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Permalink

<https://escholarship.org/uc/item/3hw2h5hn>

Journal

Journal of the Experimental Analysis of Behavior, 95(1)

ISSN

0022-5002

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Publication Date

2011

DOI

10.1901/jeab.2011.95-119

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WHAT IS RADICAL BEHAVIORISM? A REVIEW OF JAY MOORE'S CONCEPTUAL FOUNDATIONS OF RADICAL BEHAVIORISM

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B. F. Skinner founded both radical behaviorism and behavior analysis. His founding innovations included: a versatile preparation for studying behavior; explicating the generic nature of stimulus and response; a pragmatic criterion for defining behavioral units; response rate as a datum; the concept of stimulus control; the concept of verbal behavior; and explicating the explanatory power of contingencies. Besides these achievements, however, Skinner also made some mistakes. Subsequent developments in radical behaviorist thought have attempted to remedy these mistakes. Moore's book presents a "party line" version of radical behaviorism. It focuses narrowly on a few of Skinner's concepts (mostly mentalism and verbal behavior) and contains no criticism of his mistakes. In fact, Moore adds a few mistakes of his own manufacture; for example, he insists that the mental realm does not exist—an unprovable and distracting assertion. The book's portrayal of behavior analysis would have been current around 1960; it mentions almost none of the developments since then. It also includes almost no developments in radical behaviorism since Skinner. Moore's book would give an unwary reader a highly distorted picture of contemporary behavior analysis and radical behaviorism.

Key words: radical behaviorism, behavior analysis, molar view, private events, mentalism, B. F. Skinner

Suppose you were writing a book about the teachings of B. F. Skinner. What would you include? I would begin with an appreciation of the huge contributions Skinner made to understanding behavior—that is, to behaviorism and behavior analysis. I would want to present both the strengths and the weaknesses of Skinner's views, and I would want to present at least some subsequent developments, to emphasize that the development of radical behaviorism and behavior analysis did not end with Skinner.

What is Radical Behaviorism?

The central proposition of behaviorism—the idea that all behaviorists agree about and that defines behaviorism—is the idea that a science of behavior is possible (Baum, 2005). Radical behaviorism takes a further step and asserts that a science of behavior can be a *natural science*. This is what makes radical behaviorism "radical." In a natural science of behavior, behavioral events are natural events, an idea with two counter-intuitive implications: a) behavioral events, like tides, oxidation, cell division, and evolution, are not *done*—involve no agency—but just *happen* (Baum, 1995); and b) behavioral events, like tides, oxidation, cell division, and evolution are to be explained by

other natural events (i.e., not by ghostly inner entities or essences). From these two implications comes the conclusion that behavioral events may be understood and analyzed in relation to past and present environment and evolutionary history *without residue*. That is, accounts of behavior with respect to environment and evolution leave nothing out: no internal states, intervening variables, or hypothetical constructs are required. Neurophysiology may be omitted too, not because it is hypothetical, but because it reveals only mechanism and not how present behavior came to be. No amount of understanding of mechanism can substitute for an understanding of history. This definition is compatible with Moore's (by his own admission "tortuous"; p. 431), except that he includes a gratuitous assertion of the nonexistence of mental things and events (discussed below).

Skinner's Strengths

In explicating his point of view, Skinner made several advances that one may fairly say established a conceptual base for behavior analysis. To say these made the science possible would be no understatement.

To begin, we have Skinner's methodological contributions. First of all, he invented a preparation for studying behavior in the laboratory (Skinner, 1961/1956). A simple,

