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.

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# *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 current 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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and to develop useful perspectives on what has been accomplished.

In forums like this, one is tempted to jump headlong into the fray—to begin a point-by-point examination and critique of the assertions, arguments, and interpretations that appear in the target article. Before succumbing to temptation, we will consider first some broader issues relevant to evaluating Horne and Lowe's paper and stimulus equivalence research and theory as a whole.

*Behavior Analyses of Human Development*

We find it noteworthy that many of Horne and Lowe's references concerning early child development have been drawn from cognitive developmental psychology. A few exceptions notwithstanding (e.g., Lipkens, Hayes, & Hayes, 1993), behavior analysts have not been much attracted to the empirical analysis of early child development. Particularly lacking are the longitudinal descriptive studies and analyses that have been accomplished by other disciplines. This omission is unfortunate, given that the literature of behavior analysis contains many worthwhile studies of behavioral processes in young children. Horne and Lowe have done us a service in reminding us that many cognitively oriented developmental psychologists make routine use of operant methods in their work. Within that field, one can even identify debates that have a distinctive behavior-analytic flavor (e.g., the controversy on the role of consequences in correcting atypical language forms; Bohannon, MacWhinney, & Snow, 1990; Penner, 1987; Pinker, 1989). One is led to ask, therefore, why more behavior analysts have not embraced this subject matter and made the kinds of contributions that seem within their grasp.

With respect to equivalence research specifically, little research has looked at stimulus equivalence and related phenomena in children in the age ranges between, say, 18 months and 36 months, when new language (and likely other) skills undergo "explosive" expansion (see Horne & Lowe, p. 202). Similarly, there has been no adequate study directly comparing children who do and do not show typical language development. The report by Devany, Hayes, and Nelson (1986) is now nearly 10 years old. Its numerous methodological limitations are widely discussed among equivalence researchers (e.g., too-brief testing periods, failure to maintain critical baselines during testing, possibility of inadvertent cuing, etc.). Given this study's limitations, many citations, and central role in theoretical discussions, one would have expected by now a number of systematic replications and extensions, perhaps even many. The follow-up, however, has been limited, and the results are equivocal at best (e.g., Barnes, McCullagh, & Keenan, 1990). Devany and colleagues' study remains effectively alone in providing support for arguments that there is a direct relationship between verbal skills and equivalence class formation. As a consequence, Horne and Lowe must argue their position from a thoroughly inadequate empirical foundation.

We believe that the relative lack of longitudinal behavior analyses of early child development and the failure to follow up on the study by Devany and colleagues may have a common explanation. These studies are extremely hard to accomplish, at least with current methodologies; the work is time consuming, expensive, and methodologically intricate. Over the years, our own group has set out several times to conduct equivalence testing in a very difficult population, nonverbal adolescents with severe mental retardation. We have encountered all of the obstacles just mentioned and more, and we have not yet been able to bring such an individual to the point where the critical tests can be conducted in the context of a stable and reliable baseline. Our meager results, however, are not readily interpretable. Individuals whose development has gone so severely awry have profound learning problems that are evident even on the most basic tasks. Further, practical limitations often constrain efforts to develop the necessary baselines. For example, invasive operant procedures that may succeed in establishing such baselines with laboratory animals are out of bounds for ethical reasons. Perhaps Horne and Lowe might argue that our subjects have learning problems because they have no language to help them perform the tasks. Equally plausible (and more parsimonious in our view) is that our subjects' atypical language development is secondary to more basic problems of attending, observing, remembering, and so forth.

*Behavioral Analyses of Nonhuman Animals*

Horne and Lowe are led to their position in part because, they argue, nonhuman animals have not generally succeeded in passing equivalence class tests. But what can be concluded from studies accomplished thus far? Suppose this was a legal proceeding rather than a forum for scholarly discourse. Could any of us defend the position that testing has been adequate? One can almost hear the examining attorney: "Isn't it true that only a few studies have been reported? Isn't it true that the animals were given experience with only a small number of arbitrary stimuli? Isn't it true that several studies used procedures that would set up competing control by the animal's own behavior? And finally, isn't it true that the work accomplished so far provides a completely inadequate basis for reaching any conclusions about laboratory animals' capabilities?"

