

Has Radical Behaviorism Lost Its Right to Privacy?

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The role of privacy and private events is a most challenging problem in the analysis and understanding of behavior, and many questions remain unanswered or, at the least, controversial. The set of papers on this topic included in a recent issue of *Behavior and Philosophy* (2009) illustrates that even among those who are quite sympathetic to a behavioristic perspective, there is much diversity and disagreement over many fundamental issues.

Baum's (2011) position is that private events "constitute a trivial idea," are "not useful," and are "peripheral and inessential" (pp. 185, 186). Moreover, they simply have no place in a natural science of behavior, and if we would just look at behavior in a large enough context, we could ignore such events altogether.

Baum's paper is a great muddle, full of confusions, conflation, and contradictions, strange broad-brush accusations and misattributions, vague assertions and assumptions, unfounded conclusions, and a view of behavior that fails to address adequately a significant portion of human activity. Baum's position is essentially a mutation of methodological behaviorism.

First, some important history relevant to the label "radical behaviorism." Baum seems to define this philosophical position as: There can be a natural science of behavior, and that science is thoroughgoing, that is, it encompasses all essential aspects of behavior. Surely, Skinner would agree that any behaviorism worth defending would possess these qualities, but this is not what he was after

in proposing a "radical" behaviorism, as Baum certainly knows. The term finds its origins in Skinner's (1945/1972) paper, "The Operational Analysis of Psychological Terms." This extraordinary work put the "radical" in radical behaviorism—indeed, it is the first place, to my knowledge, that the expression was ever used—to distinguish it from "methodological behaviorism" (also, to my knowledge, the first time that label was used) when Skinner wrote the following:

The distinction between public and private is by no means the same as that between physical and mental. That is why methodological behaviorism (which adopts the first) is very different from radical behaviorism (which lops off the latter in the second). The result is while the radical behaviorist may in some cases consider private events (inferentially, perhaps, but none the less meaningfully), the methodological operationist has maneuvered himself into a position where he cannot. (Skinner, 1945/1972, p. 383)

Skinner's radical view removed any functional distinction between public and private events, except the feature of privacy itself, that is, the problem of accessibility. In addition, special contingencies are required to set up a self-descriptive repertoire. Behavior analysis could now include private events, at least in an interpretative if not empirical sense. And as Palmer (2009) reminds us, natural science is more than experimentation, prediction and control; science also helps us to make sense of the world. As verbal humans, a significant part of that world consists of private events, and a "thoroughgoing" science of behavior should encompass what is, in fact, a major portion of our lives; they should be considered "meaningfully," in Skinner's word. But, contra

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Skinner, Baum asserts, “the role of private events in radical behaviorism is peripheral and inessential” (p. 186). Perhaps Baum should invent another term for his “radical” behaviorism, a behaviorism that has lost its right to privacy. (Unless otherwise noted, in this commentary I use the term *radical behaviorism* to refer to Skinner’s, 1945/1972, formulation.)

Skinner, I think, did have a proper term—methodological behaviorism. To quote:

It is agreed that the data of psychology must be behavioral if psychology is to be a member of the United Sciences, but the position taken is merely that of “methodological” behaviorism. According to this doctrine the world is divided into public and private events; and psychology, in order to meet the requirements of a science, must confine itself to the former. This was never good behaviorism. (Skinner, 1945/1972, p. 382)

In a bizarre comment, Baum asserts that the radical behaviorist position on private events is “brought to the center in a misguided effort to render behaviorism acceptable to laypeople by suggesting they offer an account of mental life” (p. 186). Perhaps he got this strange idea from Skinner’s popular book *About Behaviorism* (1974), but this is an absurd accusation. Was Skinner’s purpose in “The Operational Analysis of Psychological Terms” to mollify lay critics of behaviorism?!

