SEARCHING FOR A FUTURE FOR BEHAVIORISM: A REVIEW OF
THE NEW BEHAVIORISM BY JOHN STADDON

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In 1933 Edna Heidbreder published her classic book, Seven Psychologies. There she regretted that psychology remained, at that time, a fractionated, ill-fitting assemblage of endeavors, with each segment finding the others wanting, and each “appalled” at the “flimsiness and shoddiness of much material they are asked to accept as genuine fact” (p. 3). “Psychologists,” she added, “are continually looking upon the work of their colleagues and finding that it is not good. And with little hesitation, or with none at all, they expose the weaknesses and flaws they discover” (p. 3). Notwithstanding all that, she was optimistic. In time “enough fact,” she thought, would provide a basis for psychology “to make a single, solid system” (p. 17).

Close to a century has passed since then but her optimism remains unfulfilled. If anything, the condition of the academic discipline of Psychology is now more muddled and conflicted than it was then. The intervening period has brought enormous improvements in the technology available for psychological research, and theoretical and conceptual insights that have provided signposts for psychology to emerge from the doldrums of a century of conceptual muddle: genetics, the neurological sciences, and theoretical physics, for example, and the philosophical insights of Wittgenstein, Austin, Ryle, and others. Sadly, however, psychology in general has taken little note of them, and those segments that have, have taken unto themselves fashionable names such as “evolutionary

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1 By “Psychology” I refer here only to academic psychology as practiced in the academy, that is, mainly in the universities; and even more narrowly, to psychology of that kind in the English-speaking world. As the English language increasingly becomes universal, those of us who think and write in English are tending to become intellectually ethnocentric, overlooking the psychologies of other cultures, even those of mainland Europe—much to our loss. There are, moreover, other psychologies such as dynamic psychologies rooted in the works of Freud, Jung, neo-Freudians and others that have gone their own way with little, if any, contact with the psychology I consider here. I note these matters but do not discuss them here as they are beyond the brief of the present paper.
psychology,” “genetic psychology,” “behavioral toxicology,” “artificial intelligence” and the like and have in large measure taken up their wares and gone to establish their own meetings, publications, etc. None of these splinters are untouched, however, by behaviorism. Behaviorism explicitly emerged in 1913, and had a stormy career in and beyond psychology. It is against this background that Staddon’s book offers a version of behaviorism that, he anticipates, will give a new theoretical and empirical coherence to the study of behavior.

Customarily “scientific” or “experimental” psychology dates its beginning to 1879 when Wilhelm Wundt established his experimental laboratory in Leipzig. Broadly speaking, from that time to the present the progression of psychology has been in three successive but overlapping phases: introspective psychology, behavioral psychology, and cognitive psychology. Behaviorism was basically a protest movement against the then ubiquitous method of introspection, and cognitivism was a protest movement against, as its proponents saw it, the severe constraints imposed by behaviorism on the range of phenomena open to scientific psychological investigation.

Introspective psychology based its method on the then universal assumption that psychology is the study of the mind, and, narrowing the term, of the experiences of an individual. From this it followed, quite rationally, that a science of psychology would obtain its data from the reports of individuals on their experiences: how a particular experience, say a simple experience of seeing a color, arose, how it stayed its course, how it mixed with other sensations, and how it ended. From such humble beginnings the range and complexity of introspective psychology rapidly grew, giving rise to various theories and to theoretical disagreements, notably on method; for example, to seemingly interminable argumentation on whether it was scientifically more sound to train the subjects about how to observe and report their experiences or to work with experimentally “naïve” subjects. By the very early 1900s, however, it was becoming evident that this kind of psychology was getting bogged down and that no way could be found independently to verify the mounting collections of data. Psychology had not, after all, become a science. A growing dissatisfaction was abroad.

