THE BEHAVIOR OF ORGANISMS AS WORK IN PROGRESS

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"The book represents nothing more than an experimental analysis of a representative sample of behavior. Let him extrapolate who will" (Skinner, 1938, p. 442).

The first printing of *The Behavior of Organisms* by B. F. Skinner, on September 2, 1938, consisted of 800 copies, not all of which were bound (Skinner, 1979, p. 219). It did not receive another printing until well after the second world war, when the book began to be used by F. S. Keller and W. N. Schoenfeld in their undergraduate courses at Columbia College (Skinner, 1979, pp. 318-319). It appeared in a paperback edition in 1966, and has only recently gone out of print. It is reasonable to guess that it will not remain out of print for long.

The reviews of the time were mixed. Of those, one was so perceptive a commentary that it is reprinted here (Hilgard, 1939). Of the others, Finan characterized the position of the book as positivistic and operationalistic, and was not persuaded by Skinner's distinction between respondent and operant behavior:

Conditioning Type *S* serves to warn the organism (signalization) whereas Type *R* does not prepare for the reinforcing stimulus but *actually produces it*. . . . Unless, however, it turns out that the two types of behavior can be demonstrated in terms of their subsumption under different sets of laws, to be unequivocally distinct, it would appear likely that most investigators will continue to hold the opinion that there is some economy of hypothesis in assuming the identity of the two behavior types. (Finan, 1940, p. 442; italics in original)

Too many others also missed the point and were still calling Skinner a stimulus-response theorist decades later. Jones, however, was persuaded, and acknowledged Skinner's argument that "the attempt to force behavior into a stimulus-response explanatory system has delayed its adequate scientific treatment" (Jones, 1939, p. 659). Jones went on to say that

this work should receive a ready welcome. The emphasis upon operant rather than respondent behavior should be hailed by all who object to the forcing of behavior into falsely simplified explanatory systems. Perhaps the specific laws will be modified with the extension of research, but surely the basic position should always deserve respectful consideration. (Jones, 1939, p. 660)

On the other hand, Wolf, a biologist, thought Skinner had overreached himself:

It therefore seems doubtful whether a system of behavior can be of any value as long as its applicability has not stood the test with numerous other animals besides the white rat. Skinner really ought to furnish proof of this before elaborating a *system*. It has to be said, however, that the experimental data as they stand, and the simplicity of the experimental procedure, are certainly of significant value and vastly better for analytical purposes than learning curves and the like. From the standpoint of the reviewer as a physiologist the title of the book promises a lot, but after having read it one is inclined to propose for it a title which would rather read, "Properties of certain specific complex reflex mechanisms in the white rat." (Wolf, 1939, pp. 478-479; italics in original)

The harshest review was, perhaps, Krechevsky's, although his concerns were about the system rather than the findings: "These data mark, we believe, a new 'high' in experimental achievement. . . . The regularity and lawfulness of behavior which Skinner discovers should serve as an inspiration and guide to any experimenter" (Krechevsky, 1939, p. 404). But he closes by stating that

it is to be profoundly regretted that Skinner did not attempt to tie up his work and his thinking with the rest of psychology's experimental data and concepts. He should have given us a more

"social" and a more generalized product. This reviewer believes that such a product would have been a really significant contribution to systematic psychology. As it is, we have a collection of data which have yet to be patterned . . . into a useful system describing the behavior of organisms. (Krechevsky, 1939, p. 407)

The Behavior of Organisms was abstracted in *Biological Abstracts* (Schneirla, 1938) and in *Psychological Abstracts* (Hunter, 1939); other commentaries of the time treated specific aspects of the book (e.g., Ellson, 1939, on the reflex reserve, and Loucks, 1941, on Skinner's treatment of the relation between behavior and physiology), and more detailed analyses were to follow later (e.g., Verplanck, 1954). But, mixed reviews notwithstanding, *The Behavior of Organisms* had clearly taken its permanent place among the significant books of 20th-century science. We celebrate here its first half century.

