And Yet ... : Further Comments on Distinguishing Positive and Negative Reinforcement

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Baron and Galizio (2005) usefully review and update Michael's (1975) observations concerning the problems surrounding the distinction between positive and negative reinforcement (hereafter, the $\pm$ distinction). In the end, they, like Michael before them, conclude that the valence is unjustified. And yet, despite the fact that the logical and empirical underpinnings of the distinction have been questioned for more than 30 years, it has persisted. Here we comment on some of the underpinnings of the distinction, some further practical reasons for its persistence, and how the inconsistencies between what is known about the distinction and how it is described in both professional and lay settings might be addressed.

Logical Inversion, Empirical Quagmire

If we assume, for the sake of the present argument, that the operations of "presentation" and "removal" can be distinguished formally or structurally, then there is a basis for exploring functional or behavioral differences between the two operations. Because behavior analysis is a data-based discipline, it follows that the distinction should be put to experimental test. Considering the problem of the $\pm$ distinction as an empirical problem leads to two further ques-

tions: How is the problem framed in terms of an experimental question? And what evidence is there of functional differences between positive and negative reinforcement?

In the experimental analysis of behavior, as in other sciences, it is assumed that a variable has no effect until shown otherwise. Thus, a difference between baseline and manipulation phases is not considered to exist until it has been shown in a properly controlled experiment. This is nothing more than the application of the time-tested logic behind the null hypothesis. By this reasoning, the assumption should be that positive and negative reinforcement are functionally the same until shown empirically to be otherwise. And yet, the logic implicit in maintaining the $\pm$ distinction historically has been that of rejecting a priori the null hypothesis, awaiting experimental evidence that it should be accepted. An odd inversion indeed.

The closest approximation to empirical evaluations of the $\pm$ distinction are those similar to ones conducted by Dinsmoor (1962) and Azrin, Holz, Hake, and Ayllon (1963). These authors are among a small number to investigate schedules of escape from electric shock arranged in ways roughly comparable to those studied using food as a reinforcer. But, even here, one cannot be certain that the two procedures are indeed comparable. Perone, Galizio, and Baron (1988),

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for example, have outlined a number of methodological problems in comparing human behavior to that of other animals, many of which could be extrapolated to the question of making direct comparisons between nominally positive and negative reinforcement. Nonetheless, using squirrel monkeys as subjects, Azrin et al. concluded that "fixed-ratio escape reinforcement appears to be comparable to fixed-ratio food reinforcement in the manner in which responses are maintained, as well as the manner in which the response is acquired" (p. 456). Earlier, Dinsmoor was slightly more circumspect, noting sometimes mixed maintenance of the operant bar pressing of rats maintained on variable-interval (VI) escape schedules. He suggested that such responding might be

contaminated by responding that is maintained ... by differences in the probability of shock following different classes of behavior. This contamination is relatively small when long intervals intervene between successive shocks in the series. ... More severe shocks or a more sensitive organism may be needed to ensure satisfactory performance under these conditions. (p. 47)

Dinsmoor's results may or may not be qualitatively different from those in analogous VI schedules of food reinforcement. The difficulties noted by Dinsmoor may indicate a difference in behavioral process between shock- and food-based responding, or they may indicate that the use of electric shock is simply not as well understood as is the use of food. Thus, Dinsmoor's results do not necessarily mean that the two nominal types of reinforcement are different; only that little remains known about escape from shock relative to food-maintained responding. The limited types of schedules and schedule parameters, however, do not allow clear conclusions either in favor of or opposed to the assertion that positive and negative reinforcement

differ from one another. We note that the comparisons noted herein were between food- and shock-related responding. It could be that such behavioral, or even physiological (e.g., Donahoe & Palmer, 1994), evidence of differences between positive and negative reinforcement under these circumstances may have more to do with the behavioral differences between shock and food exposure than with those between presentation and removal per se. And yet, the difference persists.

