On Books

Whither Behaviorism?

A Review of *Modern Perspectives on B. F. Skinner and Contemporary Behaviorism*,
James T. Todd and Edward K. Morris (Eds.)

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Where Watson hath slain his thousands, Skinner hath slain his tens of thousands. Or vice versa: Readers of the two books edited by James Todd and Edward Morris can decide for themselves. My assignment is Skinner, so for me Watson is just a link. The book has five types of chapter: philosophy of science, historical accounts, personal experiences, interpretations of what has been said in the past (mainly by Skinner), and expositions on what Skinner should have said. The resulting lack of unity invites the reviewer to impose an organization on the history and present status of Skinnerian behaviorism that takes the chapters as points of departure. I accept the invitation.

Two chapters, one by Stephen Coleman and the conclusion written by Edward Morris, James Todd, Bryan Midgley, Susan Schneider, and Lisa Johnson, are about the practice of history itself. As a career-long teacher of the history of psychology, I have worried for years about doing history without formal training in that discipline. This feeling was reinforced by Coleman's chapter, for he is a trained psychologist who sounds like a real historian. Coleman's insightful analyses of the use of history and its value go well beyond behaviorism. The Morris et al. chapter is more specialized in its concern with the types of documents

that can be used in writing the history of behavior analysis. I do take issue with their statement that "Behavior analysis is, today, contemporary behaviorism" (p. 195). Contemporary behaviorists still number productive researchers who follow the stimulus-response (S-R) model, or consider themselves to be associationists, or feel that they continue theoretical analyses of behavior in the traditions of Hull, Spence, and Tolman, or incorporate ethological concepts in their analysis of learned behavior and its maintenance, or in other ways do not identify with the behavior-analytic approach. I think it preferable, therefore, to restrict *behavior analysis* to the behaviorism initiated by B. F. Skinner and carried on by the group of behaviorists who call themselves by that name.

Having said that, I need to preface what follows by admitting that it is history that falls somewhere near Coleman's category of "History is a crystal ball" (p. 136). I will be using the chapters to help to understand why current psychology no longer revolves around behaviorism, even in its behavior-analytic form. Why is the approach no longer central in psychology? Why is it seen by some as a school that had its day and now is of largely historical interest? Where may behavior analysis fit in the future?

The Behaviorist Revolution: The Promise

John Watson redefined psychology:
Psychology was to be the science that

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studies behavior. The target of Watson's Behaviorist Manifesto (Watson, 1913) was psychology defined as the study of the conscious mind. Some of Watson's criticisms had been foreshadowed by his functionalist mentors, who did not dispute the scientific acceptability of the then dominant approach of Wilhelm Wundt and E. B. Titchener, but who felt that it ignored the role that mind played in life. For functionalists, the important questions were (a) what is the adaptive value of a mental process? and (b) how does the process operate? Watson agreed with the functionalists that psychology should focus on utility, but he thought that overt activity rather than mental process was what mattered. Either psychology should become the science of behavior, or a new science dealing with behavior should be established at once. Watson argued that a science of behavior could be rigorous and unambiguous, everyone would know what everyone else was talking about, laws could be determined, and the science could have an applicability to important human affairs that a psychology of the mind, necessarily tied to introspection, could never enjoy. Titchener (1914) agreed with Watson on the feasibility of a science of behavior, but he believed that it should be part of biology. For Titchener, the survival of psychology as an independent field was tied to its being the experimental science of conscious experience.

Watson won: Psychology lost its mind, and behaviorism dominated psychology for many years to follow. Application to practical human affairs was not long in coming. Behaviorism was dynamic, quickly changing from the Watsonian form to learning theory and then adding behavior analysis à la B. F. Skinner. After narrating the history of *The Behavior of Organisms: An Experimental Analysis* (Skinner, 1938), Terry Knapp documents its pioneering impact within behaviorism. The reviews highlighted how different Skinner's work was from that of his predecessors. As Todd and Morris say in

their chapter, Skinner moved behaviorism far beyond simple stimulus-response psychology.

Skinner later departed even further from prior behaviorists by reintroducing mental events as a legitimate concern. Before Skinner, those people who were still interested in their minds found their interest unshared by mainstream psychologists. But Skinner argued that although mental events are apparent only to the individual having them, that does not render them unsuitable for science. Private behavior, just like public behavior, now was fair scientific game. Titchener would have smiled.

