

about it does so generally via naming, that is, names are first established and then are functionally extended to mand objects and events" (Horne & Lowe, p. 211). In contrast, Skinner, relying here on Sense 3, takes the

mand (1957, chap. 3) as the basic form of verbal operant and treats the tact (1957, chap. 5) as a later and more sophisticated development.

NAMING AND EQUIVALENCE RELATIONS

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Horne and Lowe argue that naming skills account for positive equivalence test results in the laboratory, making the construct of equivalence redundant. In its place, they identify naming as the basic unit of verbal behavior, and suggest that efforts be directed towards studying naming. To this end, they offer a definition and an elaborate theory of the development of naming, and then apply a naming analysis to existing studies of equivalence relations. In comparing Horne and Lowe's ideas to those of Sidman (1986, 1994) and Hayes (1991, 1994), we have been struck by many similarities. When stripped of their unobservable or untestable elements, the theories have a lot in common. Here, we compare the three major theories in several key areas.

Naming Relations: Where Do They Come From?

Like equivalence relations and relational frames, naming is a relational concept. The tests that Horne and Lowe propose are closely congruent with Sidman's tests for equivalence

relations. The congruence has greatly increased now that Sidman's (1994) definition is no longer restricted to matching-to-sample procedures. Moreover, some of the tests that Horne and Lowe described have been carried out by Lipkens, Hayes, and Hayes (1993).

Horne and Lowe's analysis of the specific case of naming is a valuable exercise. It makes clear the real-world importance of emergent performances, and may encourage ties to the language literature. Naming, as they have defined it, is an extremely important example of an equivalence relation.

Their efforts make clear that the naming relation involves more than just symmetry. When a child learns to act on an object upon hearing its name and subsequently tacts the object, an echoic relation is also a prerequisite. Figure 1 shows the essential observable events that define naming. Support for the importance of echoic relations is provided in the aforementioned study by Lipkens et al. (1993): When visual-vocal relations were trained, auditory-visual relations were shown, but the opposite did not occur *until echoic relations were trained*. The explicit emphasis on the echoic has not occurred in previous discussions of naming within the equivalence framework, although echoics are generally considered to be an important element of language training.

Horne and Lowe's analysis suggests that

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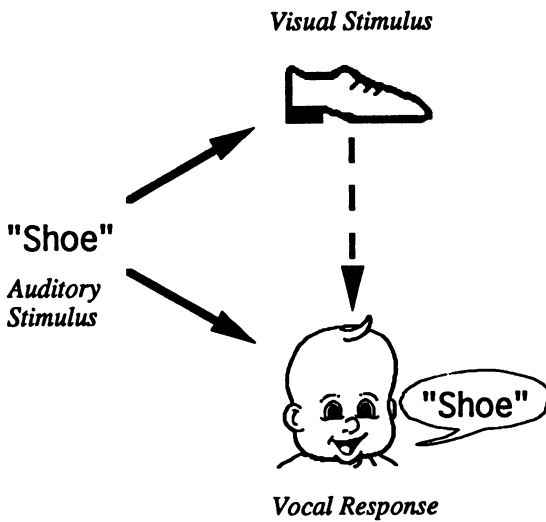


Fig. 1. A schematic of naming. The bold arrows show trained performances, and the dashed arrow shows an emergent performance.

tact relations in humans are almost invariably names (i.e., equivalence relations), because of the way they are acquired. This observation illustrates the importance of human vocal behavior to the development of symbolization: It is fairly easy to acquire echoics because of the point-to-point correspondence between the utterance produced and its discriminative stimulus. It also allows speculation as to the difficulties inherent in the use of sign language in ape language studies. Apes came to emit signs through training involving the physical manipulation of their hands. According to the present analysis, without learning to imitate hand signs, equivalence relations between signs and objects would not be possible. In contrast, the visual symbols used in the ape language research of Savage-Rumbaugh and colleagues can serve as either the first or second term in a relation without any change in form. This seems to be a major advantage in procedure.