This project was initiated during the editorial term of Philip N. Hineline. I do not recall when it was brought to our attention that 1988 would be the 50th anniversary of *The Behavior of Organisms*. As Review Editor, I had for some time been concerned with setting the occasion for appropriate *JEAB* book reviews. There was ample precedent for retrospective reviews, so once the date had been noted an anniversary treatment of the book seemed inevitable.

After some preliminary discussion with the then Editor-Elect, Edmund Fantino, we decided that we could not restrict the contribution to any single reviewer, and that if multiple reviewers were used they would best be self-selected. We made some informal and unsystematic contacts during the 1987 meeting of the Association for Behavior Analysis. For our more formal sampling, we began with the list of current *JEAB* editorial board members; after we had each contributed a few additional names, we sent out a mailing in the summer of 1987. That invitation included information about the deadlines entailed by the *JEAB* publication schedule and suggested some guidelines for the retrospective reviews: "(a) reviews should not be appreciations, but rather should make some substantive contribution of archival value, and (b) reviews could deal with the book as a whole or could concentrate on some specific topic (e.g., the reflex reserve; the evolution of the operant language from the language of

the reflex; schedules and schedule theory; the structure of the book, as compared to those by Jennings, Sherrington, Pavlov, and others)."

With varying degrees of certainty and of specification of possible content, a dozen or so responded to the invitation; of those, some later dropped out of the project on their own initiative because constraints on their time prevented them from meeting the editorial deadlines. In one instance, a potential participant had a prior commitment: Travis Thompson had already prepared a retrospective review for *Contemporary Psychology* (Thompson, 1988). A tentative review section was taking shape. What followed thereafter involved the usual intricacies of editorial correspondence dealing with length, overlap of contents, and other matters of style and substance.

Although Skinner himself was not explicitly informed about the project, it seemed unlikely that it could be kept secret from him. He might have guessed that his Preface would not be the only piece related to his book that would appear in this issue of *JEAB*, and he undoubtedly knew of other anniversary events. For example, a symposium entitled "'The behavior of organisms' at fifty," chaired by James T. Todd and Edward K. Morris and with Laurence D. Smith, Terry J. Knapp, and the present author as participants, was scheduled for presentation at the 1988 meeting of the Association for Behavior Analysis; Skinner had been asked to be a discussant. We do not know whether he will be surprised by this *JEAB* project, but perhaps he will later make a public report of the relevant private events.

Early in the project I had promised to write an introduction to the set of reviews; if my time permitted, it was to be a short one. Long before the project began I had gotten into the story through my undergraduate work in psychology, which started in 1954 during my sophomore year at Columbia College. My introductory course in psychology had been taught by Fred Keller. The text was *Principles of Psychology* (Keller & Schoenfeld, 1950), and the course included laboratory experiments with rats in the fall semester and with humans in the spring (Keller & Schoenfeld, 1949). Throughout the course the laboratory assistants made frequent references to a book they called "B of O."

Many students later took psychology lecture courses, but only a few elected to concentrate

in psychology in those days, and those who did so immediately proceeded into advanced experimental courses. Those courses were taught by W. N. Schoenfeld, who devoted the first semester to discrimination. This course was known at other universities as experimental psychology and psychophysics; its text was *Experimental Psychology* by Woodworth (1938). The second semester was devoted to motivation, and it used two texts: *The Behavior of Organisms* by Skinner (1938) and *Principles of Behavior* by Hull (1943).

I cannot say with any assurance whether other students who took that course in motivation had reactions similar to mine, but my recollection of my first encounters with those texts was that they were difficult. They were difficult in different ways, however: Hull was easier to read but harder to understand. My guess is that the reason was that Hull's work, although mathematically straightforward, was so much more distantly related than Skinner's to the behavior that was its subject matter. Hull spoke of the nervous system, of excitation and inhibition, of drive stimuli and reaction potentials, but none of these were things that any of us had seen in the laboratory. Skinner's work, on the other hand, gave us ways to talk about behavior that were consistent with the discriminations that we were beginning to learn in the laboratory. The empirical findings were only a part of what we were taught; we were also mastering the elementary terms of an effective language for the science of behavior.

