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THE UNCONVENTIONAL PHILOSOPHY OF SCIENCE OF BEHAVIOR ANALYSIS

A. CHARLES CATANIA

UNIVERSITY OF MARYLAND BALTIMORE COUNTY

In a nutshell, my problem with Staddon's essay is that it endorses an implicit philosophy of science that is not behavior analytic and that makes no reference to how analysis in terms of verbal behavior may bear upon scientific practice and the generation of technical vocabularies. The essay is couched in terms of explanations and theories and models and fundamental knowledge, but does not say how these terms bear on the behavior analyst's behavior when the behavior analyst explains or theorizes or models or knows.

But first, a rhetorical matter: I was puzzled by Staddon's account of "behavioristic correctness" in the "insistence on particular terms" (Staddon, p. 442). He uses the term *consequential* from an editorial of mine as an example, when that word is hardly part of the mainstream behavioral vocabulary, in that many other behavior-analytic writers would have used *reinforcing* in its place. My shift to the term *consequences* was a move toward a more colloquial usage, chosen in large part because it does not prejudge behavioral effect and because it is *not* technical (it is in most standard dictionaries). If Staddon thinks that saying that a response has consequences means something more than the response has some environmental effect, he should say what he thinks that something more is. And if he wishes to speak of "behavioristic correctness," he should document the phenomenon more carefully. I have prepared some behavior-analytic glossaries, but have routinely qualified them with statements such as the following: "A set of definitions must be treated as a preliminary guide to the basic classifications and concepts in the relevant literature rather than as an inflexible set of rules" (Catania, 1992, p. 362). Perhaps behavior analysts are not as homogeneous a group as Staddon seems to think.

Address correspondence to A. Charles Catania, Psychology Department, University of Maryland Baltimore County, 5401 Wilkens Avenue, Baltimore, Maryland 21228.

Let us get back to the main issue. A behavior-analytic philosophy of science must begin not with assumptions about truth and knowledge but rather with the behavior of the scientist. There are behavioral alternatives to the major categories and assumptions of traditional philosophies of science, including those upon which Staddon's essay seems to be based. In the space allotted here I can only sketch them in part; more detailed treatments are available elsewhere (especially in Skinner, 1950, and in the later chapters of Skinner, 1957).

For too many students, the starting place for contact with behavior analysis is through verbal behavior: They listen to lectures or read texts. But that verbal behavior was originally established through direct contact with nonverbal behavior, especially in the laboratory. Vocabularies treated mainly in terms of definitions cannot deal adequately with such origins. The following quotation may be relevant:

> Because the framing or mastering of a definition is primarily verbal, it cannot be counted upon to produce the discriminations upon which development and evolution of that verbal behavior was based. For example, the student who has learned to define *reinforcement* may be able to offer a correct definition, but it does not follow that the student will then be able to discriminate reliably between actual instances of reinforcement and nonreinforcement in laboratory or real-world settings. (Catania, 1992, p. 362)

Consider an investigator who has been watching an organism behave in the laboratory. In the beginning, descriptions of what the organism does will be in terms of vocabulary already available. Perhaps the organism will be said to show reflexes or to respond to stimuli; perhaps its responses will be said to be elicited or inhibited or strengthened or weakened.

Suppose then that this investigator begins to see that what makes these responses occur more or less often is not so much what precedes as what follows them. The behavioral relations begin to exert discriminative control over the investigator's behavior, at first perhaps only with respect to arrangements of experimental procedures. The subsequent transition to a vocabulary consistent with the newly relevant properties of behavior may take some time, perhaps years. Eventually the investigator begins to speak in terms of emitted responses and their consequences rather than eliciting stimuli, and begins to distinguish this kind of behavior from other sorts by calling it by some new name, such as *operant*. The terms cannot be defined until they have been invented, but they can be invented only if the appropriate behavioral relations have been discovered. (This apocryphal account is consistent with some events early in the history of behavior analysis: cf. Catania, 1988, pp. 279–280; Hineline, 1990, pp. 316–317.)