Behaviorism had proven its worth, and it encompassed all forms of activity. Skinner had developed a powerful research methodology—the experimental analysis of behavior—more or less simultaneously with radical behaviorism. Experimental analysis of behavior refers to the techniques for rigorous study of individual behavior that began with *The Behavior of Organisms* (1938); radical behaviorism consists of Skinner's views about the nature of behavior. Skinner's book showed that a rigorous science of individual behavior was a reality, and Skinner's chapter in the present volume provides a fascinating discussion of the motives, intentions, and clarifications that generated that book. Fred Keller engagingly reproduces his introductory lecture to the famous Columbia course based on the book. Keller's influence is exemplified by James Dinsmoor's account of serving as an assistant in that course. As a result of this exposure, Dinsmoor became one of the central contributors to Skinnerian behaviorism. Although he does not discuss his own contributions, Dinsmoor's important research on aversive control and observing responses as well as his technological skills have had big effects.

The Problem

Success and acceptance have not continued unabated. As in all sciences,

one would expect great leaps forward at times and periods of slow advance or even retrenchment at others. Skinner's endeavors (see Ernest Hilgard's introduction, Robert Epstein's list of Skinner's publications, and the concluding chapter for ample documentation of his accomplishments) resulted in increased recognition and acceptance of behaviorism, but behaviorism was struggling for its very existence even before Skinner's death. Some who once were enthusiastic have turned away, and new adherents are relatively few.

Gerald Zuriff even points out the lack of continuity between the research of the Skinnerians (the term *behavior analyst* had not yet been invented) working in the peak days and the contemporary efforts of those successors who now identify themselves as behavior analysts. The focus on moment-to-moment behavior has virtually disappeared. Classic problems have re-emerged to take precedence over behavior as it occurs in real time, and ethological concepts have become central to the investigation of motivated behavior. The intensive analysis of individual behavior as it is modulated over time has been replaced by relative measures of behavior, molar analyses, and dependence on hypothetical or inferred distributions. The powerful techniques for rigorously analyzing individual behavior so well described by Murray Sidman (1960) no longer routinely characterize behavior-analytic research. The uniqueness of the experimental analysis of behavior has been diluted to the extent that it seems only marginally different from the learning theories of the past.

Zuriff argues persuasively that the uniqueness and power of the approach actually were driven more by the technology developed initially by Skinner (1938) than by his views about the nature of psychology. I would add the emergence of schedules of reinforcement as the most potent independent variables ever found in the animal learning laboratory to the list of forces

that made the approach seem so compelling. Although the use of schedules as procedures for developing and maintaining behavior began with *The Behavior of Organisms*, it reached prominence with the awesome experimental analyses reported in *Schedules of Reinforcement* (Ferster & Skinner, 1957). That work put contingencies at center stage in understanding behavior. Vicki Lee apparently agrees with this assessment of the centrality of contingencies, because her examples of contingencies are all schedules. Her thoughtful analysis of the meaning and possible redefinition of contingency deserves more detailed treatment. The time is ripe for a discussion that relates her thinking to contemporary research on causation, contingency, and continuity in Pavlovian conditioning. This could well result in important insights and generalities.

Why The Decline? The Public Is Uninformed

One response to the decline of behaviorism is to maintain that the enemy is misguided, that behaviorism now and always has been misunderstood. The critics of behaviorism are either glaringly ignorant or exceptionally naive. They confuse Skinner with Watson, they think Chomsky buried Skinner once and for all, and so forth. Willard Day and Jay Moore offer a Talmudic analysis of the realities of Skinner's thought, while showing how poorly it is understood by philosophers. Another weapon is the linguistic purification rite. If only people would speak and think carefully, all problems would vanish. That ritual consists of telling ourselves and others exactly what is meant or is not meant by the words we and they use. Two examples of this are the chapters by Howard Rachlin and Philip Heline.

I confess that I do not fully understand what Rachlin is saying. His theme, as I see it, is essentially that of properly defining behaviorism under the name of *teleological behaviorism*.

Here mental terms refer to overt behavior and not to private events. Is Rachlin saying more than that what we mean by someone else's mental events are certain patterns observed in their public behavior? Skinner said that early on in his career, so I do not see what is new here. But Skinner proposed something different when he introduced the concept of private behavior. He now was referring to the experiences that only can be known directly by the experiencer, or what most of us mean by mental events or conscious experiences. Skinner insisted that an explanation of private experiences (consciousness) belongs in psychology. Maybe the lack of progress in dealing with private behavior since Skinner implanted it in behaviorism has motivated Rachlin to once again banish consciousness from psychological science. If so, teleological behaviorism sounds more like 1913 Watson reborn than an improvement on Skinner. Skinner might wonder if Rachlin has missed his point; Titchener might wonder why anyone other than Watson should have found the study of consciousness to be particularly difficult for a well-trained scientist. Rachlin needs to say what he proposes to do with the apparently universal experience of consciousness.

