JOURNAL OF THE EXPERIMENTAL ANALYSIS OF BEHAVIOR 2001, **75,** 335–365 NUMBER 3 (MAY)

SAFE PERIODS BOTH EXPLAIN AND NEED EXPLAINING

MURRAY SIDMAN

NEW ENGLAND CENTER FOR CHILDREN

Dinsmoor's (2001) stress on the response-produced safe period as a reinforcer for avoidance behavior is a positive contribution, even though several questions about such safe periods remain to be answered.

Key words: avoidance behavior, free-operant avoidance, safe period, response-produced stimulation, shock density

Although many years have elapsed since I have been deeply and personally involved with data or theory in the area of avoidance behavior, I find myself both admiring Dinsmoor's latest contribution to that area (Dinsmoor, 2001) and agreeing with important aspects of his theory. Although some of the discussion puzzles me a bit, I believe that his most important points are going to stand as enduring positive contributions.

I will, however, start with a minor quibble. I wish Dinsmoor had avoided the term *aversive stimulus.* He uses the term legitimately, to specify a stimulus whose termination is reinforcing—a negative reinforcer (Dinsmoor, 2001, p. 312). Some, however, will be tempted to carry the term beyond its descriptive function: ''*Because* the stimulus is aversive, its termination is reinforcing.'' I know from personal experience that it is easy to fall into the trap of giving aversiveness a causal status as the source of reinforcement for avoidance behavior. When one does that, aversiveness takes on the same status as hypothesized anxiety, fear, or expectations. I consider this a minor quibble because calling a negative reinforcer ''aversive'' does not reduce the force of any of Dinsmoor's arguments. On the other hand, neither does it add to the force of those arguments.

My doctoral thesis, a quantitative analysis of avoidance behavior that used a free-operant avoidance procedure (Sidman, 1953a, 1953b), was sparked by Schoenfeld's (1950) theory of avoidance. The theory held that in an avoidance procedure, shock follows closely upon all of the subject's behavior except the act that the experimenter selects as the avoidance response. Because that response always prevents the next scheduled shock, it is the one act that shock never follows closely. Schoenfeld therefore postulated that natural stimuli (proprioceptive, etc.) produced by nonavoidance responses come eventually to serve as warnings of impending shocks, and that the avoidance response is reinforced because it terminates the warning signals that arise from other behavior. This theory implied that avoidance behavior should not require exteroceptive warning signals; it should suffice simply to present unsignaled brief shocks according to some schedule, and to arrange for any occurrence of a specified response to prevent the next scheduled shock. The successful conditioning of avoidance behavior with such a procedure appeared to constitute a strong confirmation of Schoenfeld's theory.

With continued work on free-operant avoidance (much of it summarized in Sidman, 1966), I became increasingly dissatisfied with the original statement of Schoenfeld's (1950) formulation. Dinsmoor (2001), however, has now provided potential solutions to some of the problems that bothered me. I had become disenchanted with the explanatory utility of response-produced stimuli. The reason I rejected response-produced stimuli was not that I considered them hypothetical; rather, they struck me as excess baggage as far as the explanation of avoidance behavior was concerned. I felt that it would suffice just to talk about responses producing or not producing shocks, without adding intervening stimuli that are perfectly correlated with the responses that produce them. An advantage of this pared-down conception was that one

Address correspondence to the author, 1700 Ben Franklin Drive, Apartment 9E, Sarasota, Florida 34236-2303 (E-mail: msidman@aol.com).

could then view the avoidance response simply as being selected out of the subject's repertoire as the one surviving unpunished act—an appealingly lean and elegant conception of the origin of avoidance behavior.

Schoenfeld (1950) never elaborated on his suggestion that stimuli produced by the avoidance response become conditioned reinforcers; only the stimuli produced by nonavoidance responses, and removed by the avoidance responses, seemed critical to his formulation. Dinsmoor (2001), however, with his emphasis not just on stimuli produced by nonavoidance responses but also on a separate role played by stimuli emanating from avoidance responses, restores a legitimate explanatory status to response-produced stimulation. He makes the point quite conclusively that the avoidance response not only terminates warning signals produced by nonavoidance behavior that has been closely correlated with shock but also produces a safe period that functions as a conditioned reinforcer.

