursued, it forces us to emphasize external variables that are functionally related to the behavior. Indeed, if the critics of a behavioral position were right and the emphasis on control represented merely an emphasis on technological advancement, there would have been no reason not to emphasize control alone as the issue. The criticism is a fairly common one. For example, Wessells (1981) objects to an emphasis on control, saying "the kinds of predictions one aims to make need not be dictated by the pragmatic desire to change behavior for the better" (p. 161). The concern over control, however, is not primarily with the development of technology, though the technological outgrowth of behavior analysis is impressive and a legitimate source of intellectual support for the position. At a theoretical level, the concern is primarily with the completeness of the account according to the scientific goals embraced by behavior analysis. A behavior-analytic position on the goals of science in turn derives its dignity from the necessary relation between these particular goals and the overall philosophical integrity of the world view represented by behavior analysis (cf. Reese, 1984).

Behavior analysis is based on a pervasive use of a particular explanatory model: the "act in context." In behavior analysis, any event is to be understood and even defined through a contextual analysis. The three-term contingency of radical behaviorism is a dynamic spatiotemporal contextual unit—none of the terms can be defined independently of any of the others. Radical behaviorism is so thoroughgoing in its attempt to analyze context that even the behavior of scientists as they conduct contextual analyses is to be understood through more contextual analyses (Skinner, 1945).

The explanatory model of the "act in context" is shared by many perspectives on behavior, from certain forms of evolutionary biology (e.g., Dawkins, 1982), to some types of cultural anthropology (e.g., Harris, 1979), to Marxism. Because a basic explanatory model (or "root metaphor") is at the core of any well-integrated and consistent world view, Pepper (1942) has suggested that all perspectives that rely on the "act in context" as an explanatory model be thought of as types of the world view he calls "contextualism." According to Pepper (1942), the underlying "truth criterion" of contextualism is "successful working" or pragmatism (Pepper, 1942). A term, concept, or statement of a relation is not true or false simply according to public agreement about the correspondence between it and other events, but according to the impact that the use of the term, concept, or statement has on dealing successfully with the phenomena of interest. Radical behaviorism clearly encompasses such a view (e.g., Skinner, 1945).

The nature of contextualism and of other world views, in Pepper's fullest sense, is a complicated topic. It would take us beyond the scope of this paper to defend fully the claim that behavior analysis fits all of Pepper's defining characteristics for contextualism (but see Hayes & Reese, in press). For our purposes, we will refer to behavior analysis as a type of contextualism only in the limited and uncontroversial sense of a view of the world consistently based on an analysis of the spatiotemporal context of acts, and driven by a pragmatic theory of truth.

The problem with the "act in context" as an explanatory model is that it does not and cannot specify the scope of the act or the context. Context can proceed outward spatially to include all of the uni-

verse. Context can proceed backward in time infinitely to include the remotest antecedent, or forward in time to include the most delayed consequence. The "act" in question can vary from the finest muscle twitch to the most elaborate and extended behavioral sequence. Consequently, in behavior analysis, an operant can be of almost any size and, in principle, can be influenced by contingencies that are extremely remote or indirect. Under such circumstances, one might ask how we are to know that a particular contextual analysis (in behavior analysis, a particular "contingency analysis") is adequate?

"Successful working" provides an answer to this question. An analysis need proceed only to the point at which successful action can be based on it. Successful action confirms the value of the analysis—it changes the indeterminent units of contingency analysis in the abstract into the determined units of a given instance of successful contingency analysis. In behavior analysis, the name for such successful working is "prediction and control." We know that a given event should be called a discriminative stimulus, for example, because the effects of doing so are that we can deal more successfully with the behavior of interest. While prediction alone provides some confirmation of the value of an analysis, only control "proves" that the units we have selected (the divisions we have made in context and behavior) are valid. One could ask, for example, how do we know that *this* is the relevant stimulus for *this* behavior? The answer is of the general form that when we change *this* stimulus (and not *that* stimulus), we get a change in *this* behavior (and not *that* behavior). Without manipulation, the units we select could be completely mistaken. Because the explanatory model in behavior analysis does not and cannot specify the precise nature of these units a priori, this would present a grave problem. Without manipulation, we could guess, for example, that event X is a discriminative stimulus, but instead it could be part of event Y and not a separate event at all, or it could covary with behavior due to coincidence, or the wrong construction could have been placed on the proper units of behavior and thus on its antecedents, and so on. It is primarily the confirmation of the validity of the units of the analysis that requires that control be emphasized in successful forms of contextualism.

An additional problem in behavior analysis (or any contextualistic perspective) is that even the behavior of a scientist analyzing behavior is itself behavior that can be analyzed. Contextual analysis can easily be paralyzed, however, by the need to analyze context, and then to analyze the context of the analysis of context, and so on *ad infinitum.* Prediction and control both confirm the value of the analysis and provide an endpoint to the need for analysis—it need continue only to the point at which prediction and control is possible in principle. Without control as an endpoint, behavior analysis could be caught up in an intellectual whirlpool of infinite regressions of ever more massive contextual analyses and would lose contact with "successful working" as its truth criterion. The argument can be made that the relative lack of empirical work springing from J. R. Kantor's extremely contextualistic view of psychology shows what can happen if contextualists let go of control as a primary goal of science. It is the successful operation of contextualism that pragmatically requires that control be emphasized—it is not a postulate of the philosophy.