easily repeated response, coupled with a feeder and the means for presenting stimuli, proved to be an excellent preparation for studying all sorts of behavioral phenomena and a springboard to gaining some insight into everyday behavior of humans usually called "voluntary." Second, Skinner elucidated the generic nature of stimulus and response (Skinner, 1961/1935). He argued that a stimulus or a response is a class, not a unique event. A stimulus or response should be defined by what it does, rather than how it looks. Thus, "red key light" is an adequate specification, even though the light may appear differently when viewed from different angles. "Lever press" is an adequate specification, even though the movement of the lever might be accomplished by left paw, right paw, or nose. This recognition freed behavior analysis from physiology and allowed definition of behavior by function. Without it, a science would have been practically impossible. Third, Skinner argued that such definitions need not be fixed in advance; one can choose one's definition according to what works, what produces "smooth curves" (Skinner, 1961/1956). In other words, adding to the idea of functional definition, he took a pragmatic approach to specifying behavior, allowing definition to be influenced by results. We take this for granted now, but the idea that one should tailor activities to produce orderly results was radical at the time. Fourth, I would say that we could hardly have a science without the idea of response rate as a datum. This contributed a variable that could vary over a wide range across time and situation. A wide range of phenomena became available for study as a result.

Skinner also made contributions that were foundational but conceptual. Most important is the concept of stimulus control. It presupposes the idea of response rate as a datum and so is intimately linked to it. The revolutionary part was the notion that stimuli control response rate—that they need not stand in a one-to-one relation with responses. The power of this more flexible view was to free researchers to study more extended relations, instead of having to focus on momentary events. Skinner's (1957) second important conceptual contribution was the idea of verbal behavior. The strength of this idea was to bring the conventional notions of language, reference, and meaning into the behavioral

fold. Skinner's idea that verbal behavior is operant behavior and not distinct from other operant behavior was radical; if taken seriously, it would lead to huge changes in the practice of philosophy, because philosophical analysis usually relies on words' having fixed meaning. A third conceptual contribution was Skinner's emphasis on selection by consequences as a causal mode. Skinner was not the first to point out that behavior might be shaped by consequences, and his theory of reinforcement has proven inadequate, but his emphasis on contingencies of reinforcement in explaining behavior provided a firm base for research and interpretation of behavior in everyday life.

The list of Skinner's accomplishments might be longer, and others might make slightly different lists, but my point here is that one can make such a list and applaud the accomplishments. Moore's book, however, limits its appreciation of Skinner's accomplishments only to mentalism and verbal behavior, which seemed a pity to me. Instead of a critical appraisal, we find in the book a "party line," a setting out of what Moore takes to be Skinner's ideas, as if they were the final word—one might almost say scripture.

The book's lack of any broad positive evaluation is matched by a lack of negative evaluation. Reading this book, one would get the erroneous impression that radical behaviorism consists only of Skinner's ideas, that his ideas have received no criticism from other radical behaviorists, and that nothing has happened since Skinner finished laying down the framework in the 1950s. In response to this view of the book, one might argue that Moore takes radical behaviorism to be whatever Skinner said, and therefore he was under no obligation to present any subsequent developments. Putting aside the problem that Skinner was often ambiguous and possibly even self-contradictory in his writings, Moore's book, in fact, doesn't simply represent radical behaviorism as if it had ended with Skinner—Moore himself has attempted to add to it. For example, he argues that private and public events differ only in the size of their audience, a potentially misleading idea that Skinner would probably have avoided and which I will discuss below. Thus, Moore's exclusion of other radical behaviorists does not extend to himself.

Skinner's Mistakes

Skinner was deft at meeting criticism, but his eagerness to confound his critics sometimes led him into mistakes. For example, anticipating the criticism that behaviorism must surely lead to a 1984-style repressive society, he not only pointed to the undesirability of punishment as a means of control, but went on to insist on the inefficacy of punishment. Even after research by Azrin and others in the 1950s (Azrin & Holz, 1966) and later by Rachlin (1966; 1967) showed that punishment is just as effective as reinforcement, Skinner (1971) still wrote that punishment was ineffective. He may have been unaware of these experiments, but that seems unlikely, because some of them occurred in the laboratory on the same floor as his office.

