Through the Looking Glass: Symmetry in Behavioral Principles?

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Baron and Galizio (2005) have reprised and updated Michael's regrettably neglected (1975) paper on the lack of functional or interpretative distinctions between positive and negative reinforcement. They conclude that 30 years of subsequent intensive research into contingencies of consequences and related areas have not modified Michael's original thesis: Positive and negative reinforcement are equivalent. As the following commentary will attest, I am of two minds on this effort. On the one hand, I applaud a move toward coherence in a fundamental concept; on the other, the means by which this is achieved is not always clear and in some ways abandons a functional account of behavior.

The Search for Symmetry

The issue raised by Baron and Galizio (2005) is reflected in a much wider context. Behavior analysis may share with other sciences in having its own basic principles of symmetry. As Randall (2005) remarked in her recent popular book on contemporary physics: "When a physical system has symmetry, you can describe the system on the basis of fewer observations than if the system has no symmetry" (p. 193). But this is not merely a matter of convenience. The very structure of physics is founded on symmetries, from mechanics to electromagnetics to relativity to particle physics (see, e.g., Park, 1988). Such symmetries lie at the heart of the conservation laws of momentum, energy, and so on. But

sciences as seemingly far apart as embryology, crystallography, botany, and organic chemistry are also dependent on principles of symmetry. What, then, is meant by symmetry? Basically, a system shows symmetry when there exists at least one transformation that leaves the system unchanged. This condition most obviously applies to certain figures and shapes, but, more important for this discussion, to principles as well. The laws of physics, for example, do not depend on position, time, or, more generally, states of motion; we say they are invariant under these transformations.

How might symmetry principles be reflected in behavior analysis? There are seven that come to mind as possibilities and readers may think of others. First, if, as Michael (1975) and Baron and Galizio (2005) assert, there are apparently no functional distinctions to be made between positive and negative reinforcement, then reinforcer effectiveness (by various measures) is invariant under a simple inversion of procedure, that is, arranging, say, onset as opposed to offset of pertinent events. Second, the degree to which this applies to reinforcement, we should extend the principle to include positive and negative punishment, although I suspect the pertinent research here is sparse. Third, there is some evidence that reinforcement and punishment are, in some sense, mirror images of each other, that is, they are antisymmetric, just another form of symmetry (e.g., de Villiers, 1980; Farley, 1980; Farley & Fantino, 1978). Fourth, discrimination and generalization are in a virtually perfect symmetrical relation, being two sides

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of the same coin we call “stimulus control.” Whether we refer to one or the other depends on selected procedures as well as the degree to which we wish to emphasize differences or similarities in behavior as we vary a stimulus. Fifth, in a three-term contingency the antecedent stimulus, operant class, and reinforcing consequence possess a kind of circular symmetry in which none of the terms can stand alone, but have meaning only through their interaction; each, in some sense, controls the other. Sixth, in shaping behavior, we recognize the symmetry between the behaviors of the shaper and the shapee. As Skinner pointed out long ago, the behavior of each controls the other in a kind of acquisitional dance (e.g., Skinner, 1972, pp. 122–123). Seventh, just as the laws of physics do not depend on time or place, the laws of behavior appear to operate over an enormous range of species, a biological feature shared perhaps only with certain biochemical pathways. As a corollary to this invariance, behavior analysts often assume that, for example, principles of reinforcement apply not only to the behavior of individuals but to groups of individuals as well—a kind of scale invariance.

Why might these possible symmetries be important? Not only could they have the advantage of our needing fewer observations, as Randall (2005) suggested, but they would confer an internal and consistent unity to the field as well as affirm the enormous range of its applications. But how are we to show these symmetries convincingly? Have Baron and Galizio (2005) succeeded? Basically, yes, but I believe there are significant conceptual challenges, not to say empirical ones.

Tweedledee and Tweedledum

At the outset, we may ask how we make distinctions of any sort in the development and practice of a science. Some are based on fairly direct

observations—shoes and ships, cabbages and kings, wombats and wallpaper. Others emerge from more detailed study. For example, rain and snow can certainly look very different, but are found to be simply different phases of the same substance, H_2O . If we are interested in phase transitions or crystalline structure, then we consider the distinction important, but if we are interested only in the atomic composition of water, this distinction is not important. With extensive and systematic study, some initially clear distinctions may become less so, or even disappear. The motion of the moon and a falling apple turn out to be reflections of the same mechanism—the moon is also falling toward the earth. Electricity and magnetism, initially thought to be quite distinct, turn out to be intimately tied together, as Maxwell so beautifully instructed us. When a science has not yet reached an adequate understanding of the phenomena of interest, distinctions may be vague and provisional, at best, and ultimately may change radically or disappear altogether; indeed, that volatility is part of what it means to grasp for understanding. Behavior analysis is in that place.

