Can a Statement in Cognitive Terms be a Behavior-Analytic Interpretation?

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Interpretations in cognitive terms have appeared to a modest degree in the *Journal of the Experimental Analysis of Behavior* (JEAB), which is the major forum for basic behavior-analytic research and its interpretation. Acceptances of such departure from radical-behaviorist traditions within the core radical behaviorist journal have not been lapses of editorial vigilance, but rather part of a policy of nurturing variety within the work of behavior-analytic researchers (Nevin, 1980; Hineline, 1984). This rationale was well stated in an editorial by J. A. Nevin (1980):

I feel strongly that publications resulting from a variety of classes of activity should continue to appear in this Journal The principal criterion for publication is simply whether the work contributes to an understanding of the behavior of individual organisms or enhances the likelihood of effective analyses in the future, in the judgment of our reviewers and editors.

Recently a number of researchers have interpreted their work in relation to theoretical terms derived from cognitive psychology. Because of its mentalistic flavor, cognitive language may seem quite out of place in a behavioral journal. More importantly, the connotations of cognitive terminology may lead an author into pseudo-explanations of data, when in fact the terms merely rename the results. The real question, however, is whether a cognitive orientation leads to novel behavioral analyses or permits integration of findings that would otherwise remain unrelated. So long as the methods and findings of empirical work are described objectively and unambiguously, investigators should be free to use whatever theoretical language is most effective in making the significance of the work clear. If cognitively oriented language does not advance the experimental analysis of behavior, its use will extinguish The effectiveness of our communal enterprise can only be enhanced by encouraging variability in our aggregate activities, so that novel but successful variants can be selected and strengthened. (p. 134)

A couple of minor points in this passage may bear elaborating for readers who are unfamiliar with the history of the behavior-analytic approach. First, the issue regarding data on individual subjects' behavior is relevant to the identity of JEAB, for it was partly the problem of publishing experiments based on within-subject designs that led to that journal's beginnings. In fact, provided that individual organisms' behavior is adequately portrayed, JEAB does accept group designs in cases where they are especially appropriate to a question at hand. It should also be noted that a focus upon the individual organism does not distinguish radical behaviorism from all other psychologies, for Freudians can also claim a focus upon the individual, and experimental work on sensory processes is traditionally published with emphasis on data from individual subjects. Second, when Skinner was initially developing and advocating behavior analysis as a coherent approach, the dominant paradigm in academic psychology was Hull's formalized hypothetico-deductive theory. Skinner challenged this, under the title: "Are theories of learning necessary?" with his answer being: "No." (Skinner, 1950). What has not been widely understood is that he was rejecting only a particular type of theory, albeit a pervasive one. The behavior-analytic approach involves theory, and Skinner has acknowledged this (Skinner, 1969), but he has not explicitly presented his assumptions and premises *as such*.

Two other points from Nevin's statement are more central to the present concerns: First, "If cognitively oriented language does not advance the experimental analysis of behavior, its use will extinguish." I find this unlikely, for cognitive terminology and its accompanying patterns of prose conform more closely to the patterns of ordinary prose than do

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those of behavior analysis. Thus from childhood on, we learn the explaining and interpreting of behavior in mentalistic terms, and mentalistic accounts will always "sound explanatory." To state the situation more technically, fluent repertoires of cognitive language are consistently maintained in one set of environments (the culture that surrounds us); those repertoires will not be easily extinguished in another, especially if that other set of environments is not clearly delineated. This identifies a cost of adherence to purely behavior-analytic language: inevitably, in interactions with students or colleagues behavior analysts will punish or selectively extinguish behavior that is reinforced in most settings. To be sure, this can change the ways people write, talk and think within the situations where behavior analysts participate. But it also can result in those students and colleagues "leaving the scene"—not publishing in behavior-analytic journals, not taking behavior-analytic courses, or not reading the behavior analysts' books and articles. Even putting aside these secondary but important aspects of behavior change, there is risk of our foreign mode of expression being rejected as merely arcane. With behavior-analytic prose having become a distinct dialect, as I believe it has (Hineline, 1980), we find ourselves asserting in its defense: "You cannot validly criticize our interpretation if you cannot speak the language."

