JOURNAL OF THE EXPERIMENTAL ANALYSIS OF BEHAVIOR 1984, 42, 353-362 NUMBER 3 (NOVEMBER)

CONCEPTUAL APPROACHES AND ISSUES

M. JACKSON MARR

GEORGIA INSTITUTE OF TECHNOLOGY

The experimental analysis of behavior has lagged far behind mainstream psychology, particularly cognitive psychology, in the study of complex behavior—remembering, thinking, imaging, problem solving, and the like. Yet it is the study of these kinds of behavior that will provide the greatest justification of our continued existence in the community of behavioral scientists. Focusing primarily on remembering as a complex performance, aspects of (1) radical behaviorism, (2) the methodology of the experimental analysis of behavior, and (3) the special contributions of B. F. Skinner are assessed as explicitly or implicitly discouraging the experimental treatment of such complex behavior. Although there are encouraging signs of advancement into the present domains of cognitive psychology, future success of the experimental analysis of behavior in this endeavor will require aggressive pursuit by investigators and more effective training of their students.

Key words: methodology, radical behaviorism, cognitive psychology, memory, verbal behavior

---

The evident timeliness of a *Journal of the Experimental Analysis of Behavior* issue on the topic of "Future Directions in Behavior Analysis" is an expression of at least two very different conditions. First, one may view the experimental analysis of behavior and its attendant philosophical base as a kind of Aladdin's lamp by means of which we simply choose approaches to solving significant behavior problems. From this perspective, we could paraphrase Skinner (1953) and say "The methods of the experimental analysis of behavior have been enormously successful wherever they have been tried. Let us then apply them in new directions." In a sense this issue reflects a kind of fertility ritual in which we celebrate and anticipate the fecundity of the experimental analysis of behavior.

But there is a darker side to the issue of future directions, which only the naive would fail to see. Despite its apparent successes, the experimental analysis of behavior and the radical behaviorist philosophy have never been appealing to psychologists in general, or to many others for that matter. Two well documented and related reasons are that our field is both conceptually difficult and contradictory to much previously established philosophical dogma. Those psychologists the first doesn't scare away, the second one will! This condition, in turn, is partly a result of the sad fact that many psychologists are not really well trained in science or philosophy; thus they are unable to appreciate or critically analyze their own conceptual approaches, much less anyone else's.

Radical changes in how Nature is to be approached have always caused problems to the practicing scientist in any field. Some physicists had great difficulty accepting the consequences of relativity theory; and more than 50 years after the development of quantum mechanics there is considerable controversy over its interpretations. But no one would question the effectiveness of these theories in having solved some of the most significant problems a physicist could pose. Herein lies a major contrast. Many experimental psychologists view the experimental analysis of behavior and radical behaviorism as conceptual systems that have contributed little to the solution of significant problems in the field and this view grows stronger each

---

This paper is dedicated to the affectionate memory of Don Hake—colleague and comrade. Reprints may be obtained from the author, School of Psychology, Georgia Institute of Technology, Atlanta, Georgia 30332.

day. True, there is grudging respect for behavior modification among some practitioners in the field, but generally it, too, is a suspect venture.

What significant problems are our critics talking about? Primarily those presently residing in the domain of cognitive psychology: memory, thinking, imagery, problem-solving, language, perception—most, if not all, the traditional areas of psychology. Skinner, the founder of our movement, has addressed these problems interpretively in his theoretical works, but his thinking on these issues has received little empirical attention by his own flock; they have, by and large, either ignored them or been content with expressing Skinner's interpretations. The book *Verbal Behavior* (1957) is an interesting example. This most original and incisive of Skinner's works has inspired few empirical studies and, indeed, remains largely unread. We might blame Chomsky for the lack of interest in *Verbal Behavior* by cognitive psychologists, but how do we account for our own indifference? Language (or "verbal behavior" as we prefer to say) has become almost the exclusive province of the cognitivists with the result that even though the functional relationships they discover might be consistent with those suggested by behavior-analytic theory, proper credit is unlikely to be given or, indeed, acknowledged. Situations such as this lead one to consider the possibility that there may be little future to the experimental analysis of behavior and radical behaviorism, at least as we have come to know and appreciate them. Skinner says in *Beyond Freedom and Dignity* (1971) that if a culture does not arrange contingencies for its own survival, then so much the worse for that culture. At the 1981 Association for Behavior Analysis meeting he expressed concern over the lack of attention given to the experimental analysis of behavior outside the field. There is irony in the fact that we who are supposed to be experts in behavioral control exert little positive control over our critics, if indeed we can get their attention at all!

