

HOW, THEN, SHALL WE CHARACTERIZE
THIS ELEPHANT?

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At various times when working with manuscripts for the Horne and Lowe essays and related commentaries, I have been reminded of the childhood fable of several blind men attempting to describe an elephant. As I recall, the man who had the elephant by the tail found it to be rope-like; the one who encountered its trunk described it as rather like a snake; another, hugging a leg, argued that it was more like a tree, and so on. There clearly are interesting and important goings on in the behavior patterns that prompt us to speak of language, equivalence, and the like, and that confront us with apparent qualitative differences between human and nonhuman behavior. The problem is to discern how many different types of phenomena or how many distinct principles are involved, and how best to characterize them.

Within the culture at large, these differences and distinctions are not even recognized as a problem. Verbal functioning as well as consciousness and rational thought are taken as basic to distinctly human functioning; ordinary language invokes them as directly accessible via self-observation, implying that they are straightforward. Unconscious human action is acknowledged, but it is commonly viewed as more mysterious, and its origins are often misdescribed as *instinctive* (originating in evolutionary history) rather than by the more appropriate term, *intuitive* (originating nonverbally in one's personal history). The behavior-analytic tradition, in contrast, has accepted nonconscious human action as basic and nonmysterious, but for the most part as being not dependent upon verbal involvement. Awareness and logical functioning are treated as products or aspects of an overlay of verbal behavior—as phenomena to be explained rather than as special bases

for explaining human action. Following Skinner's strategy, behavior analysts have attempted to account for verbal phenomena as much as possible in terms of the same principles that apply to more basic functioning. Integral to this has been a more general strategy of strongly emphasizing parsimony as well as generality of interpretations, striving to identify similarities that disparate phenomena have in common rather than focusing upon obvious differences; this, too, supports an emphasis on principles that are applicable to the behavior of both human and nonhuman species.

Against this background, the report by Sidman et al. (1982) of attempts to demonstrate symmetrical transfer between conditional discriminations based upon arbitrary stimuli, inspired great interest in the community of behavior analysts. Despite their expert and persistent employment of behavioral techniques of shaping, fading, and prompting of discriminative performances, those authors encountered what appeared to be intractable differences between humans and nonhumans regarding the transfer of conditional discriminations when stimuli were interchanged. Furthermore, it appeared that these differences might be getting at the basis of what it *is* to function symbolically, whereby one stimulus functions in place of another without a basis in physical similarity. This possibility of identifying distinct operative principles in the human-nonhuman differences was a very exciting one, because proposed principles could be evaluated by means of the match-to-sample procedure, which can readily be used with humans and nonhumans alike. Thus, one could attempt to address fundamental human-nonhuman differences without assuming the very human-nonhuman difference that needed to be accounted for, and without depending upon the distinctly human repertoires of instruction-following for the conduct of experiments. Indeed, the

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experiment by Sidman et al. was part of a series that had begun several years earlier, involving verbal and nonverbal humans as well as members of other species. A burgeoning literature over the ensuing decade came to be organized to a large extent around two conceptions, *equivalence* and *relational frames*, both of which were characterized as more fundamental than verbal behavior per se, but as being operative in verbal functioning in ways that give it special characteristics.

Also working within the behavior-analytic tradition, Lowe and his colleagues first encountered apparently intractable differences between humans and nonhumans in studying behavior generated by schedules of reinforcement. In contrast with researchers working mainly with the match-to-sample paradigm, they have taken verbal behavior per se as basic to differences between human and nonhuman behavior—indeed, as fundamental for understanding most aspects of human behavior. Thus, in their reply to Tonneau and Sokolowski's commentary they identify their primary agenda as being “to understand how language is learned and has its effects on other behavior” (Horne & Lowe, 1997, p. 288). They identify their approach as being in accord with Skinner's analysis of verbal and other human behavior, arguing that to fail to include a detailed account of verbal behavior and its role in human functioning would be to miss part of what distinguishes radical behaviorism and behavior analysis from other behaviorisms.