Several aspects of Dinsmoor's emphasis on the safe period that follows the avoidance response do, however, puzzle me. First, I wonder whether his formulation actually requires any special mention of the termination of nonavoidance responses (or their consequent stimuli) by the avoidance response. For example, in his conclusion, Dinsmoor states,

> (c) In the absence of avoidance behavior, the experimental environment (vs. the home cage) is positively correlated with the receipt of shock. . . . (d) In the presence of the avoidance response and for a short time thereafter . . ., there is a negative correlation between response-generated stimuli and the receipt of the shock. . . . (e) The termination of stimuli positively correlated with shock and the production of stimuli negatively correlated with shock have been shown to be reinforcing. (2001, p. 328)

It seems to me that Dinsmoor's theory requires only two environments, one ''dangerous'' and one ''safe,'' each defined by its temporal relation to shock. Nonavoidance behavior, although not excluded from the dangerous environment, is not specifically included either. If I am correct about this, eliminating the need to appeal to stimuli produced by an unlimited number of nonavoidance responses would be an enormous simplification, justifiable not only on that ground alone but on the basis of another consideration that I will expand on briefly.

A second source of my dissatisfaction with the original statement of Schoenfeld's (1950) formulation was my frequent observation of extremely rapid free-operant avoidance conditioning. It seemed to me that a subject must receive many shocks before enough of its repertoire could be correlated with shock to ''squeeze out'' the only unshocked act. Although some subjects did take many shocks before learning to avoid, others avoided successfully after receiving only a few shocks. Dinsmoor's theory seems to imply, however, that the development of environment-plus-avoidance-response as a safe period depends only on a contrast with environment-without-avoidance-response. Such a discrimination could occur quite rapidly. It would not be necessary for innumerable nonavoidance responses to be correlated with shock before most avoidance responses would be reinforced. Dinsmoor's emphasis on the safe period, which requires only a discrimination between the experimental environment with and without the avoidance response, solves the problem of the sometimes rapid learning of free-operant avoidance.

Another aspect of Dinsmoor's (2001) theory that arouses my interest is the definition of a safety signal by its negative correlation with shock. Besides the avoidance response, another event also produces stimuli that are negatively correlated with shock. That other event is the shock itself. It, too, is followed by at least a brief period of time during which a shock never comes. I think, therefore, that Dinsmoor's theory implies that stimuli present during a brief period immediately after a shock will also serve as reinforcers. A test of this implication might follow a procedure like that of Dinsmoor and Sears (1973): Present a brief exteroceptive stimulus after each shock; then, make that stimulus contingent on some other response, perhaps the avoidance response itself. If that stimulus proves to function as a reinforcer, it will add considerable weight to Dinsmoor's thesis.

Then, however, there arises the question of how safe periods become reinforcing. Dinsmoor (2001) asserts that the avoidance response produces stimuli that are negatively correlated with shock and are therefore reinforcing. I can go along with that as an em-

pirical finding, but its theoretical status is not so clear to me. Why should a stimulus that is negatively correlated with shock become reinforcing? A safe period is, of course, safe only in the sense that it marks the termination or absence of a danger period; without danger, safety has no meaning. In spite of the demonstration that the safe period can function as a conditioned reinforcer (Dinsmoor & Sears, 1973), without any concomitant termination of a danger signal, negative reinforcement is still the basis for the original creation of the safe period.

I have no simple answer to this puzzle. It does, however, leave me with the feeling that we are dealing here with a phenomenon that has not yet been explained. It comes as no surprise, of course, that safe periods will function as conditioned reinforcers; if they did not, our species would be in sad shape. But it looks to me like a weakness in theory if behavior analysis cannot account for the derivation of positive from negative reinforcement.

Another major source of my early unhappiness with avoidance theory was the experiment in which subjects more often pressed a lever that produced a briefer shock postponement than a lever that independently programmed a longer postponement (Sidman, 1962). From this seemingly counterintuitive finding, I concluded that a critical variable in avoidance was the reduction in shock density over a period of time. Although I was and still am uncomfortable with that conclusion, for reasons that Dinsmoor elaborates thoroughly and eloquently (2001, pp. 318–319), the empirical fact still seems clear to me. I do not think Dinsmoor's attempt to reinterpret the data really changes the original interpretation. In pointing out that the important variable in that experiment was not the response–shock (RS) but the shock–shock (SS) interval, I believe Dinsmoor is still saying that shock frequency is critical. He stresses (pp. 317–318) ''the time between successive shocks,'' ''the scheduled time to next shock . . . (as determined by the SS interval),'' ''a series of shocks that were closely spaced in time,'' and ''a more widely spaced series of shocks,'' and concludes, ''the animal pressed the lever that produced the greatest increase from the average time to shock during SS intervals to at least one full RS interval or, commonly, the interval produced by the summation of a series of RS intervals.'' With these statements, Dinsmoor is describing high shock frequencies before the avoidance response and low frequencies after. Whenever one has to take into account many shocks over an extended period of time, one is willy-nilly talking about shock frequency.