Baum would have us believe that private events are not meaningful or, at best, inessential for a “thoroughgoing” natural science of behavior. If these assessments were true, then our behaviors (and accounts of them) would not be significantly affected if what we typically call private events did not exist. But try to imagine that. There would be no descriptions or even mentions of bodily sensations such as those of pain, taste, smell, tactile (e.g., the feel of silk), kinesthetic, proprioceptive, or even visual and auditory experiences. Contemplating a Jackson Pollack, or quietly listening to a recording of Beetho-

ven’s violin concerto, or puzzling over the meaning of an Emily Dickinson poem—all of these and much, much more would be severely limited, if not impossible. Much of literature, especially fiction and poetry, would be superficial, if not pointless; think of Joyce’s *Ulysses*, for example, a novel largely comprised of “interior monologues,” and considered thereby “realistic” by some critics. A child quietly sitting and listening to her mother read from Grimm’s fairy tales would be unthinkable. (No, this child is not deaf!) Much of what occurs in a chess or bridge tournament would also be unthinkable, if such games were indeed ever to be invented. A major portion of what we call “problem solving” involves what Skinner called “precurrent” activity (e.g., thinking, imagining, and the like). But there could be no thinking, imagining, reminiscing, musing, dreaming (day or night), silent reading and rehearsing, and numerous other activities we associate with being a verbally competent human. Words like *wistful*, *vicarious*, *nostalgic*, *bemused*, and countless others that can describe both momentary and extended private reactions to events, real or imagined, would likely be missing from dictionaries or, at least, have their definitions radically altered. We would essentially be, at best, reduced to strange verbal creatures who only talked (or read) out loud; the rest would truly be silence. We would no more ask a companion, “What are you thinking?” than we would our cat. Indeed, the word *thinking* would not exist.

What about the meaning of *privacy*? Rachlin (2003), for example, distinguishes two sorts of privacy, which he calls Privacy A and Privacy B. Privacy A is exemplified by my writing this paper alone in my office (others are not ordinarily privy to these events, but they *could* be). In Privacy B, according to Rachlin, no such access is possible. Depending on one’s theory of privacy, Privacies A

and B could be considered functionally equivalent in that the same behavioral principles equally apply to each (no new principles are needed), and this is the view of the radical (i. e., Skinnerian) behaviorist. But, in contrast to Rachlin's dichotomy, Baum presents a confused picture. He seems to want to address a putative distinction, but he accuses radical behaviorists of not wanting to make a functional distinction between "practical" and "in principle" privacy on the one hand (which is correct), and on the other by strangely equating them with folk psychologists who are said to insist on a distinction. To my knowledge, few, if any, radical behaviorists have insisted on any such distinction, including the notion, however true or false, that Rachlin's Privacy B is, in principle, inaccessible. Nor is it the case that radical behaviorists must fall back to a position that all private events are public in principle. For them, accessibility is not the point. When radical behaviorists speak of lack of distinctions they refer to the operation of equivalent behavioral principles, functional relations, and so on. That is, no new principles or mechanisms need to be evoked in characterizing, for example, covert (both Privacy A and Privacy B) from overt actions. If I'm asked to multiply 25 times 36 without aid of a calculator or pen and paper, I might perform this out loud or silently; whether the computations are done out loud or silently may require the invocation of different contingencies. (Baum's view ignores the contingencies and rules that set up private as opposed to public behaviors. But these support the assumption that such actions share controlling variables; it's the privacy that needs to be accounted for.) Anyway, silent computation doesn't appear to require some new or novel principles or phenomena to distinguish it from that executed out loud. Such is a reflection of what Palmer (2009) calls "uniformity." Certainly,

Skinner's approach to private events is an assumption, and no doubt, the simplest one, but what reasons do we have *not* to assume uniformity in the application of behavioral principles? This defines the "thoroughgoingness" said by Baum to characterize his "radical" behaviorism.

A major thrust of Baum's rejection of private events in a natural science of behavior depends on some coherent notion of "observability." There is much confusion here. Baum admits, as he must, the question is not one of existence of private events, but of their lack of observability—presumably direct observation available for consensual validation—truth by agreement (also a methodological behaviorist stance). Assuming direct observation has a clear meaning (and it doesn't), just how many observers does a consensus make? For Baum, at least one other observer, apparently. The possibility of a Robinson Crusoe scientist, as Skinner (1945/1972) was to argue, seems to be out of the question, although I suppose that would be an instance of "practical privacy." The direct observation criterion is a very severe one; indeed, much of natural science would be crippled by such a requirement. Evidence may abound where direct observation is impossible, in principle. No one has ever "seen" an electron, nor could one. Atomic and particle physics events, curved space time, black holes, the interior of a star, potential energy, temperature, entropy, moment of inertia, the Krebs cycle, the Cambrian explosion, trilobites, *T. rexes*, Neanderthals, the origin of life, the birth of the moon, and innumerable other phenomena incorporated into explanatory structures (i.e., largely what we deem as scientific verbal behavior) of various natural sciences are all observed, inferred, modeled, or theorized by various indirect means. None of these can we know anything about simply by direct observation. In many cases, we make inferences and generate

plausible hypotheses based on known sciences and their modes of observations and deductions to engender a consistent and empirically justified account, which are all expressions of Palmer's (2009) uniformity.