The most extreme instance of purification in this book appears in Philip Himeline's chapter. He does provide perhaps the clearest extant description of what it means to deal with behavior at its own level rather than as an index of something else. Unfortunately, some of his other clarifications are most puzzling. First, Himeline states that Skinner's (1935) generic definitions of stimuli and responses freed the reflex from its mechanistic connotations, and thereby freed organisms from billiard ball causation and being purely reactive machines. Is Descartes the enemy? It is not at all clear that reflexes ever meant anything that simple thereafter; it surely was not true of Sherrington's and Pavlov's reflexes. Note that in his own chapter Skinner regrets having

tied his thinking to the reflex. He now says that he should have been describing operants instead (discriminative operants probably would encompass stimulus-response relations even better). So, Skinner did not liberate the reflex as much as he challenged S-R theory. Second, what does it mean to say that "in behavior-analytic theory the world is characterized through categories of transaction with behavior" (p. 86)? Are categories of transaction something other than scheduling arrangements or contingencies? Third, Himeline says that response rate is transparent. What does that mean? What does it mean to say that "You can look right through a rate as it proceeds" (p. 86)? What do you see when you look through a rate? Surely not the individual responses as they occurred in real time, as you do when you look at a cumulative record. Rate is a derived measure involving both responses and time, but what makes it an abstraction? Is weight an abstraction? Is height? Is speed? All are measures made up of collections of units. And why does dividing responses by time result in an alternative to mental or physiological processes?

For several years Himeline has been arguing that organisms are the locus rather than the agents of behavior. Behavior is to be treated as an entity that merely requires an organism for display. As I understand Himeline, it is as if animals are like a book or movie screen, in the sense that the written words or pictures themselves are not the ideas but are only the vehicle for seeing the ideas. (I am not concerned here with arguments against inferred process, although I also would think it strange to avoid thinking of a book or movie as the product of an author.) I am bothered by the nonbiological nature of Himeline's argument. Books themselves are inanimate objects; they are not living creatures. Behavior is not imposed on an animal as images are imposed on film; it is part of the definition of a living animal. It is what a living creature does. Behavior requires

anatomy and physiology. It also depends on the animal's history. All of these interact with present environmental conditions to determine behavior. (Is this what is meant by categories of transaction with behavior?) What we mean by an organism (or by a species) is a particular combination of these features. Environmental events, be they lights, words, schedules of reinforcement, or anything else, do nothing to behavior until they are registered by an anatomical-physiological-historical entity. Only then does an animal come to behave in certain ways and thereby reveal environmental influences and effects of contingencies. If Hineline agrees with this (and features of his discussion indicate that he does), then he really should not refer to an animal as the locus of behavior rather than its agent. That is an unnatural way of talking about behavior, and its value is questionable in the first place. It is a straw man that produces rather than reduces verbal confusion, and it once again begs the ever-fruitless nature-nurture distinction.

Preferences for intervening variables (Hineline's view of response rate?) or hypothetical constructs or for simply describing functional relations are irrelevant to the central issue of whether organisms do or do not produce their own behavior. The hypothesis that individual differences or their absence in responding to stimuli may be responsible for attributing effects to organisms or to environments may well be correct, but it has no bearing on the plausibility of an organism-centered over an environment-centered approach to behavior. It proposes an interesting idea about attribution and verbal behavior that is well worth further study.

Why the Decline? Behavior Analysis Is Primarily Technology

Neither misunderstanding of behaviorism nor poor definition nor improper use of language truly explains why behavior analysis has become peripheral

in contemporary mainstream psychology. The most valuable aspect—namely, the experimental analysis of individual behavior—still is prized. What has faded from general interest is the conceptual underpinning (i.e., radical behaviorism).

After suffering through the years of theory-dominated research and unresolved grand experimental battles, many behaviorists began to lose interest in traditional learning theory. Waiting on the periphery was the incredibly powerful methodology that had been developed largely by Skinner. It was welcome and refreshing to use this methodology to try to understand the factors that directly determined real behavior instead of speculating in the rarefied atmosphere of process theory. The 1950s and 1960s were the real heyday of behavior analysis, and it truly seemed as if it was the path for psychology. Psychology had something real and important to offer the world: We knew how to control behavior.