Another important issue raised by Horne and Lowe is that of the extremely rapid acquisition of naming that is observed in toddlers. Toddlers can often name an object that merely has been named in their presence. Child language researchers refer to this phenomenon as *fast mapping* (Rice, Buhr, & Nemeth, 1990). It is likely that Figure 1 identifies some of the important features of this performance. Note that the absence of ex-

plicit reinforcement for any of the prerequisite stimulus control relations does not set this finding apart from the equivalence literature (Harrison & Green, 1990; Saunders, Saunders, Kirby, & Spradlin, 1988). Studies of the exclusion phenomenon also have relevance to this phenomenon (Wilkinson & McIlvane, 1994).

We have thus far covered the part of Horne and Lowe's analysis of naming that is observable and compatible with the views of other theorists. We now turn to the part that diverges from that of other researchers. Horne and Lowe's full description of naming includes covert responses (CRs) and stimuli (CSs) in an explanatory role. No procedures are given for determining whether or not these events are occurring. Moreover, to be useful in predicting and controlling behavior, it would also be necessary to know how the CRs and CSs relate to antecedent experiences and how they relate to the responses produced. Horne and Lowe suggest that these covert responses can occur in response to visual, auditory, gustatory, or olfactory stimuli. Once they occur, they can control a variety of responses such as saying the word, orienting toward the object, or initiating any other object-directed action. Although couched in behavioral terms, these covert events seem to function in much the same way that the term *idea* functions in the interpretive system of the layman. With a system that includes covert responses and stimuli, it is possible to "account for" or "explain" any possible behavioral outcome. But "accounting for" and "explaining" behavior may have no value in predicting or controlling behavior. Thus, although there are many behavioral outcomes that would give the illusion of supporting the Horne and Lowe theory, there appear to be none that would lead to abandoning the theory.

Is Equivalence an Unanalyzable Primitive?

Sidman has recently speculated that equivalence relations represent a basic behavioral process (1990, 1994). In contrast, Hayes (e.g., 1991) and Horne and Lowe propose an essential behavioral history. (In addition, Horne and Lowe assume that some form of verbal mediation is necessary.) But, as has historically been the case with nature-nurture issues, these possibilities may be difficult to sep-

arate empirically. A study reporting positive tests for equivalence in a sea lion is illustrative (Schusterman & Kastak, 1993). Tests were conducted after training of the type specified in Hayes' relational frames account. Ultimately, the sea lion showed strong evidence of emergent stimulus control. On the surface, this study seems to provide evidence for Hayes' position. However, Sidman (1994) notes that a subject's history helps to determine whether equivalence relations are demonstrated. Given this possibility, the study does not clearly support Hayes' theory and refute Sidman's. Moreover, if it turns out that multiple examples are sufficient to produce emergent stimulus control, then one would have to accept learning by multiple examples as a basic behavioral process (as noted by Sidman, 1994). So, in essence, both Hayes and Sidman propose that equivalence relations represent a newly identified behavioral process.

Verbal Mediation: How Is It Involved?

The issue of verbal mediation in laboratory equivalence performances arises on two levels. The first is the question of whether verbal mediation is necessary; the second is the question of what role verbal mediation plays when it occurs.

Sidman considers verbal mediation, whether it be in the form of naming or more elaborate self-instruction, to be unnecessary to the demonstration of emergent performances. He notes that, when all of the performances are an outcome of the training procedures, it is not possible to attribute one to the other (Bush, Sidman, & deRose, 1989; Sidman, Willson-Morris, & Kirk, 1986). Given current knowledge, this seems to be an appropriate scientific conclusion.

Hayes also finds a lack of parsimony in using language to explain emergent performances: "such attempts end up imagining implausible training histories, appealing to strong effects for backward conditioning, or explaining derived relations by another form of derived relations" (Hayes, 1994, p. 20). Indeed, Horne and Lowe's theory of the role of naming in equivalence relations involves the acceptance of numerous unproved assumptions about the development and occurrence of verbal behavior.

