Skinner May Be Difficult, But . . .

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It is true that Skinner's work is subject to misinterpretation, but my assumption has always been that misinterpretation resulted largely from a tendency on the part of other authors to read or reference Skinner too selectively, thus failing to take into account more of his writing and the contexts in which the writing occurred. Moxley's paper suggests that just the opposite may be true, that abundant familiarity with the details of Skinner's and others' writing can lead to confusion and misinterpretation. Whether the confusion of others over Skinner's work followed the torturous path laid out by Moxley is debatable, however. My guess is that misinterpretations do not usually reflect readers' detection of subtle *changes* in Skinner's writing over the decades, but rather reflect the fact that culture does not prepare most of us for such strange notions as Skinner's. I am not even convinced that there *were* important changes in Skinner's words, at least not in the ones upon which a current radical behaviorism is based.

That said, I quite enjoyed reading Moxley's scholarly work, and although I am not wholly convinced of his assertions, I am unable to amass the references with which to argue against them very forcefully. Admittedly, then, my comments boil down to saying, "That's not how I see it," several times. Here are my disagreements:

1. There is an important sense in which Skinner developed from reflexologist to *Skinnerian*, but this development was clearer and occurred more rapidly than suggested by Moxley.

2. The power of the concept of the *discriminative stimulus* did not wane over the decades of Skinner's writing, and the more inclusive *setting* lacks utility when speaking about most behavioral research.

3. The argument that Skinner moved from determinism to random variation as a foundational principle is overstated; much of Skinner's research concerned the role of random variation on operant behavior.

Regarding Disagreement 1: Moxley's careful history is interesting, and it certainly makes the point that Skinner's writing about the operant changed, particularly during his early research career. I had interpreted those changes (found particularly in Skinner, 1931, 1935a, 1935b, 1937) as a more-or-less clear development of Skinner's thought from a reflex-based characterization of behavioral units of analysis to the three-term contingency. Also, I had assumed that that development was fairly complete by the time *The Behavior of Organisms* was published (Skinner, 1938). Finally, I had taken Skinner at his word (1956) that his early development as a scientist was guided primarily by his contact with his procedures, subjects, and data. That development is marked by apparent discomfort from the start with the reflex model's applicability to operant behavior. Although Skinner (1935b) initially characterized Type I (i.e., operant) conditioning as the conjunction in time of two reflexes (lever → press and food → salivate, eat), he immediately introduced complexity at odds with a reflex formulation by describing the "pseudo-type" of reflex. The pseudo-type has come to be known as a discriminated operant, in which, for example, the presence or absence of a

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light is associated with reinforcement or extinction following a lever press. Skinner's point (1935b) was that it was a mistake to refer to the light as a conditioned stimulus for lever pressing, because to do so is to ignore the process of discrimination (in which the lever $\rightarrow$ press reflex is extinguished in the absence of the light) that must take place for a light $\rightarrow$ press relation to exist. The light $\rightarrow$ press relation was referred to as a "discriminative relation" (*Cumulative Record*, 1972, p. 484), thus foreshadowing the three-term terminology that would be one of Skinner's most important legacies.

Interestingly, Skinner applied the pseudo-type model to Type II, or respondent conditioning, as well, and the result is what is currently studied extensively using respondent conditioning procedures known as "occasion setting" (Rescorla, 1985). Research on occasion setting suggests strongly that discriminative processes in operant and respondent conditioning overlap each other considerably and may be identical. An argument against Moxley's assertion that Skinner was a reflexologist well into the 1950s might be that Skinner never asserted that Type I/operant and Type II/respondent conditioning were identical processes.

