

PHILOSOPHY OF BEHAVIORISM

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I was privileged to have been associated in one form or another with the Harvard Pigeon Lab from September 1964, when I arrived as a graduate student, until my friend and colleague Gene Heyman moved out of William James Hall on June 30, 1998. After completing my graduate degree in 1968, I continued to live in Cambridge and was therefore able to continue my affiliation with the Pigeon Lab, frequenting the lab meetings to learn about the latest research and occasionally to present some ideas of my own. During the academic year 1981–1982, I was a full-time visiting scholar at the lab, regularly attending lab meetings as well as weekly conversations with Fred Skinner. These latter meetings, or “pow-wows” as Fred called them, were attended by a small group consisting of Will and Maggie Vaughan, Pere Julia, another visiting scholar at the time, and me, as well as occasional invited guest speakers (Skinner, 1983, p. 394).

When I arrived at Harvard in 1964, the Pigeon Lab was about to move from the basement of Memorial Hall into William James Hall, and it had completed its transition from Skinner’s leadership to Herrnstein’s. Skinner had accepted a government Career Award and was retired from teaching and departmental responsibilities, although he remained Professor of Psychology. Not only was this a change in style, but it also meant a basic shift in research direction. For Skinner, the fundamental dependent variable in operant research was the absolute response rate of the free-operant response. He conceptualized response rate as closely associated with response probability, or response strength, the successor to the earlier reflex reserve. As a visual representation of the moment-to-moment changes in response rate, the cumulative record was the ideal recording instrument. Under this paradigm, the research

program consisted of the systematic exploration of how response rate changes as a function of contingencies of reinforcement, typically in the form of schedules of reinforcement.

However, it gradually became apparent that this conception of response rate is internally problematic. On the one hand, response rate was supposed to measure response strength as a function of reinforcement contingencies. On the other hand, however, research was revealing that response rate was itself susceptible to control by contingencies of reinforcement. If response rate, or more technically, the interresponse time, could be manipulated by changes in the contingencies, it could not at the same time reflect response strength. The seeds of this contradiction were already apparent in Skinner’s own work (Ferster & Skinner, 1957, pp. 7, 10; Skinner, 1938, p. 284), which was devoted to the study of response rate as a measure of response strength but also often explained response rate as due to response features prevailing at the time of reinforcement.

Herrnstein’s early work with two-key concurrent schedules of reinforcement offered a way out of this dilemma. In contrast to the previous research program, Herrnstein’s dependent variable was the *relative rate* of response on one of the keys, and this variable was a molar variable in that it was calculated over a session rather than moment to moment. Skinner had long opposed the use of relative frequency measures, or what he called the behavior ratio (Skinner, 1950), as it was used by Hull and Tolman in their maze-running experiments. Furthermore, relative response rate was a steady-state variable, and did not lend itself to the analysis of change revealed by the cumulative records Skinner favored (Skinner, 1976). Moreover, Skinner (1983, p. 362) doubted that a molar principle, rather than a simple process, could have figured in natural selection.

The transition from absolute rates to relative rates of response and reinforcement and

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from moment-to-moment changes to more molar measures had profound implications. First, and foremost, it initiated a coherent research program that characterized the pigeon lab for its remaining years. Herrnstein's matching law, a direct consequence of this transition, became the organizing principle for decades of research, much of it emanating from the Pigeon Lab and its graduates. A second consequence was the effect on Skinner's relationship to the lab. Although there was a host of reasons for his estrangement from the lab, it seems likely that his opposition to the change in research direction contributed to his absenting himself from lab meetings. As a result, graduate students in the 1960s onward had very little, if any, contact with Skinner and any direct influence he might have exerted on their thinking, experimental or conceptual.