Behaviorism arose in this context. It was already in the air, beginning just before the turn of the 20th century through the influence of Darwin’s theory and the introduction of animal research into psychology on the one hand, and reports of Pavlov’s discoveries on the other. It became an explicit, named, and unified movement with the publication, in 1913, of Watson’s paper, “Psychology as the behaviorist views it,” sometimes known as the \textit{behaviorist manifesto}. It opened with the sentence, “Psychology as the behaviorist views it is a purely objective experimental branch of natural science” (p. 158). After an interruption caused by

\footnote{It is worth noting, by way of an example, that there is no basic difference between “evolutionary psychology” of the present and the influential “functionalism” of early 20th century. To make matters worse, the former is hardly informed of the literature of the latter. One of the less attractive features of contemporary psychology is the practice of giving new names to old matters and treating them as if no previous knowledge existed about them.}

\footnote{I list these not because they are alike by any criterion but as examples of a variety of “areas.”}
the First World War, in 1919, Watson published his first book, by the same title. By then he had further developed and substantially broadened his views of behaviorism. The book began “Psychology is that division of natural science which takes human activity and conduct as its subject matter. It attempts to formulate through systematic observation and experimentation the laws and principles which underlie man’s actions” (p. 1, my italics). This new behaviorism was for all psychology, not a separate scientific enterprise. The italics highlight the essential points of Watson’s vision. Classical behaviorism was to give equal place to experimentation and systematic observation, to consider its subject matter to be both “activity” and the more complex “conduct,” and to discover laws underlying the observed phenomena and not merely build up an inventory of whatever behavior is observed. There remained, however, the unavoidable, one may say pestilent, question: what was to be done about phenomena that were not merely behavior, namely phenomena such as feeling, perceiving, understanding, comprehending, etc., that had been the object of study of the to-be-displaced introspectionist psychology? Watson’s answer was ambiguous: generally he ignored them, and on occasion, when he was reacting to strong criticism, he claimed he did not know what the words referring to them meant. (In this paper these are collectively named “mind-words.”)

From the 1920s to the late 1940s, with the advent of the grand learning theories, behaviorism became the dominant practice, though not the dominant philosophy in psychology. Then, Skinner’s first significant book, The Behavior of Organisms, published in 1938, initiated a series of gradual changes in classical behaviorism, arriving at a different form of behaviorism with the publication of Science and Human Behavior in 1953—perhaps his most influential work. This new behaviorism, known at various times as “Skinnerian behaviorism,” “the experimental analysis of behavior,” “radical behaviorism,” and most recently, “behavior analysis,” departed from its classical roots in numerous ways. Some changes, profound in their influence on psychology, were subtle and almost unnoticed. Significant amongst these were the following.

1. Watson’s behaviorism had clearly been a form of philosophy of science, prescribing what kind of a discipline psychology should be and what it should study. It was wedded neither to a specific method of research, nor

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4 Throughout his career, in the academy and thereafter, Watson was ever-prepared to, and did, revise his views in the face of new evidence: a quality of good scholarship sadly not as usual in psychology as one might wish.
5 By this term I mean Watson’s behaviorism. Some writers, notably those persuaded by the later, Skinnerian, behaviorism have used the label “methodological behaviorism,” which by implication misrepresents and dismissively reduces the significance of Watson’s behaviorism. The book under review distinguishes classical behaviorism from methodological behaviorism by applying the latter to post-classical and pre-radical behaviorism. The term still has problems, and it would be best to eliminate that usage. (See Harzem & Miles, 1978, for a detailed discussion of varieties of behaviorism.)
6 For practical or moral reasons or both, much of important human behavior cannot be experimented upon, just as Astronomy, could not experiment upon its subject matter for most if its existence. This did not hinder the illustrious progress of Astronomy through the centuries.
to a specific theory of behavior, nor to any set of empirical findings. Skinner’s behaviorism, in its philosophical assertions, did not differ in any detail from Watson’s. The difference arose from intermingling with that philosophy (i) Skinner’s theory of behavior, best named “reinforcement theory,” most distinctively the theoretical assertion that mind-words are names of behavior, and, (ii) his innovations of method, such as the development of the Skinner box; the method of generating for purposes of research a new form of behavior, that is, repeated occurrences of a discrete, brief movements; the method of recording them; and his empirical discoveries such as the orderly patterns that emerge when reinforcement is scheduled in particular ways. Skinnerian behaviorism prescribed how behavior was to be investigated, and how data were to be analyzed. Those practices continue in the work of almost all radical behaviorists, and it is by them that radical behaviorism can be distinguished from other kinds of behaviorism.

