

*WHY ENVIRONMENTALLY BASED ANALYSES ARE
NECESSARY IN BEHAVIOR ANALYSIS*

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Both Staddon's article and Branch's original editorial show the need for philosophical clarity within behavior analysis. Staddon raises at least a dozen issues, organized as a statement about the adequacy of behavior-analytic "conventional wisdom." But by failing to be clear about his own underlying assumptions, Staddon has confused logical or empirical issues on the one hand with philosophical topics on the other. I will focus on the central topic Staddon seems concerned about: the need for environmentally based accounts. Contrary to his position, I argue that environmentally based accounts are necessary in behavior analysis for entirely nontrivial reasons.

Behavior-Analytic Environmentalism

Nothing is more characteristic of behavior analysis than its commitment to environmentally based analyses. The commitment has little to do with Darwin, Mach, Newtonian mechanics, or the role of theory versus induction. I believe, and have argued elsewhere (e.g., Hayes, 1993; Hayes & Brownstein, 1986; Hayes & Hayes, 1992; Hayes, Hayes, & Reese, 1988), that it has to do with the inherent contextualistic quality of the approach, when combined with behavioral influence as a consequence one is attempting to produce.

The core of my argument is simple: (a) For a contextualist, truth is a matter of successful working; (b) successful working means the production of a specified consequence; (c) in the area of truth criteria, at least, Skinner is clearly a contextualist; (d) prediction and behavioral control are the consequences Skinner said were important for his science; (e) only environmentally based accounts can directly and completely produce behavioral control as an outcome; (f) Skinner's environmentalism is therefore neither arbitrary nor dogmatic. I will very briefly defend each of these points. I will

quote Skinner often, not because I am appealing to authority, but because his writings are so universally recognized as a major part of the original core of the "conventional wisdom" of behavior analysis.

Contextualism: Points (a) and (b). Unlike all other worldviews, the truth criterion of contextualism is subordinated to something else: the consequences one is trying to produce.

Serious analysis for [the contextualist] is always either directly or indirectly practical. . . . If from one texture you wish to get to another, then analysis has an end, and a direction, and some strands of relevancy to this end and others do not, and . . . the enterprise becomes important in reference to the end. (Pepper, 1942, pp. 250-251)

Analysis ends not with a discovery of the truth, but with the production of verbal constructions that help achieve an effect. Thus, in contextualistic behavior analysis, "truth" is that achievement.

Skinner's embrace of successful working: Point (c). Much can be said about contextualism and whether or not Skinner is usefully viewed as a contextualist (e.g., Hayes et al., 1988; Morris, 1988), but the present argument need not depend upon any special appreciation of Pepper's thinking. Behavior analysts themselves seem clear that the truth of concepts has to do with their utility in producing consequences:

[Scientific knowledge] is a corpus of rules for effective action, and there is a special sense in which it could be "true" if it yields the most effective action possible. . . . [A] proposition is "true" to the extent that with its help the listener responds effectively to the situation it describes. (Skinner, 1974, p. 235)

Because this is the key issue for our purposes, one only need agree on this point to continue through the argument.

Behavioral influence as an outcome of importance: Points (d) and (e). Only certain events can be thought of as "causes" within a be-

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havior-analytic approach, contextualistically conceived. Skinner was clear that he sought analyses that produced certain kinds of effects, and these effects included behavioral influence. For example, he said that an event "is useless in the control of behavior unless we can manipulate it" (Skinner, 1953, p. 34). Searching for organism-based or mental causes is thus a "tiresome and exhausting digression" (Skinner, 1953, p. 35) when considered in terms of the effects of behavior analysts are trying to produce: "mentalism has obscured the environmental antecedents which would have led to a much more *effective* analysis" (Skinner, 1974, p. 165, emphasis added).

The key issue here is the consequences that one is trying to achieve through science, and the key distinction is between mere prediction as a consequence of scientific analysis versus prediction and behavioral influence as integrated consequences. Skinner was extremely clear on this point when discussing the difference between his approach and psychological analyses that are based on mental events or behavioral traits:

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. (Skinner, 1974, pp. 208–209)

Most modern cognitive psychologists seek predictive verification (because most rely on a correspondence-based truth criterion). When prediction is the primary measure of adequacy, particular analytic practices may succeed that could not produce behavioral influence as an outcome. Thus, a completely adequate analysis for a cognitivist will often be incomplete for a behavior analyst. Both are "right," but as measured against different criteria. Purely organismic or even mental theories can in principle work just fine for prediction. If one seeks to change behavior, however, we must have a theory in which psychological adjustments made by the organism can ultimately be traced to manipulable events.

