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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months in age (much earlier than the chimpanzees in Dugdale and Lowe's study, who began training at 1.5 and 2.5 years). The significance of this early exposure to a language-rich environment agrees well with the stress laid by Horne and Lowe on the importance of early learning in our own species. It would be good if they would provide a more detailed analysis of exactly what elements may still be missing, either in the environment, or in the subjects themselves, which might preclude such apes from attaining equivalence.

Even if a particular nonhuman animal did successfully pass the tests of stimulus equivalence, Horne and Lowe suggest it might be doing so in a different way from verbally competent humans. Perhaps naming then is sufficient to support stimulus equivalence, even if it can be shown that it is not necessary. Some subjects, after successful formation of equivalence classes, fail to report assigning common names to the stimuli in the same class, and show no awareness of using names during equivalence class formation. To Horne and Lowe this does not indicate that they were not doing so. Here now in their favor is the principle that absence of evidence is not evidence of absence, but surely the onus upon proponents of a theory is to do all in their power to falsify it.

Horne and Lowe cite Mandell and Sheen (1994) who found that phonologically structured pseudo-words were easier to combine into equivalence classes than were unstructured groups of letters that were not susceptible to pronunciation, or arrays of punctuation marks. This suggested a role for overt or covert saying of the "word," reminiscent of naming. The fact that some of the formally unpronounceable stimuli were nevertheless combined into equivalence classes Horne and Lowe explain in a similar way, by postulating some kind of operation, presumably more difficult, to render them pronounceable. Though it could be true, we suspect the apparent parsimony of such an explanation. Similar behavioral feats in the same organisms may well be attained in a multiplicity of ways. In our work we found more errors and longer response latencies in testing for equivalence using abstract stimuli (unfamiliar items from exotic scripts) compared with pictograms representing familiar objects (Bentall, Dickins, & Fox, 1993). This effect could

be explained similarly by postulating that the abstract stimuli were simply harder to name, not that some other process was involved. It is true that when we taught subjects *group* names for the abstract stimuli (the same name for each of the three stimuli in a potential equivalence class) prior to testing equivalence class formation was facilitated, but no such advantage was conferred upon subjects who were taught *individual* names for abstract stimuli.

Can the overt or covert use of names explain the ways in which equivalence relations can be established with different sorts of material? Consider, for example, the effects of interference by means of conflicting articulatory or spatial tasks during tests of emergent responses (Dickins, Bentall, & Prescott, 1994¹). More impairment was found in subjects using easily nameable pictograms than in those using abstract stimuli if they were required to vocalize an irrelevant word repetitively during testing. By contrast, disruption of the "visuospatial scratchpad" (Baddeley, 1986) by means of an interfering tracking task had a greater effect on tests with the abstract stimuli. Perhaps further research using these techniques would help to extend the account of naming given by Horne and Lowe, beyond the overt or covert speech and hearing, rather as they say they can do for sensorily impaired compared with normal children.

Another line of inquiry has led us to consider the dynamics of equivalence class formation. Typically, after training relations between visual stimuli using a matching-to-sample (MTS) technique, we find, during unreinforced testing for emergent relations, effects compatible with the nodal distance account of Fields and his colleagues (e.g., Fields, Adams, Verhave, & Newman, 1990; Fields & Verhave, 1987). Both higher error rates and longer latencies are found with transitivity tests (as a function of the number of nodes involved), compared with the infrequent errors and fast latencies in nonnodal tests of symmetry, which are indistinguishable from tests of the trained relations themselves.

¹ Dickins, D. W., Bentall, R. P., & Prescott, K. (1994, April). *Interference with two types of working memory during emergent responding in equivalence class formation*. Paper presented at the meeting of the Experimental Analysis of Behaviour Group, London.

On repeated testing, these nodal differences diminish. To use a chemical metaphor, it is as if the equivalence classes gradually crystallize into their permanent form.

Horne and Lowe cite our study (Dickins, Bentall, & Smith, 1993) in which, after MTS training, paired associations (PA) were taught orally between the individual names that the subjects themselves (not the experimenters) gave to the pictograms. In some conditions these associations were incompatible with the MTS-trained relations. When MTS testing for emergent relations was subsequently carried out, considerable and sustained disruption was found. MTS-based trained relations and symmetrical inversions of these were replaced by PA-based selections, and many of the choices on tests of transitivity could be accounted for in terms of hybrid PA- and MTS-based linkages. Were the MTS-compatible choices based upon associations—intraverbals—between the names of the stimuli that were formed during MTS training while the visual stimuli themselves were shown on the screen? And did these associations then conflict with the subsequent PA training, even though this was established with very few errors to a strict criterion? Or did these resistant MTS-based associations form by some other pathway, bypassing the names of the stimuli?

In a subsequent study (Smith, Dickins, & Bentall, *in press*) we have found that if some MTS-testing of emergent relations is conducted beforehand this disruptive effect of training incompatible PAs between the dis-

closed names of stimuli is not found. Here all the MTS-based choices are maintained. To mix our chemical metaphors, it is as if the role of names is a catalytic one, in which the reagents (the visual stimuli themselves) can indeed in some way be brought together by the names, and thereby form into a compound (an equivalence class) that is subsequently unaffected by the antics of these or other catalysts. This immunity did not, however, extend to the effects of interpolated incompatible PA-like training with the visual stimuli themselves. In the same study, elements from such training obtruded in subsequent MTS tests, whether earlier MTS tests had been given prior to incompatible visual training or not.

We must therefore conclude that although some findings are compatible with and supportive of a role for naming in the formation of equivalence classes as sketched out by Horne and Lowe, a very unparsimonious explanation would be required to account for all of the relevant results in the same way. There are at least suggestions here that equivalence relations may be established in alternative ways, or at least that a critical experiment to rule this out has yet to be done. In the meantime perhaps it is premature to transfer stimulus equivalence to the dustbin of history (where, after all, according to one body of contemporary psychologists, verbal behavior itself resides!). Research may still have some way to go in this area, as well as, undoubtedly, in the broader area of language acquisition and its precursors in humans and in other animals.