There are, of course, reasons for this. The primary focus of this paper is on the relative lack of experimental analysis in those areas that are likely to provide the greatest justification for our continued existence as behavioral scientists. To treat this issue in terms of future directions we must look retrospectively at the Holy Trinity of operant theory: radical behaviorist philosophy, the methods and results embodied by the experimental analysis of behavior, and B. F. Skinner himself. It is my thesis that aspects of this Trinity have exerted rather sharp stimulus control over our scientific behavior and, as such, have implicitly or explicitly discouraged research in areas now principally dominated by cognitive psychology. The physicist Prigogine has asserted that "Scientific work consists of elective exploration rather than discovery of a given reality; it consists of choosing a problem that must be posed" (1980, p. 51). What follows is a brief consideration of the concurrent contingencies that have controlled our choice of scientific problems.

For expository purposes, it is useful to provide a motif. In particular, I would like to reflect from time to time in this paper upon how operant theory has treated the problem of "control at a temporal distance," better known as "memory." This seems appropriate since memory is the topic that the cognitive psychologists would claim with pride as their domain in terms of both empirical and theoretical effort. (In what follows I will use the term "memory" simply for ease of discourse, but with due apologies to Marc Branch, 1977).

BEHAVIORIST PHILOSOPHY AND MEMORY

Memory was there from the beginning. This should not be surprising since, as Skinner reminds us at the start of *About Behaviorism* (1974), radical behaviorism is not the science of behavior, but the *philosophy* of that science. As epistemology, its concerns embody what we deem complex behavior: perception, thought, language, consciousness, and memory.

When Skinner arrived as a graduate student at Harvard, he was already a behaviorist, having been oriented in that direction primarily by selective reading of Bertrand Russell's book *Philosophy* (1927/1974). Although behaviorism at that time was dominated by Watson, Skinner notes that Russell's treatment of behaviorism was considerably more sophisticated than Watson's. Indeed it was, and important aspects of Skinner's thinking in a number of areas may find their origin in Russell's criticisms of Watson.

In referring to Watson's theory of memory as embodied in a "verbal habit," Russell says:

> The theory is preferable to ordinary psychological theories in many ways. In the first place it is not an attempt to treat memory as some sort of mystical faculty and does not suppose that we are always remembering everything that we should remember if a suitable stimulus were applied. (p. 76)

It is not difficult to see the link between this statement and Skinner's question: "Where is the dog's trick when he isn't performing it?" This position, which clearly eschews the notion of memory as a "thing" *or* a "process," continues to characterize many post-Wittgensteinian philosophies. For example, Malcolm (1977) supports Russell's concept of "mnemic causation," which essentially assigns causes of present behavior to history and context rather than to some continuing intervening representational process. This view is, of course, contrary to an information-processing analysis based upon computer metaphors of "encoding," "storage," and "retrieval."

Russell comments, however, that memory cannot refer simply to a verbal habit because "we recount a past incident in words we never used before. In this case it is not the actual words we repeat, but only their meaning" (p. 77). It is from an observation of this kind that one could derive the concept of a *functional class* of responses, of basic significance to the analysis of operant behavior and achieving its ultimate theoretical status in *Verbal Behavior* (1957).

In a somewhat different vein, criticizing Watson's subvocal theory of thinking, Russell asserts:

> It should be realized that behaviorism loses much of its attractiveness if it is compelled to postulate movements that no one can observe and that there is no other reason to assume. . . . When the behaviorist assumes small occurrences which are needed solely in order to safeguard his theory, he is in a less strong position. (pp. 79-80)

Here is a direct attack not only on the naiveté of Watsonian behaviorism, but on subsequent developments in methodological behaviorism with its "r_g's", and on cognitive psychology and its "representations." Again, we encounter the issue of intervening processes, presumably unobservable in practice if not in principle, and placed in a different domain of analysis, to provide an explanatory medium of causation. This issue is fundamental to an understanding of why radical behaviorism and experimental analyses of behavior have had little to say about memory and about cognitive processes in general.