Another mistake he made, responding to the criticism that radical behaviorism fails to account for purpose, was to insist that the delivery of a reinforcer strengthens whatever behavior it happens to coincide with—that contingency consists of “order and proximity” alone (Skinner, 1961/1948; 1953). This view undermined behavior analysts’ abilities to analyze and explain behavior plausibly. It forced an atomistic view of behavior that treated complex or skilled performances, such as verbal utterances or catching a ball, as sequences of small units, instead of the integrated wholes that they are. Worst, for the present discussion, it offered no way to deal with gaps of time from one behavioral event to another or between behavior and consequences without postulating hypothetical constructs like “response strength” and unobservable stimuli. Why does a pigeon respond steadily on a variable-interval schedule, when food occurs so rarely? Why do people work for wages, when they get paid only once a week or less? Skinner’s insistence on the “stop-action” view of reinforcement rendered his accounts of behavior both in the laboratory and the everyday world tenuous at best.

Dealing with the gaps-of-time problem and responding to the claim that radical behaviorism has no account of thoughts and feelings, Skinner made perhaps his biggest mistake: conceding that accounts of public behavior are incomplete without private events and that private events may cause public behavior.

Private Events

Trying to avoid the accusation that behaviorism ignores thoughts and feelings, Skinner often wrote of events “within the skin.” Doubtless factors such as blood sugar level and body temperature affect behavior, but Skinner went far beyond factors like that, to write of sensory events like seeing a light and sub-vocal speech (thinking) as private events. Skinner insisted that these private events were just like public events, except that they were private, saying, for example, that his toothache is just as physical as his typewriter. Following Skinner’s lead, Moore asserts that public and private events differ only in the size of their audience, private events being confined to an audience of one. It is an enticing view, because we all experience the ability to talk to ourselves and imagine to ourselves without other people being privy to these events.

Problems arise, however, when private events are taken to affect public behavior. Skinner, for example, considered both private sensory events (e.g., pain) and sub-vocal speech to generate discriminative stimuli that affected public behavior. Skinner, Moore, and other philosophers consider public behavior under the control of private stimuli to be exemplified by verbal reports, such as “My tooth hurts.” Many philosophers consider introspection incorrigible: incapable of correction, necessarily correct. For example, someone who believed in incorrigibility might assert that although another person’s pain might be in doubt, he can have no doubt about his own pain. Although this might seem like common sense, it is false. I may have doubts about any introspection, including pain. (For example, “Do I have a toothache, or was that just a momentary twinge?”)

Behaviorists since Watson have regarded introspection as unreliable, because a person may report differently at different times, and because two people may report differently in similar circumstances. Moore’s idea that private events are confined to an audience of one seems like an assertion of incorrigibility, but presumably he would deny this. Denying it, he would have to accept that so-called “verbal reports” are unreliable—may be mistaken or even be lies. If I see a person writhing on the ground and declaring, “I am in agony,” I may say, “That person is in pain,” but the person may be faking, and I will only find out from

subsequent behavior, such as a sudden recovery, if I find out at all. The faker's actions constitute verbal behavior, but not necessarily controlled by private events. If I say, "I think I will go home" or "That looks like a duck," am I reporting on a thought or a percept? A better explanation will derive from present and past public circumstances with going home and ducks. Radical behaviorists take verbal behavior to consist of natural events, just like other behavior, and to be explicable in the same way. Utterances happen, and they are to be understood in relation to (natural) environmental events, past and present.

Common sense might seem to prove the efficacy of private events. For example, if I ask you to multiply two numbers together without paper and pen, you might sit quiet for a while, and then announce an answer. Doesn't that show that you privately visualized or verbalized the problem and produced the solution? What we know is that you came up with an answer, and your introspection as to how you came up with the answer cannot be relied upon. After all, what can we say about the idiot savant who multiplies two three-digit numbers together in a second? You were just slower. Besides, the main question for a behavioral analysis ought to be why you complied with my request in the first place.