In reading Hull, the student's difficulty was in understanding how terms and concepts only indirectly derived from behavior could be related to behavior. In reading Skinner, it was rather in understanding how these new behavioral terms were related to traditional behavioral concepts in the everyday language.

I particularly recall worrying over Skinner's treatment of the term "hunger" (Skinner, 1932). It seemed so much a term for a private felt state that even with Schoenfeld's help it took me a long time to acknowledge the significance of the argument for how the term must have evolved from observations of behavior. The point was that the term could not have acquired any function in the colloquial language except to the extent that it was based on public observations of the likelihood of eating as a function of such variables as the time since prior access to food. I later saw my getting

the point as a personal breakthrough of sorts that prepared me for the more crucial arguments about the origins of our vocabulary of private events that I was eventually to encounter in "The operational analysis of psychological terms" (Skinner, 1945).

What happened to me at Columbia and to other students there and at other places was part of the history that makes it appropriate to speak of *The Behavior of Organisms* as a work in progress. As Keller and then Schoenfeld presented it, the system provided crucial beginnings but each new finding raised new experimental questions. They spoke of how, at Harvard, Skinner was expanding his analyses of reinforcement schedules (Skinner, 1938, chapters 4, 5, and 7) in collaborative research with a former Columbia student, Charles B. Ferster. Although it was probably never stated explicitly, it was clear that new participants were welcome to join in that work in progress.

The Behavior of Organisms also stands as a work in progress in another sense. Although it is tempting to treat the book as a finished work, it is nonetheless a verbal sample from a researcher whose contact with behavior was growing more subtle and sophisticated with time. The book incorporated previously published papers at relevant points, and the interested reader can trace the development of the work in progress through the changing treatments of various concepts and phenomena, thereby supplementing Skinner's own account of that history (Skinner, 1956, 1979).

For example, the earlier papers sometimes included complex subscripted or superscripted notations for stimuli and responses (e.g., Skinner, 1934, 1936). Such notations were still to be found in *The Behavior of Organisms*, along with those that had begun to evolve in Skinner's exchange with Konorski and Miller (Skinner, 1937). And the discriminative stimulus, S^D , was still preceded by the s that denoted the eliciting stimulus of the reflex, although that s had become vestigial. Only later would it be completely expunged. The notation parallels Skinner's account:

I had come to psychology devoted to Pavlov, and I had soon discovered Sherrington and Magnus. They seemed to be closer than any of their contemporaries to a true science of behavior. The concept of the reflex had served them well, and in my thesis I had said that it was all that was needed in the study of behavior.

I knew better by the time I began to write my book. My field was the operant rather than the respondent. . . . I could not break my chains, however. I went on talking about reflexes. (Skinner, 1979, p. 201)

Other aspects of the work in progress are also best examined by considering the corpus of works both before and after *The Behavior of Organisms*. It is interesting, for example, to trace the way in which some of the text of "The generic nature of the concepts of stimulus and response" (Skinner, 1935) is worked into the book, and then to see how the same concepts emerge in different form in later works. It is also instructive to examine the changes in frequency of the term "conditioning" and its derivatives, especially as applied to operant behavior, from the earlier papers through *The Behavior of Organisms* to subsequent works such as *Science and Human Behavior* (Skinner, 1953).

