

SOME FURTHER THOUGHTS ON THE PRAGMATIC AND BEHAVIORAL CONCEPTION OF PRIVATE EVENTS

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ABSTRACT: For behavior analysis, ontological concerns are linked with pragmatism, rather than any stance on the inherent nature of the thing studied. Thus, behavior analysis regards as troublesome ontological commitments that formulate certain covert events as “mental” rather than behavioral because those commitments are antithetical to the effective prediction and control of natural phenomena. Moreover, a helpful recognition is that neither behavior nor environment need be publicly observable. Rather, they may be accessible to only one, but nevertheless in the behavioral dimension.

Key words: radical behaviorism, eliminativism, methodological behaviorism, pragmatism, ontology, Quine, Skinner

Professor Reed invited three distinguished scholars to comment on my recent article (Moore, 2001b), and then afforded me the opportunity to reply. I thank my colleagues for taking their valuable time to comment so thoughtfully on the article. In what follows, I would like to address what I see as two interrelated themes in the comments. The first is ontology. The second is the relation between behavior analysis and physiology in a science of behavior.

Ontology

With regard to ontology, Professor Leigland quite correctly notes that even raising the question of a traditional dualistic ontology in a discussion of private events is distracting and possibly unnecessary. Certainly, a *mea culpa* is in order here. Nevertheless, the dimensions of a science of behavior have always been of some concern in behavior analysis. For example, consider the following passage from Skinner (1953):

Modern science has attempted to put forth an ordered and integrated conception of nature. Some of its most distinguished men have concerned themselves with the broad implications of science with respect to the structure of the universe. The picture which emerges is almost always dualistic. The scientist humbly admits that he is describing only half the universe, and he defers to another world—a world of mind or consciousness—for which another mode of inquiry is assumed to be required. Such a point of view is by no means inevitable, but it

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is part of the cultural heritage from which science has emerged. It obviously stands in the way of a unified account of nature. (p. 258)

The article's expressions of concerns about ontology were intended simply to point out the pragmatic problems (i.e., relating to prediction and control) that stem from assuming another dimension to the world. I suggest it is the other kinds of behavioral science that have raised the issue of ontology, either explicitly or implicitly, and behavior analysis is simply trying to point out the problems of verbally implying multiple dimensions. Thus, we have Skinner (1969) saying that

The objection is not that these things are mental but that they offer no real explanation and stand in the way of a more effective analysis. (p. 222)

Questions of ontology tend to arise, I believe, because of the way we define the terms "behavior" and "environment." Let us first consider the definition of behavior. It is tempting to define it physicalistically. Indeed, this is what Skinner (1938) himself did, at least initially:

Behavior is what an organism is doing—or more accurately what it is observed by another organism to be doing. . . .By behavior, then, I mean simply the movement of an organism or of its parts in a frame of reference provided by the organism itself or by external objects or fields of force. (p. 6)

The definition above of behavior was presumably composed at a time when Skinner (1938) was trying to make the case for behavior "as a subject matter in its own right" (p. 440). Public observability (e.g., of physically measured properties) was the respectable way to establish a science of behavior and distinguish it from, say, the vagaries of introspective structuralism. Indeed, at the time of the composition of this definition, the physicalism of logical positivism and operationism were in the air, and Skinner (1979) recounts that as far as he "was concerned, there were only minor differences between behaviorism, operationism, and logical positivism" (p. 161).

However, most definitions in behavior analysis are functional definitions, rather than physicalistic. A functional definition identifies behavior as a class of activity that is a function of a class of environmental circumstances. The circumstances in question entail one or more of a stimulus presentation, stimulus control operation, or consequential operation (e.g, Catania, 1998, pp. 13-28). To be sure, some of this activity is observable to a second organism, particularly if the second organism mediates reinforcers for the first, but not all need be. Some of the behavior may have an impact on the behaving organism itself, for example, by exerting discriminative control on other behavior.

Similarly, consider a definition of the environment. In this regard, Chapter 18 in *Science and Human Behavior* begins as follows:

When we say that behavior is a function of the environment, the term "environment" presumably means any event in the universe affecting the organism. But part of the universe is enclosed within the organism's own skin.

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Some independent variables may, therefore, be related to behavior in a unique way. The individual's response to an inflamed tooth, for example, is unlike the response which anyone else can make to that particular tooth, since no one else can make the same kind of contact with it. Events which take place during emotional excitement or in states of deprivation are often uniquely accessible for the same reason; in this sense our joys, sorrows, loves, and hates are peculiarly our own. With respect to each individual, in other words, a small part of the universe is private.