Given the difficulties of direct comparisons of food- and shock-maintained responding, perhaps a more indirect approach might be useful in comparing their relative effects. Consider, for example, an experiment reported by Hearst (1962) in which rhesus monkeys trained on multiple shock-avoidance food-reinforcement schedules subsequently were tested for stimulus generalization around the stimuli correlated with the two "types" of reinforcement. Hearst did find differences in generalization gradients (avoidance-based stimulus generalization gradients usually were more flat), but the methodological point is that behavior maintained by different "types" of reinforcement was examined under a common condition to both—changes in the dimensional stimulus correlated with reinforcement. Nevin's (e.g., 1974) analysis of response strength follows a similar logic in that the relative resistance to change of two different training conditions is tested as a function of a common disrupter. Such an approach also may have merit with respect to examining whether there are empirical differences on responding maintained by the nominally different conditions of presentation and removal of stimuli.

The Persistence of the $\pm$ Distinction

Cultural contingencies and the persistence of verbal behavior. If the formal differences between positive

and negative reinforcement are not supportable and the functional differences are at least questionable, it remains to be answered why the distinction persists. Numerous examples can be noted, in addition to the $\pm$ distinction, of behavioral processes and descriptions that have persisted even though they also have flown in the face of logical or empirical analysis. Hearst (1975) presented strong arguments opposing the operant–respondent conditioning distinction that had been a part of the psychology of learning for the previous 40 years. Dinsmoor (1954) deconstructed the negative law of effect by suggesting that punishment was, in fact, not a separate behavioral process but rather the suppression observed when punishment as instituted was the result of the negative reinforcement of some other, unmeasured behavior. In what has become a terminological tug-of-war with applied behavior analysts, both Zeiler (1972) and Lattal and Poling (1981) argued against describing response-independent events as “noncontingent reinforcement” (cf. Poling & Normand, 1999; Vollmer, 1999). And yet, all of these distinctions persist.

In each of these cases, as well as with the $\pm$ distinction, the persistence can be attributed in no small part to historical-cultural contingencies, those to be found in the cumulative and tonal verbal community. Established verbal practices are hard to change, as Vollmer (1999) has noted. Moreover, the notion of categorization of events as positive and negative, carrying along with the categorization the complementary notion of additive and subtractive processes, is engrained in western culture from Job 1:21 in the Old Testament to the Spencer-Bain principle to Thorndike’s law of effect to Skinner’s operant conditioning.

Cultural and historical practices in turn persist because they, in some sense at least, work within the culture

(cf. Harris, 1977). If they did not, there likely would be different practices, be they cultural institutions or, in the present case, terms and distinctions. In describing the selection of traits, Dawkins (1986) discussed the “good enough” criterion whereby a trait or structure may not be theoretically “ideal” but it persists because it is sufficient for the purpose at hand (e.g., a bird’s wing is not aerodynamically perfect, but it allows flight). Certain process and procedural distinctions may not be perfect, but they allow progress. So we suggest it has been with the $\pm$ distinction. It may not be perfect. It may in some senses be wrong. But, in other important cultural ways, it works in both the scientific and broader cultures in which behavior analysis is discussed.

Practical considerations. Baron and Galizio (2005) suggested that “the distinction is serving a useful communicative function” and “is so well embedded within discussions of operant behavior that one cannot navigate the literature without being familiar with it” (p. 96). We concur. The persistence of the terms suggests that the communication they allow in most circumstances weighs more heavily than the obscurity they interject. But, there are other contingencies within the behavior-analytic culture that may contribute to the persistence of the practice of distinguishing positive and negative reinforcement.

Increasingly, legal considerations in treatment contribute to the $\pm$ distinction in applied settings. Legal mandates (e.g., least restrictive treatment) have incorporated cultural values about what is *best* or *right* more broadly than what might be argued from a pure science perspective and have institutionalized and thereby perpetuated the distinction in the applied realm as few other things could have. Beyond legal institutions, working in applied settings brings the behavior analyst and behavior-anal-

lytic language into contact with broader cultural institutions: schools, workplaces, clinics, hospitals, prisons, and any other organizational setting in which behavior invites systematic management. The $\pm$ distinction persists in such settings because it is easily understood in the broader cultural context described in the previous section, even though the distinction may be conceptually ambiguous.