The evidence currently available is incon-

clusive as to whether verbal mediation is necessary for equivalence. In fact, as one looks more closely at the arguments and counterarguments, one becomes less sanguine that this question will ever be answered to everyone's satisfaction. In this climate, it is perhaps inevitable that the same data inspire different reactions, and that these reactions reflect the theoretical preconceptions of the theorist. (That is one reason exchanges such as the present one are valuable.) For example, Schusterman and Kastak's (1993) report of the demonstration of equivalence relations in a sea lion appears to be quite damaging to Horne and Lowe's position that naming is necessary. Horne and Lowe, however, minimize the importance of the sea lion demonstration. Their critique seems somewhat strained. Two features of their discussion are of particular concern. First, the description of the procedures suggests that a comparison selection was scored when the sea lion simply swayed in front of the comparison stimulus. Moreover, the stimulus-compounding interpretation of the outcome seems to depend on this aspect of the procedure. The stimuli were presented in boxes (10 cm deep) that were recessed into the panel, however, and the sea lion stuck his nose into the comparison box, a much more discrete response than simply swaying in front of it.

A greater concern is the implication that the demonstration is tainted by the reinforcement of test trial responses. But Schusterman and Kastak's (1993) data presentation focused on the first four trials of testing across numerous stimulus sets, with emphasis on the first trial. The reinforcement procedures could not have determined the response to the first trial, except in the nonspecific sense of maintaining responding in general. It is extremely important that we not dismiss this method of studying emergent stimulus control. The study of verbally limited subjects, both human and animal, may require continuous reinforcement over many stimulus sets. Few studies have used these procedures. We doubt that even humans would continue to exhibit emergent stimulus control if no instance of emergent stimulus control was ever reinforced.

Ultimately, Horne and Lowe suggest that, even if the behavior of nonhumans meets the definition of equivalence, nonhumans may

be doing something quite different from humans. They offer no means of objectively determining these differences, however. Although the sea lion study requires replication, Horne and Lowe's reasons for rejecting the evidence it provides are unclear.

Horne and Lowe place considerable emphasis on a recent failure to demonstrate symmetry in chimpanzees (Dugdale & Lowe, 1990). The chimps' failure is said to be especially convincing as to the inadequacy of "frame training" because the chimps' extensive language training seemingly provided the requisite history. The negative tests might be more indicative of the limitations of the training and testing procedures, however. The chimps had previously learned scores of conditional discriminations involving real objects, pictures, and abstract symbols. Despite these histories of training in a somewhat different format, 1 chimp did not learn the prerequisite conditional discriminations under the more standard equivalence procedures. This strongly suggests that the teaching and testing format used by Dugdale and Lowe was functionally different from that used by Savage-Rumbaugh, and argues against assuming that the outcome of the framing history should have transferred to this situation. In addition, an examination of previous reports suggests that 2 of the chimps had previously demonstrated transitivity (Cerutti & Rumbaugh, 1993; Savage-Rumbaugh, 1981) and the 3rd had demonstrated symmetry (Savage-Rumbaugh, Pate, Lawson, Smith, & Rosenbaum, 1983).

We also caution against accepting the Devany, Hayes, and Nelson (1986) study as evidence that nonverbal humans do not demonstrate equivalence. In 10 years, these findings have not been reproduced. Replication attempts have met with extreme difficulties in teaching the baseline conditional discriminations (e.g., Augustson & Dougher, 1992), leaving some to wonder whether the success of some of the Devany et al. subjects was due to inadvertent experimenter cuing (a table-top task was used). An equally important concern is that testing was not conducted under standard conditions of stable, high accuracy of the prerequisite conditional discriminations. Testing began after a single block of 10 trials (of the full baseline) with accuracy of at least 90%. Further, the main-

tenance of the prerequisite relations was not assessed once testing began. At minimum, a convincing negative outcome should show a discrepancy between test trial and baseline accuracy. This does not characterize the Devany et al. study.

Currently, there is not enough good evidence that primates and verbally limited humans *do not* demonstrate emergent performances to rule out the possibility. Most of the studies have involved only one series of tests with one set of stimuli. When normal adult human subjects fail after brief testing, we do not assume capacity limitations. Moreover, all of the studies have used highly abstract matching procedures, which Horne and Lowe find generally problematic.

