plained in a like fashion. Thus, when a set of already-named objects (chair, table, etc.) are given an additional and common name (i.e., "furniture") and the subject is then asked to select the furniture objects from a mixed set, he or she may simply do so by selecting each object that evokes a tact that enters into joint control with the self-echoic of the sample name "furniture."

As illustrated in Figure 1 (Panel C), the capacity of joint control to mediate generalized bidirectional responding extends to stimulus equivalence as well. Let us assume, along with Horne and Lowe, that during training subjects apply common names to both sample and comparison stimuli. So, during A-B training Name 1 (N1) is generated for both A1 and B1, and during A-C training, for A1 and C1. Thus all the stimuli of Class 1 evoke a

common naming topography. Given this, each should control selection of the other under joint control, thereby producing all the relations that define stimulus equivalence.

Conclusion

Horne and Lowe have identified the component responses of the bidirectional word-object relation but not their articulation, because the joint control event resides within their formulation of the naming relation. Thus, in Figure 1 (Panel D), before the correct comparison stimulus is encountered, the subject's behavior is illustrated by the self-echoic loop on the left. When the named object is then encountered, the self-echoic comes under joint control of the tact emitted to the object, as illustrated on the right by the converging arrows. Joint control is thus a fundamental component of the naming relation.

ON THE ORIGINS OF MISGUIDED THEORIES OF NAMING AND OTHER SYMBOLIC BEHAVIOR

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Horne and Lowe's lengthy treatment of the origins of symbolic behavior has two general themes. The first is criticism and ultimately dismissal of the paradigm of stimulus equivalence and its associated conceptual framework. They say it is "a highly artificial set of circumstances of a sort infrequently, if ever, encountered by most children and thus is an odd experimental paradigm upon which to

base a general understanding of new . . . relations or language itself" and "an elaborate set of abstractions" that create "a false sense of security" and "introduce conceptual confusion and impede research" (p. 238). The second theme is a presumptive developmental account of the origins of naming. Naming, as defined by Horne and Lowe, is said to account for the emergent phenomena that have generated so much interest in the stimulus equivalence paradigm and to be a worthy target of a systematic program of developmental behavioral research.

I am sympathetic to the gist of Horne and Lowe's criticisms of the stimulus equivalence paradigm. It is a paradigm that does not pass the Aunt Sarah test, which has become for me an important first line filter of the prom-

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ise of a line of psychological inquiry. The Aunt Sarah test is a simple thought experiment. Your Aunt Sarah is intelligent but untutored in the culture of psychological research. She asks you what you're doing at the university. You say, "Mostly research." She says, "What's the research about?" You do your best to explain briefly what you're doing and why it is important. Does your narrative leave her politely trying to cover her befuddlement about why anyone would be doing that? You've failed the Aunt Sarah test. The Aunt Sarah test is useful because psychology is an embarrassingly multivariate field with an infinite number of problems that psychologists can investigate, and the problem of choosing useful ones is not trivial. That one's advisor, or a famous psychologist, or a lot of psychologists have deemed a particular paradigm worthy of pursuit is a tempting garden path that can lead, absent countervailing forces, to an increasingly incestuous inwardly spiraling science. The Aunt Sarah test is a useful countervailing force.

Unfortunately, my accord with Horne and Lowe's view of the stimulus equivalence paradigm does not last very far into their account of naming. The problem is that much of what they propose is demonstrably wrong. This is something I would not be able say except that developmental behavioral research on naming, which the authors hope to inspire through their theoretical account, already exists in number. They simply seem to be unaware of it. Here are just a few points in the Horne and Lowe theory that run up against empirical contradictions.

Their Figure 4 is a schematic of their conceptual model of the name relation, which "involves speakers responding as listeners to their own speaking" (p. 191). This "fusion of speaker and listener behavior within the individual" (p. 240) is touted by Horne and Lowe as their main extension of Skinner's (1957) analysis of verbal behavior. The problem here is that speaking is not necessary for the development of language, including understanding of names. The dissociation of language development in general from speaking is demonstrated most dramatically in the rare condition of congenital anarthria. Children with this condition, who cannot speak, nevertheless can develop normal comprehension of complex spoken language (Lenne-

berg, 1962). The specific dissociation of naming from speaking is demonstrated in the not-so-rare condition of specific expressive language delay (Whitehurst & Fischel, 1994). From 9% to 17% of 2-year-olds (varying with socioeconomic status) meet a criterion of fewer than 30 expressive vocabulary words and no word combinations at 24 months of age, an age at which most children have large expressive vocabularies. Many of these children have no more than one or two words in their expressive vocabularies and yet have normal nonverbal intelligence and receptive abilities that include the understanding of hundreds of names. This condition is self-correcting over the preschool years for the vast majority of children whose delay is specific to speaking (Whitehurst & Fischel, 1994). If speaking is not necessary to the development of a mature understanding of names, then Horne and Lowe predicate their account of the name relation on a model that is fundamentally at odds with the natural phenomenon to which it is addressed.

