*ON NAMING AND SYMBOLIC BEHAVIOR:
IN THE BEGINNING WAS THE NAME?*

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This is an exciting time in behavior analysis. Several converging lines of research and interpretation have raised the possibility of a genuinely behavior-analytic account of language and symbolic functioning. That specific research problems in behavior analysis have some general relation to language has long been recognized. But how does it all fit together? Is one relationship more fundamental than others, or do they all perhaps derive from something more basic? Will new principles be needed, or will older ones suffice?

In this broad and scholarly paper, Horne and Lowe outline a theoretical framework to address such questions. The paper is impressive in its breadth, drawing for support on a wide range of research findings both inside and outside behavior analysis. This kind of interdisciplinary approach is frequently called for but seldom accomplished so well. But the paper is more than merely a review of the research literature; it is a theoretical reconceptualization that tries to make sense of the literature in light of current behavior-analytic thinking. The paper is thoughtful, timely, and provocative, and is sure to inspire a good deal of future work.

Horne and Lowe have provided a plausible account of the development and maintenance of naming, and how it might be related to other straightforward verbal processes. Things *could* happen this way. But *do* they? These are questions about the kinds of explanatory statements that can be made about certain kinds of evidence. In identifying some of the conditions that may give rise to the development and maintenance of naming, Horne and Lowe are engaged in a scientific interpretation, as opposed to a scientific analysis. Unlike an analysis, which aims to specify

the actual conditions necessary to produce some behavioral outcome, the goal of an interpretation is to specify conditions that are sufficient to produce the outcome; to specify a path—but not necessarily the only path—the outcome could have followed (Donahoe, 1993). The task here is not unlike that faced by an evolutionary biologist attempting to reconstruct the history of a species. Like interpretation in that realm, a behavioral interpretation is constrained by what is known under better controlled conditions. Horne and Lowe's account of naming fares well on this measure, in that it is based on extensions of well-established principles and appears to be consistent with (or at least not inconsistent with) a fair body of data.

A problem to which interpretations of this sort are susceptible is a confusion between causes and effects, that is, between characteristics that are necessary in bringing about some outcome and those that are incidental to that outcome but that end up in the final product. In evolutionary biology, this distinction is cast as one between *selection for* and *selection of* (Sober, 1984; see Smith, 1986). A parallel distinction exists in behavior analysis between descriptive and functional operants (Catania, 1973); the former refers to the class of responses for which consequences are arranged, and the latter refers to the class of responses generated by contact with those consequences.

This distinction may shed light on some of the present issues. For example, consider Horne and Lowe's account of the development of conventional behavior established with respect to a stimulus class. We have selected this particular behavior because it figures prominently both in their account of a listening repertoire and of the subsequent naming that it makes possible. In fact, in the absence of conventional behavior, the child's listening repertoire would be impoverished, consisting mainly of an ability to comply with mands (p. 192).

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Once conventional behavior has been established (largely through imitation) with respect to some object, say, a ball, the child's further encounters with the ball may well result in conventional behavior, whether or not it has first been identified as such. To the extent that the child is responding neither to a caregiver's utterance nor to his or her own (speaking has not yet been established), the conventional behavior may not involve listening at all; the child is simply rolling the ball, which produces consequences of its own. Thus, a crucial part of the listening repertoire does not appear to depend on listening *per se*, but rather on consequences that grow out of a child's interaction with objects in his or her environment.

Conventional behavior appears to serve an even more critical function in the development and maintenance of naming. Here, it serves both as a discriminative stimulus in the presence of which naming is said to occur (e.g., rolling the ball is discriminative for saying "ball") and as a consequence of naming (e.g., saying "ball" produces conditions under which rolling the ball is more likely). But if the relationships described above hold, then there is no necessary linkage between the name and the conventional behavior with respect to the named object: The name of the object ("ball") and the conventional behavior with respect to it (rolling it) may both be related to the presence of the object (the ball), but they are not necessarily related to each other.

If one accepts the possibility that conventional behavior with respect to the ball can occur in the absence of naming, then why not other relations as well (e.g., pointing to the ball when someone asks, "where's the ball?"). In other words, this line of reasoning may raise more general questions about the *necessity* of naming. Note the emphasis on necessity. The critical matter is not whether naming sometimes accompanies other behavior (it undoubtedly does); what is at issue is whether naming is necessary to hold together the response class. Said another way, is naming part of the descriptive operant class—part of what is selected for? Or is naming incidental to selection—a concomitant effect of selection acting on a different response unit? Or are naming and the other behavior it accompanies separate responses to the situa-

tion, each maintained by its own pattern of consequences? (See Hineline & Wanchisen, 1989, for a similar point.)