2. With the growing use of the new techniques developed by Skinner, attention shifted from “learning,” which had been the subject matter of learning theories, to be replaced by the much broader “behavior” as the object of research. This had a momentous effect, one may colloquially say, “psychological effect,” so that the focus of experimental researchers, especially those working with animals, was no longer confined to the study of a particular set of phenomena, learning, important though it was. This, by itself, is sufficient to celebrate Skinner’s influence on behavioral research.

Perhaps most significantly, the very concept, “behavior,” changed with Skinner’s dictum that mind-words, which he labeled “mentalistic terms,” were to be eliminated from the language of behavior analysis, not merely ignored. The phenomena to which they may be thought to refer were behavior, and behavior of that sort occurred “inside the skin.” This was a valiant move to make radical behaviorism encompass all psychological phenomena, to address the question of what was to be done with phenomena seemingly excluded by classical behaviorism. It brought, however, new conceptual and empirical problems that are discussed in Staddon’s book, and briefly considered below.

Cognitivist psychology was in protest of the elimination of Skinner’s “mentalistic terms” from psychology and seeming neglect of the phenomena named by those terms: memory, emotion, knowing, decision making, etc. The cognitivist approach was taken by its adherents to free them from having to consider what is meant by such terms. The unfortunate consequence of this has been a climate of conceptual free-for-all, cognitivist researchers setting out experimentally to study such grand phenomena as consciousness without first

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7 I write “almost all” as a customary precaution but cannot think of an exception. There have, of course, been some minor modifications such as increasing the size of the Skinner box to cubicle dimensions to accommodate humans, and using computer buttons to replace the customary lever, etc. However, the principal techniques remain unaltered. Skinner was the first innovator of instrumentation and method in radical behaviorism and, to date also the last.
bothering to take into account the remarkable insights of 20th century philosophy on how to, and how not to, understand the words associated with them.

There are now indications that a fresh wind is beginning to blow through the world of psychology, across previously established barriers and from all quarters that until now have been theoretically disparate. An early harbinger of this is John Staddon’s book, *The New Behaviorism*. It bravely cuts across old-established borders and seeks to bring together areas of psychological thought and research that so far have remained uninformed of each other. The flavor of this is given by the book’s subtitle: *Mind, Mechanism, and Society*. This is an enormous task that calls for broad scholarship, beyond the confines of any one “field.” John Staddon, a distinguished scholar known for the intellectual breadth and the experimental originality of his work, is quite up to the task he has undertaken. As one may anticipate from its 181 pages of text, the book delivers considerably less than its subtitle promises but what it does deliver is must-reading for anyone interested in psychology, whether behaviorist or not.

The book has two parts although it is not explicitly divided as such. The first part comprehensively discusses behaviorism in many of its manifestations, dwelling on some issues and briefly touching on others. This is the fundamental core and bulk of the book, occupying the first 123 of its 181 pages. It will interest and stimulate any thoughtful behaviorist whether in agreement with Staddon’s views or not, and it will displease some behaviorists who are inexorably settled in their thinking. It is, however, this second, relatively small group of behaviorists who would especially benefit from reading it—not because they necessarily will be persuaded by Staddon’s position but because, persuaded or not, they will emerge from the book with an appreciation of the issues that must be addressed by any behaviorist position. The second part of the book briefly but interestingly ranges over a number of areas and topics that are relevant in any general discussion of behaviorism—particularly so if behaviorism is to escape from its self-prescribed confinement.

The first part opens with a brief chapter outlining the history of behaviorism up to radical behaviorism. This is the most disappointing chapter of the book due, perhaps, to its brevity relative to its subject matter. It repeats a number of inaccuracies, now firmly established in the literature, about the ideas and life of John B. Watson. Staddon cannot be faulted for all of these because the correct version is not, to date, available in the literature. There are other inaccuracies, however, that a cursory look at original material would have corrected. For example, Staddon writes “. . . Watson thought that most human behavior is not instinctive” (p. 8). In his first book, Watson devoted an entire chapter to considering instinct under the title “Hereditary modes of response: instinct” and further, concluded the book with the following paragraph: “Our personality is thus the result of *what we start with* and what we have lived through. It is the ‘reaction mass’ as a whole. The largest component of the mass, if we are normal, consists of