If we have explained why control must be emphasized as a goal of science in behavior analysis, why should prediction be included at all as a goal? There are two reasons. First, prediction is pragmatically useful and thus naturally part of the truth criterion of contextualism. It would be arbitrary to exclude it. Second, control cannot be the only proximate outcome of successful science, because technical limitations often exist on our ability to manipulate events. Predictive relationships are not *by their nature* incomplete when prediction is based upon variables that are in the domain in which the potential for control exists because *in prin-*

ciple they can provide a complete account. They take us outside the behavioral system. Whether they in fact do provide a complete account in a specific instance can be assessed as additional techniques allow direct manipulation and as additional knowledge creates networks of mutually supportive observations.

Critics of the goals of science embraced by behavior analysis have often misunderstood the basis for these goals and the integral role they play in behavior analysis. It is to such criticisms that we now turn.

THE BEHAVIOR-ANALYTIC VIEW OF EXPLANATION

Wessells (1981) has objected to the criterion of control by pointing out that "behavioral control can be achieved in the absence of explanation and vice versa" (p. 161). Astronomy is a commonly used example (Martin, 1978; Wessells, 1981) because we cannot test astronomical principles based on our ability to control cosmic events. The literal meaning of explanation comes from the same root as the word "plane"—literally, explanation means to lay out flat before us. According to the behavior-analytic position, "explanation" ultimately refers to prediction and control with adequate scope and precision. Thus, seen from the standpoint of behavior analysis, Wessell's statement is at least partially incorrect. We cannot have control in a sophisticated manner without what behaviorists take to be "explanation." With thorough and general control must also come prediction, and to a behaviorist, prediction and control of sufficient scope and precision *is* "explanation"—it is how behaviorists "lay out" behavioral systems. Control that is limited in scope and precision does not, of course, justify the term "explanation" and in that sense Wessell's observation seems obviously valid. The observation that pointing a gun at people and saying "Your money or your life" will often control monetary exchange is hardly an explanation of charitable giving. Such coercion fails as an explanation for monetary contributions in general because it has insufficient scope: a wide variety of instances are not encompassed by the "explanation."

Behavior analysts also agree that at times science can have explanation without control. Skinner uses the term "interpretation" to describe such cases. It occurs when a system can be thoroughly described, but for technical reasons control is not possible. Even then, behaviorists insist that an adequate explanation or interpretation be based on events that are *of the same kind* as those permitting control. As we have emphasized above, this insistence is not an arbitrary component of behavior analysis. It is also consistent with the strategy of the natural sciences under such circumstances. Modern astronomy, for example, is based fundamentally on the controlled observations of the physicist, and the observations are of the same kind. Someday we may even manipulate cosmic events. The limitations that occur in astronomy are technical, not problems with the kinds of events themselves.

The Mechanistic View of Explanation

This view of explanation can be contrasted with others that are popular in psychology. To put the best face on it from the point of view of behavior analysis, some forms of cognitivism are usefully considered as efforts to describe the structure of behavior (Catania, 1973). A description of the structure of behavior, however, is a description of the phenomena to be explained. Knowing the phenomena to be explained is vital, but it must not be mistaken for a contextual explanation of these very phenomena. The distinction is critical because if behavioral events are orderly, they themselves can allow prediction of behavioral phenomena, without ever going outside the particular behavioral system. This is not adequate behavior analytic explanation even though in a more limited sense it clearly can be useful. No amount of description of behavioral events will directly provide for control of behavioral events in the same individual. To think otherwise is to make the "structuralist

error" (Skinner, 1974). We cannot control the behavior of an individual without considering events other than the behavior of that individual.

Thus, the cognitivist agenda does not necessarily meet the goals of science according to behavior analysis. Cognitivists themselves have no quarrel with this point: "Most [cognitive] investigators agree that their current theories are descriptive and that an explanatory theory, which would accommodate the effects of the environment, is a distal goal" (Wessells, 1982, p. 77). Just as it is unfair, however, to criticize behavior analysis without considering its goals and underlying philosophy, so too it is unfair to criticize cognitive perspectives in the same manner. Cognitivists do not believe that they are confusing description and explanation. Nor do they believe that the lack of concern for control is a weakness. They are correct in both instances, but only in the context of their own underlying philosophical position.

