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THE UNCONVENTIONAL WISDOM OF B. F. SKINNER: THE ANALYSIS-INTERPRETATION DISTINCTION

JOHN W. DONAHOE

UNIVERSITY OF MASSACHUSETTS-AMHERST

More of the points made in John Staddon's commentary merit attention than can be addressed here. Accordingly, I restrict myself to one issue that, in my view, makes contact with most of the points—Skinner's distinction between the experimental analysis and the scientific interpretation of behavior.

For Skinner, science consists of two interrelated enterprises. The first is the experimental analysis of the subject matter of the science. In order to meet fully the demands of experimental analysis, all of the efficacious antecedents of the events under study must be independently manipulated or controlled (or, such conditions may be approximated in nature, as in celestial mechanics) and the events themselves must be directly observed and measured. Although these idealized conditions are never completely achieved, experimental analysis seeks to approximate them as closely as permitted by technological developments and theoretical limitations (as in Heisenberg uncertainty) (Skinner, 1957a, 1966). Thus, all worthy experiments are not experimental analyses, and all experimental analyses are not experiments.

The second aspect of the scientific enterprise is interpretation. In interpretation, principles induced from experimental analyses and constrained by formal (i.e., logical/mathematical) considerations are used to provide an account of events that occur under conditions that preclude experimental analysis. As Skinner (1974) put it: "Obviously we cannot predict or control human behavior in daily life with the precision obtained in the laboratory, but we can nevertheless use results from the laboratory to interpret behavior elsewhere" (p. 251). Scientific interpretation should be distinguished from other explanatory efforts in that interpretation makes use of *only* principles that are the fruits of experimental analysis. This distinguishes interpretation in behavior analysis from superficially similar activities in psychology in which complex events are discussed in terms of processes and structures inferred from observations of the complex events themselves (cf. Donahoe & Wessells, 1980, pp. 63–64; Donahoe & Palmer, in press). Because the conditions required for experimental analysis are more the exception than the rule, the greater part of the scientific enterprise is interpretation. Indeed, the greater part of Skinner's writings are interpretive rather than experimental-analytic (e.g., Skinner, 1953, 1957b, 1971).

The relation between experimental analysis and interpretation assumes special characteristics in the historical sciences, of which the sciences of cosmology, evolutionary biology, and behavior are examples (Donahoe & Palmer, 1989, in press). In the historical sciences, complex phenomena are the cumulative products of the action of processes acting on initial conditions and in sequences that are incompletely known. Because of imperfect knowledge, interpretations in historical science rarely yield accounts in which the events under consideration are *necessary* consequences of the processes identified by experimental analysis. Instead, the principles ordinarily provide accounts that are *sufficient* to accommodate the observed events (cf. Anderson, 1978; Donahoe & Palmer, in press). A further characteristic of interpretation in some historical sciences is, as Staddon notes, that the particular sequence in which past processes occurred affects the manner in which contemporaneous events affect present processes. Thus, both birds and bats fly by moving their forelimbs, but the same present environmental conditions may affect their flying differently because of their different evolutionary histories. Similarly, a given event may function as a reinforcer at one time but not at another for the same organism.

I now consider some implications of the analysis–interpretation distinction for the

Address correspondence to John W. Donahoe, Department of Psychology, University of Massachusetts-Amherst, Amherst, Massachusetts 01003.

treatment of three points raised in Staddon's commentary.

1. Behavior analysis restricts itself to observable events, whereas "even Darwin was not averse to postulating entities that are not directly observable" (Staddon, p. 440).

Although *experimental analysis* does indeed restrict itself to observed events, *scientific interpretation* does not. Interpretation may have recourse to unobserved events if (a) events of that type have previously been subjected to experimental analysis, (b) the antecedents of the interpreted behavior include conditions sufficient for the occurrence of the unobserved events when such events were observed, and (c) the characteristics of the unobserved events and their contributions to ongoing processes are confined to those that have already been demonstrated when such events were observed. For instance, I assume that Staddon has a brain even though I cannot directly observe it. I may confidently interpret his verbal behavior as the product of mediation by his brain because, upon previous occasions in which an organism has emitted verbal behavior of such high order, that organism has been observed (upon X-ray or autopsy) to have a brain.

Two additional comments should be made in connection with observability. First, Darwin's greatest error occurred when he departed from experimental analysis and inferred unobserved entities, the "gemmules" of his theory of heredity. Because gemmules implemented what is known as the "blending" theory of heredity, Darwin's own theory was inconsistent with evolution by natural selection, as the Scots engineer Fleeming Jenkin was quick to point out. Second, Skinner explicitly denied that operational definitions, for which observability of terms is required, were sufficient for scientific definitions.

> The public-private distinction emphasizes the arid philosophy of "truth by agreement." . . . The ultimate criterion of the goodness of a concept is not whether two people are brought into agreement but whether the scientist who uses the concept can operate successfully upon his material. . . . What matters . . . is whether he is getting anywhere with his control over nature. (Skinner 1945, p. 293)

Thus we cannot "define things in any way we please" (Staddon, p. 441).

2. "In the experimental analysis of behavior, *history* almost invariably refers only to present, or recent, environments—not to events in the remote past" (p. 439). This view of behaviorism leads to a conception of the organism as "simply the passive confluence of forces, like a Ouija® board pushed by intoxicated seancers" (p. 446).