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8 Another chapter, discussing hereditary phenomena, is entitled “Hereditary modes of response: Emotions.”
clean-cut and definite habit systems, *instincts* that have yielded to social control and emotions which have been tempered and modified by the hard knocks received in the school of reality” (1919, p. 420, my italics). This statement, allowing for the language of its time, surely holds as well now as it did at the time it was written. On a different point, Staddon misinterprets Watson’s statement about psychology, that “its theoretical goal is prediction and control of behavior” and objects on the grounds that prediction and control is not at all theoretical. (See pp. 4-5 in Staddon.) However, here the word theoretical is in its ordinary language sense, to mean “in theory but not in reality, the goal is . . .” and not that the goal is developing theories to predict and control behavior. The antitheoretical theme to which Staddon refers later in the book is not in fact in Watson’s writings, except his objection to theory in the absence of any empirical evidence. That theme emerged in Skinner’s writings and overlaid much of radical behaviorist research up to the present.

The remainder of this part of the book, chapters 2 through 5, is devoted to critically examining radical behaviorism. This is where Staddon excels. He presents here an intellectual *tour de force*, beginning with an exposition of the basic behavior-analytic procedures at an elementary level, and proceeds with equal clarity to present quite complex arguments, supported by accounts of his own logically intricate and experimentally complex analyses.

Staddon, in his classic paper with Simmelhag (1971), was the first scholar emerging from the Skinnerian tradition to question Skinner’s account of “adaptive behavior.” That study was the first to add actual observation of the behavior of pigeons placed in a Skinner box to the by then standard procedure of mechanically recording responses—more accurately, operations of a key mounted on a wall. The observed data were classified into two types: “interim activities” and “terminal responses.” These two types of behavior were found to be distributed in an orderly fashion in the inter-reinforcement interval. This was interpreted as evidence that there is more to behavior that develops when reinforcers are delivered to an organism at both regular and irregular intervals and that the simple contingency account is insufficient to account for these observations.

Although an overwhelming volume of research into response-reinforcer contingencies has been devoted to schedules of reinforcement where the

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9 In radical behaviorist circles generally the “prediction and control” notion is attributed to Skinner, though it is Watson’s.

10 Skinner, in fact, had doubts about the antitheoretical effect of his famous, seemingly antitheoretical paper (Skinner, 1950). In a conversation he told me that he had deliberately titled the book he published in 1969, *Contingencies of reinforcement: a theoretical analysis*, so as to emphasize his approval of “certain types” of theories.

11 As Staddon notes later in the book Skinner was the first to observe the behavior of the subject when reinforcers are delivered noncontingently. However, Staddon and Simmelhag were the first to record standard contingent responding and systematically to observe other behavior.