Also, Dinsmoor seems to be arguing (pp. 326–327) that if avoidance behavior is maintained by shock-density reduction, then that consequence should cause each avoidance response to be followed quickly by more responding. That is to say, the behavior should never stop. But surely, if shock-density reduction is to have any effect, there must be a baseline density to be reduced. As time elapses without a shock, shock density automatically falls, and more shock will be required to reinstate a baseline density, reduction of which can then be reinforcing.

After I had proposed the shock-density reduction hypothesis, Herrnstein, in a social conversation, asked me if I would predict avoidance conditioning as the result of a procedure that, as he described it, turned out to be one later reported by Herrnstein and Hineline (1966). In response to my ''yes,'' he expressed considerable skepticism. When he finally did the experiment, with its positive results, he must have recognized immediately that shock-frequency reduction could not serve as an explanation within a traditional molecular analysis. His response was boldly and creatively to propose a molar account not just of avoidance behavior but of all reinforced behavior (e.g., Herrnstein, 1970), an account that did not require the kinds of contingency analysis that, until then, had claimed the full attention of most behavior analysts. Dinsmoor's (2001) excellent critique of Herrnstein's molar approach may well turn out to produce the most heated responses to his article, and may be its most lasting contribution. Without involving myself in that dispute, I wish only to point out that shock-density reduction may function as an important source of reinforcement for avoidance behavior even if a general molar analysis proves to be the wrong path for behavior theory to travel. The two controversies are not necessarily linked.

Have I endorsed Dinsmoor's theory? Yes and no. I had previously concluded that the reinforcement for avoidance behavior can

arise from several sources (Sidman, 1966), not necessarily all at the same time but selectively, given the context. I believe avoidance behavior can be reinforced by the termination of external, internal, or response-produced stimuli that have been closely correlated with shock, by escape from behavior that has been closely paired with shock, by the reduction of shock density, and now, by the production of a safe period. For me, Dinsmoor has ruled out none of these possibilities, but with the response-produced safe period, he has added a powerful and perhaps more widely applicable explanatory principle to the others that are available.

REFERENCES

Dinsmoor, J. A. (2001). Stimuli inevitably generated by behavior that avoids electric shock are inherently reinforcing. *Journal of the Experimental Analysis of Behavior, 75,* 311–333.

Dinsmoor, J. A., & Sears, G. W. (1973). Control of avoidance by a response-produced stimulus. *Learning and Motivation, 4,* 284–293.

Herrnstein, R. J. (1970). On the law of effect. *Journal of the Experimental Analysis of Behavior, 13,* 243–266.

Herrnstein, R. J., & Hineline, P. N. (1966). Negative reinforcement as shock-frequency reduction. *Journal of the Experimental Analysis of Behavior, 9,* 421–430.

Schoenfeld, W. N. (1950). An experimental approach to anxiety, escape and avoidance behavior. In P. H. Hoch & J. Zubin (Eds.), *Anxiety* (pp. 70–99). New York: Grune & Stratton.

Sidman, M. (1953a). Avoidance conditioning with brief shock and no exteroceptive warning signal. *Science, 118,* 157–158.

Sidman, M. (1953b). Two temporal parameters of the maintenance of avoidance behavior by the white rat. *Journal of Comparative and Physiological Psychology, 46,* 253–261.

Sidman, M. (1962). Reduction of shock frequency as reinforcement for avoidance behavior. *Journal of the Experimental Analysis of Behavior, 5,* 247–257.

Sidman, M. (1966). Avoidance behavior. In W. Honig (Ed.), *Operant behavior: Areas of research and application* (pp. 448–498). New York: Appleton-Century-Crofts.

Received November 27, 2000
Final acceptance December 8, 2000

MOLAR VERSUS MOLECULAR AS A PARADIGM CLASH

WILLIAM M. BAUM

UNIVERSITY OF NEW HAMPSHIRE

The molar view of behavior arose in response to the demonstrated inadequacy of explanations based on contiguity. Although Dinsmoor's (2001) modifications to two-factor theory render it irrefutable, a more basic criticism arises when we see that the molar and molecular views differ paradigmatically. The molar view has proven more productive.

Key words: molar view, molecular view, contiguity, atomism, two-factor theory, paradigm

---

Behavior analysis inherited from 19th-century psychology an atomistic view of behavior and environment. Although we no longer talk about the association of ideas, the terms *stimulus* and *response* are still with us. Hand in hand with this atomism went the principle of association by contiguity, which moved by analogy from classical conditioning to instrumental and operant conditioning (Baum, 1995). As a principle of association or reinforcement, contiguity served to get the science going, but eventually showed itself to be insufficient. Dinsmoor (2001) defends 19th-century atomism against the onslaught of a new conceptual framework that arose in the 1960s and 1970s. For the present discussion,

Correspondence should be addressed to the author, 611 Mason #504, San Francisco, California 94108 (E-mail: wm.baum@unh.edu).