Why Staddon Cannot Distinguish Environmental Analyses from Anything Else

The failure to specify the consequences he is trying to achieve through his analysis leads Staddon to miss the point by conceptualizing the conflict between behavior-analytic and other positions over "environmental analyses" as involving internality, observability, and concepts other than manipulability. For example, he reassures us that "internal states" can be conceptualized historically. Contextualistic behavior analysts assume that *all* psychological events are to be understood as interactions of organisms in and with historical and situational contexts. A person with this view will interpret "internal events"—or any psychological event—contextually. This is a matter of assumption. The conflict about the role of the environment is thus not about denying internal events (or "states") per se.

The idea of "equivalent history" is also not a problem within behavior analysis. All actual histories are unique, and all theories are based on class concepts. Thus, any theory that has scope will organize many unique events (such as the particular set of events we call a "history") into functional equivalents.

What distinguishes adequate from inadequate theory within a behavior-analytic tradition is the degree to which it guides effective action. If you are a behavior analyst for whom prediction and control are consequences of primary importance, then adequate theory is based on events that are manipulable, at least in principle, and no theory will have been "proven" until successful behavioral influence in terms of the theory is shown. A theory that cannot tell us specifically how to produce equivalent histories via manipulation of events is not adequate as measured against prediction and control as consequences.

Consider this statement about the cumulative effects (CE) model from Staddon's paper: "Given the model, plus the results of a test, we can therefore estimate the state—hence the model's future behavior—without knowing the details of its past history. This is a direct, practical benefit . . ." (p. 445). Staddon goes on to note that although such a model would probably be considered environmentally based "because its state is so closely linked to observable behavior," it is actually equivalent to a mem-

ory trace model, which no one would call environmentally based. He concludes with a key claim: "The point is that the environment-based versus organism-based distinction is often impossible to make in practice" (p. 446).

We can restate the above paragraph as follows: (a) The CE model permits prediction; (b) it is "environmental" because it is observable; (c) it is the equivalent of cognitive models that are clearly not "environmental"; therefore, (d) one can't distinguish an environmentally based theory from anything else. This conclusion follows (but for different reasons than those Staddon lists) if we simply measure theories against prediction alone. This is why behavior analysts sometimes "go cognitive" when they haven't developed their own philosophical assumptions carefully. If predictive verification is the only consequence of importance, very often the best way to predict future behavior is to note present behavior, and often this is more elegantly done within a cognitive than a behavioral position. Distinguishing environmental from nonenvironmental theories thus becomes arbitrary.

But the conclusion is incorrect. There is in fact a very reliable way to distinguish an environmentally based theory from anything else: attempt to influence behavior by *directly* manipulating *nothing* other than the variables specified in the theory. Because no behavioral event can be manipulated directly, if the theory tells you precisely how to do this, it is environmentally based. "Our 'independent variable'—the causes of behavior—are the external variables of which behavior is a function" (Skinner, 1953, p. 35).

Within behavior analysis, *environmental* does not always refer to the world outside the skin (for that reason Staddon's use of the word *internal* is a distraction). Rather, an environmental event is part of the manipulable world outside of the behavior we are interested in (most of which, of course, is outside the skin). This manipulable world is never inside the behavior of the organism one is interested in, no matter how much the *dependent* variables

of psychology (behavioral interactions) are dressed up in cognitive clothing, emotional entities, response states, or mathematical formulae.

I am not sure if the CE model can pass the test of behavioral control, but it appears to me that it does not yet do so. It does not say precisely where the initial values come from. Until it does so it may be worthwhile for prediction, but it is also incomplete, as measured against behavioral control as an outcome. If this is correct, the theory is not fully "environmentally based," despite its behavioral appearance. To rework an earlier quote from Skinner, internal states "are . . . useless in control but they permit us to predict one kind of behavior from another kind" (Skinner, 1974, p. 209). It should not be surprising if some seemingly behavioral theories cannot be distinguished from cognitive theories in instances in which the important outcomes and philosophical base of both are the same.

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