As Horne and Lowe noted, those who believe that verbal mediation is not necessary for the demonstration of equivalence have accepted soft evidence. First, the subjects of early studies were so limited in verbal skills that self-instructional strategies were unlikely (Sidman, 1971; Sidman & Cresson, 1973; Spradlin, Cotter, & Baxley, 1973). Second, the demonstration of equivalence sometimes occurred prior to or in the absence of naming, and sometimes naming was not accompanied by the demonstration of equivalence (Sidman, 1971; Sidman & Cresson, 1973; Sidman, Cresson, & Willson-Morris, 1974). The fact remains that the subjects exhibited some verbal behavior, so these results do not provide hard evidence that verbal mediation is not involved in the equivalence performances.

Additional evidence against the necessity of naming comes from subjects who exhibited symmetry even when differential naming was prevented. In our laboratory, one performance of a subject with moderate mental retardation showed symmetry even though she had been required to say the same name in the presence of both samples throughout training (Saunders & Spradlin, 1990). However, if one accepts the possibility that the subjects were engaging in helpful subvocal verbal behavior along with the vocal behavior, these results do not preclude the possibility of verbal mediation.

In summary, the issue of whether or not naming or verbal mediation is necessary for equivalence relations remains unresolved. If one allows for the possibility of unobservable verbal mediation, and if one assumes that

nonhuman subjects are doing something different than humans, the issue may never be resolved.

Do Emergent Performances in the Laboratory Represent a Single Process?

Although there is no conclusive evidence that verbal mediation is necessary, the question remains as to whether or not important differences exist between performances that include self-instruction and those that do not. Horne and Lowe are concerned that similar outcomes may be based on different behavioral mechanisms. This concern may be the most important part of their paper. Most studies have involved subjects with verbal repertoires, often normal adults. Self-instruction may be involved in the performance of many human subjects. This differs greatly from what both Hayes and Sidman believe are the minimally sufficient conditions for the demonstration of laboratory performances. Horne and Lowe are concerned that much of the field is proceeding as if everyone is studying the same thing, and we share that concern. Moreover, one wonders how much information that is relevant to fundamental issues of symbolization—the issues that Sidman originally sought to define objectively—can be gained from studying verbally sophisticated subjects.

Are Any of the Theories Refutable?

Each of the theories can ostensibly be supported by data. Yet, are there definitive studies that would lead to refuting any of them? Horne and Lowe maintain that their theory gives rise to a number of predictions. Only subjects who name should demonstrate equivalence performances. However, they also state that, even if the sea lion data are reproduced, that does not mean that human subjects are doing the same things as the non-

verbal subjects. This suggests that, even if demonstrations of equivalence among non-verbal subjects became a reliable finding, Horne and Lowe could still maintain their theory of equivalence performances in humans. Sidman's theory that equivalence is a primitive seems to be refuted if subjects who initially did not exhibit symmetry came to do so after multiple exemplars. However, he notes that a subject's history plays a role in determining emergent performances. So perhaps, rather than symmetry being learned through multiple exemplars, symmetry may be a primitive that is demonstrated only after a set of specific experiences narrows the options. Hayes' relational frame theory also seems to be adaptable to any outcome.

Because each of the theories seems to be identified with a set of studies aimed at determining variables related to the demonstration of equivalence performances, each may ultimately contribute to the generation of data that will clarify the conditions that facilitate or inhibit the demonstration of such performances. However, it is unlikely that any of the theories will be refuted (to everyone's satisfaction) in the near future.

Conclusion

In summary, when stripped of its unobservable elements, Horne and Lowe's account of naming provides an extremely important application of Sidman's definition of equivalence. Their suggestion that laboratory demonstrations of equivalence relations involve differing behavioral mechanisms certainly merits attention by equivalence researchers. We recommend overlooking, however, the aspects of their theory that attempt to explain observed behavior by "appeals to events taking place . . . at some other level of observation" (Skinner, 1972, p. 69).