



For the Radical Behaviorist Biological Events Are Not Biological and Public Events Are Not Public

Author(s): Dermot Barnes-Holmes

Source: *Behavior and Philosophy*, 2003, Vol. 31 (2003), pp. 145-150

Published by: Cambridge Center for Behavioral Studies (CCBS)

Stable URL: <https://www.jstor.org/stable/27759452>

REFERENCES

Linked references are available on JSTOR for this article:

https://www.jstor.org/stable/27759452?seq=1&cid=pdf-reference#references_tab_contents

You may need to log in to JSTOR to access the linked references.

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact support@jstor.org.

Your use of the JSTOR archive indicates your acceptance of the Terms & Conditions of Use, available at <https://about.jstor.org/terms>



Cambridge Center for Behavioral Studies (CCBS) is collaborating with JSTOR to digitize, preserve and extend access to *Behavior and Philosophy*

JSTOR

FOR THE RADICAL BEHAVIORIST BIOLOGICAL EVENTS ARE NOT BIOLOGICAL AND PUBLIC EVENTS ARE NOT PUBLIC

Dermot Barnes-Holmes
National University of Ireland, Maynooth

I should declare at the outset that Dr. Moore is one my intellectual heroes. In fact, I recall well that it was only after reading his 1981 article, *On Mentalism, Methodological Behaviorism, and Radical Behaviorism*, that I became fully convinced of the value and importance of the philosophy of radical behaviorism and the science of behavior analysis. Not surprisingly, therefore, I find myself in broad agreement with his argument that a firm distinction should be made between radical behaviorism and what he labels logical behaviorism and conceptual analysis. Unlike these latter two intellectual traditions, the radical behaviorist does not assume that mentalistic terms must refer to events that are publicly observable, such as underlying physiological states, publicly observable behavior, or dispositions to engage in publicly observable behavior. Rather, the radical behaviorist approaches mentalistic terms in the same way as any other verbal behavior—by focusing on the behavioral history and current conditions that give rise to the use of such terms within the verbal community, including the scientific community itself. From this perspective, therefore, the use of a mental term (i.e., a verbal response) may be under the control of public or private stimuli, and the role of the radical behaviorist is to analyze the historical and current behavioral contingencies that establish such verbal control. Only in so doing will the use of mental terms be understood from within the world-view of radical behaviorism.

Although I find myself in general agreement with Moore's thesis, I would like to elaborate upon two of the key issues that arose in his article: (1) the role of physiological events in behavior analysis, and (2) the concept of the private event in radical behaviorism. In the first case, Moore discusses the problems inherent in treating private events as purely physiological, and I certainly agree with the points he makes in this regard. However, I think it should also be emphasized that the study of physiological events, *per se*, needs to be incorporated into the experimental analysis of behavior, not as underlying or explanatory mechanisms but as behavioral events in and of themselves. In the second case, Moore points out, “. . . the distinction between public and private events in behavior analysis is

AUTHOR'S NOTE: The writing of this article was supported in part by a grant from the NUI, Maynooth Research Enhancement Fund. I would like to thank Yvonne Barnes-Holmes for her constructive comments on initial drafts of the current article. Please address all correspondence concerning this article to Dermot Barnes-Holmes, Department of Psychology, National University of Ireland, Maynooth, Co. Kildare, Ireland. Email: Dermot.Barnes-Holmes@may.ie. Telephone: +353 1 7084765. Fax: +353 1 708 4767.

at heart not an ontological distinction between physical and mental. Rather, it is a distinction of access” (p. 177). I think this is a fundamentally important argument because it highlights the nonrealist and intensely pragmatic approach to psychology that both radical behaviorism and behavior analysis embody. In what follows, I will elaborate on each of these two issues, and in so doing will further bolster, I hope, Moore’s thesis that radical behaviorism is fundamentally different from the other behaviorisms that he discusses.

The Role of Physiological Events in Behavior Analysis

Some behavioral researchers may be seduced into thinking that the physiologists and neuroscientists of the future will in due course make our behavioral science more complete. The temporal gaps between stimulus and response, for example, need to be filled with physiological events, and to do this we should simply pass the baton of behavioral research on to the physiologist and neuroscientist. Indeed, Skinner (1974) seemed to suggest this very strategy:

The physiologist of the future will tell us all that can be known about what is happening inside the behaving organism. His account will be an important advance over a behavioral analysis, because the latter is necessarily “historical”—that is to say, it is confined to functional relations showing temporal gaps. Something is done today which affects the behavior of an organism tomorrow. No matter how clearly that fact can be established, a step is missing, and we must wait for the physiologist to supply it. He will be able to show how an organism is changed when exposed to contingencies of reinforcement and why the changed organism then behaves in a different way, possibly at a much later date. What he discovers cannot invalidate the laws of a science of behavior, but it will make the picture of human action more nearly complete. (p. 215)