Moxley puts considerable weight on the historical development of Skinner's diagrams in which lines or arrows connect behavioral and environmental terms. In the first instance in which Skinner diagrammed the operant relation, there were, as noted, four terms (Skinner, 1935b, *Cumulative Record*, 1972, p. 479). In delineating differences between conditioning Types I and II, however, Skinner writes on the very next page, "1. In Type I, $S_o \rightarrow R_o \rightarrow S_i$, where R_o necessarily intervenes between stimuli" (*Cumulative Record*, 1972, p. 480). At the risk of nitpicking, I wish to highlight the fact that, in this passage, R_i is absent, which may suggest that Skinner came "to conclude that only three terms were needed," as Moxley puts it (p. 75), at a very early stage indeed.

Skinner's tendency to misinterpret Russell on the stimulus-substitution issue may, perhaps, be traced to Russell's characterization of the behavioral function of "object-words": "Generally, when you hear an object-word which you understand, your behavior is, up to a point, that which the object itself would have caused" (as cited in Skinner, 1957, p. 87). Skinner goes on to point out that both Watson and Russell were wrong about this particular issue (i.e., words are not simply surrogates for the things they indicate), and that a more complete and accurate account requires an analysis of the behaviors that have been reinforced in the presence of the verbal discriminative stimulus, *fox*. (Skinner's reliance on the three-term contingency when faced with the most complex behavioral phenomena is a hallmark of *Verbal Behavior*.) It may be conceded that Skinner did not read Russell as carefully as did Moxley.

Moxley misinterprets Skinner (1945) when he states that,

Skinner (1945) referred to "the contingencies of reinforcement which account for the functional relation between a term, as a verbal response, and a given stimulus" (*Cumulative Record*, 1972, p. 380) and identified only two terms in the contingencies of reinforcement, a stimulus and a response. (p. 78)

Earlier in this important paper Skinner makes it clear that "There are three important terms: a stimulus, a response, and a reinforcement supplied by the verbal community" (*Cumulative Record*, 1972, p. 373), and that an understanding of *meaning* requires consideration of all three. Also, the S-O-R diagram and Moxley's accompanying argument have little impact when all of Skinner's work is considered. The S-O-R model citation is from Skinner's 1956 paper, "What is psychotic behavior?" (reprinted in *Cumulative Record*, 1972). The diagram served a didactic purpose, but sometimes such tools obscure important facts en route to revealing other ones. I look at that diagram and I think I see Skinner's point, even though he left out the C, or con-

sequence, this time. What should one make of this diagram, then? Is there really any doubt whether Skinner placed tremendous importance on *consequences* in his understanding of behavior? Is there any doubt that that important point has been heard and understood by even the most casual readers of Skinner? Even *New York Times* reporter Dava Sobel, in an obituary printed in the *Minneapolis Star-Tribune* 3 days after Skinner's death, got it mostly right:

By his own and others' reckoning, Skinner's most important achievement was his full explanation of the theory of operant behavior. It holds that any behavior, from a rat's pressing a bar to the steps in composing a symphony, is selected and reinforced by certain positive consequences in the environment. (*Minneapolis Star-Tribune*, August 20, 1990, p. 1A)

The upshot of all this is that scholars who have asserted that Skinner was an S-R psychologist have ignored Skinner's repeated characterization of the special role of the discriminative stimulus (they "set the occasion" for behavior that is "emitted," they do not "elicit" behavior) and the fundamental importance of consequences in accounting for operant behavior. Perhaps it could be said that they have somehow ignored the concept of the operant altogether. That said, however, Moxley's review makes it clear that anyone who is initially inclined to assert that Skinner was an S-R psychologist would be able to find the references necessary to back up their assertion.

Regarding Disagreement 2, Moxley argues that Skinner adopted the term *setting* in his later writings and used the phrase *discriminative stimulus* less. Moxley based the argument on three main points: (a) Skinner expressed dissatisfaction with the phrase *discriminative stimulus*, (b) Skinner used the word *setting* a whole lot, and (c) for a number of reasons, *setting* is the preferred term. It is safe to say that Moxley has convincingly established his second point, but I will argue about the other two points.

