On Skinner's Rejection of S-R Psychology

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Although I admire Moxley's scholarship, I do not agree with his claim that Skinner's position evolved gradually over several decades from that of S-R psychology to the view that behavior is selected by its consequences and is evoked only probabilistically by many interacting variables. Specifically, I believe that this change occurred rapidly, in a few saltations, in the mid-1930s and that he did not substantially alter his views thereafter.

We differ in our interpretation of the role of the environment for Skinner before and after this change. Moxley states that in following stimulus-response psychology, Skinner was "follow[ing] Watson and the conditioned reflex rather than Russell and the law of effect" (p. 76), but I contend that it is a mistake to identify S-R psychology with respondent conditioning or with a theory of stimulus substitution. For Skinner, the defining feature of S-R psychology was that responses and stimuli are linked in pairs, like the two terms of a reflex, that every response could be predicted from its corresponding stimulus and that the observation of a response allowed one to infer the occurrence a prior stimulus. Thus he assumed in his doctoral thesis that "every movement of an organism was in response to a stimulus" (cited in Skinner, 1979, p. 102). Clearly Skinner did not think the law of effect was inconsistent with this stance. Rather, in his first published studies of instrumental behavior, he interpreted reinforcement as the strengthening of the S'-R' reflex, that is, the putative reflex of which the instrumental response was

assumed to be a part (1932, p. 274). Thus he saw the lever as an eliciting stimulus and lever pressing as a reflexive response. Indeed, Thorndike himself interpreted the results of his puzzle box experiments as the stamping in of analogous S-R associations (Thorndike, 1911).

However, Skinner's satisfaction with this formulation diminished as he continued to apply it to his data. Very early he discovered that a rat's behavior can change in orderly ways even in a constant environment. A food-deprived rat will visit a food bin less and less frequently, at a predictable rate of change, as it consumes pellets of uniform size (Skinner, 1930). Of course from the rat's point of view the environment is not perfectly constant, but changes as it moves about the chamber, as it sniffs, as it shifts its gaze. But Skinner found that the orderliness of behavior appeared to be independent of these presumed stimulus changes. The rate of responding was not merely a function of eliciting stimuli, but of what he called "third variables" (1931, p. 452). "The only trouble was," Skinner noted, "in a true reflex, rate was no measure at all; it was determined by the rate of stimulation" (1979, p. 69).

His lever-pressing experiments strained the formulation further: "The stimulation arising from the lever did not seem to 'elicit' pressing as a shock elicited flexion or a bell the flow of saliva. The stimuli did not start and stop; I could not turn them on or off; I could not see them act" (1979, p. 102).

However, it appears to have been Skinner's studies of discrimination that finally led him to abandon his allegiance to the reflex. He noted that, "The stimulus in stimulus-response psychology had long since succumbed

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to theoretical fudging. In order to support the principle of 'no response without a stimulus' . . . various internal states had been included in a total stimulating situation" (1979, p. 143). When he had been free to assume the existence of eliciting stimuli, Skinner had found it possible to fit his data to the Procrustean bed of the reflex, but discriminative stimuli were all too real. Their onset and offset were not hypothetical; they could be controlled precisely but did not bear the fixed relationship to responding expected of eliciting stimuli. Stimulus control could vary over a wide range according to schedules of reinforcement, extinction, deprivation, and so on. That is, the orderly changes in behavior were often a function, not of its antecedents, but of other variables.

I was obviously abandoning a stimulus-response psychology. The pseudo-reflex [discriminated operant] was the entering wedge of a new formulation according to which the greater part of the behavior of an organism was under the control of stimuli which were effective only because they were correlated with reinforcing consequences. The control they exerted was more subtle than "elicitation" and capable of modulation over a much wider range. (1979, p. 143)

Skinner's main conceptual contribution in this regard, then, was not a renewed emphasis on the law of effect but a clarification of the nature of stimulus control: The environment controls behavior probabilistically, over a wide range, with experimental and predictive control lying in "third variables," such as reinforcement, establishing operations, and so on. As Skinner himself emphasized (1938, p. 20), this new conception did not mean that there were no antecedents evoking each bit of behavior, but that they could often be neglected as independent variables. That is, the experimenter could not specify them and had no need to do so; one could demonstrate control by manipulating consequences and leaving antecedents experimentally constant. It was at this point and in this way that Skinner finally broke free from S-R psychology.