The kind of influences ultimately to be exerted on Skinner in his formulation of an epistemology are reflective of classical physicists' attempts to deal with action at a distance (Laudan, 1981). Two general approaches to science emerged from this issue. The first, heavily influenced by Bacon's inductivism and Newton's "*hypothesis non fingo*," was articulated by Thomas Reid in the 18th century. Reid was not only interested in applying Newton's *Regulae Philosophandi* to guide inquiry into problems of physics, but in founding thereby a "scientific mental philosophy, that is, psychology" (Laudan, 1981, p. 89). Consider some of Reid's maxims (as expressed by Laudan, pp. 90-93):

1. As a matter of historical fact, hypotheses and conjectures have not been very productive, and have tended to mislead rather than enlighten us.

2. The adoption of an hypothesis prejudices the impartiality of the scientist.
3. The hypothetical method presupposes a greater simplicity in nature than we find there.
4. Hypotheses can never be proved by the "reductio" methods.
5. Any positive causal account must be sufficient to explain the relevant appearances and postulate only entities and mechanisms whose existence can be *directly* ascertained.
6. The method of hypotheses substitutes premature theoretical ingenuity for painstaking experimental rigor.

The reader of "A case history in scientific method" (Skinner, 1956) and "Are theories of learning necessary?" (Skinner, 1950) will find all of the above principles imbedded in these works. Reid's emphasis on inductive, experimental methods and on avoidance of hypothetical and unobservable entities and processes simply finessed the problem of mysterious mediation and paved the way for an approach based upon functional analysis. The alternative of how science should proceed was espoused by Reid's contemporary, David Hartley, who was a substantial contributor to the development of the hypothetico-deductive method. The exercise of this method led some physicists of the 19th century to develop theories of the aether and of the kinetic activities of atoms, much to the amusement and even disdain of their colleagues, the most influential of whom was Ernst Mach.

Skinner's behaviorism was to be strongly influenced by Mach, whose book *The Science of Mechanics* (1893/1960) Skinner read while a student at Harvard. This work probably introduced Skinner to operationism and, more significantly, to the notion of behavioral analysis via *functional relationships*. Mach said it in another way:

> Faithful adherence to the method that led the greatest investigators of nature . . . to their greatest results restricts physics to the expression of *actual facts* and forbids the construction of hypotheses behind the facts where nothing tangible and verifiable is found. If this is done, only the simple connection of the motion of masses, of changes in temperature, of changes in the value of the potential function, of chemical changes and so forth is to be ascertained, and nothing is to be imagined along with these elements except the physical attributes or characteristics directly or indirectly given by observation. (p. 599)

Adherence to views of this kind led Mach to reject atomic theory. "Atoms . . . are things of thought," he said (p. 588). Mach was more correct in this assessment than he could have imagined; but in the present context, views of this sort, based upon the concept of "economy of thought" and the functional relationship, were to heavily influence radical behaviorism's relative *non*-treatment of a topic like memory.

Radical behaviorist philosophy as explicit, extant doctrine can reasonably be timed from Skinner's 1945 paper, "The operational analysis of psychological terms." It was this extraordinary work that put the "radical" in radical behaviorism. For Skinner, the psychological world inside the skin could not represent a machinery of mediation, but rather emerged through the individual's interaction with environmental contingencies. It was the world outside the skin that gave us a world inside the skin. Because private events (e.g., covert behavior) were placed on no more than an equal footing with public behavior, they could not properly be considered *fundamental causes* of public behavior. To represent fundamental causes, the private events would presumably have to be of different "stuff" from the public events that emerged from them. (Does it make sense to say that the brain makes a mistake? On a more elementary level, mercury atoms, whatever they may be, are certainly not silvery and slippery.) This removed the old issue of observable versus unobservable entities; if both are of the same stuff, then, in a sense, nothing is hidden. An egg inside a refrigerator is still an egg. Removing any privileged status from private events and shunning any extra- or sub-behavioral theorizing in "Are theories of learning necessary?" (1950), Skinner rendered memory essentially a non-problem or, at best, one reserved for the physiologist.