First, Moxley cites Skinner's state-

ments that *discrimination* did not really describe what he was interested in as an indication that Skinner was unhappy with the term. Skinner did appear unhappy with the term, but not, I would argue, because he came to believe that a more inclusive term was desirable. Skinner was saying that he was less interested in discriminative abilities, capacities, and so on, than he was in discovering the circumstances that produced different performances in the presence of different stimuli. It might be said that he was more interested in subtleties on the reinforcement side of the relation than those on the stimulus side. He was still very interested, however, in the processes by which an animal would behave differently when a single feature of its environment was altered.

Skinner may have preferred the term *setting* in instances in which the form of the behavior could not be attributed to single features of the environment, that is, cases in which a consideration of multiple sources of control is forced by the behavioral phenomenon under investigation. *Verbal Behavior* (Skinner, 1957) provides no shortage of such examples, but even in such instances Skinner did not seem to imply that the multifeatured setting operated in a unified or holistic way in the control of behavior. It might be a more faithful presentation of Skinner's views to say that sources of control may be multiple, but separable. Consider, for example, his treatment of the audience as part of the setting in which verbal behavior is reinforced:

In analyzing these effects of an audience in determining the particular subdivision of a verbal repertoire, we assume at least two alternative responses are available in a given situation, apart from the audience variable. The audience selects one set of responses in preference to another. (p. 174)

In this passage Skinner identifies two separable sources of control, the "given situation" and the audience. As required by any behavioral experiment in which most aspects of settings remain constant across experimental condi-

tions, an account of the behavior usually requires special attention to the features of settings that are functionally distinctive. If use of the term *setting* increases the likelihood that researchers will attend to a range of relevant factors that control behavior in a situation, I suppose that is a good thing. Based on my impression alone, however, I would suggest that researchers in the experimental analysis of behavior have not ignored the fact that behavior is controlled by multiple variables.

In summary, I am guilty of, in Moxley's words, "interpret[ing] Skinner as simply using a term in the vernacular (*setting*) as a casual synonym for a technical term (*discriminative stimulus*) without any further implications" (p. 83). Skinner never explicitly argued that *setting* was the better term, and I believe that he would argue the contrary, except in casual discourse in which an explicit experimental analysis of the functions of antecedent variables was not necessary.

Disagreement 3 is less a disagreement about Moxley's basic conclusion regarding Skinner's views on causation and more a disagreement about emphasis. Although I had assumed that Skinner's views on determinism were never really made all that explicit, Moxley's review has convinced me otherwise. Still, discussions about determinism always seem to me to be especially troublesome, and I usually prefer just to listen. A couple of things about Skinner's career and writing occurred to me as being relevant in this context, however. They may suggest that Skinner was not quite as committed to a position on this topic as Moxley suggests.

First, Skinner made an unusual break with Laplacean determinism when he asserted that first instances of operants were "spontaneous" (Skinner, 1937, *Cumulative Record*, 1972, p. 491; I thank Glen Sizemore for pointing out to me the importance of this fact), an assertion that also calls into question Moxley's conclusions regarding Skinner's lingering identity as a re-

flexologist. Second, I wonder whether Skinner's comments about behavioral control by eliciting and discriminative stimuli (1953, p. 112) are properly seen as indicating a position on determinism. My impression is that they are merely statements of fact. We tend to think of eliciting stimuli as inexorable forces producing involuntary behavior and discriminative stimuli as controlling behavior differently, producing what may be called voluntary behavior. Skinner's point was that it is not difficult to arrange experimental conditions that guarantee that the probability of *operant* behavior in the presence of a given stimulus is 1.0, thus rivaling the level of control observed with eliciting stimuli. This is not a theoretical position, it is a statement of fact. I have pigeons responding in delayed matching-to-sample procedures with very short delays who may go several sessions in a row without making an error. The $p(R|S)$ in such procedures is as close to 1.0 as is the corresponding quantity in respondent procedures, whether or not you adhere to a doctrine of determinism.

