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ON THE RATIFICATION OF STATES: A RESPONSE TO STADDON

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In his essay, Staddon takes issue with four practices that he identifies as characteristic of behavior analysis. Although the conventional-wisdom status of three of his attributions could be questioned, the fourth and most heavily emphasized certainly could not. Regarding the elevated status of environment-based accounts of behavior, Staddon argues that a more informed approach would respect the utility of organismic states as explanatory devices. For many readers of this journal, the crux of the issue will lie in the utility of internalizing determinants of behavior for furthering an experimental analysis. After carefully considering Staddon's arguments, I remain unconvinced that an increased acceptance of "state" explanations would be of benefit. In this brief commentary, I will address why each of Staddon's major arguments in favor of state explanations seems less than compelling.

Equivalent histories. Staddon argues that a state explanation has benefits beyond the mere reiteration of an organism's specific behavioral history because a *state* is more encompassing. Here, summary statements and internal states are treated as if one necessarily implied the other, but these are actually independent dimensions of a theoretical account. Thus, when descriptive equations are reified as internal mechanisms, some justification beyond that of summarizing is required.

Indeed, summarizing histories in terms of their most critical behavior–environment regularities is a defining characteristic of Skinner's scientific strategy. (According to Skinner, e.g., 1947, 1950, organization and integration beyond the individual facts of behavior, but not beyond the level of behavior, are the necessary goals of our science. Thus, the "antitheoretical" position of behavior analysis is often misrepresented.) As functional units of behavior–environment interaction are experimentally identified, the infinite range of historical variants can be reduced to more meaningful order. Optimally, the interactions will be expressed in a minimal number of quantitative terms. At issue here, then, is not the utility of quantitative descriptions. The advantages of summary statements are certainly not the unique province of internal states, and the rationale for internalizing an environmental history has received no explicit support from this segment of Staddon's argument.

Initial conditions. A state explanation is also said to go beyond mundane historical cataloguing in the sense that certain key conditions of the model/state can be known only through estimates from the data. Even in principle, these conditions cannot be directly observed or measured, only inferred. But, does the necessary inclusion of estimated terms imply that the model/state is really "more than the history" or, alternatively, that the model represents a less-than-complete history, in that controlling variables have not been fully identified? (This seems to be a particularly important issue when the values assigned to estimated terms influence the forms of predicted functional relations, as well as their absolute values.) Although estimates might be as necessary for quantitative environment-based descriptions as for state explanations, the potential for ending an experimental analysis prematurely seems more pronounced when explanations are assumed to exist within the organism from the outset.

Further, whenever at least part of the "explanation" for a behavior pattern must be derived from the very data the theory attempts to explain, the possibility of tautological restatement should be considered. "Conventionally," the questions of greatest interest to behavior analysts have involved how specific dimensions of behavior come to be; accounts that remain silent on the origin of critical explanatory structures are insufficient for such

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interests. Of course, it may be argued that there is real benefit to model/states that allow treatment of behavior prior to complete specification of all controlling variables, but that issue bears more directly on the third suggested advantage of internalized states.

Prediction in the absence of historical information. A third argument offered in favor of state models is that, armed with the model and a current test of behavior, the state may be derived at any time, allowing accurate prediction of future behavior without the tedious necessity of tracking historical details. That there are important practical benefits of a low response-cost basis for prediction is a given. Indeed, the efficacy of accurate prediction as a potent reinforcer for scientific behavior may be seen in the very frequency of correlational approaches to the study of behavior. However, the ability to predict need not imply that controlling variables have been delineated (particularly when predictions are made from "effect to effect"; Skinner, 1953, p. 199).

Further, when prediction is what a model is good at, the experimental questions asked of that model may come to be exactly those that are satisfied by predictions, as opposed to questions concerning the variables that gave rise to a particular outcome. In short, the predictive abilities of a state model may shape the direction of research programs that follow from it, and in a direction that is inconsistent with conventional interests of behavior analysts. A model that is simultaneously good at predicting and at avoiding direct observation of functional relations seems unlikely to generate research that reveals previously unnoted and perhaps critical behavior–environment interactions. Again, the state approach appears ill-suited to the particular interests of behavior analysis.

