"equivalence" between the category names and the objects they designate!

Nor does the unfulfilled explanatory agenda stop there, for there is another mystery about language (over and above the mystery of how we manage to name things), and it too is a form of theft. If "Darwinian theft" is the source of the categories we are born already able to detect and "honest toil" is new category learning through trial and error with feedback, then once we have a repertoire of category names earned by honest toil, language gives us the unique further symbolic capacity to acquire new category names from strings of prior names alone: To use an example I have used many times before (Harnad, 1990), if you have learned, by honest toil, to call horses "horses" when you see them, reliably distinguishing them from members of other categories with which they might be confused, and you have learned, likewise by honest toil, to call "stripes" stripes, then, even though you have never encountered one, you are in a position to correctly name your first zebra upon merely being told that a "zebra" is a "horse" with "stripes." That's symbolic/propositional

theft; it can spare you an awful lot of honest toil; and it is the true power of language. Nor is it explained by (or equivalent to) associative equivalence. It will be explained by a successful explanation of what internal structures and processes give us the capacity to learn to categorize and name classes of inputs by detecting the invariance in their sensory projections (Harnad, 1992; Harnad, Hanson, & Lubin, 1991, 1995), and then how strings of names in the form of propositions about category membership can give us the capacity to name new members of categories we have not encountered before.

An explanation like this is impossible from just the experimental analysis of behavior. One must also hypothesize and then analyze the internal structures and processes that generate the capacity to exhibit the behavior (Harnad, 1982, 1984). It is only from such research that we will come to understand the origins of naming and symbolic behavior (Harnad, in press). An internal representational analysis of the honest-toil versus symbolic-theft distinction might even cast some explanatory light on Skinner's named but unexplicated distinction between contingency-based and rule-governed behavior (Skinner, 1984a).

*NAMING AS A TECHNICAL TERM: SACRIFICING
BEHAVIOR ANALYSIS AT THE ALTAR OF POPULARITY?*

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Horne and Lowe present a gold mine of information and deserve our admiration for presenting their material so clearly. Neverthe-

I would like to thank Bryan Roche for discussing with me many of the main issues addressed in this commentary.

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less, I have two reservations about their work. First, Horne and Lowe propose that using the term *naming* would "foster productive interaction with scientists from other traditions" (p. 186). I, too, favor greater cooperation with nonbehavioral researchers (e.g., Barnes & Hampson, 1993, in press; Barnes & Holmes, 1991; Cullinan, Barnes, Hampson, & Lyddy, 1993), and would happily adopt naming as a nontechnical term to foster such co-

operation. To adopt naming as a technical term, however, sacrifices behavior analysis at the altar of popularity. Mainstream psychology has spawned different psychologies for almost every western category of human activity, each with its own idiosyncratic approaches, mostly framed by vernacular terms. In contrast, the "verbal calculus" of behavior analysis has evolved with well-defined and coherently interrelated terms and principles that allow us to describe and explain behavior in general and not just behavior limited to a particular type (e.g., memorizing, thinking, naming, etc.). My concern, therefore, is that if we adopt naming as a technical term, we undermine a major strength of behavior analysis. My second reservation about Horne and Lowe's work relates to their critique of relational frame theory (RFT), and in the remaining commentary I will deal with this issue.

Horne and Lowe's name relation, as an analytic unit, is conceptualized as a self-contained circle with no role for a stimulus, contextual or otherwise, that determines precisely the conditions in which the name relation as a behavioral unit will occur. Thus the unit does not specify a stimulus or setting event that the behavior analyst can manipulate to occasion a particular instance of naming. Furthermore, the naming account requires that events be spatiotemporally contiguous as bases for explanation. That is, true naming can only emerge when a child tacts a stimulus, which then allows the uttered word to function as a stimulus for orienting towards the object, which can then function as a stimulus for uttering the name, and so on. In effect, emergent naming is explained by linking up stimuli and responses in a "join-the-dots" fashion. In contrast to the naming account, (a) the analytic unit of relational frame theory specifies a contextual stimulus (or stimuli), and it is this stimulus (or stimuli) that occasions particular instances of naming, and (b) because RFT represents an operant analysis it does not have to specify spatiotemporally contiguous events to explain behavior (see Barnes, 1994, p. 97; Barnes & Holmes, 1991, pp. 26–27). In short, RFT conceptualizes naming as a type of operant behavior, whereas Horne and Lowe do not. Many of Horne and Lowe's criticisms of RFT emerge from their failure to appreciate the

operant nature of RFT. Consider the following two criticisms.