The investigator who comes to respond differentially to classes of behavior modified by their consequences may then begin to define relevant properties: Are functional properties more important than topographical ones? Are some types of classes easier to create than others? In other words, operant classes exist in the behavior of the observed organism, but once the observer begins to respond differentially to these classes, they become the controlling stimuli for discriminated operants in the observer's verbal behavior. Presumably the origins of theories and models and explanations are to be found in such features of the observer's behavior.

I am not claiming that we know enough about these processes. Part of the difficulty is that the discriminable dimensions discovered by the scientist are not easily specifiable (if they were, the process of scientific discovery would be unnecessary). Another part of the difficulty is that the processes that lead to the coinage of terms inevitably involve multiple causation of verbal behavior and its attendant complexities. For example, metaphorical extension is common (reinforcement as strengthening, an operant as something that works, shaping as a kind of sculpting), but we seldom know enough about a coiner's verbal history to offer more than a plausible guess about how the coining came about.

None of this yet involves explanations or theories or models. Let us now consider them briefly in turn, recalling as we do so that each also arises via metaphorical extension from colloquial vocabularies (see also Catania, 1978, and *explanation* as indexed in Catania, 1992, p. 440).

Explanation is derived etymologically from roots implying the laying out or display of something (*ex-*, out, and *plain*, flat), and is typically defined in terms of giving reasons or of clarifying. As an example of the former usage, an auto mechanic might offer one of several explanations for the failure of a car to start: dead battery, empty fuel tank, bad starter, and so on. We accept the explanation if, after action based upon it, we are able to start the car.

The latter usage, however, is probably closer in sense to scientific explanation. For example, to explain how a car worked we might proceed by showing its systems in operation (perhaps with visual displays rather than actual components): how the burning of fuel in the cylinder moves the piston, how that in turn moves the crankshaft, how crankshaft motion is transmitted through clutch and differential to the wheels, and so on. The most effective demonstrations clarify each part a system by relating it to familiar phenomena with which it readily generalizes, as in showing the similarity between fuel ignition and the everyday varieties of combustion. (In traditional philosophies of science, explanations are judged to be effective or valid when they relate what is to be explained to other familiar and well-established phenomena.)

If asked how fuel is introduced into the cylinder or how its ignition is timed, we could show how valves or fuel injectors work or how distributors cycle the activation of spark plugs. For some, a given level of explanation might be satisfactory; others might ask for more detail. For example, only a few might be concerned with how exhaust valve timing is coordinated with fuel injection and ignition; of those, fewer still might care about the detailed working of timing belts.

Explanation in the sense of the car example is not very different from explanation in biology. Whenever a biologist demonstrates how strands of DNA combine and recombine, or how they can act as recipes for proteins, or how they replicate themselves, the biologist has successfully explained some part of the genetic functions of the cell. The double helical structure of DNA is no longer theory; it can be shown to us, and it has become part of the definition of DNA that it can take this form. And precisely when this aspect of its structure stopped being theory, it became maximally effective in explanation.

Both the car example and the DNA example illustrate that explanations vary in depth. The point is that, at any level, explanation is showing how something works. In this sense, behavior analysts sometimes offer explanations of behavior. We sometimes show how shaping works by studying effects of consequences on subclasses of the responses being shaped, and we sometimes show how molar relations come about by showing how simpler processes combine to produce them (e.g., Catania, Sagvolden, & Keller, 1988). It is perfectly legitimate not to be satisfied with this level of explanation, and we can expect in the long run that some will pursue explanations at physiological levels (presumably it will make a difference whether the neural systems sought are selectionist rather than associative ones).

What then about theories and models? Whenever we achieve explanation in the above sense, we have achieved a lot. But surely we made guesses about the possibilities before arriving at an explanation. Guesses are an instance of weakly determined verbal behavior, but perhaps when we call them theories or hypotheses they sound as if they are more strongly determined.