In the context of the present discussions, it appears to this reader, at least, that Horne and Lowe are working from a set of starting assumptions that differ from those invoked by those focusing mainly on relational frames or equivalence. Specifically, they take language and verbal behavior as the *basis for* the phenomena that have been interpreted in terms of equivalence or relational frames. In contrast, others propose that those phenomena are at the root of what it *is* to function symbolically, and of some key features of verbal behavior and language. The kinds of disagreements that arise from this contrast are illustrated by comparing what different people are likely to make of the list that Horne and Lowe offer as “problems with the data” (1997, p. 277). Horne and Lowe assert that equivalence as well as relational frame theory

do not account for various empirical findings. A rejoinder from one who takes equivalence as “a basic stimulus function, not derivable from more fundamental processes” (e.g., Sidman, 1997, p. 259) would probably characterize most of those empirical findings as identifying *characteristics* of that basic stimulus function. That is, the implications of these findings differ, depending upon whether the phenomena identified with the term *equivalence* are to be explained via other principles as Horne and Lowe propose, or whether, as Sidman suggests, equivalence is considered to be an irreducible basic relation, in which case the enumerated findings are understood as identifying characteristics of that relation.

Another result of differing assumptions concerns judgments of what counts as adequate evidence to support a viewpoint. Horne and Lowe require the most stringent standards for demonstrations of symmetry between sample and comparison stimuli in matching-to-sample experiments. Drawing upon Saunders and Green's (1992) analysis of the various alternative discriminations that can be operative in match-to-sample procedures, they assert that because successful performance on an identity test *can* be based upon cues other than those of symmetry relations, that “among all the many studies purporting to show stimulus equivalence there are none, in fact, in which all the three criteria for equivalence enjoined by Sidman are satisfied” (Horne & Lowe, 1997, p. 276). That is, given that symmetry is not a *necessary* characteristic of the observed discriminative relations, they assert that equivalence is an untenable interpretive concept and should be discarded. From a contrasting viewpoint, Saunders and Green (1992) were not so quick to discard the term, despite their critique of control procedures in experiments claimed to demonstrate it:

Clearly, certain training histories produce more than trained conditional relations; new performances indicate that sample stimuli set the occasion for other stimuli to function as discriminative stimuli in configurations that were not explicitly trained. . . . A great deal of research is needed to analyze the full range of training and testing contingencies, in addition to match to sample, that produce this general outcome. It is our hope that such work will lead to a *new definition of stimulus equivalence*

that embraces this diversity. (p. 240, italics added)

In several laboratory studies, reciprocal transfer of match-to-sample discriminations has been reported as occurring in the apparent absence of correlated verbal behavior (see, e.g., commentaries by Saunders & Green, 1996; Stromer, 1996). This has led equivalence and relational frame theorists to provisionally conclude that those discriminations were not dependent upon verbally based categorizing, a conclusion that is consistent with more general cautions about assuming relationships between what human subjects do in experiments and what they say about what they do or have done (Shimoff, 1986). Conversely, in setting up the background context for their present essay, Horne and Lowe state,

Whether or not these [human experimental] subjects are explicitly provided with experimental instructions, they instruct themselves about their own behavior and its outcomes. For them, many features of that environment, including the experimental procedure itself, occasion naming and complex forms of self-instruction. . . . It is implausible that human subjects in [conditional discrimination tasks] would respond only in accordance with the experimenter-defined contingencies of reinforcement and not also to their own verbal behavior about these contingencies. (p. 272)

The reader is thus invited to assume that because naming in some form *might* have occurred in such studies, and if it occurred would be sufficient to produce the observed results, naming is probably occurring and thus should be appealed to in accounting for the observed effects, even when the evidence for it is tenuous. At one point they even suggest that proponents of an alternative viewpoint should simply concede this by explicitly supplying names as part of the training procedures. It seems to me that from the other viewpoint this would make the experiments pointless. Putting aside the issue of conflicting assumptions, however, Horne and Lowe make an important point in identifying the need for more experimentation on these topics with young infants or other nonverbal humans.