Skinner's absence from the Pigeon Lab was a source of disappointment for me. I had arrived at Harvard with the single-minded intention of learning more about Skinner's philosophy of behaviorism. As an undergraduate at Columbia, I had been inspired by Keller and Schoenfeld who offered behaviorism as not only a scientific theory and method but also as a way to conceptualize all of psychology. They led a department of true believers in this philosophy, including Bill Cumming, a radical behaviorist if there ever was one, Ralph Hefferline, who devoted an entire semester seminar to Skinner's (1957) *Verbal Behavior*, and Herb Terrace, newly arrived from the Harvard Pigeon Lab. Psychology at Columbia in those days was synonymous with Skinnerian behaviorism, and introductory psychology consisted of reading Keller and Schoenfeld's (1950) *Principles of Psychology* accompanied by a rat lab in operant conditioning, much to the consternation of many a Columbia undergraduate. But to me, a philosophy major interested in philosophy of psychology, this was exciting and challenging. After a few of these courses, and some philosophy of science with Ernest Nagel and philosophy of mind with Arthur Danto, I was off to Harvard to study with Skinner himself.

To my chagrin, not only was Skinner not accessible, but Dick Herrnstein, my mentor, did not share my interest in philosophy of behaviorism. To be sure, if pushed into a philosophical discussion, he could hold his own,

displaying the intellectual brilliance he brought to everything he approached. But his heart was with the science of behavior, not its conceptual foundations. Because of my interests in philosophy, he often compared me to a spider, spinning a web of ideas out of my own mind, in contrast to empiricists like himself, who like squirrels, construct a science by collecting one fact after another from the external natural world.

Another source of irritation in my quest to learn philosophy of behaviorism was located right down the corridor in Memorial Hall where S. S. Stevens ran the psychophysics lab. Using his magnitude estimation methods, he claimed to be measuring "sensations," or "psychological magnitudes" in contrast to physical magnitudes. Nevertheless, Stevens (1935) claimed that these apparently mental terms all had perfectly good operational definitions. What irked me was that his definitions, derived ultimately from Bridgman's notion of operational definition, were unintelligible to me. I didn't understand then, and still do not, how a sensation can be equivalent to a set of experimental operations and behavioral responses. The issue continued to trouble me, and it was no accident that my first publication in philosophy of behaviorism was "A Behavioral Interpretation of Psychophysical Scaling" (Zuriff, 1972) in which I offered a behaviorist understanding of psychophysical research.

Whereas the Pigeon Lab generated two coherent research programs, one under Skinner and later one under Herrnstein, no comparable coherent philosophy of behaviorism emerged, other than Skinner's own work. Undoubtedly, there were many reasons for this, but I will mention only three. First, as Skinner increasingly absented himself from the lab, the opportunities for him to enlist members of the lab into his vision of behaviorism decreased. His philosophical discussions were conducted primarily with his followers around the world rather than with members of the Pigeon Lab down the hall. Second, Herrnstein was committed to empirical behavioral research rather than the exploration of the conceptual foundations of behaviorism, and as a consequence, no institutional context existed for the development of a coherent philosophy. Third, graduate students and postdoctoral fellows came to Harvard for

the empirical behavioral research, not the behaviorism. Those seeking conceptual training chose to go elsewhere, for example, to the University of Nevada to work with Willard Day. Furthermore, as Skinner's interest in philosophy of behaviorism shifted to social issues, the Harvard students had even less interest because of the tenuous connection between his social ideology and the science of operant conditioning he founded. To be sure, many an animated philosophical discussion took place in the Pigeon Lab, but no consensus emanated from them.

Obviously the most important contribution to philosophy of behaviorism to emerge from the Pigeon Lab is Skinner's own corpus of writing. Much has been said about his radical behaviorism, and I will not attempt to add more here other than to outline his philosophy as a baseline to which later developments can be compared. First are his brilliant behaviorist interpretations of the mental world. This includes his interpretations of mental concepts such as meaning, purpose, and expectation in terms of contingencies and histories of reinforcement, his introduction of private events into behaviorism to account for first-person reports of internal episodes, and his behavioral epistemology. Second is his conception of psychological theory and theorizing. This includes, on the one hand, his strictures against theoretical inference, his substitution of functional relation for causation, his notion of a theory as a parsimonious set of laws, and his view of explanation as prediction and control. On the other hand are included his views of effective scientific practice as staying close to the data and observation, his opposition to the hypothetico-deductive method, his championing an autonomous science of behavior, and his interpretation of science as the behavior of scientists. Third is his social philosophy including his ideas on the application of behavioral technology, his advocacy of behavioral control, his opposition to the use of punishment, and his rejection of traditional mental concepts such as "freedom" and "personal responsibility," which he believed interfered with human progress.