Although the first statement is generally correct, it is beside the point. Although the exacting demands of *experimental* analysis typically permit only a relatively small portion of the organism's history to be manipulated and measured, the *interpretation* of behavior often extends to the distant past of the organism. Certainly this was the case in Skinner's interpretations of human behavior (e.g., Skinner, 1953, 1957b). The second statement is contradicted at many places in Skinner's writings. As but one example, the opening sentence of *Schedules of Reinforcement* (Ferster & Skinner, 1957) reads: "When an organism acts upon the environment in which it lives, it changes that environment in ways which often affect the organism itself" (p. 1).

3. "The opposition between organism-based and environment-based theories is only a difference of emphasis. . . ." This claim is stated very broadly but is supported more narrowly using models of discriminative behavior in which an "internal state" is identified as a relation among several environmentally defined variables. The resulting account is characterized as "both, depending on how you describe it. It is organism based if you focus on the fact that the behavior of the (model) organism depends on an internal state defined by [environmental variables]. But it is equally an environment-based explanation if you focus on the fact that these state variables can be computed from the animal's past history" (Staddon, p. 444).

These assertions raise a number of fundamental questions, only two of which are considered here. First, what is the status within behavior analysis of a variable that is defined as a function of other variables that refer, at least in principle, to observable events? Such variables—here called derived variables—present no special conceptual difficulties for behavior analysis. To the extent that derived variables aid the scientist to "operate successfully upon his material" (Skinner, 1945, p. 293) (i.e., to yield parsimonious and orderly functional relations between the environment and behavior) they are useful. Indeed, instances of such derived variables abound in behavior analysis (e.g., ratios of stimulus duration to intertrial interval duration, relative response rates, etc.) just as they do in other sciences (e.g., density as the ratio of mass to volume).

Second, Staddon seems to claim that derived variables may thought of as residing *within* the organism. What else can be made of the statement that they provide "organism-based" as contrasted with "environment-based" explanations? If this view of Staddon's position is accurate, then it is inconsistent with both the experimental-analytic and interpretive aspects of behavior analysis. Derived variables do not exist in the physical world apart from the behavior of the scientist who constructs them (i.e., they are instrumental fictions). Hence, derived variables cannot be found within the organism, or anywhere else except in the scientist's verbal behavior.

How are we to understand the attraction of "organism-based explanations" for Staddon? The motivation for "internal states" appears to lie in the contentions—with which I concur—that (a) "the 'internal state' of any black-box system cannot be known *except* through knowing its history" (p. 446) and (b) without knowing the internal state of some historical systems, it is not possible to predict the effects of present environmental conditions on the behavior of that system (cf. Donahoe & Palmer, 1989). As Staddon puts it, "the same set of experimental manipulations . . . may produce different results . . . if applied at t_1 than at t_2. The point is that the future behavior of a historical system cannot be predicted from observables alone" (pp. 440–441). Staddon's proposed solution to these difficulties is to infer from environmental and behavioral observations the nature of the "internal state." Unfortunately, this is the same flawed strategy pursued by cognitive psychology and, in my view, has all the prospects of jumping on a bandwagon pulled by dying horses.

If internal states cannot be validly inferred from observations of environment and behavior, then what is the solution to the dilemma that Staddon has correctly identified? Staddon inadvertently reveals what I believe to be its solution: "If we disavow an interest in either physiology or mind reading, then we cannot know anything about internal states except through the study of particular histories" (p. 446). But why should we disavow an interest in physiology? It is Skinner's point that, if we are to characterize internal states—and Staddon asserts, and I agree, that we must do so in at least some circumstances—then the experimental analysis of behavior must be supplemented by (not replaced by) the experimental analysis of physiology. Consider the following statements of Skinner, consistent over the years: "if these [he was referring to stimulus classes] are real aspects of behavior, they must also be aspects of the activity of the central nervous system, which it is the business of the reflex physiologist to discover—through some other means, incidentally, than inference from behavior" (Skinner, 1935, p. 61). "The physiologist of the future will tell us *all* [emphasis added] that can be known about what is happening inside the behaving organism. . . . What he discovers cannot invalidate the laws of a science of behavior, but it will make the picture of human action more nearly complete" (Skinner, 1974, pp. 236–237). Thus, internal events must themselves be subjected to experimental analysis as "a necessary condition for the eventual synthesis of the two fields" (Skinner, 1935, p. 61).

The proposal that "internal states" can be inferred from incomplete accounts of the individual and ancestral environments is, at best, a refinement of the cognitive agenda. The continued temptation to circular reasoning is illustrated by the "progression" from the cumulative effects (CE) model, to the exponentially weighted moving-average model, to the cumulative trace (CT) model. None of these models draws upon experimental analyses of internal events, and all of them rest their validity upon the ability to "fit" the data from which they were inferred. In short, the door has been opened for reliability to be mistaken for validity. Because none of these models is based on experimental analysis, none meets the requirements of scientific interpretation. Staddon has correctly identified some limitations of explanations based exclusively on observations of recent environments, but the proposed solution is illusory. What is needed, as Skinner anticipated, is a synthesis of the experimental analyses of behavior and physiology—what may be called a *bio*behavioral approach (Donahoe, Burgos, & Palmer, 1993; Donahoe & Palmer, in press). (Other difficulties with organism-based concepts are discussed elsewhere; see Palmer & Donahoe, 1992.)

To conclude, progress in understanding complex behavior is not impeded by limitations inherent in the philosophy of radical behaviorism. Instead, such problems as exist are largely of two origins: (a) an insufficient appreciation of the distinction between experimental analysis and scientific interpretation in historical science and (b) a mistaken belief that an independent science of behavior is somehow undermined by an appeal to subbehavioral events, even when those events are known through independent experimental analyses. Considered in their entirety, the writings of B. F. Skinner remain our surest guides in the effort to understand complex behavior.

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