Were we in court, we would be compelled to answer "Yes" to all of these questions. For us, the major problem with most of the work accomplished thus far is that it has not prepared the animal adequately for the equivalence tests. Many studies have ignored what we may call the problem of stimulus definition. As Ray and Sidman (1970) wrote,

> All stimuli are [complex] in the sense that they have more than one element, or aspect, to which a subject may attend. To ask that an experimenter be aware of all the possibilities is already, perhaps, an impossible demand. To ask, further, that the experimenter arrange conditions so that no undesired stimulus-response correlation is ever reinforced sets a truly impossible task. For these reasons, we may never have a generalizable formula for forcing subjects to discriminate a specific stimulus aspect. (p. 199)

This quotation elegantly summarizes the underappreciated problem of subject-experimenter communication that must be overcome in any teaching situation. Any effective communication, verbal or otherwise, entails the establishment of "joint regard" (or, more commonly, "joint attention" in the cognitive-developmental literature); the topographies of stimulus control for both subject and experimenter must cohere in order for the results to be consistent with the expectations of the latter.

But how does one go about establishing coherence between subject and experimenter stimulus control topographies? It has been known for some time that giving subjects experience with a variety of stimuli may help to encourage control by the experimenter-specified stimulus differences or stimulus relations. Notably, this was the approach taken by Schusterman and Kastak (1993), who have provided the most convincing data thus far that nonhumans can pass equivalence class tests. From our standpoint, their study carries unusual weight: An animal with a fairly large brain was trained with many sets of stimuli to encourage concordance with the experimenter's definition of the relevant stimuli and possibly to adapt the animal to the introduction of novel sample-comparison combinations (as routinely occurs on equivalence tests). Although their positive findings currently await replication, efforts to explain them away have not been compelling, as illustrated by those in Horne and Lowe's article.

Our own work with rats (e.g., Dube, McIlvane, Callahan, & Stoddard, 1993) suggests to us that studies with this and other small-brained species will be limited in their ability to resolve the current debate. Like our studies of individuals with profound mental retardation, the work has been time consuming, methodologically complex, and has not yet produced compelling results. The work reported from animal cognition laboratories (e.g., Urcuioli, Zentall, & DeMarse, 1995; Wasserman, DeVolder, & Coppage, 1992), although much more impressive, has some of the same obstacles to overcome. Put succinctly, it is hard to develop in rats and pigeons the baselines of precise stimulus control that are achievable with humans, higher primates, and marine mammals. We believe that Schusterman and Kastak have blazed the trail. We can only hope that laboratories with access to apes, sea lions, and dolphins can pursue their compelling lead.

Should it become evident that positive equivalence-test outcomes can be shown reliably in nonhumans, this could present a problem for theories that have used the previous failures-to-find as supporting evidence. One such theory, Hayes' relational frame theory, should be able to incorporate such findings with little difficulty. Frame theory can be reasonably characterized as an updated, extended version of traditional operant accounts of concept formation. Horne and Lowe's theory, by contrast, appears to have greater difficulty. For us, their suggestion that equivalence in nonhumans might occur via processes different from those operating in humans is not very attractive intellectually. Problems of parsimony aside, it is not obvious to us what pattern of data could prove or disprove this aspect of their proposal.

*The Role of Naming in Equivalence Class Formation*

In our view, a major weakness of Horne and Lowe's position is that it does not consider fully all of the ways in which naming might be involved in performances on equivalence tests. Although it seems to be true, for example, that naming facilitates positive outcomes, this finding has multiple interpretations. Consider the behavioral requirements of typical equivalence protocols. During training, the baseline conditional relations require a mix of successive (sample) and simultaneous (comparison) discriminations. Experimenters often assume, with questionable justification, that stimuli that have been discriminated in simultaneous presentation will also be discriminated when they are presented successively (i.e., when former comparison stimuli are presented as samples). They also may assume, also with questionable justification, that discrimination training establishes sample-S+ rather than sample-S− controlling relations. Successive discrimination of all stimuli and sample-S+ relations must develop in order for the subject to exhibit positive outcomes on standard equivalence tests. When the procedures encourage the subject to name the stimuli, they may also encourage the prerequisite successive discriminations and sample-S+ relations. Studies thus far accomplished, even those that may at first seem compelling (e.g., Eikeseth & Smith, 1992), do not allow us to choose between our "naming-as-facilitator-of-prerequisite-discriminations" account and the one offered by Horne and Lowe.