A case in progress? Recently within behavior analysis, considerable theoretical and empirical attention has been generated with respect to the topic of stimulus equivalence. In early reports of stimulus equivalence (e.g., Sidman & Tailby, 1982), emergent properties of reflexivity, symmetry, and transitivity were demonstrated following arbitrary match-to-sample training with human subjects. *Stimulus equivalence* was a descriptive, summary term for a set of interrelated stimulus-control relations, an intriguing, functionally defined phenomenon, *to be explained.*

Subsequent study indicated that equivalence patterns could be generated by a number of different training histories (e.g., Saunders, Wachter, & Spradlin, 1988; and even in the absence of explicit differential reinforcement, e.g., Harrison & Green, 1990). Thus, a set of equivalent histories was identified. Interestingly, as few procedures failed to produce equivalence (at least with human subjects), equivalence classes came to be increasingly spoken of as entities, "things" that subjects "had" or that "existed" (even prior to being tested; see McIlvane & Dube, 1990), with "properties" or "networks" that, in turn, controlled choice patterns. The equivalence "model" also proved to be powerfully predictive. Given the model and the results of a test, equivalence status could be determined, and future performances under a variety of conditions (e.g., class expansion, Sidman, Kirk, & Willson-Morris, 1985; and contextual training, Bush, Sidman, & de Rose, 1989) could be specified very accurately, even though the responsible historical variables were not completely determined. Thus, the treatment of equivalence has sometimes had much in common with a state approach.

Several trends in equivalence research are also worth noting. For example, focus on the organization of equivalence classes has increased, and not always in relation to environmental variables. In numerous demonstration studies, no true independent variable is manipulated, and tests of untrained stimulus relations may seem designed more to reveal underlying processes than to evaluate functional relations responsible for observed structure. (The actual tests have been viewed as explicitly independent of the provenance of equivalence.) Similarly, reports of the details of training and testing procedures can be quite abbreviated (this fact appears most obvious to researchers just beginning equivalence work), and data on acquisition of conditional discriminations, the theoretical basis of derived relations, are not often reported. Admittedly, there are limits on what can be included in any one report. Nevertheless, a decreased emphasis on behavior–environment interactions seems to characterize a surprising proportion of equivalence research.

Concurrent with the shift in methodological emphasis, conceptual treatment of equivalence also appears to have made a rather dramatic transition for some behavior analysts. Instead

of being an important phenomenon to explain, equivalence has sometimes become the explanation of important phenomena, with examples ranging from reference to rule governance. In such accounts, the explanatory status of equivalence is state-like in that its presence must be inferred and that it is not directly manipulable. In theoretical accounts of equivalence as an underlying organism-based variable, we must recognize the implicit assumption that behavior emerges from the subject and not from an explicit history. (To my knowledge, there has been no formal attempt to outline the adaptive significance of equivalence relations.)

The point of this discussion is not to deny the import of the work characterized above. In fact, many significant dimensions of equivalence patterns have been revealed, and the theoretical accounts may very well be correct. However, with respect to the acceptance of state explanations, there may be two lessons here. First, I see no evidence that an organism-based approach has been necessarily rejected by behavior analysts. Second, trends in equivalence work suggest that such an approach may shift attention away from the study of behavior–environment relations and the question of how equivalence performances develop.

Logical versus functional equivalence. Staddon argues that states cannot be rejected in favor of explicit histories because a state can be defined only in terms of its history. (Apparently, estimating states in the absence of historical data is incidental to their more formal identification. However, there does seem to be a certain inconsistency in identifying states as histories on the one hand, and extolling states as "more and less than history" on the other. Except at the most general level, then, the logical equivalence of states and histories might be questioned.) At the same time, Staddon is certainly not the first to point out such equivalences. Skinner (e.g., 1953, 1954, 1978) and others (e.g., Branch, 1977; Hineline, 1990) have frequently noted that much of organism-based explanatory terminology may be viewed more appropriately as reified environmental histories and the patterns of responding that they produce. One might argue that in this sense, *logical* equivalence has been a part of our conventional wisdom for quite some time.