If "verbal reports" are just behavior to be explained, what is the status of the private events supposedly "reported on"? They cannot be things or objects. When a person says, "I am in love," that is not a report on an inner love *thing*. When a person says, "I am in pain," that is not a report on an inner pain *thing*. If we posit utterances to report on inner, unobserved events, those events take on the properties of hypothetical constructs: unobservable events with undefined properties, the existence of which is inferred from the observable behavior. They become indistinguishable from the hidden mental causes that radical behaviorism rejects as superfluous and inimical to a scientific account of behavior. To be sure, sciences often posit unobservable events—at the atomic level, for example—but these must have defined properties and understood relations to observable events, neither of which can be said of reported-on private events. Your inner speech or inner imaging are never measured (then they would no longer be private!), and have no reliable

relation to public behavior. Asserting that private sensory and speech events are "just like" public behavior cannot solve this problem; no matter how much you insist a sow's ear is a silk purse except for the hair, it remains a sow's ear. The problem is the privacy.

One response to this criticism might be to argue that private events are useful in *interpretations* of everyday behavior. Thus, even though private events have no role in explaining behavior rigorously, they might enrich the account by adding plausible concomitants. If they enrich the account, they do so by appealing implicitly to the folk-psychology distinction between inner (private) world and outer (public) world, reverting to dualism—a high price to pay. After all, a strength of radical behaviorism is its denial of dualism, its assertion of "one world" only (Skinner, 1961/1945), and, indeed, if the science is to be a natural science, it must deny dualism, for the good reason that it renders cogent explanation impossible. Folk psychology and common sense notwithstanding, a natural science of behavior has to rely on observable, measurable, natural events in its explanations.

By making the radical-behaviorist view practically indistinguishable from folk psychology, inclusion of inferred private events has the very effect that Skinner hoped to avoid—making experimental study of human behavior difficult because, to lay people and critics, behavioral accounts will seem incomplete without discussion of private thoughts and feelings. Even worse, it makes the study of nonhuman behavior susceptible to the criticism that accounts of rats and pigeons should include their private thoughts and feelings. Indeed, Lubinsky and Thompson (1993), having trained pigeons to peck at one key when given Drug A and another key when given Drug B, claimed that the pigeons were discriminating on the basis of private states produced by the drugs. The states, however, were inferred from the performance and were redundant with pecking the one key or the other. Nothing is gained from positing an inner cause about which you know nothing—neither what it is, where it is inside the pigeon, nor what it has to do with the pigeon's nervous system. If a pigeon pecks one key when given Drug A and the same key given Drug C, the two drugs have a common effect—the pecking.

How do we acknowledge private behavior and stimuli without falling back into mentalism?

My own view is that we solve this problem by taking a molar view of behavior. When Skinner claimed that his toothache is just as physical as his typewriter, he could not have meant that his toothache is an object just like a typewriter. We cannot interact with a toothache the way we interact with a typewriter, but we can conclude that a person (including ourselves) has a toothache by observing their behavior, which is just as physical as a typewriter. How do we do this? A person in pain must do more than say "My tooth hurts." He must grimace, hold his face, be distracted, and ultimately be relieved by aspirin or the dentist. All of these public events, or at least some of them, must be present before we unhesitatingly agree that the person is in pain. A person who claims to be in pain but exhibits no other pain-behavior is, for all practical purposes, not in pain (see Rachlin, 1985, for a more thorough discussion). (This does not mean that another person will fail to act as if the person were in pain; the consequences of doubting could be severe.) When a person is solving a problem (needing to get to the airport but your car isn't running), he may spend time thinking covertly or overtly, but he engages in behavior (calling friends or a taxi) that ultimately solves the problem (getting to the airport). Thus, thinking and feeling are included in a radical behaviorist account, not as private events, but as patterns of public behavior. No need arises to imagine or speculate about private events—or to deny them.

Private events, real or not, only seem to be important to the account when one focuses on momentary control of momentary behavior. For example, Moore writes of a person who, on hearing a forecast of rain, carries an umbrella, "...the radical behaviorist may well agree that the individual who takes an umbrella may well emit a chain of covert responses that contributes to taking the umbrella." (P. 412.) Only when we think the account requires us to create a chain of momentary causal events do we think we need to fill temporal gaps with momentary, private events.