First, Horne and Lowe ask of RFT, what is the history and how does it work? RFT is theoretical insofar as it suggests that equivalence, naming, and rule following all can be considered forms of operant behavior (see Hayes, 1994, pp. 13–20). From this perspective the details of the history and how it works are largely empirical issues; they cannot be specified before the necessary experimental analyses have been conducted. RFT is therefore both conservative and parsimonious because it employs the fundamental and familiar concept of the operant, and its data-driven approach to science is entirely consistent with the traditional behavior-analytic approach to science (see Chiesa, 1994, pp. 53–54). Of course, whether or not RFT's operant approach is widely adopted within the behavior-analytic community will depend on how effective it is in generating useful basic, applied, and conceptual analyses. Thus, RFT's value is not diminished, as Horne and Lowe suggest, by its lack of specific predictions about the necessary and sufficient histories for naming and other derived performances. In principle, it makes generic predictions; specificity comes in working out particular cases.

Second, Horne and Lowe also complain that RFT does not specify the behavioral principles governing the establishment of relational frames. This is a strange criticism because it is like asking, what are the behavioral principles that govern an operant? To be sure, we could and should conduct experimental analyses that examine the effects of variables on the acquisition and maintenance of behavior with the generic characteristics of a relational frame, just as we do with other discriminative operants. Imagine that we arrange a history of bidirectional training in the presence of a specific contextual cue across a number of stimulus sets, and then we observe emergent behavior in the presence of the cue, using a new set. If we then define the trained and emergent behavior as a type of operant class, the effects of many variables on this type of operant could and should be conducted (e.g., the effects of different stimulus and response forms). True, the RFT approach employs a purely functional concept of an operant (hence the term *overarching op-*

erant class; see Branch, 1994, p. 54), but this approach does not involve breaking with any important conceptual or technical practices within behavior analysis (cf. Catania, 1992, pp. 148–150, on the issue of learning set).

I was somewhat puzzled by Horne and Lowe's claim that RFT suggests that a hypothesized hearing response may control the utterance in emergent naming. This is not the RFT position. Rather, a history of reinforced imitation (of which echoic behavior may be one example) allows the child to imitate the sound he or she hears privately (see Barnes, 1994, p. 96). Thus, if an object is seen that produces a hearing function and appropriate contextual cues are also presented, the child may echo the hearing function actualized by the object. In effect, the private hearing function does not control the utterance; rather, it enters into a rich network of past and present contingencies that control the utterance (i.e., these contingencies are part of the relational frame). To attach exclusive causality to one component of the behavioral system (particularly a component that cannot be manipulated directly) would be inadequate, because it would not be the most effective strategy for obtaining control (see Barnes, 1989; Barnes & Keenan, 1989, 1993, 1994; Hayes & Brownstein, 1986).

Furthermore, contrary to Horne and Lowe's position, it is not clear why a child cannot hear a sound (e.g., a name) that has been repeatedly paired with an object before the child utters the name. In fact, respondent conditioning predicts such an outcome, and strangely enough Horne and Lowe say as much (p. 194). Perhaps one might argue that a child could hear a word when presented with an object, if the child had previously been trained to look at the object before hearing the name (see object → hear name); however, one would not expect the child to hear the name if the training had occurred in the opposite direction (i.e., hear name → see object). This of course remains an empirical issue. Although backward respondent conditioning has not been obtained as easily as forward, it is now generally accepted as a weak but real effect (Catania, 1994, p. 192). Thus, it seems possible that an infant (who cannot yet imitate verbal utterances) may, in certain contexts, privately hear her mother's voice when she sees an object that her

mother has repeatedly named in the past (see object → hear name), even if that naming has occurred only in the opposite direction (hear name → see object). Furthermore, according to RFT, this effect would be strengthened if the child had a history of reinforced hear name → see object and see object → hear name pairings with a number of other objects and names. Parenthetically, this is why RFT requires that a critical test for relational responding involves both mutual and combinatorial entailment; mutually entailed transfer alone might occur by virtue of backward respondent conditioning.