12 Staddon writes they *found* two types but, of course, any set of such complex data can be classified in many different ways, depending on what is being investigated and what is to be explained. This may seem a pedantic point but it is worth noting because not infrequently theoretical arguments arise from differences in the classification.
contingency account is taken for granted, a crucial test of that account is to find what happens to behavior when reinforcers are delivered independently of behavior: Under such conditions will repetitive behavior develop in relation to each reinforcer delivery as if the two were contingently related? This is the crucial experiment. Contingency is not, of course, an event; it is a relation. It can come to influence behavior only through experiencing the events said to be contingently related, in a particular time-sequence. If contingency is a necessary condition for the emergence of stable patterns of responding then, when no contingency is programmed between the two events, no stable relation should emerge between a pattern of response and, closely following it, the reinforcer. As early as in 1948 Skinner did, in fact, conduct such an experiment where stereotyped behavior developed even though reinforcers were delivered regardless of any response. He labeled the resulting behavior “superstitious behavior,” implying that it occurred as if reinforcer delivery was contingent on its occurrence even though there was no such relationship programmed by the experimenter. Clearly, the label went beyond the empirical finding because the phrase “superstitious behavior,” imported from ordinary language, has several different meanings and brings with it a variety of notions. Moreover, when steady response-reinforcer relationship is observed, both where contingency is programmed and when it is not, the difference is only from the experimenter’s perspective, not the animal’s. Skinner’s evidence does show, in the first place, that contingency is not a necessary condition—though it may be a sufficient condition—for the establishment of consistent patterns of behavior in relation to reinforcer delivery, and it contradicts his fundamental, oft-stated belief that contingency is all-important in determining behavior. This was not, however, his interpretation. Ingeniously, Staddon notes, Skinner inverted the more obvious conclusion so as to retain intact the ubiquitous contingency account. The very term superstition implies action on a false basis, and thereby the contingency is preserved unfalsified. Staddon, too, is ingenious in his analysis of that simple experiment, and his argument is rationally sound and experimentally supported. He correctly notes that temporal contiguity of response and reinforcer is insufficient evidence for the contingency (or reinforcement) theory. To be so, two other conditions have to be met: (i) reinforcer should not occur in the absence of response (but note that this holds only if the reinforcer is not produced separately by several events only one of which is the response) and (ii) reinforcer frequency should increase if the response frequency increases (but note that there are exceptions to this suggested, for example, by the scarcity notion of some microeconomic theories). Skinner’s experiment did not satisfy those two conditions. (The provisos in parentheses do not apply in the case of that experiment.)

Staddon overlooks some other considerations that seem at first sight to refute but in fact support his position. One is that given sufficient number of trials, that is,

13 Whether animals have perspectives is not an issue here. The reader who is uncomfortable with such “mentalistic” [sic] terms would do equally well by replacing my usage with, for example, “the animal did not respond differentially in the two conditions; the difference was only for the experimenter and could affect only his/her behavior (e.g., in describing the findings)”
reinforcer deliveries, the “superstitious” response pattern drifts and disappears, to give way to other behavior. This cannot occur when there is a contingency, except when satiation, fatigue, and other such conditions intervene. Another is that how durable is the pattern of behavior depends on the species used. The pigeon is an ideal subject for the superstition experiment, for its superstitious pattern will persist long though not forever; the rat is less suitable as its superstitious pattern will change sooner than the pigeon’s; and humans tend to fall into two groups, where one group’s response pattern will last so long as the experimenter can last, and the other group’s pattern will quite quickly stop. How are these differences to be explained?

On the face of it, such evidence seems to support reinforcement theory: once the animal learns that the established response pattern is unnecessary for the occurrence of the reinforcer, that behavior ceases or changes. The difference between species might then be filed away under “species differences” in, perhaps, learning. Staddon’s account, however, fares better. Recall that evidence from the Staddon and Simmelhag (1971) study identified two different types of behavior, interim activities and the terminal response. In the case of response-independent reinforcer delivery it is likely that whatever behavior was occurring at the time reinforcement occurred functioned as terminal response for a time, temporarily and not with the strict temporal requirement of a programmed contingency. The variation in the duration of the interval between the (terminal) response and reinforcer would provide for some other behavior to coincide with reinforcer delivery, leading this new behavior to replace the previous one as the terminal response. If this is correct, then long-term observation in an extended replication of the superstition experiment should find gradual changes in established response patterns, one pattern succeeding the other. Note that no such changes can occur where a programmed contingency is in operation. This suggestion is strengthened by species differences observed in what happens to behavior previously established under a contingency, when that contingency is terminated but reinforcer delivery is continued as before. In 1977, Lowe and I compared the behavior of rats and pigeons under Fixed-Interval (FI) and Fixed-Time (FT) schedules, with parameters of 30, 60, and 120 sec. With each parameter a different group of animals was exposed to the FI schedule for 80 daily sessions, FT schedule for 50 sessions, and again the FI schedule for 40 further sessions, in that order. With each parameter rats’ responding declined almost to zero under the FT schedule. Pigeons’ pecking rate remained almost the same under FT 30 sec throughout the 50 sessions as under FI 30 sec; under FT 60 sec it declined to almost half of the rate under FI 60 sec; and almost, but not entirely, ceased under FT 120 sec (Lowe & Harzem, 1977). It is not clear what reinforcement theory would predict under these conditions—

14 Controlling, of course, for such things as satiation.
15 The persistence of the first group is controlled by factors outside the experimental conditions, such as the assumption that the behavior expected by the professor conducting the experiment—perhaps yet another kind of superstitious behavior? The human evidence given here should be taken with some caution because it is from previously unreported experiments, conducted in my laboratory several years ago.
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perhaps no change in behavior, since there was no change in any of the already established relations from the animals’ point of view.