Need the behavior-analytic enterprise incur these costs? The answer to this is also the answer to Nevin's question of whether a cognitive orientation can lead to "novel *behavioral* analyses" (italics added). There is no argument over whether the behavior-analytic community should encourage novelty, for variation is essential to progressive change. There is also little question that interpretations stated in cognitive language are novel as accoutrements of behavior-analytic theory. What *is* under scrutiny is whether these, even after the editing of a radical behaviorist journal, are in fact behavior-analytic interpretations. *Can* an interpretation in cognitive terms be a be-

havior-analytic interpretation? My answer is, tentatively, no. The assertion remains tentative to hold open the possibility that a more advanced understanding of both behavior-analytic and cognitive theory could reconcile their differences. At the same time I find the possibility to be remote, that an adequate reconciliation could be achieved. The apparent intractability of the difference is well captured by Wessells' (1981, 1982) characterization of the two positions, each from the viewpoint of the other: "For Skinner, no theory concerning inner events, even a noncircular theory, can explain behavior" (Wessells, 1981, p. 158); "... for cognitivists, functional relations between environment and behavior are not explanatory. No amount of order among observables will satisfy the desire to discover internal processes through which the environment influences behavior" (Wessells, 1982, p.75). Wessells goes on to assert that "cognitive theories are not facades of knowledge constructed in the face of ignorance; they are useful, scientific analyses of inner processes." Thus, just as study of relationships between physiology and behavior is a distinct, complementary enterprise, so also, psychology couched in terms of processes that are said to lie behind or under, rather than within, behavior is best viewed as distinct, perhaps a complementary enterprise. Detailed discussions of such complementarity have been presented by Catania (1973), by Segal (1977) and by Wessells (1981). Surely, a single scientist can do good work within more than one tradition. However, when interpreting within *one* rubric, one is *not* interpreting within the other.

So, what aspects of these differences make adoption of language from one rubric into the other so troubling? Some aspects relate to how certain classes of phenomena are handled, as detailed by Branch and Malagodi (1980) with respect to private events, and by Marr (1983) and Morris, Higgins, and Bickel (1982), with respect to causation over time. Marr (1983), Morris et al. (1982), and others have also delineated some differences as

understood within philosophical discourse. In each of these cases, the mixing of explanatory practices would risk incoherence, or at least a loss of conceptual rigor. But the aspects that I wish to emphasize here relate to features of explanatory or interpretive prose. In both vernacular and cognitive-technical language, behavior is accounted for in terms of mediation and agency of the behaving organism or its structural characteristics. Environment is treated mainly as context. In contrast, behavior-analytic language treats organism as context, focusing upon behavior-environment interaction. Agency (an attribution that arises ubiquitously in noun-verb prose patterns—Hineline, 1980) is typically parsed as on the environmental side of that interaction.

As noted in the quotation above from Nevin's editorial, a standard radical-behaviorist critique of mentalistic or cognitive language has been to characterize it as introducing "explanatory fictions" or "pseudo-explanations," which are seen as distinct from behavior and environmental events. After all, if "environment-behavior" is a proper agent-action (noun-verb, independent variable-dependent variable) statement, then a second agent is superfluous. The arguments against including cognitive terms have typically appealed to more than mere language pattern, however. One is an argument of logic, as indicated by Nevin's suggestion of their susceptibility to trivializing circularity. The other is the pragmatic argument that interpretations of behavior in cognitive language are often mere re-descriptions with loss of precision, and that interpretations of behavior in terms of environmental events are more amenable to direct assessment and to application in changing what organisms do.

