Behaviorism: Methodological, Radical, Assertive, Skeptical, Ethological, Modest, Humble, and Evolving

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Humility

Humble behavioral scientists treat scientific statements as provisional, utilize criticism and disconfirming evidence, rely more on nature than teachers, willingly submit their beliefs to test, and search for relevant evidence in areas beyond their expertise. Their humility does not require self-abasement or lowly demeanor, nor prohibit assertion, commitment, or directed labors.

However, the commentators suggest important objections to "humble" as a modifier and offer worthy alternatives: modest behaviorism (Hineline), assertive behaviorism (Green), ethological behaviorism (Timberlake & Delamater), skeptical behaviorism (Chase), sacred behaviorism, open behaviorism, functional behaviorism, praxism, methodological behaviorism, radical behaviorism—the list is rich.

Rhetoric

Green, Hineline, Nevin, and Timberlake and Delamater question whether humble language will convince others, with Hineline emphasizing the need for effective communication. Their arguments should be considered carefully. At the same time, we must remember that reinforcement derived from convincing others will be short-lived unless the convincing language is consistent with the non-verbal world. Rhetoric may lead some to join one's path, but power must follow promise or disaffection results. The humble behavioral position is that, over

the long run, straightforward descriptions of procedures, results, and applications will convince the most.

A second reason to use humble language is that overstatements and overgeneralizations by Thorndike, Watson, Pavlov, and Skinner, as well as by contemporary behaviorists, are partly to blame for the rejection of behavioral work by many non-behavioral psychologists. Laws of behavior were overstated, efficacy of behavioral control overadvertised, and relevance of reinforcement principles overgeneralized (see Timberlake & Delamater). Overstatement and overgeneralization by behavioral researchers have led to an equally unfortunate overrejection by other psychologists. Reinforcement principles may not explain all of learned behavior, but if those incompletely understood principles are ignored, much learned behavior will remain unexplained. Both the overgeneralizations and the overrejections hinder productive experimental analyses.

A third reason to employ humble language is that language is not a disinterested descriptor but an instrumental act, emitted under partial control of consequences. As the instrumental functions of a speaker's verbal behavior become increasingly apparent to the listener, language alone is less and less likely to effect outcomes desired by the speaker. Aristotle distinguished between research and argument in science. Today, because of the weakening control by argument, humble descriptions of procedures and results are increasingly important. This last point applies to many other areas of society as well.

Behaviors

Hineline writes, "I will be amazed . . . if the behavior/behaviors distinction

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turns out to be as pervasively helpful as Neuringer suggests." That distinction was offered partly in an attempt to shape the behavior of behaviorists. We acknowledge different classes of behavior with differing functional relationships and laws, and we might therefore entertain the possibility of different functional relationships in the overt and covert domains. Hineline's point is extended by another contemporary behaviorist: "The essential difference between modern behavioral theories and cognitive or physiological theories is that, according to behavioral theories, whatever actions an organism takes are actions of the whole organism. For instance, a rat's bar press is considered to be an action of the whole rat, not its paw or its nervous system or some functionally defined internal mechanism. Otherwise behavioral theory becomes indistinguishable from physiological or cognitive theory" (Rachlin, 1985, p. 46–47). If responses are defined by a microswitch, then experimental analyses will remain at the level of the whole organism. But to define behavior exclusively in terms of holistic output is to ignore complex intra-organism behavioral interactions. Is the following not a "behavioral" experiment: first measure rates of responding by the subject's left hand under an FI schedule of reinforcement. Then attempt to determine the effects on left-hand responding of a simultaneously operating VR schedule of reinforcement for right hand responses (see, e.g., Laursen, 1972; Skinner, 1986). This analysis can readily be extended to the effects of covert behaviors—thoughts, self-statements, and images—on an overt response and so forth. The holistic behavioral method has provided much information, but there are other ways to analyze behaviors experimentally. Here, as in other cases, either-or thinking interferes with a comprehensive experimental analysis. Hineline makes a similar point, that is, "We need not abandon our approach in . . . adopting technologies from other approaches." And both Czubaroff and Nevin also appear to be sympathetic.