Most, though not all, forms of a cognitive account of behavior seem to be based on the world view of mechanism (cf. Pepper, 1942). The fundamental explanatory model of mechanism is that of the machine. The type of cognitive theorizing that is based on computer metaphors and uses computer simulations to test the adequacy and operating characteristics of various theoretical models is obviously mechanistic in this sense. We will limit our discussion of cognitivism to this mechanistic variety. (Some forms of behaviorism are also mechanistic, but not radical behaviorism. A detailed discussion of this is beyond the scope of this paper, but see Hayes & Reese, in press.) In accord with its explanatory model, a mechanist shows no hesitation in explaining a behavioral system by specifying the component parts of its structure and the nature of its orderly operation; just as a person examining a car would readily explain its action by an appeal to its component parts (e.g., pistons, spark plugs) and structural organization (e.g., the spark plug wire is connected to the spark plug). In mechanism, each part can be described independently of the others and the nature of the parts does not change when they are combined into systems. The conditions that gave rise to this structure or the ways we can manipulate it are irrelevant to the description of the operation of the machine and predictions based on this description.

Mechanism has a correspondence-based truth criterion (Pepper, 1942). A term, concept, or statement of relation is true to the extent that we can agree that it corresponds with events. This presents particular restrictions to mechanistic theories. It would be improper to develop a description of a system based on close contact with it and then to treat this as an adequate theory of the system because obviously any detailed and careful description of events will be in close correspondence with these very same events. Correspondence can be used to test the adequacy of theories or concepts only by applying them to *new* situations, that is, by deductive prediction. The more abstract the relation between the original situation, the theory, and the new situation, the better (e.g., see Ericsson & Simon, 1984, on this point). When we can deductively predict with sufficient precision and scope, we have "explained" the phenomena according to a mechanistic account. The emphasis on hypothetico-deductive theorizing that exists in all mechanistic perspectives (e.g., most forms of cognitivism and S-R theory) is not arbitrary; it is integral to the functioning of the underlying world view.

Thus, for mechanists, description and theoretical prediction form an adequate basis for explanation; in contrast, contextualists emphasize prediction and control. These differences about adequate scientific explanation correspond with differences about the goals of science according to the underlying world view. Neither can be said to be "right" or "wrong" because any evaluation we can make about the adequacy of scientific goals must itself be in terms of some set of underlying values or goals. From the point of view of contextualism, all we can say is that these two views of science may be more or less useful for accomplishing particular ends, or that they might have

this or that effect on the culture that supports them. Most behaviorists probably believe, of course, that a contextualistic perspective has positive consequences for scientific subcultures and perhaps the majority culture. Evaluating truth on the basis of consequences, however, is itself a distinctly contextualistic line to take. Few behaviorists would argue that behavior analysis is leading to better and better hypothetico-deductive theories. For a mechanist, this may show the inadequacy of behavior analysis. For a contextualist, it shows no such thing.

To summarize our main points so far: Prediction and control are reasonable goals for science. Together, they allow behavior analysis to progress efficiently, providing an adequate basis for scientific explanation according to a contextualistic perspective. Unlike prediction, control requires that we go outside of the behavioral system itself. Sometimes even when we do, control will be technically impossible. For the sake of a complete account, however, it is necessary that we explain behavior in terms of events that in principle allow both prediction and control. This requirement is not due primarily to a concern with technology. Rather, it is necessary for the successful working of the explanatory system used in behavior analysis. Other explanatory systems imply other kinds of scientific goals, and alternative practices can be defended on those grounds.

MENTALISM

An arena in which to examine the implications of the preceding analysis is the study of private events. Indeed, many of the objections to behavior analysis are usually cast in terms of private events and their investigation. The dominant alternative to a behavioral analysis of private events is mentalism. Objections to mentalism can be placed in two basic categories: metaphysical objections and metatheoretical objections. The metaphysical objections are directed at the concept of literal dualism and are shared by virtually all scientific perspectives. The metatheoretical objections have to do with the incompleteness of the account mentalism engenders when measured in terms of the goals of prediction and control. Our main point is that it is important not to confuse these two types of objections. Many of the behavior-analytic concerns with mentalism are metatheoretical objections driven by the goals of science embraced by behavior analysis, and these objections are by no means unique to the topic of mentalism. We show later that they apply to topics within behavior analysis itself.

The Scientific Unacceptability of Literal Dualism

Originally, psychology was the study of the soul. The Oxford English Dictionary (OED) defines "soul" as "the spiritual part of man." The OED defines "spirit" as an "incorporeal or immaterial being" and as a "being or intelligence distinct from anything physical." The word "physical" comes from a word for nature (thus the science of physics) and is defined as "of or pertaining to the phenomenal world of the senses; matter." Thus, if you take the words literally, "soul" or "spirit" are inherently dualistic terms because they oppose matter and nonmatter. We might label this "literal dualism." Literal dualism is the belief that there are two different essences in the world—one type exists in space and time, while the second type is nonspatiotemporal.

This type of literal dualism was to some degree transferred to the concept of "mind" and thus to its study. The OED defines "mind" as "the mental or psychical being or faculty." An elaborating definition explains that "mind" is "the seat of a person's consciousness, thoughts, volitions, and feelings; also, the incorporeal subject of the psychical faculties, the spiritual side of a human being; the soul as distinguished from the body . . . . Mental being; opposed to matter." The spiritual meaning of "mind" is also shown by the fact that God has long been referred to as "mind," as for example in the quote "That eternal infinite mind, who made and governs all things" (Locke, 1690). Thus, in lay terms, the essence of

the mind is fundamentally different from the essence of the physical unvierse. "Mind" explicitly makes contact with a cultural tradition of literal dualism.

