CAN THE NAMING HYPOTHESIS BE FALSIFIED?

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Any history of the psychology of learning will reveal a number of theoretical debates, each hotly contested in its time, that proved notoriously difficult to subject to definitive experimental analysis. In some unfortunate cases the debate continued long past the point when the data being collected were able to play a useful role in resolving the issue at hand. The benefit of hindsight has taught us that progress in dealing with such stalemates often depended on learning to ask a better experimental question. I wonder if the equivalence versus naming controversy does not bespeak the need for a better question.

In their current theoretical analysis, Horne and Lowe contend that behavior analysts interested in understanding the phenomena termed stimulus equivalence would be better served by shifting their focus to the study of naming. They present a provocative argument for naming as the fundamental unit of verbal behavior and of derived stimulus relations. I agree fully with Horne and Lowe that naming, along with most other forms of verbal behavior, constitutes an important topic for study in its own right. However, I also believe that the superiority of their naming account of stimulus equivalence, in relation to those of Sidman and Hayes, may be questioned along a number of important dimensions. If we are to avoid the sorts of theoretical dead-ends alluded to above, perhaps the most critical of these dimensions is its potential for experimental disconfirmation. By the authors' own emphasis, theoretical accounts should be properly judged by their ability to inspire testable and falsifiable predictions. In this brief commentary then, I will consider tenets of the naming hypothesis vis-a-vis the extent to which they might be empirically disproved.

Horne and Lowe outline three key tests of the naming hypothesis. Unfortunately, our

most preferred strategy, direct measurement of the putative controlling variable (i.e., naming), is ruled out early on as a potentially disconfirming test. The number of ways in which naming can enter into control over equivalence performances are said to be "large, possibly infinite" (p. 222) and are often covert as well. Thus, it is argued, failure to observe a particular pattern of verbal response will not allow the conclusion that naming was unnecessary. Although this reasoning cannot be faulted in principle, it does imply that from the start, disconfirmation will have to rest on inference.

The first potentially falsifiable prediction of the naming hypothesis highlighted by Horne and Lowe is that equivalence patterns should not be shown by nonhuman animals, given their lack of naming repertoires. Not surprisingly, Horne and Lowe view the bulk of existing animal data as consistent with this prediction. Notably, they exclude as evidence of equivalence data from a study by McIntire, Cleary, and Thompson (1987), despite the fact that the cited analyses of that study (Hayes, 1989; Saunders, 1989) are directly applicable to the response-mediated (albeit operant) account that constitutes the naming hypothesis. An explanation of the functional distinctions between a child's explicitly reinforced listener behavior in the presence of a directly trained common name (e.g., p. 206) and a monkey's comparison stimulus selection in the presence of a trained and differential response pattern would be of interest.

Of greater concern with respect to falsifiability, however, is their discussion of the formalistic fallacy in the context of animal testing (e.g., p. 224). In their view, even if an animal did pass equivalence tests, there would be few implications for the determinants of equivalence performances in verbal human subjects. By this reasoning, tests with animal subjects could not falsify the naming hypothesis. Indeed, at a later point in their paper (p. 233), Horne and Lowe argue that

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the naming hypothesis should be adopted "until there is evidence" of "equivalence in humans that has been directly contingency generated." Given the difficulties in identifying the latter and in ruling out naming strategies in verbally capable humans (discussed above), it is not clear how such evidence could ever be obtained. In short, despite their statements to the contrary, the potential for animal studies to falsify Horne and Lowe's naming hypothesis is directly challenged by the authors themselves.

The second prediction of the naming hypothesis said to be open to experimental disconfirmation is that the performances of nonverbal human subjects should not meet the defining criteria of stimulus equivalence. Although one might expect the same formalistic fallacy argument described above to be applied with such subjects, it is not explicitly discussed in this context, perhaps because experimental evidence has not yet required it. Later discussion (e.g., p. 239) makes it clear, however, that as with animal subjects, only minor theoretical concessions would actually follow from a demonstration of equivalence in preverbal or nonverbal humans. The reasoning holds that such a case would require only that two types of equivalence be considered; one verbally controlled and the other "contingency generated." In my reading, this does not sound like the falsification of a theory.

