Modesty, Yes; Humility, No

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I agree with Neuringer's main premise, that we behavior analysts need to examine and improve the ways in which we present our viewpoint and our work. I disagree with his choice of "humility" as a term for organizing and characterizing the stance that we should adopt. To be sure, the term can imply "tentativeness of theoretical and methodological positions, willingness to consider alternative views, support for diversity, openness to criticism . . . a scientific stance that all knowledge is provisional and that one's most deeply held positions must continually be reconsidered." But in addition to these fine characteristics, "humility" is clustered with terms of abasement, suggesting not just the stance of respecting others, but of *deferential* respect to others. Humility is identified not only with patience, but also with timidity and submission; not only with lack of pride, but also with lack of self-esteem (Morris, 1969). In adopting a stance of humility, then, one may be understood as allowing continual reconsideration of one's own position while not obliging the same from the adherents of other viewpoints. I am concerned that these other, detrimental meanings would be functionally operative.

"Modesty" would be much better. Like "humility," this term stands in contrast with pretension and arrogance, but it is not linked with abasement. To be sure, "modesty" also has additional meanings, suggesting propriety of speech, dress, and behavior, and these may imply more social conformity than some of us would like. Still, it is unambiguous in allowing for confident valuing of one's accomplishments while respecting the contributions of others and refraining from extravagant claims for one's own importance or merit.

The topics that Neuringer discusses in support of his case are areas in which

our stances of presentation are indeed important. The stance appropriate to each may differ, however, depending upon whether a primary concern is that of gaining access to a designated audience, of effectively addressing an audience after having gained that access, or of interacting with non-behavioral colleagues on a day-to-day basis. The appropriate stance also may differ depending upon whether one's audience is uninformed, misinformed, or counter-informed regarding what we are about. It follows that we need a more analytic approach to the problems of gaining access to various audiences and of addressing those audiences effectively.

The Repertoires of Gaining an Audience

One way to get peoples' attention is to say their beliefs are mistaken, their theories are wrong, or their actions are foolish. We also can claim attention more directly by simply declaring the importance of what we have to say. (Technically, the first approach is autoclitic in character, constituting a concealed mand for attention; the second is overt mand.) While these approaches can be counted on to produce an attentive audience, they invite skeptical and hostile reactions, prompting the listeners or readers to criticize what is proposed and to defend their own preconceptions. These approaches also may result in the claimant being characterized as arrogant or pretentious, thus justifying a bias to dismiss out-of-hand what the claimant has to say. Neuringer is recommending that we eschew those repertoires—suggesting, in effect, that their advantages in efficiently gaining an audience are overridden by their costs of biasing the audience toward rejection. The stance of humility can be understood as an explicit strategy to allay aggressive/hostile reactions; I am concerned that it would result merely in

a shift from hostile to benign dismissal of what we have to say or even of our practical contributions.

One way to still invoke the "mistaken/wrong" gambit that gains attention but without evoking hostility, is to identify the speaker or writer with the audience: "We may be doing the wrong thing here." Interestingly, that is the stance of Neuringer's present essay, and I find it effective—after all, he and I are both behavior analysts. Notably, it is effective even though I recognize that Allen himself does not act in arrogant or pretentious ways. Can this approach also be effective for a behavior analyst addressing *non-behavioral* audiences? I think it can, if one identifies a given problem as one we share as ordinary citizens, as psychologists, or as other scholars or professionals. The proposed behavioral solution, then, should be advanced modestly—as in: "This approach has been effective in some situations that may be similar to the one that confronts us here" rather than implying: "This is the best possible solution, and you are foolish not to have recognized it." That is, one can be *modestly* assertive. Furthermore, one can support this with the personally modest practice of drawing attention to the work of others whenever it is appropriate, and of emphasizing the contributions of co-workers when citing one's own work. It is especially important as we are thrust into the post-Skinner period, to raise the visibility of our modest heroes, portraying them not as meek, submissive, timid individuals but rather as confident and effective people who claim no more than they can deliver.

The Specific Domains that Neuringer Cites

A recurring suggestion in Neuringer's discussion of the category, "Subject-matter of contemporary behavioral research," is that we systematically adopt a subtle shift of designation, replacing "behavior" with "behaviors" in our prose, as a strategy for reducing our vulnerability to criticism and misunderstanding. The hope is that this change

would aid in highlighting our emphasis on classes and sub-classes, in insulating us from appearing to include too much or too little within our purview, and in emphasizing the role of concurrent behavior systems in our account, thus aiding recognition of how we deal with novelty, complexity, and the verbal/nonverbal relationships. These are indeed some key areas where we need to be better and more widely understood; I will be amazed, however, if the behavior/behaviors distinction turns out to be as pervasively helpful as Neuringer suggests. In addition, I remain ambivalent about discussing "behavior" in noun form when it is intrinsically verb-like in character (Hineline, 1980); the proposed change from singular to plural appears to be an acceptance of the noun form.

