On the Relation Between Behavior Analysis and Biology

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The answers to two questions are pivotal in clarifying the relation of behavior analysis to biology. First, is behavior analysis a biological science? Second, what are the implications of the answer to the first question for the relation of behavior analysis to biology?

The answer to the first question is quite straightforward, because it hinges exclusively on definitional and logical considerations. As conventionally defined, biology is “a science that deals with living beings and life processes” (*Merriam-Webster Dictionary*). Because our science is self-described as *behavior analysis* and because the term *behavior* refers to processes exemplified by living organisms, behavior analysis is necessarily a part of biology. Indeed, Skinner explicitly held this to be the case: “The experimental analysis of behavior is a rigorous, extensive, and rapidly advancing branch of biology” (1974, p. 255). Note that most of psychology is *not* a branch of biology, because its extrabehavioral processes and structures are not “of this world.” That is, these entities in normative psychology are instrumental fictions inferred from behavior and, as such, are *unobservable* in principle and not merely *unobserved* in practice for technical reasons (as with private events in behavior analysis; cf. Donahoe & Palmer, 1994, pp. 275–276). In short, behavior analysis is a branch of biology, and only a portion of psychology may be subsumed under biology. In any case, behavior analysis is not a subset of psychology.

If definitional and logical considerations point unequivocally to behavior analysis as a branch of biology and Skinner himself held this view, then why have many resisted this conclusion? I believe that the reluctance stems from misunderstandings concerning the implications that flow from the conclusion. These misunderstandings have given rise to suspicions that the independence of behavior analysis would somehow be compromised or that behavior analysis would be “reduced” to physiology. However, such implications are not forced by the conclusion that behavior analysis is a branch of biology.

Behavior analysis is an independent science, irrespective of its being a branch of biology. For example, I have no difficulty in accepting behavior analysis as a branch of biology and, simultaneously, all of the major points in the target article (save for a few minor quibbles and qualifications not considered here). Behavior analysis is no more dependent on physiology than physiology is on cellular biology, or cellular biology on genetics. All are independent branches of biology. In short, it is generally correct that behavior analysts can “safely ignore” and “need not consider physiological processes” when practicing their science. Moreover, as Skinner observed on several occasions, no functional relation uncovered at the behavioral level can ever be undermined by findings at the neural level (e.g., Skinner, 1974, pp. 236–237). On the contrary, neural processes must be consistent with the orderly functional relations identified at the behavioral level, otherwise analyses at the neural level are either incorrect or incomplete.

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Although behavior analysis is no more dependent on other branches of biology than those branches are on each other, behavior analysis is also no less *interdependent* on them than they are on each other. Skinner was also sensitive to these interdependencies: "I am not overlooking the advance that is made in the unification of knowledge when terms at one level of analysis are defined ('explained') at a lower level" (Skinner, 1938, p. 428). "The physiologist of the future will tell us all that can be known about what is happening inside the behaving organism. His account will be an important advance over a behavioral analysis, because the latter is necessarily 'historical'—that is to say, it is confined to functional relations showing temporal gaps. . . . It will make the picture of human action more nearly complete" (1974, pp. 236–237).

The present reaction to the target article concludes by identifying two areas of interdependence that exist between behavior analysis and other biological sciences. The first is experimental; the second is theoretical (i.e., interpretative).

1. In any science, phenomena are eventually encountered at the border between the level of analysis of that science and its neighboring sciences in which information from both sciences becomes crucial for understanding. The target article acknowledges this type of interdependence when pointing to observations designated by such terms as instinctive drift, species-specific behavior, developmental milestones, and the like. At such times, understanding suffers unless the experimental analysis of the contingencies of reinforcement (and the products of such contingencies) is supplemented (not supplanted) by knowledge of the products of other contingencies (e.g., the contingencies of natural selection) at both the behavioral and other levels. As an example, Sidman has raised the possibility that behavior taken as evidence of the formation of equivalence classes might be the result of the ac-

quisition of stimulus–stimulus relations (Sidman & Tailby, 1982). However, by definition, stimulus–stimulus relations cannot be *directly* observed at the behavioral level and, therefore, cannot be subjected to experimental analysis at that level. (The behavioral consequences of the formation of stimulus–stimulus relations can be observed, of course, but these observations are necessarily affected by variables in addition to those affecting the postulated stimulus–stimulus relations.) The direct effects of stimulus–stimulus relations can be observed only at the neural level (cf. Donahoe & Palmer, 1994, pp. 145–146, 179–180, 200) where, in fact, Sidman's conjecture has been corroborated. When monkeys acquire an arbitrary matching-to-sample task, connections are selected between cells in sensory association cortex such that these cells are activated only by those combinations of the sample and comparison stimuli in whose presence responding was reinforced (Sakai & Miyashita, 1991; Tanaka, in press).

2. The general acceptance of selection by reinforcement as the primary insight into the origins of complex human behavior may depend upon the integration of behavioral observations with those from other biological sciences. The acceptance of natural selection as the primary functional insight into the origins of structural diversity and complexity was dependent upon its integration with two emerging sister sciences—genetics, which provided the mechanisms for retaining the effects of natural selection across generations of different organisms, and population genetics, which provided quantitative techniques that were capable of tracing the cumulative effects of natural selection. If history functionally repeats itself, then acceptance of selection by reinforcement will depend on the identification of its retentive mechanisms (the mechanisms of synaptic plasticity) and the development of more precise means than verbal interpretation for tracing its cumulative effects over the time span of an individ-

ual organism (i.e., neural networks; cf. Donahoe, Burgos, & Palmer, 1993). If synaptic plasticity and neural networks are to serve these purposes, both must be constrained by experimental analyses carried out by other biological sciences. It is not sufficient that they be mere inferences from observations at the behavioral level, as is the case with parallel distributed processing models in normative cognitive psychology (Donahoe & Dorsel, in press; Donahoe & Palmer, 1989). Note, however, that—although a strong case can be made that the acceptance of selection by reinforcement as the key insight into complex behavior is dependent on supplementation by knowledge of synaptic plasticity and neural networks—the functional principle stands by itself as the product of independent experimental analyses at the behavioral level (Donahoe, Crowley, Millard, & Stickney, 1982).

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