Moore fails to grasp the necessity of temporally extended accounts. He cites the example of a person sitting with eyes closed enjoying music and tries to answer the question put by

philosophers as to how one would distinguish that person from someone who was sitting and sleeping (p. 234). Moore's "answer" is that the person enjoying the music is enjoying it privately. This, however, is no answer at all, and would be met by critics of behaviorism with glee, because it would confirm their idea that accounts of behavior without mental events are incomplete. A better answer is that we judge such behavioral differences, not on the basis of some internal difference, but on the basis of a difference in public behavior in a wider context. We would judge by what the two persons did after the concert—one would comment on how lovely the music was and which parts were particularly nice, whereas the other would wake up and have nothing to say. Only when we focus on the moment are we tempted to invent inner states such as "private enjoyment." (See Rachlin, 2003, for further discussion.)

Behavior Analysis since 1957

The most puzzling and bothersome part of Moore's book is his portrayal of behavior analysis. Of nineteen chapters in the book, five—Chapters 5 to 9—are largely devoted to description of behavior analysis. Chapter 3, called "History of Behaviorism and Behavior Analysis: 1930–1980," stops its history of behavior analysis with Skinner, alluding to no developments since 1957. The trend continues throughout the chapters that concern the science. Chapter 5, "Categories of Behavior," lays out a taxonomy (tropism, taxis, kinesis, fixed-action pattern, respondent, and operant) that was current in my undergraduate years, which ended in 1961. Nowhere do we find mention of developments like auto-shaping, polydipsia, or adjunctive behavior; no research by Staddon is cited, and the name Breland is absent from the index. The book, however, is not meant to be a textbook of behavior analysis, and perhaps one might conclude that Moore is only trying to set out enough basic principles to support the behavioral accounts to be discussed. If so, this section ought to have been organized to accomplish that goal; as it stands, its relation to the rest is obscure, and it is woefully out of date.

Wanting to be fair, I thought that perhaps Moore was only trying to portray behavior analysis of 1960, without explicitly saying so. That proved untrue, however, because he

singles out for praise and lengthy explanation one relatively recent development, Relational Frame Theory. It would seem that Moore thinks this theory, which many behavior analysts consider a retreat into mentalism, is the only development since 1957 worthy of note. All the solid advances are omitted. Reading this book, one would get the erroneous idea that behavior analysis has stagnated since Skinner.

Most astonishing are two sections that discuss molecular and molar accounts of behavior (pp. 101–104 and 121–122). Moore explains molecular and molar views of avoidance without citing Herrnstein or Hineline (indeed those names are absent from the index). He discusses molar views of behavior by referring to Holt (1965/1914) and Lashley (1961/1951), but not Herrnstein or Rachlin (Rachlin is cited in the book once, in an unexplained citation to Rachlin and Green, 1972; the book is sprinkled with unexplained citations).

One will search in Moore's book in vain for treatments of advances since 1960, such as quantitative analysis, the matching relation, choice, discounting, behavioral economics, self-control, signal detection, foraging, concept formation, or delayed matching to sample, all of which are now parts of behavior analysis. One might infer that Moore regards these developments as unimportant.

The most likely explanation of Moore's distorted presentation of behavior analysis is his narrow focus on Skinner's concepts of verbal behavior and mentalism. He could have helped readers if he had made explicit the narrowness of his purview. Skinner's concerns were much broader, and he was careful to distinguish behaviorism, as philosophy of science, from behavior analysis, as science. Moore confuses matters by equating radical behaviorism with behavior analysis. For example, we see on page 51, "Skinner's form of behaviorism—behavior analysis—arose at about the same time as neobehaviorism but differed entirely." By describing behavior analysis in such primitive terms and equating it to radical behaviorism, he will give any unwary reader a highly distorted picture of behavior analysis.