Skinner’s quotation could be taken to mean that behavior analysts must wait patiently for neuroscientists to provide us with the answers we need, and these will fit perfectly and immediately into our hitherto incomplete behavioral puzzle. I am fairly sure, however, that this is not what Skinner really intended. Instead, I suspect that he was suggesting that certain questions about behavior will require the knowledge, technology, and experimental procedures of the physiologist and neuroscientist, but behavior analysts will still need to undertake the appropriate behavior-analytic research in order to advance behavioral science *qua* behavioral science. In other words, physiological events may be incorporated into the science of behavior, not as biochemical events *per se*, but as behavioral events. Skinner’s later work, cited by Moore on page 171, indicates that he certainly seemed comfortable with this notion when he wrote: “I see no reason why we should not also call the action of efferent nerves behavior if no muscular response is needed for reinforcement” (Skinner in Catania & Harnard, 1988, p. 485).

In any case, I believe that behavior analysts should study physiological events as behavioral events (Barnes-Holmes, 2000). After all, it is only a matter of convenience that lever pressing, key pecking, and button pushing have most often

been used as responses within behavior-analytic research. Thus, for example, a behavioral researcher could quite legitimately measure dopamine levels, *as a class of responses*, contingent on food delivery. In so doing, dopamine levels are not being analyzed at the biological or chemical level, but at the behavioral level (i.e., as a response class contingent on specific consequences). When approached from this angle, biological events are not treated as underlying, mediating, or modulating behavioral events. Rather, biological events are approached, measured, analyzed, and understood as part of the behavioral system under investigation, and thus they are seen as participating in functional relations with past and current behavioral contingencies.

A recent pilot study conducted in my own laboratory provides a relevant example of this approach. In this study, brain activity was recorded in the form of event related potentials (ERPs), while adult participants were exposed to respondent-type training and testing in equivalence relations across multiple stimulus sets (see Barnes, Leader, & Smeets, 1996). The study focused on the level of brain activity that occurred during each participant's first exposure to pairs of nonequivalent stimuli (see DiFiore, Dube, Oross, Wilkinson, Deutsch, & McIlvane, 2000). The data thus far indicate that the level of a particular measure of brain activity, known as the N400 waveform, decreases across successive stimulus sets, indicating a functional relation between this waveform and number of training and test exposures. When approached in this way, the brain activity is not seen as underlying, mediating, or modulating the subject's test performance. Instead, the N400 waveforms constitute a response class that participates in the behavioral contingencies that are established and manipulated within the experiment. Of course, there may be many additional functional relations between brain activity and other more "overt" responses, such as key presses, but these relations may be viewed as behavior-behavior relations (Barnes-Holmes, 2000), which have limited explanatory value for the radical behaviorist (Hayes & Brownstein, 1986). In any case, this general approach to the study of brain activity clearly separates radical behaviorism from the other behaviorisms discussed by Moore.

The Concept of the Private Event in Radical Behaviorism

The distinction between public and private events is a popular one in radical behaviorism. Paradoxically, however, all behavioral events from a radical behavioral perspective may be defined as private, and the implications of this view again serve to highlight the unique nature of radical behaviorism as a philosophy of science.

Consider an experimental participant who has been exposed to an operant contingency in the presence of a green light. If the presence of the green light now occasions a higher response rate than occurs in its absence, the light may be functioning as a discriminative stimulus. At this point, we might be tempted to argue that the discriminative stimulus is a public event, because both the subject and the experimenter (and anyone else who can perceive a green light) can observe the events in question. To draw this conclusion, however, involves confounding

common sense terms with the scientific nomenclature of behavior analysis. In the former case, anyone who perceives the green light is observing the *same* green light, but in the latter case no two individuals ever respond to the *same* discriminative stimulus. Any given discriminative stimulus acquires its behavioral properties within the behavioral history and current context of a particular organism. And because no two behavioral histories are exactly identical, no two discriminative stimuli can be defined, at least technically, as the same. Even when an experimenter attempts to provide similar histories of reinforcement in the presence of the “same” green light for two organisms, the discriminative properties that are established for the green light will not be exactly identical for both organisms (e.g., slightly different response rates or extinction curves may be observed across the behavioral streams). In this sense, therefore, no two organisms ever respond to, observe, or share the same discriminative stimulus, and thus all stimulus events in behavior analysis are “private” to the behavioral stream within which they occur (see Barnes & Roche, 1997; Barnes-Holmes, 2000, for detailed discussions of this issue).