To make our point another way, suppose that one were able to exert precise contingency control over what the subject observed, how long he or she observed it, how many times he or she looked back and forth between the comparison stimuli and between each comparison stimulus and the sample, and so forth. Suppose further that one obtained the plausible finding that establishing certain patterns of observing facilitated class formation and that such patterns were maintained even when contingencies did not explicitly require them. What could one conclude from such observations? Certainly, one could conclude that effective "observing behavior" was an important variable in obtaining positive equivalence class test outcomes. One would be skating on thin ice, however, in asserting that "observing" was the key process determining those outcomes.

Our argument is essentially this: Individuals who have acquired verbal repertoires have acquired many other behavioral repertoires that must also be considered (scanning effectively, attending selectively to features of complex stimuli, etc.). These kinds of behavior are prerequisite for acquiring a naming repertoire and may themselves become better organized by overt or covert verbal behavior. Indeed, the interaction of naming with other behavioral repertoires may encourage positive outcomes on class tests. But there is little justification in the current literature for supposing that naming is the fundamental process necessary for such outcomes. In our view, naming emerges as central in Horne and Lowe's account for the same reasons that others have been drawn to verbally based accounts in the past. Verbal repertoires are familiar to everyone, are often easy to observe in others, are obvious in observing our own overt and covert behavior, and at least superficially are plausible candidates for an important, if not causal, role in behavior. By comparison, other repertoires are less obvious and perhaps less likely to command theoretical attention.

We are also concerned that Horne and Lowe's account is in fact mediational, despite their claims to the contrary. We do not dispute that their account differs from those offered in the verbal learning literature. We do, however, disagree with their assertion that they are not proposing a stimulus-response chaining model. For us, their Figure 4 seems to diagram something very like stimulus-response (S-R) chaining. In general, their account seems reminiscent of theories of mediated generalization (e.g., Osgood, 1953) and its reliance on stimulus substitution in S-R units: The experimental stimuli are discriminative for overt or covert naming, and the naming in turn produces stimuli that control the selection response recorded in the experiment. This is mediation. The authors protest, "Indeed the primary role of naming should not be viewed as *mediating* the establishment of stimulus classes: Naming *is* stimulus-classifying behavior" (pp. 226–227). Perhaps so, but in virtually all of the equivalence articles that are reviewed in their paper, pointing or touching is the stimulus-classifying behavior that needs to be explained, and Horne and Lowe's explanation is that stimuli produced by naming controlled the pointing. Again, this sounds like mediation.

If naming does not serve a mediating role, what status does it have? Is it an intervening variable? When Horne and Lowe assert that naming becomes, in essence, an automatized behavioral repertoire, they begin the slide down the slippery slope. For example, consider the following description of a naming event: "When the child names, she not only makes an utterance but also at the same time brings into being other behavior, either full-blown or incipient, overt or covert, all of which is bound up with the word" (p. 214). For us, talk of bringing behavior into being is at odds with the basic assumptions of behavior analysis. Moreover, the other behavior referred to is listener behavior, a concept that needs further development; at present, it seems to include any and all behavior with a verbal discriminative stimulus. For Horne and Lowe, the genesis of naming is the "fusion of conventional speaker and listener functions [that] establishes a qualitatively new bidirectional relation in the child's behavioral repertoire" (p. 200). Given the multiple functions of both speaker and listener behavior identified by Skinner and by Horne and Lowe, we are left wondering about the nature and determinants of behavioral "fusion" and the status of its qualitatively new product in a behavioral analysis.

Horne and Lowe suggest that naming is a new kind of event that has characteristics of both stimulus and response, but cannot be classified as either. There are alternatives, however. One might suggest that naming is purely an abstraction, with a status like *expectancy* in some cognitive theories (e.g., Peterson, 1984). That is, one might argue that naming is theoretically useful even if the specific behavior involved in naming cannot be identified. Alternately, one might argue that all naming events could in theory be broken down into component stimuli and responses, but that practical limitations on the observation of covert or incipient behavior prevent such an analysis.