Those who favor inclusion of cognitive terminology within behavior analysis have addressed these arguments, and in so doing seem to assume that the key issue regarding these terms is one of objectivity in principle—and they have shown that *in principle*, the cognitive terms *can* be impeccably objective.

Shimp (1976) points out that behavior analysis necessarily deals in abstractions (even the fundamental concept of "the operant" is abstract), and claims that if one defines one's terms operationally, the cognitive terms are no different in principle from the ones commonly viewed as purely behavioral. Similarly, Wasserman (1983) appeals to Tolman's rationale for providing operational respectability and rigor for terms that have been viewed as mentalistic fiction. He asserts that these terms, carefully defined and used in ways consistent with the operational definitions, need not imply subjective experience or a dualistic conception of reality.

Dualism, subjectivism, and objectivity are not the main issues, as I have argued elsewhere (Hineline, 1983). But for now, accepting the issue as defined by Wasserman and Shimp, the problem is that what is true in logical principle appears not to be true in actual practice. Their discussions of operationism treat scientific prose as if it were purely rule-governed behavior. But even if a scientist explicitly defines cognitive terms with precision, the subsequent use of those terms is likely to be shaped inappropriately by the contingencies and locutions of vernacular speech. Thus a discussion solely in terms of the rules of logic or of scientific rule-governed behavior is inadequate to the case. Evidence of such vernacular influence is discernable in an exchange between Goldiamond (1976) and some advocates of "cognitive behaviorism" concerning the status of "self-reinforcement." Mahoney (1976) argues that "self-reinforcement" need not imply an inner entity or agent: "... (such terms) ... can be ... (and have been) used in operational ways to describe behavioral patterns that are culturally deemed 'selfy.' The use of the term 'self' to describe these patterns *need not* connote acceptance of free will or 'self-determination,' and it *need not be problematical* so long as clear operational definitions are provided" (p. 516; italics added). Then Thoreson and Wilber (1976) in a contribution to the same exchange that is explicitly endorsed by Mahoney, show that in practice, the inclu-

sion of mentalistic terms appears to change the nature of a presumably behavioral interpretation: "self-evaluative standards (as bases for self-administered reinforcement and punishment) are primarily acquired and developed through experience with the social environment. Such experience, however, *is* mediated by cognitive processes. External stimuli get transformed via selective attention, interpretation, and categorization. Thus, stimuli acquire much of their reinforcing properties through cognitive processes" (p. 518; italics added). My point here is not that Thoreson and Wilber's statement is totally indefensible; rather, the point is that in its appeal to mediational interpretation Thoreson and Wilber's statement is not a behavior-analytic one. Thus, while it may be possible to pursue a behavior analysis that includes cognitive terms, typically that is not what results. The behavior analysis gets transmogrified in fundamental ways; change of terms leads to a shift of agency in agent-action statements, and thus to a fundamentally different kind of interpretation.

There is still another basis for challenging the "pro-cognitive language" arguments on their own terms. Even if a writer succeeds in providing precise, non-dualistic operational definitions of mentalistic terms, and also succeeds in adhering to them, the reader or listener may be unable to adhere rigorously to them. Witness Person A talking to Person B: "I hereby define 'nerd' as a sensitive, intelligent, admirable person. You're a nerd." Person B is unlikely to react in accordance with the explicitly-supplied definition. Similarly, Person A might say: "The question of the nature of a mental representation is really a question about which aspects of the pattern of incoming data are perceived, learned, and retained for potential future use" (Pylyshyn, 1972). However, as Person A continues to talk about characteristics of representations, the audience will not be hearing an account of environmental contributions to subsequent performance.

Finally, it should be noted that the operationism invoked by Shimp, by Wasserman and by Mahoney, all in the name

of "scientific objectivity," appears not to be behavior-analytic operationism. Rather, it resembles the operationism of "methodological behaviorism" which Skinner rejected long ago, in distinguishing it from that of radical behaviorism (Skinner, 1945).

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