At this point, it might be argued that the private or public nature of discriminative stimuli for our experimental participants is not at issue in the current context—what matters is that two or more behavioral scientists can readily observe the events in question and agree about what they saw (e.g., that the green light is apparently functioning as a discriminative stimulus). For the radical behaviorist, however, the research activity of the scientist, including both scientific observation and agreement, are no less behavioral than any other activity (Skinner, 1974; see also Barnes & Roche, 1997). When two scientists observe and agree about the same event, the observation and subsequent agreement that each of them discriminates are, from the radical behaviorist perspective, stimulus events that participate in the *separate* behavioral streams of the two scientists. In this sense, therefore, even scientific observation and agreement are *private* to the behavioral streams within which they occur. Not even the behavioral scientist can escape his or her own behavioral stream and make direct nonbehavioral contact with an ontological reality about which he or she can then agree or disagree with another scientist. Skinner described the reflexive nature of radical behaviorism in this way:

It would be absurd for the behaviorist to contend that he is in any way exempt from his analysis. He cannot step outside of the causal stream and observe behavior from some special point of vantage, “perched on the epicycle of Mercury.” In the very act of analyzing human behavior he is behaving. (1974, p. 234)

On balance, stating that radical behaviorism is inherently reflexive also may be defined as a behavioral event, and thus any ontological claims with regard to its reflexivity can be seen as contradicting its own reflexivity. A radical behaviorist who claims that everything he or she says is a behavioral event, and then goes on to state that this very claim is true, in some ontological or nonbehavioral sense, can expect to be challenged vigorously by other members of the wider verbal community. It would be a mistake, therefore, to become overly dogmatic about the

reflexive nature of radical behaviorist philosophy. In doing so, one paradoxically undermines the very reflexivity that one is seeking to uphold. A radical behaviorist solution to this conundrum involves embracing what I have called behavioral pragmatism, an intensely personal, goal-driven, and pragmatic approach to science and philosophy (Barnes-Holmes, 2000; see also Barnes & Roche, 1997). The details of behavioral pragmatism are not important here—what matters, is that radical behaviorism is seemingly characterized by an extreme form of pragmatism within which no final, absolute, or ontological claims are permitted, lest they undermine the behavioral nature of radical behaviorism itself.

The intense pragmatism of radical behaviorism is also to be found in Moore's claim that the distinction between public and private events is not ontological but is derived from a (pragmatic) concern with ease of access to the events in question. For the radical behaviorist, therefore, public events are not *really* public and private events are not *really* private. Indeed, as I have just shown, a radical behaviorist argument may be mounted that all behavioral events, in one sense, are private, but on balance when we hold on too tightly to this argument the argument itself is undermined. For the radical behaviorist, therefore, any distinction that is made between or among types of behavioral events should not be ontological in a final or absolute sense. Rather, any verbal distinction should be assessed relative to the use that distinction has in helping the researcher to achieve specific analytic goals. Sometimes the distinction might be useful in this regard, and sometimes it may not.

Conclusion

The radical behaviorist approach to biological and private events clearly differentiates it from logical behaviorism and conceptual analysis as discussed by Moore. Unlike radical behaviorism, the latter traditions are both focused on identifying the publicly observable, ontological referents for specific psychological terms, and thus they are underpinned by an implicit and often explicit realist philosophy. From this perspective, the science is driven not by the personal analytic goals of the scientist but by the assumption that scientific activity will eventually reveal the true nature of reality. Pragmatic strategies may sometimes be used within these latter traditions, but upon close inspection they are often firmly based on realist assumptions (Hayes, 1992). In short, radical behaviorism is a-ontological and intensely pragmatic, whereas the latter behaviorisms are intensely ontological and a-pragmatic.

References

- Barnes, D., & Roche, B. (1997). A behavior-analytic approach to behavioral reflexivity. *The Psychological Record*, 47, 543-572.
- Barnes, D., Smeets, P. M., & Leader, G. (1996). Procedures for generating emergent matching performances: Implications for stimulus equivalence. In T. R. Zentall & P. M. Smeets (Eds.), *Stimulus class formation in humans and animals* (pp. 153-171). Holland: Elsevier.

- Barnes-Holmes, D. (2000). Behavioral pragmatism: No place for reality and truth. *The Behavior Analyst*, 23, 191-202.
- Catania, A. C., & Harnad, D. (Eds.). (1988). *The selection of behavior: The operant behaviorism of B. F. Skinner: Comments and controversies*. Cambridge: Cambridge University Press.
- DiFore, A., Dube, W. V., Oross III, S., Wilkinson, K., Deutsch, C. K., & McIlvane, W. J. (2000). Studies of brain activity correlates of behavior in individuals with and without developmental disabilities. *Experimental Analysis of Human Behavior Bulletin*, 18, 33-35.
- Hayes, L. J. (1992). Reality and truth. In S. C. Hayes, L. J. Hayes, H. W. Reese, & T. R. Sarbin (Eds.), *Varieties of scientific contextualism* (pp. 35-44). Reno, NV: Context Press.
- Hayes, S. C., & Brownstein, A. J. (1986). Mentalism, behavior-behavior relations, and a behavior-analytic view of the purposes of science. *The Behavior Analyst*, 9, 175-190.
- Moore, J. (1981). On mentalism, methodological behaviorism, and radical behaviorism. *Behaviorism*, 9, 55-77.
- Skinner, B. F. (1974). *About behaviorism*. London: Jonathon Cape.