Ideal versus Natural

Timberlake and Delamater ask behaviorists to examine instrumental behavior in the context of evolutionary adaptations. Findings from the environmentally-minimalized experimental chamber may not provide adequate predictions of behaviors in more natural environments. At the opposite end of the methodological spectrum, Skinner (1986) argued for a more homogeneous operant chamber than is commonly used, one less likely to engender species-specific behaviors. Both points of view can be helpful. Analyzing operants in idealized experimental spaces may help us to describe basic controlling relationships which can then be located in the "real" world. Observing operants in natural environments may help us to understand better the functions and limitations of these relationships as well as to identify new relationships for analysis in the idealized chamber. Ideal and natural methods of study need not compete; they can complement one another.

Covert

Chase writes that the study of covert phenomena is difficult and requires skepticism, but that an experimental analysis of covert phenomena is possible. I agree with both points. Here, as in so many other places, Skinner provided the way by indicating the possibility. Chase and his research colleagues have been doing the hard and necessary work.

Covert phenomena can be observed: I observe the covert movement of my tongue, covert clenching of my stomach muscles, covert imagining of a theme from Beethoven's Seventh Symphony, and covert visualizing of a basketball player gliding toward the basket. I observe such covert phenomena much as I observe red stop lights, symphony orchestras, numbers on a computer screen, or other overt events. Observations of covert events enable empirical research, but there are problems.

First, a scientist can use overt events

but not someone else's covert phenomena as discriminative stimuli (SD's). Thus, the scientist (a) cannot directly reinforce another's covert events, and (b) cannot emit data-recording responses directly contingent upon another's covert phenomena. Second, many covert phenomena are "emitted," that is, they are apparently analogous to voluntary operants, and therefore there is multiple and complexly interacting control over them. If you are told, "Say your name to yourself," you may or may not emit the requested covert response.

Two major strategies have been employed to study covert phenomena: Cloud-Chamber and Words-as-Windows. A third strategy, Two-Labs, will also be discussed.

The Cloud-Chamber strategy seeks traces of the covert phenomenon in the overt world. For example, Shepard and Metzler (1971) studied "mental rotation" by asking subjects to indicate whether two 3-dimensional objects were identical. Reaction times were directly related to differences in rotation between the two stimuli. MacLeod, Mathews, and Tata (1986) also used reaction times, but here as an index of the attention by anxious subjects to threatening words. In general, under the Cloud-Chamber strategy, the scientist studies covert activity by observing its effects on an overt response, much as the physicist studies atomic particles by observing trajectories in a cloud chamber. One problem confronted by Cloud-Chamber strategists is that an overt effect may result from many different covert phenomena, alone or in combination.

The Words-as-Windows strategy hypothesizes that verbal descriptions are veridical functions of covert events, and that the researcher can learn about the subject's covert world through the window of her words. One problem is that overt words are instrumental responses, as suggested above, and therefore controlled by a multiplicity of influences, for example, reinforcement contingencies, motivations, etc. A second problem concerns the difficulty of teaching verbal descriptors when covert phenomena are the

SD's. These problems, coupled with the failure of 19th century introspective psychology, have caused many behavior analysts to be wary of using verbal reports (Chase). As in the silent dog strategy described by Chase, Cloud-Chamber techniques are sometimes used to validate the Words-as-Windows method.

As analogy for the Two-Labs strategy, imagine two scientists on two different worlds. Communication between the two worlds is possible, but not travel. Scientist X performs an experiment on One World, observes a functional relationship, and reports it. Scientist Y, on the Other World, reads a journal report, performs the same experiment, and either confirms the finding or not. Scientists X and Y never observe each other's experiments—much as is the case when one lab is in La Grande, Oregon and the other in Dunedin, New Zealand—but depend upon one another's verbal descriptions of functional relationships. This two-lab example is a model for self-experimental analyses of covert phenomena. Self-experimenter W observes a covert phenomenon as dependent or independent variable, publishes the findings, and self-experimenter Z attempts to replicate. The goal of self-experimental covert research is descriptions of intersubjectively reliable functional relationships. The long history of psychophysics has led successfully to scaling of sensations—relating (what some believe to be) covert sensations to overt physical events. The same may be possible for covert instrumental activities.