The topic of memory gets exceedingly scant coverage in *Science and Human Behavior* (1953). There is a one page discussion of the "Behavior of Recall" where the focus is on the practical technique of self probes to aid recall (p. 245). This aspect is treated a bit more in *Verbal Behavior* (1957) where there is a discussion of responses to past behavior (pp. 142-148), and of forgetting from the perspective of interference theory (pp. 207-209). Skinner considered memory in more detail in *About Behaviorism* (1974, pp. 107-110), including a trenchant commentary on cognitive theory. More recently, an expansion of this critical analysis appeared in "Why I am not a cognitive psychologist" (1977). The accelerated attention given by Skinner to memory must be understood in part by the rapid growth in memory research beginning in the 1960s with the advent of information-processing models. These models and others of similar ilk were readily adopted and extended by a large community of experimental psychologists imbued with methodological behaviorist philosophy which is most sympathetic to intervening variables and hypothetical constructs. Research in memory at the advent of information-processing models was heavily laden with dreary interference theories and nonsense-syllable methodology that seemed hardly to have advanced the field since Ebbinghaus. Elaborate models patterned on computing machines and the development of sophisticated methods using *real* words, sentences, and even pictures no doubt represented a fresh tide in a hithertofore stagnant backwater. The emphasis on studying memory in "ecologically valid" contexts was (and is) exciting to these workers.

The 19th-century physicists' arguments over the status of mediating entities like the aether and unobservable atomic processes were based upon different epistemological paradigms that have obvious parallels to the present debates between cognitive psychology and radical behaviorism. The difficulties were to some extent resolved in physics by the construction of a mathematical description providing prediction and organization of *observables*, a description remote from the homey "billiard-ball" pictures of early days. The power of a mathematical account resides in the user's ability to discover properties of Nature by appropriate manipuluation of the verbal stimuli, that is, by talking about it.

To deal with the problem of action at a temporal distance, the cognitive psychologists have conceptualized a world between behavior and brain that might be described most charitably as the "software" of the brain. This is a world of baroque richness providing for both intricate and infinite variation in programs to express the same behavioral theme. Not only is this sort of activity reflective of what Skinner meant when he said "Theories are fun" (1950), but I believe it is possible to make a case for the kinds of verbal behavior emitted by cognitive psychologists on pragmatic, that is, heuristic, grounds. Such verbal behavior could be said to play a role similar to an abstract mathematical account, where appropriate manipulations might lead to specification and organization of controlling variables. This is a perspective fraught with danger, however. The metaphorical and "extra-episodic" character of the verbal behavior of the cognitivist tends to control responses incompatible with a functional account. The necessary translation from mental events to behavioral events of the kind suggested by Harzem and Miles (1978) does not tend to occur. Somewhat like modern mathematical physics, the medium is the message, without, of course, the depth, the power, or the logic of an analytic, mathematical account.

Radical behaviorists have found (as Harzem and Miles put it) the "cloud-cuckoo-land" thinking of cognitive theory both repugnant and ludicrous. Meanwhile, the cognitivists have continued to gather in the functional relationships and weave them into their gossamer theories. We have been content largely with pointing out how muddle-headed the cognitivists are. Like Einstein, the radical behaviorist has not been intimidated by an apparent need for an aether—in this case a kind of mental aether—to mediate control at a temporal distance.

We believe that remembering is a function of history and context. But one has to develop this perspective in detail. The experimental analysis of stimulus control emerged from consideration of variables rather different from those needed for detailed functional accounts of remembering. Yet, we are often guilty of "explaining" a performance like recall as an expression of "stimulus control." But this is simply catchword explanation; without knowing what functional relationships actually have been discovered in the study of recall, we have no way to evoke effectively those pertinent relationships in the area of stimulus control, if, indeed, they are available. If they are not available, *we* need to discover and examine them.

METHODOLOGY AND MEMORY

The experimental analysis of behavior is the issue of a mating between behaviorist philosophy and the methods of classical physiology. The animal chamber and the cumulative recorder, as ingenious as they are, are direct descendants of the dissecting table and the smoked drum. The family relationships apply not only to the hardware, but to design technique as well. Claude Bernard's *An Introduction to the Study of Experimental Medicine* published in 1865 encompasses much of Sidman's *Tactics of Scientific Research* (1960), including the latter's criticisms of statistical analysis. The emphasis on the study of the behavior of the *individual* organism that led to the founding of the *Journal of the Experimental Analysis of Behavior* would have seemed perplexing to physiologists inasmuch as their science could hardly have developed otherwise! The analysis of the effects, say, of acetylcholine on heart rate is an old but perfect example of the standard ABA design. Under highly controlled conditions a baseline "performance" is established (A). Then the drug is administered at various doses (B) with return to baseline following each dose (A). No one committed to the experimental analysis of behavior should be dismayed by the methodological bond with physiology; it is but one expression of our fundamental conviction that behavior is a biological property of organisms. Our biological perspective has received too little emphasis. Cognitive psychology has attracted many researchers away from the consideration of genetic, adaptive, historical, and contextual analyses into the domain of the machine, to focus on *conceptual* internal processes that are more appropriate to a computing device than to a biological system.