Although the Pigeon Lab did not prove to be a place for the continued incubation, discussion, and development of these ideas as it was for Skinner's science of operant condi-

tioning, many members of the lab made important individual contributions to the discourse on the philosophy of behavior over the years. For example, of the more than 200 articles published in the journal *Behaviorism*, a journal devoted to philosophical behaviorism, between its founding by Willard Day in 1972 and its demise in 1989, more than 10% were authored by members and former members of the Pigeon Lab. Nine of them also served on the editorial board.

In addition, several of them published important textbooks on learning and behavior. Although these books were devoted primarily to the science of behavior, they typically included an introductory chapter describing the history and nature of behaviorism. Often brief passages touched on philosophical issues from a Skinnerian perspective. Two examples are textbooks by Reynolds (1975, pp. 2, 3, 131) and Catania (1998, pp. 254, 374). In addition to the several textbooks on learning and behavior, five graduates of the Pigeon Lab also published books primarily devoted to the development of philosophy of behaviorism. To be sure, many other alumni also published articles with philosophical content, but I shall restrict myself to these major works, which alone are sufficient to illustrate both the important role played by alumni of the Pigeon Lab in articulating the philosophy of behaviorism and the lack of any single unifying conception.

Robert Boakes (1984) authored *From Darwin to Behaviourism*, a very readable history of the study of animal psychology from Darwin to about 1930. The later sections of this book deal with the beginnings of behaviorism, especially the work of John Watson, but they also touch on the thought of Tolman, Lashley, and Hull. Skinner, however, is not discussed. During the period covered by Boakes, researchers in animal psychology had to struggle with many philosophical issues central to behaviorism, including the definition of *behavior*; the inference of mental operations and contents from behavior, the relations between behavior and physiology, scientific method, the nature of consciousness, and the relations between mind and body.

William Baum's (1994) *Understanding Behaviorism* comes very close to representing a direct descendant and developer of Skinner's thought while modernizing and clarifying

Skinner's radical behaviorism (Zuriff, 1995). Baum reconceptualizes several of Skinner's basic empirical concepts, including reinforcement, and embeds them into modern evolutionary biology. For example, he works out in detail the parallels among natural selection, reinforcement, and cultural selection, and he substitutes the contemporary theoretical framework of biological fitness for Skinner's earlier concept of survival. Similarly, Baum presents the best exposition available of Skinner's concept of rule-governed behavior, filling in the gaps in Skinner's thought and correcting the weaknesses. It is in the area of social philosophy that Baum makes his most original contribution to Skinnerian behaviorism. In his discussion of freedom and happiness, Baum brings to bear the latest studies in adaptation-to-the-norm and equity theory to deal with some of the anomalies in behaviorist treatments. Finally, in a somewhat idiosyncratic philosophical analysis, Baum identifies radical behaviorism with a version of pragmatism.

Whereas Baum attempted to extend Skinnerian behaviorism, John Staddon's (2001) *The New Behaviorism* sets out to refute nearly every aspect of radical behaviorism and to replace it with his "new behaviorism" (Zuriff, 2001). Staddon calls into question Skinner's basic concept of reinforcement contingency, challenging the adequacy of contiguity to explain the effects of reinforcement. He rejects prediction and control as sufficient goals for a scientific theory, and he argues for a deeper theoretical understanding. Contrary to Skinner's proscriptions against theorizing, Staddon advocates theoretical inferences of internal states and mechanisms to supply causal explanations rather than the teleological explanations provided by contingencies of reinforcement (and see discussion of Rachlin below).