The discussion of predictions for nonverbal humans focuses mainly on congruent developmental trends in language abilities and in equivalence performances. However, even these data might be seen as raising some questions for the naming account. For example, although the "naming explosion" occurs at about 18 months, only half of Lowe and Beasty's (1987) 3- to 4-year-old subjects are reported to have shown equivalence patterns, despite the use of familiar and presumably easily labeled stimuli like colors and simple shapes. Of course, the potential to disconfirm the naming hypothesis by demonstrating equivalence with nonverbal humans was never argued for the converse finding (failing to obtain equivalence in verbal humans), and even verbally sophisticated subjects sometimes fail to demonstrate equivalence patterns, but these points may relate most directly to the next testable prediction.

The third and final key test of the naming hypothesis is identified as the prediction that naming interventions used during match-to-sample procedures should influence equivalence performances. Here we are reminded that interventions may either facilitate or interfere with class patterns on match-to-sample tests, depending on whether the naming generated is "congruent with experimenter-defined classes" (e.g., p. 226). This conclusion makes sense and is consistent with data from a number of studies. Unfortunately, it also provides an out that renders this prediction and a number of others unfalsifiable. As long as naming can have any possible effect on equivalence performances, inferences about naming can be made no matter what the test outcome. Suppose that a naming intervention failed to have its predicted influence on equivalence patterns. Would researchers conclude that the naming hypothesis was wrong, or that the intervention had been ineffective in generating the intended naming relations (see, e.g., pp. 224–226), or even that preexisting alternative naming relations had exerted greater control (see, e.g., pp. 216–217)? Given that the naming relations at issue are inherently unamenable to direct observation, these lines of reasoning become impossible to disconfirm. Thus, it is not clear that the study of naming interventions could falsify the naming hypothesis. In addition, an equitable consideration of the formalistic fallacy might question the validity of assuming similar determinants in cases in which equivalence class performances require naming interventions and in cases in which they do not.

As a final note, it seems important to recognize that the capacity for naming to both facilitate and prevent equivalence carries fundamental implications for the naming hypothesis. Although the point receives little explicit attention (see brief mentions on pp. 217, 226, and 237), the status of naming abilities shifts abruptly from providing a complete account of equivalence phenomena to playing a necessary but insufficient role. Still to be accounted for are the critical variables that control congruence between naming and experimenter-defined classes. (Despite the length of the present treatment, I found only two sentences that took up this most basic of issues.) Interestingly, a major criticism of the relational frame account was that it

failed to specify the complete history necessary for equivalence patterns to emerge. I think it could be safely argued that the naming hypothesis begs the same question.

To conclude, Horne and Lowe have offered an interesting, novel, and in-depth look at how naming relations might be established from more basic verbal units. The developmental analysis is sure to spark research ideas, and thus may fulfill certain goals of the paper.

With respect to accounts of stimulus equivalence, however, I remain unconvinced that the naming hypothesis provides a significant departure from or improvement on the alternatives. Incomplete facets of a theory can always be developed further, but I particularly question the ability of the suggested research agenda to further a data-based resolution of equivalence accounts, given the difficulties of potential disconfirmation outlined here.

METHODOLOGICAL ISSUES IN THE STUDY OF NAMING

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Horne and Lowe provide a well-developed account of the acquisition of the naming relation and an interpretation of its role in determining various forms of verbal and symbolic control of behavior. Their account is rich and includes many controversial points, so there is much more that warrants attention than can be discussed in any single commentary. Accordingly, I will leave debate of many of the conceptual and theoretical issues to others, and focus on the parts of Horne and Lowe's article that I believe may provide direction for much of the research that is sure to be generated.

Horne and Lowe outline three research areas that they take to be key tests of the naming hypothesis: (a) equivalence in nonhumans, (b) equivalence in nonverbal humans, and (c) naming manipulations and equivalence in verbal humans. Although they rec-

ognize that "much of the critical experimentation remains to be done" (p. 240), they clearly contend that the available evidence supports the naming hypothesis. I will review these three areas here, with the aim of considering methodological problems that have made critical experiments difficult to conduct. In my view, the currently available studies are insufficient to differentially support Horne and Lowe's position relative to the rival hypotheses. Research strategies that might clarify these issues will be considered.

Equivalence in Nonhumans

Certainly any theory must account for the observation that nonhuman organisms generally fail the tests of stimulus equivalence on which verbal human subjects so easily succeed. For Horne and Lowe, this state of affairs is a predictable consequence of nonhumans' lack of naming skills. On the other hand, accounts of the nonhuman data can also be developed from Sidman (1994) and Hayes (1991), so that the simple failure of nonhuman subjects' performances to show symmetry and transitivity after conditional discrimination training may not differentiate these theories. Nevertheless, because the

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