The important point here is that from the perspective of RFT, emergent naming may occur in the way suggested by Horne and Lowe, but it may also occur in the way suggested above (via private hearing). Furthermore, from the perspective of RFT, these issues are largely empirical and perhaps not that important, because if emergent naming occurs only when the child can publicly utter the name and not before, RFT as an operant account could easily accommodate this effect.

A related, troubling issue is that the foregoing type of debate may raise theoretical "nonissues" concerning the status of the private hearing and saying functions (e.g., how might we distinguish between privately hearing and privately saying a word?). This is precisely the sort of theorizing that predominates when an explanation requires spatiotemporal, contiguous cause-and-effect relations between stimuli and responses. Insofar as RFT does not require contiguous cause-and-effect relations (although it can accommodate them; see Barnes, 1994), it can circumvent these nonissues that lead us so easily away from achieving prediction and control in both experimental and applied settings. Finally, although it might be argued that my commentary may contribute to the sort of theorizing that I am criticizing, it is my hope that drawing attention to this matter will alert the reader to the possible pitfalls involved.

In concluding, there are three final issues related to Horne and Lowe's treatment of RFT that I will only briefly examine here. First, many of Horne and Lowe's criticisms of RFT arise because the authors ignore the transformation of function as a defining characteristic of arbitrarily applicable relational responding. For example, Horne and Lowe argue that

naming is not symmetry. As I have argued elsewhere, however, terms such as symmetry are used to describe a particular pattern of a transformation of functions (Dymond & Barnes, 1994, p. 264, 1995, in press; Roche & Barnes, in press), and thus symmetry, by definition, cannot occur independently of a transformation of functions. Symmetry per se is never behavior. Attention to this feature of RFT explains why I might salivate when I see the written word "chocolate," but I certainly do not attempt to eat the word (see pp. 234–235).

Second, Horne and Lowe suggest that RFT allows for "two basic kinds of equivalence" (p. 233). This criticism arises from a failure to appreciate the operant nature of RFT. From this perspective, patterns of relational responding may be conceptualized as possessing fractal-like qualities (see Mandelbrot, 1992). Once a simple pattern of relational responding has been established, such as emergent naming, more complex patterns may be built from the simple pattern, so that eventually a particular stimulus by virtue of its participation in one or more relational frames may establish additional frames (e.g., Kohlenberg, Hayes, & Hayes,

1991). The elegance of RFT, therefore, is that even the most complex human behavior may be conceptualized as multiple layers of relational frames that are all reflective of the same operant process. There is absolutely no need therefore to invoke, as Horne and Lowe do, contingency versus verbally controlled forms of equivalence.

Third, Horne and Lowe may be correct about the now famous sea lion study (Schusterman & Kastak, 1993) (see also pp. 223–224). From the RFT perspective, however, if a nonhuman animal produced reliable evidence of a transformation of functions in accordance with equivalence relations following an appropriate reinforcement history, then by definition we have a relational frame. The frame may not involve vocalizations in the presence of certain objects, but from a functional-analytic perspective it would still be a relational frame. Of course, it is possible that relational frames that include spoken, written, or signed functions may be established more or less readily than frames that require only pointing functions (see Catania, 1994, p. 41), but this again is an empirical issue, and will not change the operant nature of the relational frame.

NAMING AS A FACILITATOR OF DISCRIMINATION

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Upon reflection, it seems time for a paper like Horne and Lowe's to appear in *JEAB*. In our view, its value lies not so much in the positions that it espouses but in the opportunity it affords for broadly considering cur-

rent behavior-analytically oriented research on stimulus equivalence. Perhaps even more important is the authors' ambitious attempt to build upon what Skinner accomplished in *Verbal Behavior* (1957). In responding to Horne and Lowe, we want first to thank them for providing a prod for those doing research in stimulus equivalence and related areas. Their paper and the commentary that it will inspire are likely to help to define better the most important immediate research objectives, to establish priorities for future study,

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