

## EDITORIAL: TECHNICAL LANGUAGE IN CULTURAL ANALYSIS

All sciences require common language to advance. One reason behavior analysis has been so successful scientifically (though its success culturally may be questioned) is because of the precision of behavior analytic language. One of our core journals, *The Behavior Analyst*, irregularly features a section "On Terms," and lack of precision or confusion in discussions of behavior analysis are assertively addressed—sometimes to the point of pedantry. As a result, behavior analysts share common understandings and definitions of such terms as reinforcement, discriminative stimulus, and tacting. One of the major advantages of behavior analytic conferences is the opportunity for the field collectively to construct, refine, and clarify the terminology by which our science is communicated. As that language evolves (consider, for example, the importance in recent years of terminology related to motivative/establishing operations and equivalence relations), behavior analysts attend carefully to the changes.

As the field moves toward developing a rigorous science of cultural analysis (see *Behavior and Social Issues*, Volume 15, No. 1), similar precision is clearly required. Certainly in its earliest days, it appeared that common language was emerging. B. F. Skinner defined culture as "the contingencies of social reinforcement maintained by a group" (1984/1987), and suggested that what was selected in cultural selection was "practices—better ways of hunting, gathering, growing, making tools, and so on" (1988, p. 36). While not quite providing a definition, Glenn (1991) attempted to further specify what cultural practices were, indicating that they "involve repetition of analogous operant behavior across individuals of a single generation and across generations of individuals" (p. 60). Things were not quite this simple, however; on the same page noted above and elsewhere, Skinner discussed cultures themselves as being selected. At the least, this suggested four, rather than three levels of selection (natural selection, operant selection, selection of cultural practices, and selection of cultures), which may prove to be the best way to approach the issue. Much of the early writing on cultural analysis also drew on Marvin Harris' cultural materialism in anthropology (1979); although some of Harris's constructs and terms were difficult (the *permaclonic system* comes to mind), they were clearly defined and relatively precise.

So far, so good, in moving toward a science. Glenn and others, drawing as I see it on Skinner's work on verbal behavior, clarified the importance of interlocking behavioral contingencies (IBCs) within cultures. In IBCs, the behavior of one class of cultural actors was contingently related to the behavior of other classes of actors—and cultures typically involve many such contingent relationships (Mattaini, 1996). Up to this point, there is little if any disagreement in our literature.

Every behavior analyst knows what a behavioral contingency is; in the words of Sulzer-Azaroff & Meyer, "Contingencies are *relations* between responses and the events that follow them—their consequences—and the events that precede or accompany

them—their antecedents” (1991, p. 98, emphasis in original). The simplest behavioral contingency, the 2-term contingency, is the relation between a behavior and its consequence. It may seem apparent that there cannot be a 1-term contingency since a contingency is a relation (which implies an X and a Y that are related) ... but wait.

The next level of complexity that is commonly discussed in cultural analytic literature is the *macrocontingency*—and here the trouble begins. Ulman first defined the macrocontingency in 1978 as “a set of differing actions (topographies) of different individuals under common postcedent control” (Ulman, 1998, p. 209). (Loosely, “postcedent” is a term used by behaviorologists to communicate what other behavior analysts communicate using the term “consequences.”) He later refined the definition of the macrocontingency to refer to “conjoint actions of two or more individuals under common contingency control” (Ulman, 2006, p. 96). Ulman uses the example of two persons playing a video game, in which playing the game involves interlocking behaviors and produces reinforcement for both players. Glenn (2004), by contrast, described the macrocontingency as “the relation between a cultural practice and the aggregate sum of consequences of the macrobehavior constituting the practice” (p. 143).

There are many interesting and problematic differences between the two usages; see Ulman, 2006, for discussion. Two critical points are relevant here; first, as noted by the authors themselves, the two definitions are significantly at variance (tracking the meanings of the constituent terms in both cases is important to elaborate those differences). The second point, however, is even more important. Both variations agree that there is a *relation* involved—but it is not clear that this relation is always a contingent one in some versions of Glenn’s usage. Malott and Glenn (2006), for example, indicate that “If the behavior constituting a cultural practice has a product that can affect other people, then the aggregate product of the behavior can become a social problem ... the relation between the operant lineages of all people engaged in the cultural practice and the aggregate product is a macrocontingency” (p. 37). The discussion does not appear to suggest that the aggregate product—the social problem—is selecting the operant lineages; the only “relation” appears to be that the cultural practice produces the outcome, but there is no feedback loop, the outcome is just a result, without a reciprocal impact on the practice. That happens all the time—but there is no contingency involved when it does, and no selection by consequences.

Although the example cited above is probably simply not fully developed, there is potential mischief aplenty afoot here. Elsewhere in this issue the cacophony produced by an orchestra of individuals each playing without coordination is described as a macrocontingency, and the authors specifically referred to the section from Malott and Glenn in the previous paragraph when asked by a reviewer if they really meant that. But clearly nothing is being selected by the cacophony—there is no contingent relation involved, nothing is being selected, and the use of the term macrocontingency can only be misleading under such circumstances. There are other similar examples; one recent submission to this journal referred to “wandering children” as a macrocontingency, apparently attempting to expand on the social problem usage noted above; the authors again cited this section of Malott and Glenn. All of which raises a crucial point:

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*At a minimum, any phenomenon in behavior analysis that is labeled a contingency of any kind simply must involve a contingent relation.*

There is also confusion around the term *metacontingency* (originally suggested by Glenn; for the most recent statement of Glenn's thinking, see Malott & Glenn, 2006). Because these issues have been discussed in depth elsewhere (e.g., Hayes & Houmanfar, 2004, Houmanfar & Rodrigues, 2006; Mattaini, 2006), I will just note some current concerns, while acknowledging that there is substantial controversy about these concerns:

- The selection process involved in the standard definition of the metacontingency as commonly defined is not clear, but does not appear to be analogous to the behavioral contingency (Houmanfar & Rodrigues), or to Skinner's selection by consequences
- Contingencies cannot be both the causes of selection and the outcomes of selection, although they are discussed in this way in common descriptions of metacontingencies (Hayes & Houmanfar)
- There are serious questions as to whether the metacontingency as commonly defined involves any classes of potentially manipulable variables—suggesting a possible scientific dead end (Hayes and Houmanfar; Mattaini)

These issues can be addressed (see, for example, alternatives discussed in Houmanfar & Rodrigues, 2006), and doing so will certainly advance the field. One small step would be for every scholar involved in cultural analysis to simply make their own definitions clear in their work, and we will at a minimum ask that authors do so henceforth in *Behavior and Social Issues*. That, however, is a far cry from having a common, accepted language for the field. Imagine if every behavioral author defined reinforcement for him or herself, and if in some cases the definition involved contingent relations while in others it referred to a class of persons ("wandering children"). This clearly would not do. At a minimum, it is essential that all discussions of "x-contingencies"—of whatever kind—involve clearly contingent relations, and that discussions of selection clearly state what is selected, and what the demonstrated or hypothesized selecting consequences might be.

Mark A. Mattaini, Editor  
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