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IT'S AVERSIVE TO HAVE AN INCOMPLETE SCIENCE OF BEHAVIOR

ES AVERSIVO TENER UNA CIENCIA INCOMPLETA DE LA CONDUCTA

THOMAS S. CRITCHFIELD* Y ERIN R. RASMUSSEN**

*ILLINOIS STATE UNIVERSITY, **IDAHO STATE UNIVERSITY

From the earliest days of behavioral psychology (e.g., Thorndike, 1911), aversive control — what we now call punishment and negative reinforcement — was recognized as a partner to what we now call positive reinforcement. Although much is controversial about aversive control (Dinsmoor, 2001; Johnston, 1991), one thing is not in doubt: Aversive control is a prominent component of the natural world (e.g., Skinner, 1953) and therefore deserves serious scientific scrutiny.

The first half of the 20th century saw the emergence of key ideas about aversive control, including precursors to what remain the major families of theories of punishment and of negative reinforcement (e.g., Dinsmoor, 1954; Mowrer, 1949). As the 20th century reached its midpoint, researchers were learning how to apply to aversive control the free-operant methods that Skinner and colleagues (e.g., Ferster & Skinner, 1957) pioneered for studying positive reinforcement (Azrin & Holz, 1966). This set the occasion for a remarkable period — a sort of Golden Age of aversive control research. Across roughly 25 years, much of what now are seen as the fundamental principles of aversive control were fleshed out. Within about 25 years, however, things began to change: Key investigators left the aversive control laboratory, and by the 1980s, basic behavioral research on aversive control had thinned to a trickle. It remains rare today (e.g., Baron, 1998).

The reasons why aversive control research became unpopular in behavior analysis probably are complex, and certainly are beyond the scope of the present discussion, but the consequences of this seismic shift in scholarly focus are easy enough to assess, and unpleasant to consider. Below we briefly describe three ways to conceive of the status quo.

IT'S AVERSIVE TO BE IRRELEVANT

In 1928, a youthful B.F. Skinner set as his goal nothing short of remaking the entire field of psychology (cited in Bjork, 1997, p. 81). Whatever its successes, behavior analysis is unlikely to achieve Skinner's goal if it continues to ignore such an important part of the natural world. Scholars outside of behavior analysis have not made this mistake, as three examples illustrate. First, neuroscientists are actively pursuing the neuroanatomical correlates of aversive control (e.g., Gehring & Willoughby, 2002). Second, psychologists have marshaled diverse evidence that suggests that aversive events are psychologically more potent than non-aversive ones (Baumeister, Bratslavsky, Finkenauer, & Vohs, 2001). Third, in recent years reviews in several major journals have evaluated the effectiveness, and impact on mental health, of corporal punishment (e.g. Gershoff, 2002). In light of Skinner's goal for the field, it is sobering to note that prominent psychological research programs such as those just mentioned make *virtually no reference to the behavior analysis literature*. Given the recent scarcity of behavior analytic research on aversive control, however, workers in these domains can be forgiven for assuming that behavior analysts have little to say about their topics of interest.

IT'S AVERSIVE TO HAVE UNFINISHED BUSINESS

Let us be gracious and allow that a remaking of psychology is, perhaps, too ambitious a goal for a relatively young science. Even within more modest ambitions of studying behavior for the sake of behavior, there is plenty of incentive to innervate a behavior analytic study of aversive control, because the field's *own questions* about aversive control have not been answered. For example, like their counterparts of 50 years ago, basic researchers argue about the relative merits of one-factor and two-factor accounts of aversive control (e.g., Dinsmoor, 2001). Similarly, when Lerman and Vorndran (2002) reviewed the empirical literature on punishment, they found inadequate guidance regarding many factors that are important in designing interventions. Thus, even if behavior analysis will never supplant traditional approaches to psychology, and even if operant learning comprises only a small slice of psychological functioning, a complete science of behavior (for the sake of behavior) demands a thorough exposition of aversive-control processes.

IT'S AVERSIVE TO BE INCONSISTENT

To place the status quo into a somewhat different context, note that there has been no *general* demise of empirical progress in behavior science. Consider just the 15 years or so immediately following the end of the Golden Era. This was a time of dramatic change in approaches to studying, talking about, and organizing concepts regarding positive reinforcement. As Figure 1 illustrates, this period saw a shift in emphasis toward concurrent behaviors, multiple-term contingencies, and varied time frames of behavior control. Yet most of the progress involving the topics represented in Figure 1 occurred after the aversive control laboratory had been largely abandoned. As a result, it remains to be determined whether unique insights regarding aversive control might be derived from these advances, or even how aversive control processes might be expressed in these domains.