Laurence Smith, Jay Moore, and M. J. Marr provide scholarly accounts of the degree to which Skinner followed Ernst Mach's dictates in his conception of proper science. Smith traces the evolution of Mach's thinking from that of Francis Bacon. The goal of science is the formulation of descriptive laws based on induction, and these provide the rules for mastering nature. Science is the economic description of facts, and the value of scientific laws is to benefit people in their interactions with the world. Theories and hypothesis are just provisional aids to a complete description. Smith shows that *The Behavior of Organisms* (1938) fits the Baconian-Machian tradition in being largely confined to description and immediate observations. Skinner's later work further emphasized application and technology, and his social utopianism also reflects Bacon. Moore complements Smith by comparing Machian positivism and logical positivism. Skinner replaced the neobehaviorists (Tolman, Hull, Spence) with the Machian positivism of radical behaviorism,

and Moore then goes on to critique any type of mediational theory that deviates from this orientation. Marr begins his important chapter with a reiteration of Mach's position and its relation to Skinner's thinking about behavior.

But in this orgy of Machian experimental control lurked the downfall of the kingdom. We seemed to believe either that we knew the basic principles of behavior already (mainly from Skinner, 1938) or that what was missing would automatically emerge in Baconian fashion from masses of good solid individual data. We became trapped by the power of our methodology (as Zurriff points out so well) and our ensuing ability to control behavior in and outside of the laboratory. Robert Bolles once said that it was as if some evil god had condemned psychologists to forever be experts in methodology but never to know what their data meant. Machian proscriptions really may not provide the ultimate answer to the nature of good science. Technological power does not lead inevitably to the compelling integrating principles that constitute fundamental scientific knowledge. Whether we were misguided or were insufficiently sensitive to the limitations of our leaders will never be known. We came to know a lot about the effects of specific contingencies of reinforcement, but we never had any good idea of what they were all about. Whenever someone did offer us a conceptual principle that suggested that maybe the curse had been lifted, as when Richard Herrnstein generated the matching law, our appreciation was evident from the number of experiments that followed. If even we were not really happy with being behavioral engineers, why should we expect others to develop a lasting appreciation of the importance of what we were doing?

Marr's very instructive chapter has powerful implications. Because Mach saw the goal of science as finding functional relationships, he was against atomic theory and the theory of relativity and the other developments that

now dominate our understanding of the physical world. Einstein, Bohr, and Heisenberg began to theorize with unobservables. In other words, the Baconian-Machian approach did not provide the last word in physics. Classical physics shifted to quantum physics, where unobservables moved to center stage, and classical physicists often were unable to make the transition. (For a poignant picture of what that meant to the old guard, see McCormach, 1982, which describes how a physicist trained in the tradition of Newton, Maxwell, Helmholtz, and Hertz was essentially destroyed by the new theoretical physics.) Marr suggests that the approach of classical physics characterizes much of Skinner's work, and that the common influence was Mach. Given the turning away from Machian principles in the enormously successful modern physics, where does this leave a Machian-based behaviorism?

Marr points out that another revolutionary aspect of quantum physics was the discovery that systems can have no intrinsic properties but instead are determined by how they are observed. Light is a wave or a particle depending on the particular conditions under which it is studied. Something similar to this complementarity happened in the experimental analysis of behavior when an accumulation of data began to suggest that the functional relations pertaining to a particular behavior changed radically depending on the conditions under which the behavior was studied. In addition, many instances now exist of functional relations depending strongly on the specific behavior observed. If the laws describing key pecking by hungry pigeons working in a confined environment perhaps describe only that species operating under those exact conditions, the lack of generality may make the data interesting to only a small group of specialists. The extent to which that level of specificity of functional relations is true is still unknown, and it still is not obvious how these effects are to be interpreted and reconciled. But a substantial body of

data of this sort did appear, and they have been the major force that undermined both learning theory and the principles of radical behaviorism, all of which were extrapolated from highly idiosyncratic experimental conditions.

No one denies the importance of a science of behavior, and no knowledgeable person denies the methodological power of the experimental analysis of behavior, at least in the form that focused on the moment-to-moment activity of individual organisms. What is missing, though, is what quantum theory provides to physics. The absence from the present volume of substantive integrating principles that organize facts and help us to understand what behavior is all about highlights the fact that methodology and philosophical purity do not fill that gap. Neither does any other existing form of behaviorism, nor do any of the types of cognitive theory (some of which, i.e., Gibson's ecological approach to perception, can sound quite behavior analytic), nor does any other existing organized approach to psychology do the job. The result is a badly fractionated psychology. The dilemma is not new; it was expressed in 1969 by Edna Heidbreder:

Reconceptualization must occur in the psychological enterprise as a whole as must occur in the development of any natural science if it is to achieve the kind of research which over the years has come to be expected of a well-established productive scientific discipline. A comparable reconceptualization must occur, but one that is more thoroughgoing and comprehensive and more nearly acceptable to all competent psychologists than any now available to students, to their instructors, or to psychologists generally. For no such reconceptualization has occurred in psychology, or, if it has, it has not been recognized and accepted as such. Psychologists have succeeded, in some cases brilliantly, in effectively reconceptualizing one or another portion of their domain. But they have produced no achievement which, in the manner of Newton's in physics and Darwin's in biology, has given psychology a conceptual scheme with reference to which, over a considerable period of time, all or nearly all of its available knowledge could be integrated, its ongoing research directed and to some degree coordinated, and its workers united into a single group sharing, despite diverse interests and specialized practices,

a common outlook on their common domain. (pp. 45-46)

Here we have an eloquent expression of the persistent failure of psychology: the absence of compelling organizing principles or integrating themes. We do not find it in behaviorism or in cognitive science or anywhere else. None of these schools provides or derives from a satisfying conceptual framework that brings all of scientific and technological psychology into one coherent body. The answer does not lie in methodological purity or the social and philosophical speculations of great thinkers. And without it we are condemned to eternal parochiality. We continue to await our Niels Bohr, our Albert Einstein, our Werner Heisenberg, our Charles Darwin.

Prologemenon to Unification

A fundamental advance was the unequivocal demonstration that behavior can be studied scientifically, but a fundamental mistake was the idea that behavior can be understood independent of biological processes. It is no wonder that behaviorists are often accused of extreme environmentalism: Even though no one ever said that the environment is all that matters, in practice behaviorists have tended to act that way. The environment does not directly impose behavior on an organism; the environment imposes constraints and opportunities that act on a living behaving organism. Anatomical, physiological, and biochemical states are at least as, if not often more, important than are present environmental demands and opportunities. Does any clinician doubt that the effects of a behavioral intervention are strongly affected by or negated by the client's physiological state? Do drugs work directly on behavior or do they influence behavior by affecting the nervous system? The brain is just as real as the environment, and it is critically important in determining if and how environmental events affect behavior. Hormonal level influences responsivity to a potential sexual partner, dopamine influences performance under

fixed-ratio schedules, brain damage determines the success we will have with various procedures for teaching language. Behavior is the product of a biological organism interacting with its environment. Variables do not have a certain effect on organisms; they have certain effects on organisms operating in the context of certain internal and environmental states. The analysis of behavior is part of biology, and it must be treated as such.

Behavior analysts have been talking about evolution recently, so one might think that they already have a biological perspective on behavior. Indeed, A. C. Catania asserts that Skinner moved behavior from physics to biology. Various writers have noted analogies between natural selection and reinforcement. Natural selection involves variation and selection; so does shaping. Natural selection entails an active role of the environment in determining when some variation is effective; so does stimulus control. But to put principles of behavior analysis at the same level as natural selection is to fall into a trap. Natural selection can explain the evolution of any biological property; behavior-analytic principles explain only behavior. The principle of reinforcement does not have the same status as the principle of natural selection: Reinforcement is the product of natural selection, not its equal. Natural selection can explain why the ability to be reinforced has evolved; reinforcement does nothing comparable with respect to natural selection, except maybe to explain why people accept it.

Evolution can be a starting point for psychology, but it needs to be an informed evolution if psychology is not to follow sociobiology in degenerating into storytelling. Biology does not stand or fall on the generality of natural selection. Different processes may be responsible for the diversity of species (macroevolution) and the adaptations of individual species to their environments (microevolution). Evolutionary biologists are very much concerned with neutral genes and alternative mecha-

nisms for evolution such as genetic drift, chance or random change in gene frequency, gene flow, mutation, or unequal frequencies of allelic forms. It may be that an ecological orientation to behavior is a more appropriate starting point for a biological perspective than is evolution. Here the starting point is that behavior (public and private) is the product of an active organism bringing with it a set of systems for interacting with its environment.

Was behavior analysis necessary for a real science of behavior? Ethologists, who also study behavior, share little common ground either theoretically or empirically with any psychological behavioristic tradition. Those biologists never doubted either the reality or importance of a science of behavior, and their origins precede behavior analysis. But behaviorism was necessary. Whereas biologists and anthropologists previously only thought to study behavior with naturalistic observational methods, behaviorism has shown unequivocally that behavior can be studied in the laboratory. Laboratory analysis goes far beyond naturalistic observation and inference, and it shows that it is possible to describe laws of how individuals really behave. Methodology reappears as the primary contribution of behaviorism. It is time for this powerful tool to be used to understand the biological nature and function of behavior.

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