Persuading agencies and institutions to consider behavioral treatment programs seems more likely when positive features of the program are emphasized, including describing the procedures as involving "positive" as opposed to "negative" reinforcement. In lay terms, this might be described as expressing the "tone" of the program or the "intent" of the implementer or manager, and in more behavioral language it describes the context or framework in which the reinforcement procedure will be implemented. Behavioral programs based on what is described as "positive reinforcement" may be no different or effective on the targeted behavior than those based on "negative reinforcement," but the former are more likely to be accepted, implemented, and followed through (e.g., Daniels, 2000; Iwata & Bailey, 1974). Other useful, more general behavioral differences may accrue from procedures framed positively, such as overall productivity, verbal reports of satisfaction, and retention. It is important to keep in mind that the focus of this discussion is on the terms themselves and not on the actual behavioral process. The positive results just described accrue in large part, we suggest, by the use of "positive" terms as opposed to "negative" ones in behavior management settings. This practical outcome of the language contributes significantly to the persistence of the terms *positive* and *negative* reinforcement in applied settings.

More generally, and practically speaking, the use of the language of

positive reinforcement widens the acceptance of behavior analysis in a broader culture that *does* distinguish, and places moral value on, circumstances construed as positive as opposed to those that are negative. Indeed, *coercion* and *manipulation* are words still strongly associated with behavior analysis. *Positive* reinforcement may give practitioners a verbal hook for scaling the bias against behavior analysis to the goal of changing behavior through reinforcement.

Addressing the Seeming Inconsistency Between What is Known and What is Said

Addressing the $\pm$ distinction is akin to the broader question of using lay language to describe scientific concepts derived from behavior analysis. Hineline (1980), for example, made a carefully crafted argument for the precise use of language vis à vis descriptions of behavioral processes. Bailey (1991) offered a more practical approach in advocating that language must communicate the concepts in a useful way, regardless of the precision of the language used to describe them. The $\pm$ distinction, although imprecise, is likely to persist as long as it is useful, as noted in the previous section, following the general scheme described by Bailey.

It is critical that students and professional practitioners of behavior analysis understand the logical and empirical difficulties inherent in distinguishing positive and negative reinforcement, just as they must understand other technical intricacies of the discipline. At the level of the science, not to understand technical issues and the proper descriptions of technical issues and procedures is not to understand behavior analysis. In communicating with others, be they beginning students, clients, or parents of clients, however, the goal of communicating the concept effectively is primary. Such communication,

we suggest, can occur at a simplified level only if the theoretical subtleties of the concept can be articulated by the communicator, teacher, or therapist under appropriate circumstances. Communication in practical settings often means putting precision on the back burner in favor of more user-friendly descriptions than those employed with colleagues. As long as the behavior analyst understands the concept in a technical and scientific sense, it seems both acceptable and even necessary to distill the language used to describe the concept at a communicable level. If the behavior-analytic speaker does not understand the concept a priori, then the distilled version is not a translation but rather may be a bastardization and misrepresentation of the concept.

Of course, the danger of distilled behavioral concepts is always present. It was described by Kennedy (1992) with respect to anthropomorphic behavioral descriptions: A description that starts off as useful shorthand for the real thing may be reified by either speaker or listener. In the case of the $\pm$ distinction, its use is likely to continue for the reasons already noted herein and by others, but such use must be done with an understanding of the conceptual baggage that the distinction carries. It is the case in other disciplines as well that, at the cusp, concepts are not always as simple as they are portrayed further away from the cusp and in general communication. After all, in physics, prediction and control sometimes are better served when light is considered as a wave and at other times when it is considered a particle.

Even if the $\pm$ distinction could be negated empirically, it likely would continue on practical grounds alone for the reasons discussed herein. By the same token, the celestial mechanics of the sun and planets in our solar system have been known for hundreds of years, and yet, knowing to the contrary, behavior analysts still

describe the sun as rising and setting each day.

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