The objection to literal dualism is twofold. First, such a view is not scientifically tenable. This objection is shared by virtually all scientists, including cognitivists. By definition, nonspatiotemporal events cannot have form, mass, acceleration, beginnings, or ends. How they can be regarded as "events" in such a case is problematic. Any event that can be discerned is being observed in the world of space and time, and must in that sense be regarded as "physical." Literal dualism also raises the impossibly difficult issue of how nonspatiotemporal events can cause physical events to occur.

This argument is not about terms. If someone wanted to call all events "mental," there could be no objection, though it would distort our normal understanding of the term. The concern is more directed at literal dualism: "What is lacking is the bold and exciting behavioristic hypothesis that what one observes and talks about is always the 'real' and 'physical' world (*or at least the 'one' world*)" (Skinner, 1945, p. 276, emphasis added). As the last phrase in this quote shows, radical behaviorism is monistic, but not physicalistic in the sense of naive realism or related perspectives.

The second major objection to literal dualism is that it leaves a gap in the domain of science. Who is to predict and control mental events and relate them to behavioral events if they are not in the purview of science? (Skinner, 1953, p. 258). This objection is a metatheoretical one, and it applies equally forcefully to mentalistic theorizing that explicitly denies dualism.

Mental Physiology

For the above reasons, virtually all scientists avoid obvious forms of literal dualism in their scientific work. A variety of linguistic practices, however, have the same metatheoretical problems as literal dualism. One such practice is the creation of a pseudophysiological analysis to replaced literal dualism (Skinner, 1969, pp. 280–284).

It is very popular to use the word "mind" to mean "brain." Television shows or magazine articles on the action of the human brain, for example, are almost always said to be about "the human mind." Cognitive psychology has made no bones about the connection. A recent authoritative text on cognitive psychology asserted that in cognitive psychology "the contemporary view [is] that mental processes are synonymous with brain processes" (Ellis & Hunt, 1983, p. 11). Study of the brain and the nervous system is, of course, worthwhile and relevant to a behavioral analysis. But it is no less troublesome simply to substitute the word "brain" for "mind" and then to engage in precisely the same kinds of analyses as before.

Cognitivists use talk of the brain for two purposes. One is simply to claim that "mental functioning is not a mysterious, nonphysical event" (Ellis & Hunt, 1983, p. 7), that is, to emphasize the rejection of literal dualism. The second reason is more subtle. As we have described above, in the kind of hypothetico-deductive theorizing naturally promoted by mechanism, there is no requirement that terms used in the analysis of events refer to other events outside the original domain of interest. In order that there can be a proper division of labor among the sciences, however, scientists must take a complete scientific account to the point at which the determinants of a given phenomenon are themselves being analyzed by other scientists.

In psychology, contextualism naturally does this because it leads to the identification of events outside the behavioral system, as we have already discussed. These events are themselves to be explained by others. Behavior analysis, for example, does not attempt to understand how operant chamber key lights work. It is enough to known the conditions under which key lights can function as stimuli. The study of electrical lights is turned over to another science. Conversely, mechanism (though it need not do so)

can leave a hole in the fabric of science because scientifically adequate analyses from this perspective can stay entirely inside the original domain. Behavioral events, or processes inferred from them, can be explained by other behavioral events, or processes inferred from them. Although philosophically permissible in mechanism, this kind of circularity is intuitively unappealling, even to cognitivists.

By appearing to study brain processes, however, cognitive psychologists can identify causes that appear to go outside of the behavioral system itself. Cognitive psychology can superficially present itself under the umbrella of the neurosciences. If we actually understood how the structure of the nervous system was influenced by events, and exactly how any current structure produced behavior, we would indeed have one type of complete account (in fact, one quite comfortable to mechanists). But studying behavioral activity called "mental" is no more inherently a study of the brain than studying any behavioral activity. In order to maintain a proper division of labor among sciences, the connection between brain and mental activity would have to be studied explicitly. It is clear that cognitivists do not actually intend to study the brain or its connection with mental activity:

> Of course, brain activity can be studied physiologically, but cognitive psychologists use a different approach. Since the brain activity of interest cannot be directly observed (for example, we have no idea what happens in the brain when a person remembers a grandmother), we must infer the existence of these processes and then describe the processes in abstract language. (Ellis & Hunt, 1983, p. 7)

In summary, mental activity can be thought of as nonspatiotemporal activity, in which case it steps outside of science altogether. It may be thought of as a brain activity, but then psychologists sometimes act as if we need not explain how the structure of this activity itself came to be. There is a third way to view "mental activity," however—as behavior.This is the view taken by radical behaviorism, to which we now turn.

Mental Activity as Private Behavior

Watson and "behavior." "Behavior" is commonly taken to refer to a certain subset of organismic action. For example, it is quite typical to hear theorists speak of "thoughts, feelings, and behavior" as if behavior can be easily distinguished from events called "thoughts" or "feelings."