If environment- and organism-based accounts are accepted as equally explanatory, what then of Occam's razor? Apparently, justification by parsimony is insufficient because the former approach may seem "narrow and doctrinaire," constraining experiment and theory. It is true that the experimental demonstration of controlling variables is a stringent criterion by which to judge a theoretical account. However, the only real constraints imposed by this approach are that a variable be directly manipulable and that some measurable dimension of individual subject performance be seen to vary as a result. This is sound scientific practice, and once accomplished, interpretations appealing to the type of relation thus verified have a much wider latitude (Branch, 1992; Hineline, 1984), as witnessed in the pages of *JEAB*. Researchers who are alienated by these "constraints" may be one indication that there are differences between organism- and environment-based approaches that go beyond their logical equivalence.

Most important, logical equivalence between environment- and organism-based accounts in no way necessitates functional equivalence with respect to scientific practices. Another long-standing wisdom of behavior analysis is that organism-based theories often give rise to experimental questions and methodologies very different from those occasioned by environment-based accounts. In the extreme case, behavior comes to be studied because of what it reveals about processes occurring at another level, rather than as the focus of interest in its own right, and independent variables can suffer a similar loss of status in the shift from controlling variable to state modulator. This dissolving focus on behavior–environment interactions was exactly Skinner's objection to theory at a level other than behavioral, and I have tried to show that the concern is still warranted. The possibility that organism-based accounts might instigate such a trend argues against loaded "shorthand" descriptions of behavior determinants, even for readers who should be "alert to what is intended."

Conclusion. I fully agree with Staddon on the need to incorporate history (current and past) more formally into accounts of behavior. Histories do indeed change the organism, but at this time, all we can know is that the organism is changed behaviorally. Thus, it seems neither necessary nor particularly beneficial to provide histories with internal representations.

REFERENCES

Branch, M. N. (1977). On the role of "memory" in the analysis of behavior. *Journal of the Experimental Analysis of Behavior,* **28,** 171–179.

Branch, M. N. (1992). On being narrowly broad. *Journal of the Experimental Analysis of Behavior,* **57,** 1–4.

Bush, K. M., Sidman, M., & de Rose, T. (1989). Contexual control of emergent equivalence relations. *Journal of the Experimental Analysis of Behavior,* **51,** 29–45.

Harrison, R. J., & Green, G. (1990). Development of conditional and equivalence relations without differential consequences. *Journal of the Experimental Analysis of Behavior,* **54,** 225–237.

Hineline, P. N. (1984). Editorial. *Journal of the Experimental Analysis of Behavior,* **41,** 1–2.

Hineline, P. N. (1990). The origins of environment-based psychological theory. *Journal of the Experimental Analysis of Behavior,* **53,** 305–320.

McIlvane, W. J., & Dube, W. V. (1990). Do stimulus classes exist before they are tested? *Analysis of Verbal Behavior,* **8,** 13–18.

Saunders, R. R., Wachter, J., & Spradlin, J. E. (1988). Establishing auditory stimulus control over an eight-member equivalence class via conditional discrimination procedures. *Journal of the Experimental Analysis of Behavior,* **49,** 95–115.

Sidman, M., Kirk, B., & Willson-Morris, M. (1985). Six-member stimulus classes generated by conditional-discrimination procedures. *Journal of the Experimental Analysis of Behavior,* **43,** 21–42.

Sidman, M., & Tailby, W. (1982). Conditional discrimination vs. matching to sample: An expansion of the testing paradigm. *Journal of the Experimental Analysis of Behavior,* **37,** 5–22.

Skinner, B. F. (1947). Experimental psychology. In W. Dennis et al. (Eds.), *Current trends in psychology* (pp. 16–49). Pittsburgh, PA: University of Pittsburgh Press.

Skinner, B. F. (1950). Are theories of learning necessary? *Psychological Review,* **57,** 193–216.

Skinner, B. F. (1953). *Science and human behavior.* New York: Macmillan.

Skinner, B. F. (1954, November). A critique of psychoanalytic concepts and theories. *Scientific Monthly.*

Skinner, B. F. (1977). Why I am not a cognitive psychologist. *Behaviorism,* **5,** 1–10.