Metaphysics

McDowell takes contemporary psychologists (other than behaviorists) to task for not being monists. McDowell's claim is wrong with respect to many contemporary psychologists: most cognitive, social, physiological, sensory, comparative, developmental and other experimental psychologists are monists.

Are thoughts and feelings composed of physical matter, energy, or mental stuff? I don't know. McDowell is correct in that I am not a monist, but neither am I a

dualist, materialist or other -ist. Issues of essence need not be considered in order to study behavior.

McDowell recalls the early years of biology and how unhelpful were the battles over the principles of life and the essences within the nervous system, the vital and animal spirits. Only when hard-working biologists openly admitted ignorance of such matters did biology advance. For example, Cullen stated in 1772 (as quoted in one of his student's class notes), "I am more anxious to explain *what* happens than to explain how it *does* happen," and he refused to take sides in the battles of his day between materialists and vitalists (Wightman, 1958, p.141). Cullen's view is shared by some contemporary behaviorists, for example, "[A] science of behavior . . . keeps as primary the questions of what an organism will do, and when; questions of *how* are treated as secondary, to be addressed through elaboration of *what* and *when* relationships of behavior-environment interaction" (Hineline & Wanchisen, 1989, p. 224). *What* questions can be asked without allegiance to materialism, empiricism, monism, or their opposites.

In a related argument, Czubaroff defines the behaviorist's empiricist position in terms of four assumptions, including "strict determinism" and the goal of "prediction and control of particular events." Nevin agrees: "According to the most central tenets of our creed, all behavior is determined. . . ." Czubaroff and Nevin correctly identify behaviorists as determinists. What evidence could modify such a belief? Human subjects can learn to emit numbers at a computer terminal which are statistically indistinguishable, according to ten major tests, from the output of a random number generator (Neuringer, 1986). This research does not "prove" that behavior is indeterminate (indeed, the subjects differ from the computer according to some higher-order statistics) but, together with animal studies (Neuringer, 1991; Page & Neuringer, 1985), has caused me to question the omnipresence of behavioral determinism. Can random behavior be reinforced? Is unpredictable behavior

controlled by schedule contingencies and discriminative stimuli? These are empirical questions, not tests of faith, and modification of the determinist creed may be liberating, not destructive.

Behavior may differ from the world of physics in that an instance of behavior can be lawfully determined at one time and lawfully indeterminate at others (Nevin correctly states my view). As is the case for other operants, the probability of engaging in unpredictable behavior may be functionally related to genetic and environmental processes, with highly predictable behaviors engendered by some environments and highly variable, possibly "random," behaviors by others. The capacity to behave both predictably and indeterminately, if it exists, no doubt has evolved, and the study of its evolutionary course would be of great interest.

Determinism is not the most humble position (see Nevin, however). The behaviorist-determinist hopes to predict and control all behavior, at least in theory. On the other hand, I hypothesize that many behaviors are unpredictable, even given complete knowledge of all controlling variables. Only after their emission can sense be made out of these unpredictable behaviors. Post-hoc explanations (as opposed to prediction and control) may be the only thing possible for *some* behaviors *some* of the time.

This "lawful-indeterminacy" position may also help to explain the "private experience of autonomy" (Nevin). To the extent that a behavior "occurs randomly," it is autonomous, that is, it cannot be predicted from knowledge of genes or environment. "Random occurrence" should be interpreted as: given a set of possible behaviors, the set being determined by genes, experience, and environment, the emitted instance is unpredictable, even assuming complete knowledge. Autonomous instances cannot be predicted by the emitter of the behavior or by external observers. On the other hand, as implied by Nevin's comments, the set may well be predicted. Thus, determinism applies to the set and indeterminism to the instance. Impor-

tant questions for future study concern the size of operant sets, how size is modified and controlled, and the distinction between instance and set.

I end, first, with a "Thank you" to the commentators. My response covers only a small part of their many excellent (I agree) and motivating (I disagree) ideas. And, last, a "Thank you" to B. F. Skinner, intellectual giant and dear friend to many, whose life's work provides a most important part of the nurturing substrate from which behaviorism evolves.

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