We need not suppose that events which take place within an organism's skin have special properties for that reason. A private event may be distinguished by its limited accessibility but not, so far as we know, by any special structure or nature. We have no reason to suppose that the stimulating effect of an inflamed tooth is essentially different from that of, say, a hot stove. The stove, however, is capable of affecting more than one person in approximately the same way. (Skinner, 1953, pp. 257-258)

For behavior analysis, then, the definitions of behavior and environment do not turn on whether they are publicly observable. Given this state of affairs, what are the differences between behavior analysis and mentalism, where the latter also emphasizes factors that are accessible to only one?

It seems to me the differences are two fold. The first difference concerns the origin of the so-called "mental" phenomenon. To the extent that typical forms of mentalism deal with the origin at all, they tend to assume that private factors are innate and unlearned, in the sense of Kantian metaphysics ("Generally speaking, cognitive explanations are biologically oriented, nonphysiological and mechanistic; they bear the unmistakable imprint of rationalism," Wessells, 1981, p. 155). Indeed, one reads much these days of the "evolution" of mental processes, for example, with respect to those held to "underlie" language. Pinker (1994) is a convenient example.

Second, many forms of mentalism tend to account for the efficacy of the mental phenomenon as an antecedent force that just somehow compels some end, for example, as an initiating, purposive power. Behavior analysis rejects this interpretation of cause (e.g., Skinner in Catania & Harnad, 1988, pp. 354, 482-3, 486-7). To the extent that behavior analysis talks about the causal efficacy of private phenomena, it identifies the discriminative control they exert, but discriminative control is not the same as an initiating, purposive power. Moreover, private phenomena exert the discriminative control they do because of the control transferred to them by public relations, which is why the radical behaviorist doctrine on private events is pragmatic and not reducible to mentalism.

To be sure, modern cognitive psychologists assert that their talk of mental states, mental processing mechanisms, and so on does not create any ontological problems. However, it seems to me they are committed to a conception of behavior and environment that implicitly endorses the criterion of public observability. This commitment creates ontological problems when they talk about activity that is not publicly observable. In short, the commitment is that if a phenomenon is not publicly observable, it cannot be either behavior or environment. Then the question is what is its nature, and that is where the ontological trouble starts. Indeed, it

seems to me it promotes methodological behaviorism, where under the guise of pragmatism scientists are prohibited from dealing with anything that is not publicly observable (Moore, 2001a). As Skinner (1945, p. 293) said, this strategy was never good behaviorism but behaviorists themselves often resorted to it because it was easy to defend.

In broad perspective, the central issue in a discussion of whether mentalistic talk is also dualistic is what controls the verbal behavior in question. Recall that meaning is to be found among the determiners, not the properties, of response (e.g., Skinner, 1957, pp. 13-14). By virtue of the commitment to the criterion of public observability for behavior and environment, and the commitment to inner events as necessarily differing from behavior and environment, I would argue that mentalists are controlled by the same social-cultural traditions as traditional dualists. Therefore, their denial of dualism is hollow.

Behavior Analysis and Physiology

With regard to the relation between behavior analysis and physiology, Professor Stemmer advances a position affiliated with Quine as well as eliminativism. This position is in one sense a monistic, materialist position. It clearly does not countenance any sense of a mentalistic orientation inspired by cognitive psychology. However, as before, the central issue is the definition of behavior and environment that it embraces. It seems that to count as behavior or environment for Professor Stemmer, the variables need to be publicly observable. Absent their public observability, the variables need to be treated as something else, namely, as purely physiology. Happily, one implication of this position is really not far from the radical behaviorism of behavior analysis. Professor Stemmer notes that the inner physiological events acquire their behavioral significance because of their relation to public events (e.g., Stemmer, 2001, p. 191). This is exactly the same argument that Skinner has made since 1945, although Skinner does not invoke neural traces or other physiological concepts in his argument. Consequently, there is a sense in which it does not matter whether one calls the events in question physiological or behavioral. (The same observation applies to Professor Barnes-Holmes' comments.) Unhappily, however, the stance invites an implicit endorsement of methodological behaviorism, wherein the commitment to public observability calls for a different approach to behavioral phenomena. In addition, Professor Stemmer appeals to ostensive stimulus-stimulus pairings to account for the learning of "mental" terms, in a sense not far from Pavlovian conditioning. Although some terms may well be learned this way (e.g., Skinner, 1957, p. 360), it seems to me that a far greater number of terms are learned through equivalence relations, as illustrated by the complex networks of metaphors that underlie our descriptions of private phenomena.