As noted above, Horne and Lowe hold firmly to the position that the term *equivalence* must be understood in the strict sense of its

origins in formal set theory, and if that strict definition does not apply to behavioral relations it should be discarded. To be even possibly workable, the term requires added conditionalities of context, which as they point out, are rather loosely specified, and which make behavioral equivalences not the same as formal equivalences. However, in adopting the term *naming* as the cornerstone of their account, they, too, have imported a term from other traditions while asserting a meaning that differs from the usage in its communities of origin. Whereas a satisfactory behavioral meaning of *equivalence* would have to mean something more than the precise interchangeability denoted by symmetry in formal relations, their preferred term, *naming*, is likely to be problematic in its own way, because its alternative domains of usage are characterized by *lack* of specificity. It took me many readings before I began to sense that I was reading (hearing?) the term in more or less the same way that they were writing (saying?) it. Each term participates in alternative sets of relations maintained by the verbal community that conflict with the proposed technical usage; thus, I anticipate that other readers will have the same problem.

Furthermore, the ordinary-language features that seem to make both terms useful may not quite capture what is functionally important about the phenomena of concern, and may obscure the applicability of the relevant principles to nonverbal behavior. Similarly, as Skinner emphasized, the vernacular term *language* may obscure the functional differences between speaking and listening, on the one hand, and the similarity of their characteristics as operant behavior generally, on the other. Moreover, just as speaking is not the same as listening, although both may involve similar stimulus patterns, touching a sample as the occasion for selecting a comparison stimulus is not the same behavior as touching the comparison stimulus when the two stimuli have been interchanged. The relation between the two repertoires might be better understood if they were termed *reciprocal* rather than *symmetrical*. Also, taking behavior as better defined by its functional than by its formal characteristics supports our looking for a parallel relation between what is called language learning and the match-to-sample procedure. Thus, although normal

language acquisition is probably the most common way in which reciprocally related repertoires are acquired, it may not be the only way. If so, the similarity of various kinds of reciprocally related repertoires might be recognized more easily if the term identifying the key process (or feature of process, of relation, or of property of relation) were something like *fusion* instead of *naming*. To consider the possibility of even greater generality, the fusion of two (or more?) repertoires may be related to other recently recognized combinatorial principles identified with the term *adduction* (Andronis, 1983), whereby two repertoires are combined by presenting their evoking stimuli together and then reinforcing the resulting behavior. We clearly have something interesting and important here: Do we have hold of this elephant by the tail? A leg? A tusk? What would be the implications of the difference?

As I understand it, the naming relation as characterized by Horne and Lowe is most fundamentally a speaking-listening fusion, irrespective of its particular functional characteristics. If so, both the core feature of the naming relation, as construed by Horne and Lowe, and the equivalence relation, as construed by Sidman, are relatively transparent. That is, although equivalence or speaking-listening fusion constitute clusters of interrelated stimuli, objects, or relations, they do not appreciably delineate the *functionality* of those clusters. It is only when a member of an equivalence class becomes part of a functional class, such as that of a mand or tact relation, that the other members or objects take on behavioral functions. Similarly, as an account of relatively unelaborated relations such as those by which a child comes to name objects that he or she has not otherwise interacted with, Horne and Lowe's account of naming invokes minimal functional characteristics, somewhat analogous to those attributed to behavioral equivalence. More elabo-

rated forms of naming, such as naming of objects that already have been in contingent relations with the child's behavior, or naming that states implicit rules (naming a substance "poison" states a rule regarding its ingestion), appear to share characteristics with Skinner's functional classes like mands and tacts, including statements of rules, and with the contingencies that are described as operative in the relational frames account. In both the naming formulation and the relational frames formulation, greater specificity is needed regarding such characteristics.

In addition to providing a promising as well as provocative extension of a behavioral account of verbal and related phenomena, Horne and Lowe's essay, along with the commentaries and their replies, have prompted a critical examination of a whole range of interrelated issues that in earlier decades were not directly confronted by behavior analysts. The sometimes distinctive characteristics of human behavior were like an elephant in the room with nobody talking about it. One thing is clear: We're talking about it now!

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