In our opinion, the probable end point of thinking like this is the inference of neural responses, neural behavioral repertoires, and the like. Such a conclusion would not necessarily be bad. The analysis of such responses and repertoires, however, would be most useful if it was conducted collaboratively with neuroscientists. They are increasingly able to help us look inside the skin. When accomplished via inference and solely by behavior analysts, there is a great risk that we will begin to exhibit behavior that is inconsistent with the fundamental principles of our discipline. The limits of a purely behavioral analysis were well expressed by Skinner (1989), who wrote that "There are two unavoidable gaps in any behavioral account: one between the stimulating action of the environment and the response of the organism and one between consequences and the resulting change in behavior. *Only brain science can fill those gaps.* In doing so, it completes the account; it does not give a different account of the same thing" (p. 18, italics ours).

### *Conclusion*

From the preceding discussion, it is clear that we see significant limitations in many aspects of Horne and Lowe's analysis. Nonetheless, there is much of value in their contribution. They have clearly made a good faith effort to give us a comprehensive presentation of their current thinking. In our view, the field of equivalence research could be much advanced by comparable efforts to present and discuss other perspectives. For example, the proposal that equivalence is a basic process, not derivable from other processes, has not yet received the detailed presentation and debate that such a radical suggestion merits. Horne and Lowe's paper may help set the occasion for accomplishing this necessary exercise. We hope that this paper will also help to inspire additional efforts to extend and update the thinking that was begun in *Verbal Behavior.* Should Horne and Lowe succeed in their efforts to inspire new interest by behavior analysts in the scientific study of early child development, their contribution will be great even if these specific theories do not survive the test of time.

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## *NAMING, STIMULUS EQUIVALENCE, AND CONDITIONED HEARING*

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Horne and Lowe suggest, in keeping with earlier formulations of the naming hypothesis (Dugdale & Lowe, 1990), that naming is necessary for passing standard tests of stimulus equivalence (i.e., for forming emergent stimulus-stimulus relations). They suggest a developmental sequence by which the naming relation is learned. First, listener behavior is established with respect to classes of stimuli. Next the child learns a generalized echoic repertoire that is said to supply a critical link in the formation of a naming (or speaker-listener) relation. Upon hearing someone else say "shoe," the child now has two types of behavior evoked concurrently (i.e., listener behavior of selecting and seeing shoes and, via the echoic, speaker behavior of saying "shoe"). Because this pairing incidentally enables the child's seeing shoes to be an antecedent to his saying "shoe" (which is consequently reinforced by the caregiver), the shoe-"shoe" relation is learned. This interlocks with the previously established listener relation to create, say Horne and Lowe, a "qualitatively new bidirectional relation" (p. 200). Moreover, they claim that "it is at this stage that we can say the child has learned to name the shoe" (p. 199). I have three observations regarding this: First, I do not see why the interlocking of two relations that have both been directly trained produces a *qualitatively* new relation. Second, because both relations have been directly trained, there is nothing emergent in the episode. Thus, the behavior of the monkeys in the equivalence study by McIntire, Cleary, and Thompson (1987) satisfies this definition of naming; their contingencies allowed precisely this kind of interlocking to occur, and their monkeys went on to pass the test. If so, we have an example of the same behavioral principles (three-term contingency) governing the success of nonhumans and humans on equivalence tests. Third, naming as defined here does not require an echoic repertoire. All that is required is to train the respective listener and speaker relations somehow. Speaker relations can be trained in subjects with little or no echoic repertoire. For example, tacting can be established (albeit slowly) by shaping successive approximations (see Manabe, Kawashima, & Staddon, 1995, for an example with budgerigars).

Later, a change occurs in the definition of naming when Horne and Lowe claim that "naming is a higher order bidirectional behavioral relation" (p. 207). They attempt to specify how it comes about, arguing that "with repetitions of the interactions [between the listener, echoic, and tact components] shown in Figure 9 the cues of the caregiver's naming of and pointing at a new object come to be sufficient *on their own* to evoke the full sequence of behavior that makes up the name relation" (p. 202, my italics). The point to note here is that direct reinforcement is

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