Exercise of our methods over the last 30 years or so has generated a rich fabric of functional relationships descriptive of schedules of reinforcement and stimulus control. Our theoretical focus on contingencies of reinforcement and the methods used to study them has exerted sharp control over the behavioral questions investigated and has reduced the likelihood of dealing directly with questions of concern to a large domain of experimental psychologists. Our long-standing commitment to the detailed analysis of contingencies is rationalized on the basis that such contingencies are *fundamental determinants of behavior* and an understanding of the controlling variables of "simple" (i.e., easily described) schedule arrangements is essential to the ultimate analysis of complex performances. In fact, the problem of isolating the controlling variables of schedule performance has been of immense difficulty. It has turned out that the behavior engendered by easily described schedules is not so easily analyzed. In some cases—for example, the fixed-interval schedule—we may not yet have *described* the performance carefully enough for an effective analysis.

Some researchers have suggested that we abandon this schedule-analysis effort altogether, not only because it is too difficult, but also—in perhaps a fox-and-thegrapes guise—because they believe that schedules are nothing but "contrivances." Contrivances or not, the fact remains that contingencies are the fundamental explanatory device of the experimental analysis of behavior. It is difficult to imagine with what the experimental analysis of behavior could replace them. Yet we are put in a peculiar position that could be described as follows: Imagine if physics had been formulated backwards so that Kepler, Galileo, and Newton (perhaps we should call them Notwen, Oelilag, and Relpek) began by struggling unsuccessfully with discovering the symmetry properties of theoretical entities called elementary particles, while others implored them to explain how the earth moved around the sun or why an apple fell to the ground. Our bizarre physicists might have replied, "Oh, that's too complicated, but our fundamental analysis of elementary particles will someday give us the answer—if we can find the elementary particles!" We seem to have discovered, as the modern physicist has, that the world of elemental processes is no less complex than the world it is supposed to explain.

We have finessed some of the problems of contingencies by adroit operations of partitioning and integration, as manifested, for example, in the various formulations of the matching law. However, the extension of the explanatory power of contingencies to the analysis of complex performances is based upon the assumption that nominal units of behavior obey the same laws regardless of the size of the units—a useful, if tenuous, assumption that needs considerable attention if the experimental analysis of behavior is to have credibility outside the field (Marr, 1979; Shimp, 1979). One of the most significant contributions to the development of modern physics was Bohr's Correspondence Principle, which provided a bridge between quantum and classical processes and with it a degree of confirmation of the quantum theory. We lack, however, any correspondence principle to take us from one level of behavioral analysis to another, and there are no consistent units of analysis or even units of measurement. Behavior, unlike a crystal, does not fracture along invariant lines. Whatever constitutes a functional unit, it is a *dynamic* entity. Skinner calls it "lively"—and we have little understanding of how complex performances emerge from putatively simple ones.

In wrestling with the problems of contingencies, we have typically used elegantly simple procedures that engendered steady-state (equilibrium) performances. We have tended to shy away from analyses of transitions and have operated largely under the explicit assumption of reversibility as embodied, for example, in the ABA design. There are, of course, notable exceptions here, including some found in Ferster and Skinner (1957), but the trend is clear and the implication is that phenomena such as memory would get little attention.

There is an interesting correspondence here with the development of classical thermodynamics, an enterprise whose theoretical structure shares much with the experimental analysis of behavior. Classical thermodynamics restricted its attention to macroscopic (i.e., observable) properties of matter and as Nash (1970) puts it, ". . . calling on only a minimal array of axiomatic postulates, it cunningly contrives to discuss material phenomena without making any assumption whatever about the constitution of matter" (p. 1). It embodies functional relationships between such variables as pressure, volume, and temperature that in turn define equilibrium states of systems. This approach characterizes much of the experimental analysis of behavior, and indeed Skinner describes the maintaining conditions for his own behavior as follows: "It is reinforcing to find variables which change in an orderly fashion and which permit one to formulate behavior as a scientific system in the sense in which that term is used, for example, by Willard Gibbs" (1969, p. 93).