It is also useful to explore the implications of our guesses. When we formalize them for such purposes, we may call them models. Models are often mathematical, but they need not be. For example, early human-scale physical models of strands of DNA played a crucial role in establishing the double helical structure of DNA.

At what point are correspondences close enough that a model is no longer theoretical? At what point did the double helical structure of DNA stop being theory? The problem is again one of verbal behavior. Here it is enough to make the point that the transition does occur. For example, synapses were once only theoretical entities, but the junctions between axons and bodies of neural cells are now studied directly, and synaptic transmission is explained in more depth whenever neuroscientists show in more detail how it works. A word that begins as a theoretical term can evolve into a name for a phenomenon.

We sometimes accept mathematical theories or models as explanatory, but it may be more appropriate to treat them as economical descriptions. For example, planetary motion is not explained by Kepler's laws; rather, those laws describe, to a reasonable approximation, the mathematical properties of the orbits (cf. Catania, 1973, p. 440). Newtonian mechanics are consistent with the properties of human-scale space, but other systems may be more general (e.g., Einstein's relativity theory) or may operate in other domains (e.g., quantum mechanics). For example, Einstein's theory of relativity describes some properties of space in the proximity of large masses. To argue such distinctions between description and theory is not to diminish the achievements of mathematicians and physicists; the correspondences between mathematical systems and physical events are remarkable. But the sense in which Einstein's mathematics explains the bending of light as it passes near the sun or the implications of saying that the light bends because space is curved are matters of verbal behavior as well as physics.

Just as these systems describe mathematically some properties of events that occur in space, we may be able to describe some properties of behavior. Such descriptions may arise from guesses about how behavior works, but if we call them theories and identify them with internal states, we may conclude that we have offered an explanation when we have offered only a different and a very indirect kind of description. The trouble is that such descriptions may lead researchers to overlook the operation of variables that should have been revealed in the ordinary course of an experimental analysis (e.g., quantitative analyses in terms of a matching-law model may summarize data in such a way as to obscure the details of contingencies or the different properties of rule-governed and contingency-shaped behavior; see Catania, 1981, for several examples).

This treatment of theory has parallels in behavior-analytic accounts of the role of thoughts and feelings as causal. If the analysis of the language of private events (Skinner, 1945) implies that feelings and thoughts are best treated as accompaniments of behavior or as behavior itself rather than as causes of behavior, so also a behavioral philosophy of science should treat theories not as causes of scientific behavior but as its products. From that perspective, the origins of theory become of special interest. For example, what leads to good guesses about how behavior works? We might assume that good theories in a given domain come most easily to those who spend the most time exploring that domain but who also have contact with a broad range of phenomena outside it.

The major objective of this commentary was to question some assumptions about the nature of scientific theory that seemed implicit in Staddon's essay. In so doing, it argued for an alternative view that deals more explicitly with the role of verbal behavior in the behavior of the scientist. In this view, it is inappropriate to assume that theories drive scientific behavior; they may instead be derivatives of it, with functions yet to be established. This is not an argument against theory; rather, it is an argument for questioning traditional assumptions about its role.

Probably we should take seriously the possibility that theories often hinder rather than enhance scientific discovery. Theories are, after all, verbal behavior. As such, they may generate the insensitivity to relevant contingencies that is a common feature of rule-governed behavior. Did phlogiston theory speed the progress of chemistry? Did wave and particle theories help in the development of quantum mechanics? Were the theories of Hull and Spence advances or digressions? Presumably theoretical space has many more valleys than peaks in it.

Mathematical models were one of the four distractors that Skinner (1959) identified among the alternative reinforcers that could lead psychologists to a flight from the laboratory. He did not include the flight to discussions of the philosophy of science, and that may be because a behavior-analytic philosophy of science will inevitably return to behavior itself as its subject matter. That subject matter, in its own right, is rich enough. If this is the wisdom of behavior analysis, it is hardly conventional.

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