Behavior analysis is often said to be, as Baum claims, an historical science and is compared in this way with evolutionary biology. There is much to argue about here, but although, as behavior analysts, we can certainly generate histories in the laboratory and in applied settings, in most situations we have never witnessed the history of any organism; the history might be inferred from the current behavior, just as characteristics of a once-living dinosaur might be inferred from its bones (actually the mineralization of the bones) and other sources of information from paleogeology to muscular mechanics. Basically, much of what might be called the exercise of historical science is interpretation, which is in turn based on sets of inferences (including quantitative massaging to yield certain molar relations) wherein direct observation may have played a minimal role. Of course, one hopes to escape the attribution made by Mach to Descartes: "A minimum of experience always suffices him for a maximum of inference" (1893/1960, p. 363). But the overall point here is obvious: In the absence of direct observation, what we know about private events comes from inferences and consistencies in indirect observations, manipulated histories, and, of course, our own actions and experiences, which we appear to share with countless others (e.g., "Does this feel like silk to you?" "Are you thinking what I'm thinking?"). Agreement (or disagreement) is meaningful and possible because of shared histories, including the acquisition and exercise of verbal behavior within a verbal community embedded in the world. As Skinner (e.g., 1957) proposed, self-descriptive repertoires and a host of other private events are an out-

come of verbal histories. However defective or limited these processes may be in particular cases, they are all assumed to reflect the operation of the same basic principles. Indeed, although we may be wrong in certain cases, the same may be true of our conclusions from observing the overt behavior of others, as well as interpreting our own. Baum places primary emphasis on how things might go wrong, when, in fact, in common everyday interactions we don't seek a verification for all assertions, descriptions, reported sensations, thoughts, and so on (from others or ourselves), nor do we need to.

An illustration of Baum's pervasive skepticism is his treatment of a proposed "antiprivacy machine" which he uses to illustrate "the biggest problem," namely, that a person might deny the results of a probe (p. 190). If such a device were possible (not bloody likely), I assume it will have been tested in ways common to other probing devices and methods. How would one know if the machine *ever* worked? One such test out of many is that the device should record "Who am I?" if it were said out loud and continue to record this query even though it was no longer heard by other listeners.

Baum asserts that unobservable events cannot serve as causes. Then, for example, what can it mean to say that the flow of charges in a circuit generates a magnetic field around the conductor? I suppose Baum would say, no, it is merely the attachment of a battery to the circuit that causes the magnetic field; that's observable, but the rest is unobservable. (Remember, too, the magnetic field is measured by its effects on a conductor.) Of course, attachment of the battery is essential, but stopping there leaves much out of the account, including what it is about a battery that makes the measured magnetic field possible. This sort of analysis can be applied to Baum's bare-bones description of the Lubinski and Thompson (1993)

studies of drug discrimination. The literature on drug discrimination is now gigantic, no doubt the largest in behavioral pharmacology. Baum's simple Drug A versus Drug B description wouldn't do that literature much justice. As mentioned previously, most of what goes on at a chess tournament can be considered to be private events that lead to overt behavior. To simply say that the pieces were in some arrangement, then a piece was moved to a new location (with a possible capture), but nothing else of any value can be said about what occurred related to this move seems perverse. Of course, there is a deep history behind activities like chess playing, and consistent rules and extensive experience give credibility to what a player might say about why a given move was made. The same can be said of numerous other cases.

More compelling still are the now-extensive studies in which monkeys can be trained to operate devices in real time via recordings from many cortical cells in regions related to motor function (for a semipopular account of some of these studies, see Nicolelis, 2011). Initially, peripheral muscular activity related to the proper movements can be recorded, but these eventually disappear. The covert behavioral events that control the device operations are, of course, unknown; current studies in humans are promising but are less precise, because the recordings (EEGs) must be done externally. With advances in technology, we may never achieve an "antiprivacy machine," but private events may come to serve a functional role in modifying the environment.