Horne and Lowe's Figure 5 illustrates their model of the development of listening. I applaud their emphasis on the importance of the development of listener behavior in the understanding of naming: The phenomena of specific expressive language delay and congenital anarthria, described above, suggest its primacy, as I believe do some of my own empirical and theoretical contributions to the literature (e.g., DeBaryshe & Whitehurst, 1986; Whitehurst, 1977, 1979; Whitehurst & DeBaryshe, 1989; Whitehurst, Kedesdy, & White, 1982; Whitehurst & Sonnenschein, 1985; Whitehurst & Valdez-Menchaca, 1988). However, the Horne and Lowe model is the three-term contingency applied to listening, and that is the wrong model if it is taken as the fundamental one. Their prototype for the development of listening is that a caregiver uses the name of an object in the presence of a child, the child points to it, and the caregiver reinforces that pointing behavior. No doubt such interactions occur and can be functional, but the last two components of the three-term contingency, the child's overt pointing response and the adult's reinforcement, are not necessary for appropriate comprehension of a word to occur. My laboratory has conducted dozens of experiments demonstrating this.

For example, Whitehurst et al. (1982) re-

Fig. 1. Percentage correct on four probes across time of comprehension of novel object names by six 2-year-olds. Objects were labeled by an adult at the same time the object was shown to a child (present condition), 10 s before the object was presented (future condition), and 10 s after the object was presented (past condition). Each child experienced each condition in a counterbalanced order. Each child experienced 15 novel object-names in all, five in each condition.

ported a series of experiments on the roles of word-referent temporal contiguity, word-referent informativeness, and referent value in the word learning of 2-year-olds. All of the studies were conducted in a natural setting over several weeks, and each involved a basic paradigm in which an adult periodically removed an object from a box, named it for the child while holding it in the child's view, and put it back into the box. There were never any contingencies for the child's pointing or labeling. Name learning was probed every session by having the child pick a referent from an array of referents as a measure of comprehension (e.g., "Can you find the wick, Sandy?") and having the child label the referent as a measure of production (e.g., "What's this, Sandy?"). No feedback was given during these probes. In the word-referent contiguity study, as an example of the series of studies, children were exposed in counter-balanced sessions to a present condition in which five objects and the names for the ob-

jects were presented simultaneously by the adult (e.g., "Sandy, see the wick"), a past condition in which the names for five other novel objects were uttered 10 s after the object was presented (e.g., "Sandy, that was the muff"), and a future condition in which the names for the five other novel objects were uttered 10 s before the object was presented (e.g., "Sandy, the next thing is the puck"). In all conditions of the study, familiar referents (e.g., dog, ball, fish) were also presented and labeled. Figure 1 displays results from that study on the measure of comprehension in terms of the percentage of the originally novel objects in each condition that were correctly identified on four successive learning probes. Note that receptive naming increased in each condition over time, with performance in the present (high temporal contiguity) condition being almost perfect by the final session. Thus, children can learn the names for things very well without response contingencies, contrary to the model in

Horne and Lowe's Figure 5. Response contingencies are necessary for children to produce names (Whitehurst & Valdez-Menchaca, 1988) but not for them to understand names.

The study reported above illustrates the more general point that Horne and Lowe's model is wrong because it is an operant model applied to phenomena that are substantially driven in their early stages by associative processes. Variables that I and my colleagues and former students have found to be important in the early acquisition of naming include temporal contiguity of an adult's naming of an object and the child's view of that object (as above), the informativeness of the relation between a name as used by an adult in the presence of a child and the occurrence of the thing to which the name refers, the value to the child of the referent of the thing named by an adult, the relation between the child's direction of attention and naming by an adult, the typicality of the thing that is labeled by an adult, the level of abstraction of the name that is provided by the adult, the degree of intraverbal context that is provided for a new name, and the frequency that an object is labeled by an adult. A theoretical home for such variables can be found in Rescorla's (1988) model of Pavlovian learning, Bandura's (1977) model of observational learning, and connectionist models of cognition (e.g., MacWhinney, 1989; Rumelhart & McClelland, 1986). This does not mean that operant contingencies are irrelevant to naming. They appear to be important to the development of the frequency and form of the child's expressive repertoire (e.g., Bohannon, MacWhinney, & Snow, 1990; Whitehurst & Valdez-Menchaca, 1988; but see Morgan, Bonamo, & Travis, 1995). They are, however, much more subtle than the "clever girl" variety espoused by Horne and Lowe. They are also secondary in the sense that children can learn to behave with respect to spoken names in ways that the verbal community would view as representing full understanding without the children experiencing contingencies for their own speech.