Likewise, in his discussion of social philosophy, Staddon (2001) disagrees with most of Skinner's recommendations. He rejects Skinner's use of survival as a selection criterion for social practices, preference for positive reinforcement over punishment, advocacy for social control, and his call for the overthrow of traditional values such as freedom and personal responsibility. Underlying Staddon's opposition are his suspicion of revolutionary change and his skepticism over grand extrap-

olations from animal laboratory research to the complex world of everyday human action.

In his densely written *Behavior and Mind* and an important series of papers, Howard Rachlin (1985, 1992, 1994) presents a highly original and intriguing version of behaviorism. Using an Aristotelian analysis, he argues for unusual behaviorist understandings of explanation and the mental. He claims that to explain behavior in terms of contingencies of reinforcement, and by extension, in terms of principles such as the matching law or maximization, is to give a final cause for the behavior rather than an antecedent efficient cause. It is comparable to explaining why the violinist played the notes she did by saying she was playing Debussy's String Quartet in G minor. This explanation places the behavior in the larger context of what was happening, past as well as future, and the Quartet is thus the final cause rather than an antecedent event that produces the violinist's playing. Similarly, to explain behavior in terms of maximization, or a utility function, or a contingency is to place it in the context of events and correlations extended over a period of time and space.

This teleological behaviorism also offers an interesting interpretation of mentalistic language. When we explain a person's behavior in terms of beliefs, feelings, thoughts, or sensations, we seem to be suggesting an antecedent efficient cause. According to Skinner, this cause may be a covert event or an antecedent history of reinforcement. For Rachlin, in contrast, the mentalistic explanation is interpreted as teleological in that it suggests a final cause rather than an efficient cause. It embeds the person's behavior in the larger context of this person's overt behavior and its interactions with the environment over extended time and space. Thus, Rachlin dispenses with Skinner's world of internal responses and stimuli (as well as the information processing apparatus of the cognitivists) and substitutes final cause explanation for efficient cause explanation. Interestingly, Rachlin's shift away from single responses and reinforcements to extended patterns of behavior-environmental interactions in his accounts of explanation and the use of mental terms parallels (and was perhaps suggested by?) the theoretical shift in the Pigeon Lab from moment-to-moment analysis to more

molar variables measured over long periods of time.

In contrast to Rachlin, Baum, and Staddon, Zuriff's (1985) *Behaviorism* does not advocate one particular version of behaviorism. Instead, it attempts to present a conceptual reconstruction of all varieties of behaviorism, revealing their logical geography. Its thesis is that behaviorism is an attempt to develop an objective and empirical psychology and that there are many ways to achieve this. In working out the meaning of *objective* and *empirical*, the behaviorist encounters a number of philosophical choice points, for example, the selection of a data language and how to conceptualize first-person reports. As each choice point is negotiated, others materialize as a logical consequence. Thus, a version of behaviorism, including Skinner's radical behaviorism, can be conceptualized as a branching tree diagram, with each choice point represented by a node. Zuriff attempts to evaluate the strengths and weaknesses of each version in light of contemporary philosophy and science and to correct flaws where possible.

Each of these books has played an important role in contemporary philosophy of behaviorism. Although these products of the Pigeon Lab do not represent a coherent behaviorism, they do share two features in common (with the exception of Boakes' history). First, each in its own way has tried to modernize behaviorism in light of recent developments in philosophy and science, especially evolutionary biology. Second, each has attempted to defend behaviorism against the avalanche of criticism leveled at it by philosophers and psychologists, especially cognitivists. Although these defenses are, I believe, successful in the sense that they are for the most part intellectually valid, they have not been successful in the sense of terminating the endless recycling of the same criticisms

or inducing psychologists to pay serious attention to behaviorism. Nevertheless, in assessing the legacy of the Pigeon Lab, these contributions to the philosophy of behaviorism should be remembered as significant achievements.

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