Classical thermodynamics in its application emphasizes reversible equilibrium processes despite the clear fact that most processes in Nature are irreversible. This irreversibility was embodied in the Second

Law of Thermodynamics, but the full implications of this law and irreversibility in general is a product of our own century. In particular, the *constructive* role of irreversible processes has only just begun to be appreciated (Prigogine, 1980). Such constructive processes, no doubt, are manifested by the changes in organisms attendant upon their interactions with the environment.

Our uneasiness with irreversible effects has not totally prevented us from examining them, but our techniques, like those of classical thermodynamics, have typically approached the problem by establishing steady-state conditions. They are best exemplified by the stimulus-control techniques of repeated acquisition and, most pertinent to the analysis of remembering, delayed matching. It has been primarily through the latter procedure in various guises that the experimental analysis of behavior has entered into the study of remembering. However, present stimulus-control techniques will have to be considerably elaborated and augmented to provide a functional analysis of remembering that is adequate to its subtleties. The great irony is that many of those best able to execute this program have already begun to apply the techniques, but have done so in the service of cognitive theory, calling themselves, appropriately, "animal cognitive psychologists." One is tempted to paraphrase an old comment on the Russian nationality, "Scratch a Russian and you'll find a Tartar" to "Scratch a behaviorist and you'll find a cognitivist." No doubt there are a number of variables controlling this retreat into the bosom of cognitive theory, from the unguent seductiveness of its explanatory prose to the practicality of communicating the results of research effort in the dominant terminology. Whatever the reasons, the implications are that the experimental analysis of behavior is bereft of explanatory appeal. Methodologically we may have gained the respect of the majority of experimental psychologists—we are the bringers of baselines—but we have made little headway against the doctrine that behavior itself is but a diagnostic sign of mediated processes. Many of our own researchers do not have adequate confidence in the theoretical and philosophical foundations of the experimental analysis of behavior to contribute anything but method. Method may, in the last analysis, be the only enduring contribution of our field. Once established, it does not depend upon radical behaviorist philosophy any more than the value of Pavlov's methods depended upon his particular view of cortical functioning. And, as pointed out earlier, we inherited the basic features of our methods from others.

What can be done? First, experimental analysts of behavior must begin to assault the difficult behavioral problems. Those who have directed their skills into applied behavior analysis have already led the way and should be commended for their courage. Their work has established the value of our philosophical convictions. Basically, I believe our methods are equal to the task; indeed, the work of researchers like D'Amato. Shimp, Rilling, Honig, and Wasserman clearly indicate this, but the analysis of complex performances will, no doubt, call for innovations and in some cases, perhaps, usurpations of method. We should not fear this, so long as the procedures are appropriate to a functional analysis. Second, advances in the analysis of contingencies have reached the all-important stage of significant analytical treatment—description and prediction—as, for example, Nevin's paper in this issue attests. We must encourage the further development of analytical models of behavioral processes and train our students accordingly. I am not advocating "a flight to mathematical models"; we have reached levels of sophistication not only in the specification of controlling variables, but also in the application of mathematical techniques, to allow for real progress in the development of quantitative formulations of behavior. Stochastic processes, time-series analyses, system theory, differential equations, Laplace and Fourier transforms, catastrophe theory, and the calculus of variations are examples of mathematical methods that should become part of the armamentaria of future behavior analysts. We should lead the way toward a future in which mathematics will be as essential to the presentation and development of behavioral analysis as it has been to physics.

Our deepest problems, however, lie not in method, but in interpretation. Those of us committed to a rigorously behavior-analytic approach must hone our philosophical tools, for herein lies the foundation of our work. Cognitive psychology, based loosely upon Cartesian thought and muddled interpretations of philosophies of physical science and the romance of the computing machine, is riddled with rational absurdities. But radical behaviorism itself is in need of careful philosophical analysis. Progress in this direction has been made by the founding of the journal *Behaviorism*, by critical discussions of relationships between Skinnerian thought and modern linguistic philosophers (e.g., Day, 1969a, 1969b), and by books like the highly significant *Conceptual Issues In Operant Psychology* (1978) by Harzem and Miles. But treatments of these kinds must reach the psychological and philosophical audience at large if we are to achieve effective credibility and criticism. Important articles are sequestered in *Behaviorism, Journal of the Experimental Analysis of Behavior, Journal of Applied Behavior Analysis,* and *The Behavior Analyst*, and thus we end up talking to ourselves. In this context we should repudiate narrow graduate training that represents psychology largely, if not solely, from the radical behaviorist perspective. Others of different theoretical and empirical persuasion have and will continue to contribute much to challenge behavior analysts who, by accepting such challenges, may strengthen their own position or, if necessary, abandon it.