I have focused on my own work in critiquing Horne and Lowe's model, but the general point that they have failed to attend to the developmental research literature can be made by referencing other research programs. Consider, for example, the literature

on fast mapping (e.g., Carey & Bartlett, 1978; Dollaghan, 1985), which describes and attempts to account for the typical vocabulary spurt during the 2nd year of life, during which children may be learning many new words each day, dozens a week, and hundreds to thousands over a year, most without explicit instruction or feedback. Or consider the extensive developmental literature concerning constraints on word learning (e.g., Golinkoff, Mervis, & Hirsh-Pasek, 1994; Markman, 1989), which addresses the general issue of how a child, hearing the sentence, "See the dog," rules out the possibility that the referent for "dog" is the color or size or weight or attractiveness or so on of the particular dog that has been labeled. Or consider the large literature on concept structure as it affects language acquisition (e.g., Davidson & Gelman, 1990; Gopnik & Meltzoff, 1993), which addresses the issue of how a child's understanding of a class of referents affects learning of names for that class. Or consider the extensive literature on language development in exceptional children or circumstances (e.g., Bishop & Mogford, 1988; Whitehurst, *in press*), which helps to locate language development in a phylogenetic context. Or consider the large, often behavioral literature on language interventions (Kaiser, Yoder, & Keetz, 1992; Whitehurst et al., 1994). Or consider the extensive research on negative evidence (e.g., Bohannon et al., 1990), which specifically addresses the role of operant contingencies in the development of expressive language forms. These and many other research programs are directly relevant to any worthy account of the origins of naming.

Horne and Lowe might respond that much of the research and theory I have cited are not behavioral and thus fall outside the bounds of their theoretical approach. To the extent that behavioral research is defined as research on operant behavior, that is partially true. However, this brings us back to Aunt Sarah, who would probably be as perplexed by this type of scientific myopia as she would by a carpenter who refused to use any tool other than a hammer to build a house. Developmental behavioral research, in my view, is most productively defined as the study of changes in the potentially observable behavior of organisms across the lifespan, with an

explanatory and pragmatic focus on input variables. Operant contingencies are a category of input with considerable importance, but they hardly exhaust the possibilities. If behaviorally oriented psychologists want to develop accounts of phenomena that are as complex as the development of naming and be more than self-stimulatory, they will need to come to the job with a full box of tools. I have argued here and elsewhere that attention to stimulus-stimulus relations will be

fruitful, but there are many other foci that will also prove to be productive, as noted in the previous paragraph. I believe that the particular strengths of the behavior analysis tradition can generate unique and important contributions to the study of the development of naming and more complex forms of language, but that work will be marginalized if those who produce it do not extend themselves to make it part of the larger scientific enterprise devoted to the development of language.

*LEARNING NAMES MAY HELP TO MAKE
THE RIGHT CONNECTIONS*

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Horne and Lowe are to be congratulated on the thoughtful effort they have made to extend the approach of Skinner (1957), incorporating names as key features of verbal behavior and postulating a plausible behavior-analytic account of the origin, development and functions of naming in childhood. Our comments, however, mainly concern the second half of their paper in which they propose that naming provides both a necessary and sufficient explanation of stimulus equivalence. It seems to us that the array of arguments used by Horne and Lowe is at best circumstantial, and fails to present a critical test of their position. Nor do they address the dynamic way in which equivalence classes are acquired.

Horne and Lowe's assertion that no non-human animal has been shown to satisfy the necessary criteria of equivalence rests on a

universal negative. Horne and Lowe mostly rely on reviews of the literature up to 1990 (Dugdale & Lowe, 1990; Hayes, 1989; Saunders, 1989). They make a spirited effort to account in other terms for the more recent results of Schusterman and Kastak (1993), and one looks forward to their response. Horne and Lowe still adduce as evidence the failure of Dugdale and Lowe (1990) to obtain symmetrical responding in 2 common chimpanzees (*Pan troglodytes*) previously trained in a "language-training" project (Savage-Rumbaugh, 1986). This seems to be inflexible in view of the updated review and reanalysis of the chimpanzee data (Cerutti & Rumbaugh, 1993) that describes, for example, how these 2 apes used "food" and "tool" lexigrams to categorize other lexigrams, a performance deemed to be commensurate with the possession of equivalence. Cerutti and Rumbaugh also review ongoing work on 2 bonobos (*Pan paniscus*) who show appropriate listener behavior (in Horne and Lowe's terminology) to many spoken words in English to which they have been exposed from a few

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