Confusion over this issue can in part be attributed to Watson. Watson's (1925) behaviorism had both methodological and metaphysical components. His methodological behaviorism essentially said that scientists must be behaviorists because science can only deal with the publicly observable. Thus, even though other kinds of human action may exist, we can only deal with behavior because only behavior is publicly observable. This position might be thought of as implicitly dualistic because it recognizes that behavior is only a subset of organismic activity, and encourages a study that is necessarily incomplete since science can only deal with that subset, rather than the entire set, due to rules of proper scientific methodology.

Watson also made a second, somewhat contradictory point. He seemed to say that even if we could solve the problem privacy presents to a scientific analysis, behavior is still all that could be studied because only behavior exists. Although it is possible to read Watson to mean simply that nonspatiotemporal events do not exist, his emphasis on the peripheral locus of such phenomena as thinking (e.g., Watson, 1920, 1925) can be and was taken to mean that thoughts, feelings, and other private events are not real in their own terms. This position can be termed "Watsonian metaphysical behaviorism."

What is important to note in all of this is that the use of the word "behavior" keeps changing. In Watsonian methodological behaviorism, behavior is viewed as a subset of organismic action that is publicly observable and is therefore subject to a scientific analysis. In Watsonian metaphysical behaviorism, behavior is viewed as the totality of organismic ac-

tion, but there is the implication that only events that are publicly observable (at least potentially) should be thought of as real.

Radical behaviorism and "behavior." Radical behaviorism can be distinguished from these other types of behaviorism in part by the view it takes in regards to "behavior" and the nature of scientific observations (Skinner, 1945, 1963). As in Watsonian metaphysical behaviorism, behavior is taken to be the set of all organismic action. The word "organismic" is important. Actions by suborganismic units (e.g., a single neuron firing) are not usually considered to be the behavior of organisms, but under certain conditions they may if they can conveniently be viewed as the integrated action of a whole organism. For example, the controlled heart rate of a person in a biofeedback training program would probably be thought of as the behavior of an organism, while the beating of a heart removed from the body clearly would not.

Unlike earlier forms of behaviorism, however, radical behaviorism makes no commitment to public observability per se as the defining characteristic of scientifically valid events (Skinner, 1945). Rather, observations are scientifically valid or invalid based on the contingencies controlling these observations. Science is an enterprise that promotes the development of verbal statements of relations between events based on verifiable experience. Scientific verbal behavior thus should be under the control of the subject matter of the science and the value of this verbal behavior is determined through the impact it has on others attempting to come under the control of that same subject matter.

Science attempts to restrict sources of control over scientific observation through the scientific method. Its purpose is to ensure that scientific observations are controlled primarily by events in the relevant subject matter and not by states of deprivation, audience factors, or similar sources of control over verbal behavior (Moore, 1981). Consistent with the "successful working" truth criterion of contextualism, the scientific value of an observation in behavior analysis is ultimately determined by the degree to which it enables prediction and control.

The essence of Skinner's (1945) criticism of operationism was thus that public agreement provides no assurance of the proper sources of control over scientific observation, nor of its pragmatic value. Conversely, in principle, observations of private events can be tightly controlled by these events themselves, given the proper history, and can be highly useful. In this sense, observations of private events are no more or less scientific than public events based on their privacy per se. In radical behaviorism, behavior can thus be defined as all observable organismic action, not all publicly observable organismic action (cf. Heidbreder, 1933, and her discussion of the flaws of classical behaviorism). Skinner (1974) has made the point quite clearly:

> [Radical behaviorism] does not insist upon truth by agreement and can therefore consider events taking place in the private world within the skin. It does not call these events unobservable, and it does not dismiss them as subjective. (p. 16)

In this view, then, no objection can be made to talk of events such as thinking or feeling. This talk is not trivialized by insisting it is only the talk itself that is scientifically legitimate (Skinner, 1945). A specific instance of thinking is viewed as a scientifically accessible event– a covert behavior. We may eventually find ways of identifying specific covert behaviors in others. For example, we currently have ways of knowing when reports of private speech are in fact occasioned by the specified private speech (Hayes, 1986).

Mental Causality as Behavior-Behavior Relation

Thoughts as behavior. One might ask: Why insist that thinking be regarded as covert behavior when the physical properties appear so different from overt behavior? Why not call thinking "mental activity" or even a "brain process"? If "behavior" is defined so broadly, doesn't

this make the concept of "behavior" meaningless? Such questions have often been forcefully raised. For example:

> The omission of [mental] states left [Skinner] with an inadequate vocabulary, which he then expanded by moving some stimuli inside the organism ("private stimuli"), and by treating all other aspects of mental states as responses. Seeing became behavior, and imagination became "seeing without the thing seen." But these are assertions, not demonstrated facts. They may serve as the axioms of a parsimonious behavioral system, and that is largely how Skinner used them. But they cannot then also be used as arguments against other systems, or against behavioral systems with augmented axioms, such as the assumption that covert events are sufficiently different from overt ones to deserve separate treatment as a separate category of events. (Killeen, 1984, p. 27)