The difference in the typical response rates of rats and pigeons probably accounts for the difference in behavioral sensitivity to absence of the contingency. Behavioral variation in the context of contingent events probably has biological value: It enables the animal to detect when a contingency is in effect and when it is not, rather than interminably persisting with the behavior once the contingency becomes effective, even after it ceases. The inter-response intervals of rats are longer than those of pigeons, and thus for them there is a longer, quite brief, period for variation of the interval between a response and the delivery of the reinforcer. In other words, there is greater opportunity for rats than for pigeons to detect the change in the response-reinforcer relation. This raises the question, when the contingency is not in effect why does responding stop? What stops is, of course, the specific response previously required by the contingency, and not all behavior. Again, Staddon’s account fares well: on that showing, some interim activity occurring at the time of reinforcer delivery will begin to function as terminal behavior. Thus, we find here an instance of what Staddon terms response competition, accounting for behavior change. This can be, though has not been, tested. If behavior were observed à la Staddon and Simmelhag during the FI and FT conditions, here, too, a greater fluidity would be predicted in the features and sequencing of both terminal response and interim behavior under the FT than under the FI schedule. As suggested above for the superstition experiment, the terminal response would be transient, becoming stable for a time and then changing.

The basic assertion of Skinner’s contingency theory of behavior, better known as the reinforcement theory, is that contingency explains all behavior. Staddon’s view is that there is more to behavior than contingency, and we must go beyond schedule-research if we are to develop a new behaviorism inclusive of previously neglected areas of research, and methods that are, no doubt, yet to be developed. One theory simply observes the effects of contingency on behavior; the other asks (i) how does a contingency come to affect behavior, and, related to it, (ii) is there more to behavior than the contingencies affecting it? In other words, Staddon’s position does not replace Skinner’s but adds to it. That, of course, is how science advances.

This part of the book then goes on, sometimes to show, and sometimes more tentatively to suggest how an integrated behaviorism may be achieved. Broad issues with reference to radical behaviorism, such as the use of the “Darwinian metaphor” in that literature, moral philosophy and what counts as value, personal responsibility, deterrence in law and the effectiveness of punishment are discussed with evident mastery of the subject, and often critically of radical behaviorism. In these ways the book, ranging widely over topics of undoubted relevance, provides a welcome antidote against the isolationist trend in radical behaviorism.

I noted above that mere programming of a response-reinforcer contingency cannot affect the behavior of the animal merely by virtue of the programming:
responses and reinforcers have to be experienced in their programmed relations. However, we know that such experiencing is not necessary for learning to occur. What, then, are the conditions under which an organism learns the relations between its behavior and other events? Answers to this question highlight an important, possibly the most important, difference in the present context, between animals and humans. Animals learn response-reinforcer relations by experiencing specific sorts of temporal relations between specific responses and reinforcers. In addition, some animals train their young, and there is evidence of learning by imitation. Humans learn these relations (i) in the same ways as animals and (ii) by verbal transmission. This latter effect, learning through language, is perhaps the most significant difference between animals and humans in the development of their adaptive behavior. How would this significant difference affect any attempt at extension from experimental findings with animals to accounting for human behavior?

The final part of the book is implicitly related to this question. It consists of three brief—too brief—chapters, ranging over an impressive breadth of topics far beyond the standard confines of behaviorism. If the previous part is necessary reading for any behaviorist, this is the enriching part, a sort of bonus. The first in this part, Chapter 6, explains in summary and critically discusses cognitivism and varieties of “contemporary behaviorism.” Rachlin’s attempt at a revision labeled “teleological behaviorism,” recent suggestions for new ways of viewing behavior from a Darwinian perspective, and “theoretical behaviorism,” which represents Staddon’s own view are included here. Theoretical behaviorism, it is said, serves “to understand the complete set of internal states of our animal, where each state is defined by a set of equivalent histories” (p. 142). This is, of course, a possibility, however remote, in animal research. It is, however, impossible for human research not only in practice but also theoretically. Theoretical behaviorism must, therefore, assign to itself a confinement within theoretical models on accounting for animal behavior. This behaviorism is fundamentally different from any of the behaviorisms we know to date. It is not a philosophy of science in the usual sense of that phrase, it is not a theory of behavior, and it does not prescribe any set of instrumentation and method. It is, in fact, a recommendation for a program of research, focused exclusively on animal behavior.