Radical Behaviorism since Skinner

Reading this book, one would also get the erroneous idea that nothing has happened in

radical behaviorism since Skinner. Much has happened, however. Books have appeared: Rachlin's (1994) *Behavior and Mind*; my own *Understanding Behaviorism* (Baum, 2005); Lee's (1988) *Beyond Behaviorism*; Chiesa's (1994) *Radical Behaviorism* (this latter is cited only to approve of its treatment of hypothesis testing). At least four symposium volumes have been published containing a variety of discussions of radical behaviorism (Lattal & Chase, 2003; Modgil & Modgil, 1987; O'Donohue & Kitchener, 1999; Todd & Morris, 1995). That Moore almost entirely neglects these works suggests that the book is not really about radical behaviorism, but rather Skinner's ideas, regardless of their strengths and weaknesses. To these Moore adds a few weaknesses of his own, such as his insistence on the nonexistence of mental things.

Mentalism

Moore does an adequate job explaining the problems with mentalism. The explanations are repeated over and over. (If the book were properly organized and edited, its length would probably decrease by a third.) Moore makes a move, however, that Skinner carefully avoided. Moore asserts again and again that mental entities and events *do not exist*. For example, his definition of behaviorism includes, "The mental dimension is rejected because it does not exist, and therefore when one talks of mental phenomena, one is actually not talking about phenomena from another dimension at all." (p. 431). In this, Moore departs from Skinner, who was much more careful about the grounds for rejecting the "mental dimension." The problem with mental causes of behavior is *not* that they don't exist. Their existence or nonexistence cannot be demonstrated one way or the other, any more than the existence of a real world independent of our experience can be demonstrated, as George Berkeley (1685–1753) famously pointed out. The closest Skinner came to denying the existence of the mental was to talk about "explanatory fictions," but when he came down to talking about the "'real' or 'physical' world," he only committed to "the 'one' world" (Skinner, 1961/1945, p. 284). The reason is that the problem with mental causes is the same as the problem with dualism of any sort.

Calling mental terms “fictional” implies that they don’t exist, and jibes with the grounding of behaviorism in pragmatism, but it doesn’t point to the problem with mental terms. Unicorns and fairies are fictional because most of us never encounter them and, thus, have no need of them to explain the world around us. The task of behavior analysis with respect to fictions is to account for talk of unicorns and fairies and for talk of beliefs, desires, intentions, visions, and pains as if they were inner objects or things. Asserting their nonexistence, as Moore does, is unnecessary. We need to be able to offer a plausible alternative to accounts like, “Tom is on the Number 4 bus because he has a desire to go home and a belief that this bus will take him there.” The solution lies in viewing such utterances as occasioned by observed patterns of behavior.

Mentalistic explanations fail because the connection between a supposed mental realm and behavior remains forever mysterious. We will never know how a wish, as a mental cause, could cause public behavior of, say, shopping. The problem of circularity is usually easily exposed, because the wish is inferred from the shopping. Yet the inference itself is less the problem than the mysteriousness of the connection that would allow something like a wish to cause shopping. Indeed, if the inference were of something in the brain, we might entertain it, if only its connection to the behavior were clarified.

An alternative to forbidding mental terms is to try to make sense of their usage. Holt (1965/1914), Ryle (1949), and more recently Rachlin (1994) suggested that to wish is to behave—not privately, but publicly. That is, we understand what a wish is by examining the behavior that leads us observers (and the person himself) to say that the person has a wish; it may include activities in addition to shopping, such as talking about shopping and looking through advertisements. Indeed Rachlin (1994; 2003) has argued that behavior analysis is the science of mental life, because it alone can explicate all talk about mental entities and events. Sad to say, none of this discussion appears in Moore’s book.

Conclusion

Moore’s book lays out a picture of behavior analysis that is incomplete and anachronistic. His depiction of radical behaviorism too falls

short, because it contains only some of Skinner’s views, no broad evaluation of his successes, and no criticism of his mistakes. On top of this, the book adds some weaknesses of Moore’s own manufacture. Anyone who relied on this book to find out about behavior analysis and radical behaviorism would get a highly distorted picture.

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