Skinner himself (1974) is quite clear that the issue is not one of parsimony per se. Asking himself how we could decide between behavior analysis and mentalism, he replies:

> We cannot say that one is simpler than the other . . . [but] accessibility [for use in control] is another matter. No one has ever directly modified any of the mental activities or traits . . . . for most practical purposes they are changed only through the environment . . . . A decision [between the two positions] is perhaps more difficult if we simply want to predict behavior . . . . [Traits] are . . . useless in control but they permit us to predict one kind of behavior from another kind. (pp. 208–209)

Behavior-behavior relations as incomplete accounts. By referring to "mental events" as behavior we do three things. First, we eliminate consideration of "mental events" that cannot be thought of as observable organismic activity, such as purely hypothetical constructs. Second, we emphasize that it is the task of psychology to predict and control these events. And third, we focus on analyses that can accomplish these two goals. If "mental events" are a separate category of events, then they can be used to explain behavioral events and perhaps need not themselves be explained by behavioral scientists. Mental events that cannot be translated into behavioral observations are particularly prone to this problem because by definition they are seemingly a separate category of events from behavior. The problem can be used as an "argument against other systems," but it is not a mere repetition of axioms. It is an identification of possibly mischievous contingencies over the behavior of psychologists, one that the history of psychology gives us every reason to take seriously.

When a radical behaviorist is less than enthusiastic about an account of behavior that predicts that someone will respond in a given way after thinking a particular thought, it should be an absence of enthusiasm resulting from the incompleteness of the account rather than from the reference to a thought. The immediate question then becomes what are the determinants of that thought and (even less obviously) what are the contingencies that lead to a relation between a given instance of thinking and overt responding in this individual.

In behavior analysis, the view that thinking causes overt behavior distills down to the view that one behavior can cause another. In these terms, when we ask such questions as "What role does thinking play in the control of behavior?", we are actually asking about the nature of a behavior-behavior relation. Behavior-behavior relations are very important in behavior analysis in a variety of areas, and they are as worthy of study as is any behavior. No matter how dynamically one behavioral event may be intertwined with other behavioral events within the same individual, however, for a contextualist a behavior-behavior relation is a phenomenon to be explained by appealing to particular contextual arrangements (e.g., contingencies of reinforcement) that might permit prediction and control of the behavior-behavior relation itself. A behavior-behavior relation cannot be a complete explanation of behavior, except to a mechanist, whose world view does not insist on control as a necessary goal of science.

"We may object, first, to the predilection for unfinished causal sequences" (Skinner, 1969, p. 240). Killeen (1984) has criticized this concern:

> Skinner notes that after we have explained a response in terms of mental states or activities of feeling, we still need to explain the mental state. But there is nothing wrong with that. Experimental

analysis of one of the links in a causal chain should not necessarily be faulted because it does not include the previous ones; analysis must inevitably stop at some point short of the Ultimate Cause." (pp. 27–28)

For a complete account, however, behavior analysts must take analysis to the point at which prediction and control are directly possible in principle. Behavior of the individual being studied can never satisfy that criterion.

Thus, despite the fact that environment and behavior are always involved in a dynamic interrelation, it is not arbitrary (Bandura, 1978, 1981) to insist that analysis proceed to the environmental level. An environmental cause can in principle be used directly; given the technical ability to manipulate it, effective action can be based on it. A rule pointing to a behavioral "cause" might help locate causal environmental events and relations, but it is also likely, if one is not careful, to stop the search for causes that could permit a complete account. Skinner (1974) has said this explicitly:

It has been objected that we must stop somewhere in following a causal chain into the past and we may as well stop at a psychic level . . . It is true that we could trace human behavior not only to the physical conditions which [cause it] but also to the causes of those conditions and the causes of those causes, almost ad infinitum, but there is no point in going back beyond the point at which effective action can be taken. That point is not to be found in the psyche. (p. 210)

Thus, "the *initiating* action is taken by the environment" (Skinner, 1974, p. 73, emphasis added; see also Skinner, 1984). If we explain behavioral events in terms of events that are in the domain of other sciences, we move toward the comprehensive knowledge that we desire. But if we explain behavioral events in terms of other behavioral events of the same individual, then a significant gap is left to be filled. If we will not fill it, who will? If not now, when?

The effect of calling thinking "behavior." By calling all organismic activity appropriate to psychological study "behavior," we naturally inoculate ourselves to some degree against incomplete accounts. Acceptance of an incomplete analysis is much less likely when all events involved are clearly in the behavioral domain. For example, a researcher may notice that many good Monopoly players are also good poker players. Few of us would be tempted, however, to claim that people play poker well because they play Monopoly well or vice versa. Both of these actions are obviously in the behavioral domain. Events outside of this domain must be found to explain the first behavior, and equally important, to explain the relation between the two behaviors. That is, what actual events lead to good Monopoly playing, good poker playing, and their interrelation? This need seems relatively clear when the two events are obviously from the same domain. Behavior-behavior relations are seldom mistaken for causal analyses adequate for the purpose of prediction and control when they are clearly stated as behavior-behavior relations.