The earlier portion of the present reply defined behavior as activity that is functionally related to the environment. The formulation of a behavioral event, therefore, includes the environment as well as the response itself. Accordingly, we explain a behavioral event by stating that the hungry rat's lever press or the hungry

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pigeon's key peck is in response to the contingent relation among some discriminative stimulus, say a 1000 Hz tone or a 530 nm light, the behavior in question, and a consequence, say a 45 mg food pellet or 3 seconds access to mixed grain, in prior similar circumstances. As Skinner (1969) put it, a formulation of probably the most important interaction between an organism and its environment, operant behavior,

must always specify three things: (1) the occasion upon which a response occurs, (2) the response itself, and (3) the reinforcing consequences. The interrelationships among them are the "contingencies of reinforcement." The concept characterizes an aspect of the environment which Tolman and Brunswik may have been trying to identify when they spoke of its "causal texture". . . . The interrelationships are much more complex than those between a stimulus and a response, and they are much more productive in both theoretical and experimental analyses. (pp. 7-8)

To be sure, when a rat presses a lever or a pigeon pecks a key, the muscles of the organism are contracting, but the event is behavioral. The muscle contractions are regarded as a necessary component in the larger interactional systems of the event, in the sense that one presumably would not want to say a response had occurred without there being some efferent activity somewhere, at some level of magnitude, inside the skin of the organism. In fact, one might even deal with such issues as muscle contraction by defining it as the response. For example, Hefferline and Keenan (1963) arranged for subjects to earn monetary reinforcement when the emg potential of a small muscle in their thumbs exceeded a particular value. The value was so small that the thumb itself did not actually move, but was detected by the measuring equipment. The behavior varied systematically with respect to the reinforcement conditions. Interestingly, although not the reason why I am citing this research, subjects were not able to state at the end of the experiment how the money was earned, even though their behavior was very orderly with respect to environmental variables.

Stemmer expressed concern in his commentary about the empirical evidence for asserting that covert events are behavioral as opposed to the subject matter of physiology only. As my target article notes, Skinner (Catania & Harnad, 1988, pp. 331, 485) once suggested that covert efferent activity may count as a response if environmental contingencies do not require overt action. Perhaps Skinner's suggestion has its origin in Watson's early analyses. In any case, a recent study with monkeys may be relevant to Stemmer's concerns and validate Skinner's conjectures (Mijail et al., 2002). The experiment was performed in a Brown University neuroscience laboratory on three monkeys. The researchers implanted miniature electrodes in the motor cortex of each monkey. The researchers then connected the electrodes to a computer. The monkeys were subsequently taught to play a video game that involved moving a cursor with their arms and hands to track a target on a computer screen. When the monkeys played the game, their brains made characteristic signals, which the computer recorded. The team wrote a program that correlated the patterns of electrical activity in the brain with the overt

movements of the monkeys' arms as they manipulated the cursor. Then, the program translated brain-wave data directly into joystick output, so that the monkeys' brain activity alone would move the cursor. The result was that after a very short period of time, the monkeys were playing the video game without using their arms and hands, but with only the same kind of brain activity that was involved in overtly manipulating the cursor. In line with Skinner's suggestion, this demonstration seems to support the idea that covert responses are carried out on a smaller scale but with the same underlying physiological structures that underlie overt, and that behavior, interpreted as activity functionally related to environmental circumstances, need not be overt.

This matter is also germane to the point that Professor Barnes-Holmes raises. Professor Barnes-Holmes suggests that behavioral science would be well served by studying physiological events as behavioral events. I agree, but would say things differently. Any nominally physiological event that is modulated by its relation to environmental factors means that it already is a behavioral event; the modulation makes it so. The precise degree to which the events at issue can be brought under environmental control is an empirical question, and the results are uncertain (e.g., Miller & Dworkin, 1974). Indeed, biofeedback techniques for control of certain functions are concerned with precisely this issue. Whether dopamine levels or specific eeg patterns can be influenced by environmental relations, as Professor Barnes-Holmes ponders, remains to be determined, but the question is certainly worth pursuing.

In any event, Moore (2002) recently suggested the ultimate relation between behavior analysis and physiology is pragmatic. That is, in principle, knowledge from either behavior analysis or physiology serve as adequate basis for attempts to manipulate, predict, and control behavior. If knowledge from one domain is limited, attempts to manipulate, predict, and control behavior on the basis of that knowledge might be made more effective if they were informed by the other science. A knowledge of underlying physiology is therefore not necessary to provide the logical or empirical grounds for validating behavior-analytic theories, explanations, or technological advances. The pragmatic relation between behavior analysis and physiology means that each science can inform the other in its characteristic way, but that physiology is not necessary to validate behavior-analytic explanations, any more than behavior analysis is necessary to validate physiological explanations. As before, I take this stance to be consistent with Professor Barnes-Holmes, in that successful working rather than an essentialist, realist commitment to conceptual systems of uncertain origin becomes the meaningful truth criterion.

With this assertion of pragmatism I close my reply.

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