The last two chapters take further steps going beyond behaviorism. They discuss topics such as internal states, the concept of consciousness and its relation to the brain, and briefly touch upon epistemology by discussing “three categories of knowledge.”

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16 This is sometimes labeled “coming into contact with the contingencies” but of course, what is contacted is not the contingency but the response and other events—contingency is derived from the experienced relations. Moreover, there is more to such situations than the contingency.

17 In behavior-analytic theory this kind of behavior is labeled “rule-governed behavior.” This is too restrictive, however, because most often the behavior to which it refers is unspecifiable, with an indefinite range of patterns, in an indefinite range of situations. “Do not enter,” for example, seems comfortably to fit this label, but obviously not “live your life to its fullest.”
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The book concludes with a postscript, entitled “alchemy of the mind” which ends with a short paragraph that summarizes the program of the new theoretical behaviorism: “The solution offered by theoretical behaviorism is simply to lower our sights. Don’t reach for the stars; reach for a telescope. Study the dynamics of simple animal behavior. Maybe the stars will arrive in due course” (p. 181).

In 2000, I published a paper entitled “Towards a new behaviorism.” Little did I know at the time that Staddon’s forthcoming book would herald the arrival of a new behaviorism. The paper concluded as follows.

When anything functions badly the only lasting way to correct it depends on understanding how it functions in the first place. Behaviorism offers the prospect of understanding how human behavior functions. The fathers of behaviorist thinking, from Hobbes to Watson, to Skinner, all sustained the hope that behaviorism would help to resolve the basic problems of human life. Now, realizing this hope is a realistic possibility, pending the behavior of the behaviorists. (58-59)

I want to plead for a position different from Staddon’s, namely that we must build a science of human behavior. Theoretical behaviorism cannot, in principle, “bring the stars.” A science of animal behavior, no matter the power of its models, will not have considered the most fundamental characteristic of human behavior: learning by verbal transmission. Human behavior is so powerfully affected by language, both by self-language and the language of others, that those phenomena have to be researched on their own right. Consider an example: Throughout history human action has been affected, and powerfully controlled, by consequences that no one has ever experienced. The effect has been through language. In all the major, established religions accounts of what consequences follow what sorts of actions after termination of one’s biological life, that is, notions of hell, heaven, purgatory, forms of reincarnation, and nirvana have been powerful enough to give rise to culture patterns, and empires have risen and fallen under their influence. A science of human behavior must, I believe, seek scientifically and not anecdotally—as has been the case this far—to understand such phenomena.

Staddon has written a marvelous book, challenging, stimulating, and perhaps even irritating to anyone who is excessively settled in their beliefs, those who have selected to be card-carrying members of any one of the multitude of “ism”s in psychology. But those who have not entirely closed their minds will find much in this book to ponder over, much reason to reconsider their own assumptions, and best of all, much reason to go on to read any of the important works they may not have read about issues of Darwinian thought, consciousness, mind, and yes, even the question of what is to count as behavior.

Many years ago Miles and I wrote “. . . many conceptual questions can fully be answered if one points out both the reasons for saying ‘yes’ and the reasons for saying ‘no’” (Harzem & Miles, 1978, p. xi). This especially applies here. In reading this well-written, engaging book of impressive breadth what is important is not to see if one agrees with Staddon, but to understand the reasons for his statements. The book amply cites and describes a broad range of differing views
and sets and example to us all by the importance given to debate than doctrinaire commitment. So, let Staddon say the last word, with this great quotable quote from his book: “Debate dissolves error; silence crystallizes it” (p. 33).

References