Finally, as students and teachers and researchers, we have too long and too extensively relied on our founder, B. F. Skinner, as an authority to explain and even to establish the significance of a behavioral issue. He has been exceedingly clever and creative in extrapolating and generalizing from laboratory results obtained with creatures like the pigeon to the most elaborate types of human behavior. Such boldness has had two opposing effects. First, in a positive direction, some researchers have been encouraged to extend an experimental analysis into the dark forest of human behavior. For others, however, imaginative but apt extrapolations have provided false comfort that the analysis of complex behavior has already been done—the "nothing-but" syndrome. We all need to look for ourselves.

REFERENCES

Bernard, C. (1957). *An introduction to the study of experimental medicine* (H. C. Greene, Trans.). New York: Dover. (Original work published 1865)

Branch, M. N. (1977). On the role of "memory" in the analysis of behavior. *Journal of the Experimental Analysis of Behavior,* **28,** 171-179.

Day, W. F. (1969a). Radical behaviorism in reconciliation with phenomenology. *Journal of the Experimental Analysis of Behavior,* **12,** 315-328.

Day, W. F. (1969b). On certain similarities between the *Philosophical Investigations* of Ludwig Wittgenstein and the operationism of B. F. Skinner. *Journal of the Experimental Analysis of Behavior,* **12,** 489-506.

Ferster, C. B., & Skinner, B. F. (1957). *Schedules of reinforcement.* New York: Appleton-Century-Crofts.

Harzem, P., & Miles, T. R. (1978). *Conceptual issues in operant psychology.* Chichester, England: Wiley.

Laudan, L. (1981). *Science and hypothesis: Historical essays on scientific methodology.* Dordecht, Holland: D. Reidel.

Mach, E. (1960). *The science of mechanics* (T. J. McCormack, Trans., 6th ed.) LaSalle, IL: Open Court. (Original work published 1893)

Malcolm, N. (1977). *Memory and mind.* Ithaca: Cornell University Press.

Marr, M. J. (1979). Second-order schedules and the generation of unitary response sequences. In M. D. Zeiler & P. Harzem (Eds.), *Advances in analysis of behaviour: Vol. 1. Reinforcement and the organization of behaviour* (pp. 223-260). Chichester, England: Wiley.

Nash, L. K. (1970). *Elements of chemical thermodynamics* (2nd ed.). Reading, MA: Addison-Wesley.

Prigogine, I. (1980). *From being to becoming: Time and complexity in the physical sciences.* San Francisco: Freeman.

Russell, B. (1974). *An outline in philosophy.* New York: New American Library. (Original work published 1927)

Shimp, C. (1979). The local organization of behavior: Method and theory. In M. D. Zeiler & P. Harzem (Eds.), *Advances in analysis of behaviour: Vol. 1. Reinforcement and the organization of behaviour* (pp. 261-298). Chichester, England: Wiley.

Sidman, M. (1960). *Tactics of scientific research.* New York: Basic Books.

Skinner, B. F. (1945). The operational analysis of psychological terms. *Psychological Review,* **52,** 270-277.

Skinner, B. F. (1950). Are theories of learning necessary? *Psychological Review,* **57**, 193-216.
Skinner, B. F. (1953). *Science and human behavior.* New York: Macmillan.
Skinner, B. F. (1956). A case history in scientific method. *American Psychologist,* **11**, 221-233.
Skinner, B. F. (1957). *Verbal behavior.* New York: Appleton-Century-Crofts.
Skinner, B. F. (1969). *Contingencies of reinforcement.* New York: Appleton-Century-Crofts.
Skinner, B. F. (1971). *Beyond freedom and dignity.* New York: Knopf.
Skinner B. F. (1974). *About behaviorism.* New York: Knopf.
Skinner B. F. (1977). Why I am not a cognitive psychologist. *Behaviorism,* 5(2), 1-10.