As Neuringer correctly anticipates in his concluding section, many of his proposals can be criticized as blurring the distinction between the behavior-analytic and other approaches. I think our strategy should be one of clearly but constructively *delineating* the relationships between our approach and other approaches—identifying and building upon features that we hold in common with them, but not overlooking the differences. Thus, his discussion of the overt-vs-covert distinction seems to obscure a fundamental problem that concerns implicit assumptions regarding causal relations. Recognition of the assumptions *as such* can enable us to agree to disagree with our colleagues, while understanding what we are disagreeing about. For example, one might say:

Most psychologies assume that causes must be contiguous with their effects, and that if an immediate external cause is not evident, an immediate internal one must be inferred. Behavior analysis accepts causal relations as occurring on multiple scales of magnitude including those that entail gaps in time, and it shares this feature with other modern sciences.

Or to address a related issue, we might say:

The arguments between cognitivist and behavioral accounts often have been couched in terms of mentalism and anti-mentalism. The difference between the two viewpoints can be understood better as one between mediational and non-mediational theory (Hineline & Wanchisen, 1989). Thus, Gibson's non-representational account of visual perception is analogous to Skinner's non-associationist account of learning (Costall, 1984), and Watkins's (1990) recent critique of "mediationism" in the cognitivist study of memory closely resembles Skinner's (1950) critique of the theories of an earlier day.

Or yet a third example:

It is axiomatic to our approach that behavior can be understood in relation to present and past environmental events. That is simply the kind of theory it is. In contrast, some other psychological theories place the causes of behavior within the organism. Attribution theorists, in social psychology, have noted and studied this kind of interpretive difference as it occurs in ordinary non-scientific discourse; perhaps the relationships of attribution theory can be brought to bear upon the characteristics of and differences between serious scientific theories as well.

Moves such as this last one can enable detailed discussion of our differences within a neutral context that may be familiar to non-behavioral readers. Doing so need not "give away the store," for in the process one can demonstrate potential contributions of our approach to the understanding of that context—as in showing how to direct the relationships revealed by attribution theory upon that theory itself (Hineline, 1990). The above-noted, and other *JEAB* book reviews of recent years, have illustrated various domains in which our differences as well as our affinities to other viewpoints can be discussed constructively.

Similarly, our approach to methodology (Neuringer's second major category of discussion) can be a basis for delineat-

ing the relationships between our approach and other approaches. We need not abandon our approach in respecting and even sometimes adopting techniques from other traditions. We should note, for example, that our basic strategy of systematic replication has much in common with the strategy of factorial designs within the tradition of statistical inference. Similarly, I think that we need not and should not abandon the attempt to keep our scientific language conceptually coherent (and thus "pure"). However, we can assert a strategy of tightly coherent theorizing without making extravagant claims for its superiority. Neuringer favors an emphasis on translation between languages in an accepting way that de-emphasizes differences. I would applaud attempts at identifying mutual relevance via the phrases "When we speak of . . ." and "When you speak of . . ." but I think that the differences need to be clearly understood, rather than obscured by attempts at translation and thus substitution of terms and concepts.

A number of recent developments in related fields are vindicating positions that we have held for years. I just noted Watkins's (1990) attack on mediationism, which fits hand-in-glove with Skinner's "Are theories of learning necessary?" (Skinner, 1950). Andresen (1990) has described a number of developments in linguistics and related fields that seem congenial with our approach to verbal behavior. There are indications within social psychology that some of its prominent authors (e.g., Gergen, 1987) are becoming sensitized to issues that Skinner addressed years ago (Skinner, 1945) when parting company with "methodological behaviorism," although the similarity is seldom recognized. There *is* increasing recognition of a common interest in contextualism as a theme that could unify psychological meta-theory (e.g., Hayes, 1986; Morris, 1988; Rosnow & Georgoudi, 1986). Similarly, a renewed emphasis on selection as a causal mode by philosophical biologists (Smith, 1986), represents another opportunity for our participation within a broader domain of scholarship. Our problem is one of gain-

ing place within these developing, inter-related domains—of making it clear that we are not simply trying to jump onto whatever bandwagons are currently moving, and yet not introducing our interests with the claim that “We told you so!”—even when we did so.

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