Note, however, that when behavioral events are apparently in different domains, the mistake is readily made. If we observed that good poker players feel self-confident (not "behave self-confidently"), have aggressive personalities (not "behave aggressively"), and are intelligent (not "show intelligent behavior"), we might feel as though we have at least a partial explanation of their skill even though this is in principle identical to the obviously less satisfactory claim that good poker playing comes from good Monopoly playing. To most people feelings, personalities, intelligence, and the like, if not explicitly termed types or qualities of behavior, often seem to be something else. Such things are then easily mistaken for causes of behavior.

Insisting that we call "mental events" by the name "behavior" should not be done to diminish the interest in these so-called mental events, any more than calling a public behavior in a behavior-behavior relation by the name "behavior" should be done to diminish interest in public behavior. Behavioral scientists should call events "behavior" to keep clear the fact that it is their job to explain such events and to avoid incomplete accounts based on these events.

A brief summary seems in order. The

purpose of this paper has been to place the behavior-analytic rejection of mental causes into the larger context of the goals of science embraced by behavior analysis. Both critics and supporters of behavior analysis seem at times to confuse the various reasons for the behavior-analytic concern with mentalism. Literal dualism is rejected because it is scientifically untenable and because it leaves claimed sources of control over behavior outside the scientific enterprise. This rejection of literal dualism, however, is common to virtually all scientific forms of psychology. The rejection of mentalism on the grounds of the requirements of scientific method, as done by early forms of methodological behaviorism, is not recognized as legitimate by radical behaviorism. Neither is the attempt to define away the independent existence of private events, as in Watsonian metaphysical behaviorism. What, then, is unique about the radical behavioral rejection of mentalism?

Our point has been that the primary radical behavioral objection to mentalism (other than literal dualism) is a metatheoretical one. Nondualistic analyses based on mental causality usually boil down either to pseudophysiologizing, to theorizing based on hypothetical constructs, or to elevating disguised behavior-behavior relations to causal status. The concern with all of these maneuvers is that they interfere with the behavior-analytic agenda of predicting and controlling behavior. As we have tried to show, this agenda is not an arbitrary element of behavior analysis. It is a required element for the successful functioning of the perspective. Thus, mental causality is a form of theorizing rejected because its pursuit threatens the successful operation of science as viewed from the standpoint of behavior analysis.

THE SLIPPERY SLOPE OF NONMANIPULABLE CAUSES

We believe that much of the criticism of the behavioral approach arises from a failure to recognize its goals and the reasons for them. Other goals or values make alternative practices and beliefs justifiable, and with these we can have no quarrel, provided only that the researcher honestly states what these goals are. For example, if a particular field of psychology wishes to eschew control as the end product of science, it has the obligation to make that clear to all, whether the person is a psychologist, a client seeking help, a congressman, or a taxpayer helping fund federal research.

We see no relative disadvantage for a radical behavioral approach when prediction and control are being pursued. We do not mean to say, of course, that research generated by nonbehavioral psychologists necessarily results in incomplete analyses as viewed from a contextualistic perspective. See, for example, Ornstein and Naus's (1978) demonstration that recall was increased after the manipulation of environmental events controlling overt rehearsal. We must guard against responding to the quality of language rather than to the quality of the analysis. The issue is not the *form* of science but its *function.* It is the quality of the analysis that is at issue rather than an attempt to urge that some simple physical correspondence exists between private and public behaviors.

Examples of the Danger

Behavioral analyses that implicate "covert behavior," rather than "mental events," are equally incomplete if they fail to extend the analysis to environmental variables. Indeed, there are several topics within behavior analysis that share with mentalistic accounts some of the same potential metatheoretical problems of encouraging incomplete accounts.

Self-rules. There can be little doubt that humans talk to themselves. Presumably, these verbal events can relate in reliable ways to other behavior. To explain other behavior simply in terms of "self-rules," however, is not adequate behavior-analytic explanation. As in the cases discussed earlier, the term "self-rules" does not obviously place these events in the

same category as "behavior." Thus, when talking of a relation between a self-rule and behavior, it is not always obvious that it is a behavior-behavior relation that is at issue. Because it is, we must explain both how external events gave rise to the private talk, and how the private talk came to control the behavior of interest. When this is done, "self-rules" may participate in an overall causal relation, but they should not themselves be seen as causes.

When behaviorists say they have located a "cause," it seems they should have identified a relation that can perform the functions required of a scientific relation within a behavioral perspective, namely, prediction and control. In a sense, causes are not objective independent facts in the world. All events in a thoroughgoing contextualistic system are "events" only because construing them that way serves a purpose. A causal relation can only be tested as a verbal construction, and only when this verbal statement of a relation serves the purposes of science should it be called a "cause." Perhaps when we have finished a complete analysis of a behavior-behavior relation, we could think of the first behavior as a kind of "intermediate cause" (e.g., see Skinner's reluctant agreement on this point, 1984), but it is surely safer not to do so. Some other term is needed. "Controlling behavior" suggests itself because of Skinner's use of the term to serve this function in his analysis of self-control. For example, self-rules might be said to function as controlling behaviors as parts of an overall causal relation. They are not themselves causes, at least not in the behavior-analytically acceptable sense of the word "cause."