Horne and Lowe clearly favor the first alternative, but the latter two strike us as plausible as well. The point to be made here is that, as an interpretation, the naming hypothesis is limited in the kinds of causal statements it can make. Short of an experimental analysis, we cannot distinguish among these alternatives. Fortunately, unlike interpretations in evolutionary biology, behavioral interpretations are concerned with relations that play out within an individual's lifetime, and thus are (in principle at least) subject to an experimental-longitudinal analysis. Horne and Lowe sketch in broad outline the characteristics a research program along these lines might include. We anxiously await additional research of this sort, which will undoubtedly provide important insights into the conditions, some necessary, some sufficient, and some both necessary and sufficient, for verbal and symbolic competence. Until then, naming remains a plausible path—but not the only path—by which such competence may be achieved.

A related but more general observation concerns scientific interpretation itself—the patterns of discrimination and generalization maintained by the scientific verbal community. As Horne and Lowe suggest, "The . . . concept of equivalence brought together . . . different behavioral relations under a *common name* and gave rise to the notion that there was a unitary psychological phenomenon called stimulus equivalence that could be assayed using match-to-sample procedures" (p. 187, emphasis added). What Horne and Lowe appear to be suggesting here is that the common name "equivalence" led researchers to respond to what were previously different behavioral relations in a similar way. This has a number of problematic consequences, as the authors indicate, not the least of which is a tendency to view dependent variables as mere indices of some more fundamental process, rather than being of interest in their own right. It also created a false sense that something had been explained, when in fact it had only been named (p. 238).

This raises a number of important questions about equivalence as a unitary phenomenon, but we have to wonder if the present

account might not be on the same slippery slope with respect to the term *symbolic behavior*. That is, might giving symbolic behavior a common name similarly bring together different behavioral relations under a single rubric, giving rise to the notion that there is a unitary psychological phenomenon called "symbolic behavior" that is in need of explanation? The common name carves out a domain with precisely the characteristics that succumb to an analysis in terms of naming. But do we know enough about the diverse array of behavioral relations wherein we speak of symbolic functioning to justify calling them all by one name? It seems to us that systematic behavioral investigations of the

many relationships that occasion use of the term *symbolic* are in such early stages that it may be premature to identify common features that justify giving them a single name. One of the hallmarks of behavior analysis has been a refusal to take mainstream psychological terms as defining a subject matter, not so much because they are wrong but because they can create a misleading picture of the facts to be accounted for (Skinner, 1945). If, as the present account suggests, naming is capable of creating new patterns of discrimination and generalization, including those of scientific inquiry, then perhaps we should be conservative in our adoption of such names until more of the empirical facts are in order.

THE ORIGINS OF NAMING: A CRITIQUE OF SELF-LISTENING

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Horne and Lowe's article is impressive because it makes the language development literature more accessible to behavior analysts. By relating the findings of developmental psychology to verbal behavior, stimulus equivalence, and rule governance, Horne and Lowe enhance the experimental analysis of verbal behavior by showing the kinds of complex behavior developmentalists have observed children emit. A unique and controversial aspect of their theory, however, is the role that self-listening plays in the development of naming and the bidirectional stimulus relations involved in verbal behavior. Many behavior analysts will reject self-listening as an important variable because it cannot be experimentally manipulated. Some may interpret self-listen-

ing as having the same function as other events that have been experimentally manipulated (Skinner, 1953). To be convincing, however, an interpretation of self-listening as a potential causal variable should conform to known experimental facts. If interpretation is to be different from speculation, it needs to specifically describe the relation between the interpreted event and manipulable variables (Donahoe, 1993; Palmer, 1991). The question addressed in this commentary is whether Horne and Lowe's analysis of self-listening functions as sound interpretation.

Verbal Relations as Operants

Figure 1 outlines some verbal relations as sets of three-term contingencies. These response classes seem to be the critical links between listening, echoing, and naming that Horne and Lowe use to build the case that self-listening can be used to explain bidirec-

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