The concern with the language of behavior was there almost from the beginning. Consider the opening paragraph of "Drive and reflex strength" (Skinner, 1932):

We say that an animal is hungry if, when we give it food, it eats. We also say it is hungry if it responds in characteristic ways to an empty food dish, to the approach of the experimenter, or, in short, to any stimulus that has been conditioned to behavior in some way concerned with eating. In the absence of such specific stimulation, we may say that the animal is hungry if it shows what has unfortunately been called spontaneous activity, although we cannot in such a case distinguish between hunger and some other drive until a stimulus leading to a specific response has been introduced. In our everyday use of the word only the first of these conditions is important, for we ordinarily attribute hunger to an animal only because it eats or because it exhibits behavior that we have frequently observed to be followed by eating. In the latter case the use of the word depends on the conditioning of the experimenter. (p. 22)

The concern with the language of behavior continued in *The Behavior of Organisms*, but it had become implicit rather than explicit, and to some extent was subordinated to other concerns. For example, hunger was treated more formally in the context of drive, where it was the basis for an account of the logic of intervening variables (would I have had an easier time with the 1932 paper than with "B of O"?). Skinner's analysis of the origins of such

terms in the verbal history of the speaker was to reemerge later, in "The operational analysis of psychological terms" (Skinner, 1945).

The recurrent theme of verbal behavior brings to mind an editorial correspondence in which I was involved some years ago. The editor of another journal had asked me to review a submitted article that proposed a distinction between Skinner's earlier and later behaviorism. Although the editor later accepted the article conditional on revision, to the best of my knowledge it was never resubmitted and published. My recollection is that the article contrasted an initial positivistic and operational position, characterized by *The Behavior of Organisms*, with the later position that is now recognized as contemporary radical behaviorism. If I remember the manuscript correctly, it is easy to argue now that its problem, notwithstanding the important role of Mach in Skinner's intellectual history, was in trying to characterize *The Behavior of Organisms* as positivistic or operational. There are too many lines of evidence to the contrary, not the least of which is that, despite its title, "The operational analysis of psychological terms" is actually a renunciation of operationism.

Even though the continuities in Skinner's treatment of the origins of our vocabulary of behavior are compelling, the argument for the two Skinners remains tempting (one is reminded of the early Wittgenstein of the *Tractatus Logico-Philosophicus* and the later of the *Philosophical Investigations*; Wittgenstein, 1922, 1953). *The Behavior of Organisms* is very different from *Science and Human Behavior* or *Verbal Behavior* (Skinner, 1957), and the difference seems to be in more than just the extent to which cumulative records and other data are included. Perhaps the key is that *The Behavior of Organisms* was written in a context of competing behavioral systems such as Hull's and Tolman's. Perhaps the enterprise was to some extent temporarily sidetracked by the attempt to construct a systematic edifice of behavioral laws buttressed by paradigmatic formulas and consistent with models from the physical sciences.

Skinner (1950) later commented on whether theories of learning were necessary; contrary to some opinions, he did not reject theory, but he did argue that some kinds of theories were better than others. We seldom now refer to the Static or Dynamic Laws of the Reflex, but the

language of operants and respondents and of discriminative and eliciting stimuli is still with us. Although it was very competently negotiated, the formal system was a detour. Once it found its proper verbal and biological context, the enterprise quickly got back on track (e.g., Skinner, 1981; cf. Catania, 1987). At its heart was its concern with origins, and when we seek our origins many of us find some of ours in this seminal book.

We cannot exhaust the ways in which *The Behavior of Organisms* continues to epitomize the work in progress, and there is no need to introduce in detail the individual reviews that follow. They speak sufficiently well for themselves. The final piece among them is B. F. Skinner's own preface to the 1966 paperback reprinting of *The Behavior of Organisms*. We must let the author have the last word. It seems appropriate to do so, but not out of courtesy; rather, we assume that the reader, upon finishing the preface, will find it appropriate to continue directly into the book. Each new reading has its consequences. Their cumulative effects will maintain and advance the work in progress celebrated here. But those who read the book to see how it ends will be disappointed. As befits a work in progress, it does not end. That is because all of us have become part of the continuing story.

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