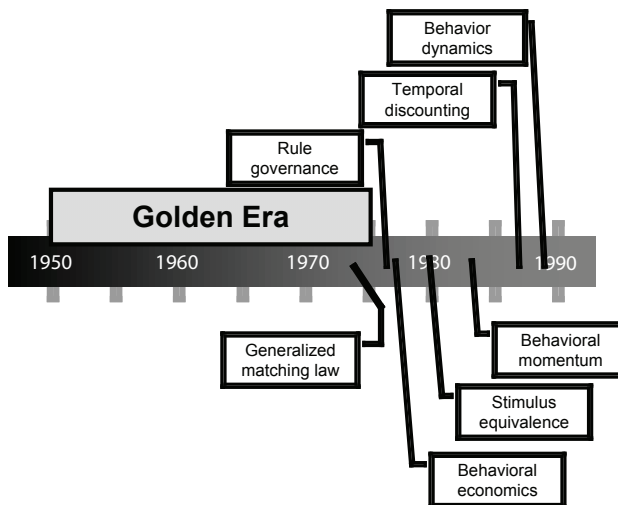


Figure 1: Some advances in the study of positive reinforcement that emerged or solidified since aversive control research in behavior analysis became rare.

Consider the generalized matching law (GML; Baum, 1974), which is the most-frequently applied framework for analyzing operant choice. After more than three decades of extensive research, no existing GML model adequately describes the simple superimposition of punishment upon concurrent sched-

ules of positive reinforcement — not because investigators have tried and failed, but rather because this question has attracted precious little interest (Critchfield, Paletz, MacAleese, & Newland, 2003). Note, too, that the GML is not the only model of operant choice. Alternative conceptions, such as contingency discriminability theory, may have unique implications for the understanding of aversive control that have not been carefully examined (e.g., Magoon & Critchfield, 2006).

It has been suggested that, in lay terms, discrimination reflects not what an organism *can* detect but rather what it *will* detect in a given circumstance (Davison & Nevin, 1999). It is clear from developments in positive-reinforcement research that experimental behavior analysis remains capable of detecting important regularities in behavior, although where aversive control is concerned, as a community we have chosen not to.

GOAL OF THIS SPECIAL ISSUE

A goal of this special issue is to illustrate the value of a renewed emphasis on the study of aversive control. By addressing an eclectic range of topics, the articles in this issue highlight not only how much remains to be learned about aversive control but also the considerable benefits of knowing. A happy conclusion to be drawn from these articles is, despite the omissions of the past, how easy it remains to envision a future Experimental Analysis of Behavior in which aversive control is given the attention that it deserves.

This issue is dedicated to the memory of James A. Dinsmoor (Figure 2), who articulated the modern two-factor theory of punishment (Dinsmoor, 1954) and contributed much important research on the role of stimuli correlated with aversive events in free-operant avoidance. Perhaps uniquely among the key players in the Golden Era of aversive control research, he remained focused throughout his career on the importance of aversive control to a complete science of behavior. Around the end of the Golden Era, his research using the observing-response procedure provided essential insights into the importance of aversive control in stimulus control (e.g., that an S- correlated with extinction or reduced reinforcement rate becomes aversive; e.g. Dinsmoor, 1983). Even toward the end of his time at Indiana University, even after retirement, even while facing challenges associated with advancing age and an automobile accident that left him physically disabled, Dinsmoor continued to explore the proper theoretical context in which aversive control should be examined (e.g., Dinsmoor, 1983, 1995, 1998, 2001). In this later work, he did as much, perhaps, as any contemporary behavior analyst to keep aversive control alive as a topic of investigation and theoretical debate.



Figure 2: James A. Dinsmoor (1921-2005). Photograph courtesy of Jack Michael.

In recognition of his profound influence on the analysis of aversive control, Jim was invited to contribute a commentary to this special issue, but he graciously declined, citing worsening health and competing contingencies: “As you know, these days I am desperately trying to grind out a very few of what you might call “heritage” articles before my wits desert me. I wish you well with the project” (personal communication, May 14, 2004). Jim Dinsmoor passed away on August 24, 2005. By virtue of its very existence this special issue on aversive control bears his indelible stamp; we hope that it honors his memory by inspiring readers to undertake their own aversive control investigations.

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