Finally, Skinner *investigated* two ways in which chance is important to understanding behavior. In 1948, Skinner published "Superstition in the Pigeon," in which identifiable patterns of behavior were produced by "adventitious reinforcement," that is, accidental temporal contiguity between a pigeon's movement and freely delivered food. In 1957, Morse and Skinner published "A Second Type of 'Superstition' in the Pigeon," which was concerned with the changing functions of discriminative stimuli due to accidental stimulus-reinforcer correlations. In both of these experimental situations, changes in behavior due to chance correlations between events were observed. Granted, this research does not prove that Skinner was not a determinist in the sense that Moxley suggests, but it does show that Skinner recognized the important role of random variation early in his career.

Perhaps the most interesting and the-

oretically compelling of Skinner's references (e.g., 1981) to random variation are found in his statements regarding the analogy between operant conditioning and natural selection. Moxley points out other interesting references, and I found particularly useful his connection of rule governance and contingency shaping with issues of variation and determinism. A question that occurs to me is whether Moxley sees Skinner's theorizing about rule governance as contributing to his movement away from a classical determinism, or was it more the other way around?

I suppose that my response to Moxley's scholarly paper is an attempt to reconcile my impressions of Skinner's work with Moxley's conclusions. My education in Skinner's work seemed to highlight positions that run counter to those of Moxley. Perhaps this is due to the fact that I received guidance from other scholars of Skinner, in particular the late E. F. Malagodi, who helped me to see the critical significance of Skinner's rapid movement away from S-R theorizing (of which the term *discriminative stimulus* was a clear indication), his firm commitment to reinforcement theory and the three-term contingency, and the theoretical importance of chance correlations among behavioral events. These issues had never been particularly troubling ones to me. Moxley has compiled an impressive history of Skinner's and others' writing that makes me very grateful that I had such guidance.

REFERENCES

- Morse, W. H., & Skinner, B. F. (1957). A second type of "superstition" in the pigeon. *The American Journal of Psychology*, 70, 308-311.
- Rescorla, R. A. (1985). Conditioned facilitation and inhibition. In R. R. Miller & N. E. Spear (Eds.), *Information processing in animals: Conditioned inhibition* (pp. 229-326). Hillsdale, NJ: Erlbaum.
- Skinner, B. F. (1931). The concept of the reflex in the description of behavior. *Journal of General Psychology*, 5, 427-458.
- Skinner, B. F. (1935a). The generic nature of the concepts of stimulus and response. *Journal of General Psychology*, 12, 40-65.
- Skinner, B. F. (1935b). Two types of conditioned reflex and a pseudo-type. *Journal of General Psychology*, 12, 66-77. (Reprinted in *Cumulative record*, 1972, pp. 479-497 [New York: Appleton-Century-Crofts].)
- Skinner, B. F. (1937). Two types of conditioned reflex: A reply to Konorski and Miller. *Journal of General Psychology*, 16, 272-279. (Reprinted in *Cumulative record*, 1972, pp. 489-497 [New York: Appleton-Century-Crofts].)
- Skinner, B. F. (1938). *The behavior of organisms*. Englewood Cliffs, NJ: Prentice Hall.
- Skinner, B. F. (1945). The operational analysis of psychological terms. *Psychological Review*, 52, 270-277. (Reprinted in *Cumulative record*, 1972, pp. 370-384 [New York: Appleton-Century-Crofts].)
- Skinner, B. F. (1948). "Superstition" in the pigeon. *Journal of Experimental Psychology*, 38, 168-172.
- Skinner, B. F. (1953). *Science and human behavior*. New York: Macmillan.
- Skinner, B. F. (1956). A case history in the scientific method. *American Psychologist*, 11, 221-233.
- Skinner, B. F. (1957). *Verbal behavior*. New York: Appleton-Century-Crofts.
- Skinner, B. F. (1972). *Cumulative record*. New York: Appleton-Century-Crofts.
- Skinner, B. F. (1981). Selection by consequences. *Science*, 213, 501-504.
- Sobel, D. (1990, August 20). B. F. Skinner, famed behaviorist, dies. *Minneapolis Star-Tribune*, 9(138), p. 1A.