In the current example of positive versus negative reinforcement, I see a conceptual knot that needs to be untied to clarify any potential distinctions, or, alternatively, to confirm a proposed symmetry. Baron and Galizio (2005) touched on this knot at certain points, but, in my view, did not untie it or even cut through it. Basically, there is a tangle of (a) operations or procedures, (b) the effects of those operations, and (c) the reasons for the effects seen. To take another, perhaps less contentious, example, consider the term *extinction*. As a procedure, this could be defined, for example, as the removal of a reinforcing consequence that is currently maintaining behavior. There are other procedures that reflect a more sophisticated definition

of extinction, such as switching from a response-dependent to a response-independent contingency. As for effects, in both cases we expect to see the target behavior to decrease in probability, although the two procedures are likely to produce different patterns in extinction that are tied to our concept of extinction. Why does the behavior change under either of these conditions? If one is focused on the presence or absence of a maintaining event as crucial to maintenance of behavior, then the former procedure is appropriate; if one sees the relation between a behavior and its consequence as the crucial determinant of a performance, then the latter procedure is the more appropriate. In either case, one may theorize further about the “causes” of extinction. For example, in the traditional procedure maybe behavior goes away because other, incompatible behaviors that supply alternative reinforcers compete with the original target behavior, or maybe responding in the absence of the reinforcer is punishing, or whatever. Are there two (or more) kinds of extinction, and on what bases do we make this judgment: procedure, effect, or cause, or some combination of these?

Now the case of positive versus negative reinforcement seems more clouded. Clearly, there are distinctive definitions tied to procedures (onset vs. offset, etc.). Moreover, because we are talking about putative reinforcing effects, in both cases we expect to see responding acquired and maintained as a result of imposing one or the other kind of procedure. But when it comes to understanding *why* this might be the case, we enter into difficult waters. Many behavior analysts have, I think wisely, tended to put that question aside with respect to *any* putative reinforcer, positive or negative, while exploring the conditions under which some contingencies serve to modify behavior. A major focus of Michael (1975) and Baron and Galizio (2005) is on what might

be called motivating operations (Michael’s term) and attendant conditions. The latter, especially, seem to engender mostly alternative and undecidable “just-so” stories about the key event that controls behavior—one emphasizes onset, the other, offset, of a hypothetical condition. Some of the scenarios hark back to now-long-dead need- or drive-reduction theories of action.

Traditionally, reinforcers were said to act through the onset of pleasure or the offset of pain. But the Tweedledee–Tweedledum characterization of reinforcement often seems contrived, if not outright implausible. The child who turns on the TV and views a cartoon is said either (a) to receive the positive reinforcer of the cartoon, which thus increases the subsequent probability of turning on the TV, or (b) to be relieved of boredom with the same outcome. Of course, both might be the case; how do we deal with that? Moreover, I would assert that the pursuit of pleasure does not imply the prior condition of pain, nor does the avoidance of pain imply the pursuit of pleasure. I happen to like Beethoven and Wagner, and I tend to listen to their music if it happens to be on the radio, but in the absence of hearing their music, I don’t ordinarily sense that I am in some active state of deprivation of 19th century romanticism that can be relieved by my hearing, say, the Egmont Overture. Similarly, I don’t take active pleasure in not having a headache; if I don’t have one, so what? But if I do have a headache, taking an analgesic takes the pain away and restores me to my former condition of relative neutrality with respect to the no-headache condition. I don’t discount the possibility of some contrast effect, but it is the headache I want to get rid of—the primary cause, if you like; what happens afterward seems irrelevant.

Of course, one can make up many examples of varying plausibility, but one is playing a game that I thought

most behavior analysts had largely abandoned with respect to behavior–consequence relations. Baron and Galizio (2005) cite Morse and Kelleher (e.g., 1977), who emphasized and demonstrated that maintaining (and punishing) events can be created through contingencies and their histories, not simply through our prior picking and choosing from a catalog of events rigidly tied to particular, usually motivational, effects. Even if Morse and Kelleher's theory needs modification, many, if not most, of the consequences that control our behavior have little, if anything, to do with any known motivating operations or conditions. For example, if I walk toward a building, I get closer to it; this behavior is not necessarily related to deprivation or any other such specific condition I know of, including the fact that in walking toward one place, I am going away from somewhere else. These conditions are built into the way nature works. Only with distorting glasses and the like do these “everyday” arrangements go awry and behaviors must adjust. In general, consequences don't come in discrete packages following discrete behaviors to yield pulsating pleasures or pauses in pain. Most behavior–consequence relations have a continuous, fluid quality and are not captured by digitizing them or invoking facile motivational and emotional rationales.

Thus, there may be little reason to argue about a distinction between positive and negative reinforcement if we are not very clear about reinforcement itself. (The same could be said for punishment.) There are numerous other examples of our confusion over reinforcement. For examples, arguments continue over putative distinctions between the discriminative and reinforcing properties of stimuli, and we are still contesting the necessary and sufficient condi-

tions for acquiring and maintaining avoidance behavior.

We might take a lesson from the symmetry between discrimination and generalization mentioned earlier. Here distinctions are based on procedures and what changes in behavior we wish to emphasize. In either case, we are talking about stimulus control. With putative distinctions between positive and negative reinforcement, procedure is obviously of significance, but we also tend to emphasize nonaversive as opposed to aversive circumstances; in either case, we are talking about reinforcement. Whatever the circumstances, perhaps we are better off simply assuming symmetry until there are good reasons not to. Reinforcement, whatever it is exactly, is just reinforcement—positive or negative. We know it when we see it.

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