It is perhaps worth emphasizing that the orderly control of behavior in a stable environment by contingencies of reinforcement is not a theory but an empirical fact. As such, it is compatible with various theoretical interpretations. Some have suggested that the fundamental effect of reinforcement is simply to strengthen behavior, possibly irrespective of antecedent events (Kemp, 1997; Stein, 1997); others have suggested that that reinforcement only alters the stimulus control of behavior, where *stimuli* include all sensory events within and without the skin (Donahoe & Palmer, 1994; Donahoe, Palmer, & Burgos, 1997). According to the latter view, behavior change is possible even if the environment is held constant, because the control exerted by this environment will vary along a gradient of probability according to contingencies of reinforcement, deprivation, and so forth. This view sees behavior evoked by its antecedents, but differs from S-R psychology by attributing variability in behavior not only to changes in stimulation but also to changes in the strength of stimulus control as a function of other variables. (We see this position as entirely compatible with Skinner's, if not explicitly endorsed by him.) However, regardless of one's theoretical stance, the empirical fact that consequences alter behavior in an unchanging context cannot be ignored. It is no longer possible to defend a pure S-R formulation that assumes a correlation between stimulus change and behavior change and omits reference to the effects of other variables on response probability.

For this reason, I feel that Moxley has misunderstood Skinner in attributing to him a lingering commitment to S-R psychology as late as 1956. His break from that paradigm had been forced by the data, and it would have been anomalous for him to recant. I do not see the diagram in Moxley's Figure 1 (Skinner, 1956) as an endorsement of S-R psychology. It is true that only two terms are shown in the diagram, but the first term includes both ambient

stimuli and independent variables such as establishing operations and contingencies of reinforcement, and, as is clear from the text, it includes historical variables as well, both phylogenetic and ontogenetic. This was not the paradigm that Skinner abandoned in the 1930s. When we say that behavior is controlled by its consequences, we mean, of course, that it is controlled by a history of consequences—now antecedents of the present instance of behavior. The third term in the three-term contingency plays no role in explaining the prior response, only in explaining future instances of similar responses. At any moment behavior is indeed a function of its antecedents, but among those antecedents is a history of contingencies of reinforcement.

As for Skinner's putative change in emphasis from single discriminative stimuli to complex settings, I find it hard to believe that Skinner, early in his career, failed to appreciate the complexity of control by the natural environment. Certainly *Verbal Behavior* (1957), which he began composing in 1934, following a conversation with Whitehead, shows no such deficiency. I believe the difference in emphasis reflects Skinner's shift of interest from experimental analysis to interpretation. In the interest of experimental control, Skinner manipulated only one, or only a few, discriminative stimuli in his basic research, but when speculating about human behavior he seems to have been sensitive to multiple sources of control. This sensitivity is reflected in his paper on the operational analysis of psychological terms (1945), in which he identifies a number of overlapping ways in which the verbal community can teach one to tact private events; it can be seen in *Walden Two* (1948), in which it is observed that there are many variables that jointly contribute to the happy outcome of their child-raising efforts.

Finally, I do not believe that Skinner's selectionism implied an abandonment of determinism. Perhaps prior design is implicit in Laplace's determin-

ism, but I know of no evidence that Skinner believed this. Rather, he seems to have endorsed determinism only in antithesis to free will, the notion that behavior cannot in principle be predicted and controlled because it is a product of something outside of natural law. Selectionism is just a naturalistic explanation for complexity, and is fully compatible with the assumption that there are no occult forces at work in nature. In *The Behavior of Organisms* he wrote,

The work is "mechanistic" in the sense of implying a fundamental lawfulness or order in the behavior of organisms. . . . It is not necessarily mechanistic in the sense of reducing the phenomena of behavior ultimately to the movement of particles, since no such reduction is made or considered essential; but it is assumed that behavior is predictable from a knowledge of relevant variables and is free from the intervention of any capricious agent. (1938, p. 433)

The uncertainty of our observations of subatomic particles may be problematic for the determinism of Laplace, but it left Skinner untroubled:

Since human behavior is enormously complex and the human organism is of limited dimensions, many acts may involve processes to which the Principle of Indeterminacy applies. It does not follow that human behavior is free, but only that it may be beyond the range of a predictive or controlling science. Most students of behavior, however, would be willing to settle for the degree of prediction and control achieved by the physical sciences in spite of this limitation. (1953, p. 17)

In this passage Skinner reveals himself as a pragmatist, with little interest in the fine print of a theory of determinism. His increasingly frequent references to evolution in later years may have reflected not a shift in his position, but an attempt to remind a skeptical public of the generality and power of principles of selection.

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