For Baum (and Rachlin, e.g., 2003) the solution to the problems of privacy is to finesse them through "molar behaviorism." Molar behaviorism certainly was not invented to address issues of privacy; indeed, eliminating privacy is not even an essential aspect of such a movement; one could still espouse a molar

behaviorism and keep private events as addressed in radical behaviorism. Rather, molar behaviorism is based on a background of certain aggregated data measures of steady-state performances revealed, for examples, by the matching law and shock-frequency reduction theories, starting with animal experiments long ago. How such performances are actually established appeared to be of less interest, and still is.

As with animals, the concept of *molar* as applied to humans is also slippery and seems to apply from saying "ouch" or wincing following perhaps an unseen injury: to being "honest," that is, manifesting "honest" acts, just how many of what sort is needed is not clear, nor is it clear how such a pattern might be established in the first place. Presumably, if we stand back far enough and consider only extended patterns of behavior (I guess one "ouch" doesn't establish a "pain"¹), then all will be well; private events (and presumably a good bit of overt behavior too) will be, at best, epiphenomenal. Both *extended* and *pattern* beg for definitions as well as conditions for their establishment, observation, and measurement; moreover, Baum hedges more than a bit when he talks about "local" events and issues of scale (e.g., "every reinforcer counts"; Baum, 2010; Davison & Baum, 2000).

Of course, we can be said to exhibit extended patterns of behavior; this aspect of molarism is obvious. "Personality," along with various attributions, like honesty, are but descriptive terms that characterize relatively consistent patterns of behavior of an individual under various circumstances. But my objection is declaring that *only* vaguely specified molar

¹ I find Baum's treatment of pain bizarre, as would any physician or others who deal with pain in its many forms (e.g., where is the pain of grieving?), but space limitations do not allow a careful critique here.

patterns of behavior are to be considered in a science of behavior, a view even elevated by its advocate to the exalted status of a new paradigm (Baum, 2002). Unfortunately, space does not allow me to examine that astonishing claim here.

The emphasis on the molar, as the only scientifically respectable position, and the associated “*all* behavior begins in the environment” reflect a lack of distinction between proximal and distal causes or explanations, or, more generally, the layering and nexus of an explanatory scheme. I may have the sudden thought that I have forgotten some papers and return to my office to retrieve them. Does my recalling the papers play *no* causal role in my subsequent behavior? Of course, we should rightly ask the question, “What brought about such a thought?” There are myriad possibilities and embeddings, including previous thinking, maybe about the content of the papers as I walked along. This raises a question about why this might have occurred, and so on. We can carry on this process indefinitely, but to what end? The Big Bang, the ultimate environmental cause?! Moreover, the more one attempts to get back to distal causes, the more speculative and uninformative they become.

The molar view also appears not to address in any useful way what might very loosely be described as *actions* as opposed to *habits*. I may be said to manifest the habit of being punctual, but I cannot be said to have the habit of saying “ouch” on being pricked with a needle or weeping when told of the loss of a loved one. The word *habit* simply does not have those functions in the language. Much of our behavior is occasioned by local, if not singular, events. Of course, we manifest both our unique and our shared histories in relation to these events, but a strictly molar account seems anemic in the context of a good share of everyday behavior; it is somewhat like looking through the wrong end of a telescope. Moreover,

highly skilled performances often call for virtually continuous control by prevailing as well as changing conditions. Any notion of molar feedback here seems useless. Consider both the challenging acquisition and ultimate mastery of a Chopin etude by a concert pianist, including, commonly, perusal of the score in the absence of actual playing (part of which is called “virtual practice”). Molar behaviorism, including its exclusion of private events, seems to have little that is enlightening to say about such accomplishments.

I have essentially argued that Baum’s “radical” behaviorism is radical by lopping off a substantial part of human behavior (overt as well as covert) and couched in a muddled molarism. It solves no problems, but both raises and obscures many; it is a mixture of the obvious and the obscure. Compared with those of other natural sciences, it is an austere and arid philosophy. Most of all, it is a retreat, not an advance, in our attempts to understand behavior. It is a behaviorism not worth defending.

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