Response-produced stimuli. At times, behaviorists have used the term "response-produced stimuli" to explain behavior-behavior relations. The apparent effects of private behaviors on other behaviors may be explained on this basis. For example, we may say that a person visualizes one's bedroom, and due to the stimuli produced by this behavior, remembers where the car keys were left. Even if the analysis avoids the idea of an image seen (in favor of the more "behavioral" term "response-produced stimuli") it is troublesome.

Responses obviously usually produce stimuli. For example, all operants, by definition, do so. The concept of response-produced stimuli, however, is usually invoked when there is no possibility of discerning or manipulating the stimuli independently of the behavior itself. The danger is that, because stimuli are apparently in a different class than behavior, explanations based on "response-produced stimuli" will be accepted as complete even when we have in principle no hope of manipulating or even discerning these stimuli independently. Explanations are not complete under these conditions. We move no closer to prediction and control simply by replacing a behavior-behavior relation with a "behavior–response–produced stimuli–behavior" relation unless we have independent access to the stimuli. It is useless to "explain behavior by appealing to independent variables which have been inferred from the behavior thus explained" (Skinner, 1969, p. 264). The radical behavioral objection to hypothetical constructs (when they are used as other than merely a shorthand for behavior) has the same metatheoretical basis as the primary objection to mentalism. In some cases, "response-produced stimuli" can have the status of a purely hypothetical construct.

This objection does not necessarily mean that we should stop using the term "response-produced stimuli." We should distinguish, however, between three types of situations. In the first, the "stimuli" referred to are clearly in the domain in which control is possible in principle. An ordinary operant is an example. A key-peck that produces food is producing food-related stimuli. That is presumably why this behavior occurs in the first place. We could speak of the food as response-produced stimuli. In this case, we are using the term "response-produced stimuli" in a manner totally consistent with the goals of science as seen by behavior analysts because we can manipulate these stimuli independently from the behavior

that produced them and discern their effects on behavior.

In the second situation, the stimuli cannot currently be controlled due to technical limitations, but control is possible in principle. Analyzing backscratching in terms of sensory reinforcement maybe an example. Here, when we claim that the back scratch is due to response-produced stimuli we are engaging in interpretation, and in the future we may or may not find the interpretation to have provided a complete account. We may ultimately find ways to block the suggested sensory stimulation and to discern the effects of this manipulation.

Finally, there are times when direct control is impossible in principle. A claim that we have unconscious thoughts and that these produce stimuli might be an example. Here, we are using the term "response-produced stimuli" solely to provide a consistent account, but at a considerable cost. We have disguised an analysis that cannot in principle meet all the goals of science from a behavior-analytic viewpoint in the cloak of terminology that suggests these goals can be met.

Behaviors as self-reinforcers. Even radical behaviorists sometimes claim that one behavior can be maintained by the person involved providing other behaviors as "self-reinforcers" (e.g., Malott, in press). The task of behavior analysis must be to explain both behaviors and their relation. When the contexts giving rise to such behavior-behavior relations have been manipulated experimentally, however (e.g., Hayes, Rosenfarb, Wulfert, Korn, & Zettle, 1985), the apparent influence of self-reinforcing behaviors have always resolved into the effects of environmental events (Sohn & Lamal, 1982).

An example of an apparently causal behavioral consequence is the Premack principle. Superficially, this principle seems to suggest that one behavior can reinforce another in the same organism. But if the external environment manipulates the opportunity to engage in a particular behavior following another, then this is not a simple behavior-behavior relation. It is really best thought of as a behavior—environmental restriction—behavior relation.

The examples of self-rules, response-produced stimuli, and self-reinforcement show that behavior analysts are also susceptible to the tendency to dress up behavior-behavior relations in the cloak of nonbehavioral events and then to forget that they have done so. The cost of this action is the same as the cost of mentalistic talk or pseudophysiological talk—incomplete analyses are inappropriately accepted as complete and a resultant gap in knowledge is produced.

CONCLUSION

An embrace of mentalism is not a sure road to an appreciation of the richness of private phenomena, and trivialization is not the necessary result of behavioral translation. Behavior analysts should reject mentalistic terms precisely in order to study the actual phenomena associated with them in a more thorough way and in a way more satisfying to the goals of science as viewed by behavior analysts. Allowing behavioral causes made seemingly less incomplete by calling them "mental" ultimately tends to stop causal analysis before the point at which effective action is possible. We need to understand the actual phenomena pointed to by mentalistic terms, or terms such as self-rules or self-reinforcement for that matter. The analytic discipline supplied by the assumptions inherent in radical behaviorism is needed most in exactly such difficult endeavors, not in order to pursue analytic discipline for its own sake, but in order to develop a more thoroughly adequate explanation of human behavior.

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