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SAFE PERIODS BOTH EXPLAIN AND NEED EXPLAINING

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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

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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

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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 empirical 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.

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MOLAR VERSUS MOLECULAR AS A PARADIGM CLASH

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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

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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,

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I will refer to 19th-century atomism based on contiguity as the molecular view and the newer framework, based on extended patterns and relations, as the molar view.

Rescorla (1967, 1968), in both experiments and arguments, clarified the explanatory insufficiency of contiguity. Contiguity of the conditional stimulus with the unconditional stimulus cannot and does not predict classical conditioning, because the unconditional stimulus must also be less frequent in the absence of the conditional stimulus. If the unconditional stimulus is just as likely to occur in the absence of the conditional stimulus as in its presence, no conditioning occurs, despite the contiguity of the stimuli. Although Rescorla made the point for classical conditioning, it is readily extended to operant conditioning (Bloomfield, 1972). If the reinforcer is just as available in the absence of responding as in the presence of responding, we expect no operant responding. In molecular terms—that is, in the terms of occurrence and nonoccurrence—the time periods before and after a response must differ in the frequency of the reinforcer.

Occurrence versus nonoccurrence is just the crudest and most obvious way in which a reinforcer or punisher may vary. Creatures will behave so as to enhance the efficacy of a reinforcer or diminish the intensity of a punisher. They will behave so as to bring a reinforcer nearer in time or to delay a punisher. They also will behave so as to increase the likelihood—hence, the rate—of a reinforcer and to decrease the likelihood (rate) of a punisher.

The molar view is thus an extension of the necessary comparison that invalidates the contiguity-based law of effect. Suppose, however, one granted the invalidation of contiguity while still insisting on a molecular view, as Dinsmoor (2001) apparently does. What advantage lies in the molar view? Dinsmoor's attempt to defend two-factor theory illustrates well the advantage, because his arguments render his theory irrefutable and redundant.

Let us agree that behavior produces proprioceptive and kinesthetic feedback. It is rarely observed, but Dinsmoor (2001) has a solution to that: Assume the stimuli and observe the response. His explanation of the avoidance found by Herrnstein and Hineline (1966), with no exteroceptive stimulus and random delivery of shocks both before and after responses, is that the response-produced stimuli are less often paired with shock than preresponse (i.e., postshock) stimuli. The translation, taking into account that the response is the measure of the stimuli, is that shock occurs at a higher rate before responses than after. That, however, is the shock-rate reduction explanation. Dinsmoor cannot explain the avoidance without reference to shock-rate reduction, even though he prefers to talk about delay, which he himself recognizes is simply the reciprocal of rate. The statement, "In the absence of responding, the time intervals from one shock to the next are shorter on average," is exactly equivalent to the statement, "In the absence of responding, the shock rate is higher."

Dinsmoor (2001) insists that the response-produced stimuli, which seem redundant, in fact are essential, because reinforcement must be immediate. Behavior is maintained less well when unsignaled delays are introduced between reinforcers and the responses that produce them. I discussed this in 1973, because it seems to imply a role for contiguity within the context of the molar view. I suggested that the effect of unsignaled delays is to degrade the correlation between rate of reinforcement and response rate, and I showed some sample records from an experiment testing this idea. Dinsmoor ignores that part of the article and the feedback function, the essential ingredient in appreciating the organism–environment feedback system. Ironically, after insisting on the necessity of immediate consequences, Dinsmoor tries to explain away the maintenance of responding that only eventually, after a minimum of 2 min, shortens the avoidance session, by suggesting that the delayed escape reinforces the responding (Mellitz, Hineline, Whitehouse, & Laurence, 1983). This looks like trying to have it both ways.

I said before, and I say again, that two-factor theory cannot explain avoidance without resorting to hypothetical entities (Baum, 1973, 1989). The hypothetical entity in Dinsmoor's (2001) attempted explanation is the reinforcement. His appeals to "aversiveness" and "safety" are no more defensible than was Mowrer's (1960) appeal to "fear." We know that under certain circumstances creatures will behave so as to avoid electric shock. To say that the reason for this is the ''aversiveness'' of the shock is to add nothing to the account. It is exactly like saying that objects are heavy because they possess letharge, are hot because they possess caloric, or burn because they possess phlogiston. Dinsmoor needs this imaginary essence only because he needs something to transfer from shock to signal. A parallel point applies to ''safety'' and to conditioned reinforcement, where the imaginary essence might be called ''reinforcingness.''

The experiments that Dinsmoor (2001) cites as evidence for his theory are conceptually flawed, because they depend on failure of discrimination. They are fragile results, because they depend on a host of factors, any one of which might facilitate or prevent the formation of the discrimination of the lack of contingency or shock. In the experiment by Weisman and Litner (1969), for example, Dinsmoor omits to mention that the rats were pretrained to avoid shock. No doubt there are many ways to confuse a rat.

The conflict here, however, is not between theories, as Dinsmoor (2001) seems to suggest. The conflict is paradigmatic. Dinsmoor's defense of two-factor theory should be read as a defense of an atomistic view of behavior analysis. He shows that two-factor theory can explain anything, much as followers of Ptolemy, in defense of the geocentric view of the solar system, showed that epicycles could explain all the perturbations in the paths of the planets. Copernicus's heliocentric view actually predicted the planets' motions no better than the geocentric view, but it prevailed in the end, because it was the more productive view. Similarly, the molar view of behavior analysis has proven to be the more productive view.

What Dinsmoor (2001) takes to be a weakness of the molar view is actually a strength. There is no question that extended relations may be overridden by local relations. Humans and other animals sometimes behave so as to obtain immediate small reinforcers even when by doing so they cancel the possibility of later larger or more frequent reinforcers. They also sometimes behave so as to postpone immediate small punishers even when by doing so they produce later larger or more frequent punishers. The molar view allows these observations to be cast in terms that may be studied: Under what conditions do short-term small reinforcers exert more control than later large reinforcers? More generally, under what conditions do local reinforcement and punishment prevail over extended reinforcement and punishment? The question ties the overriding to a large body of research on self-control (Logue, 1995; Rachlin, 2000). Pigeons' behavior, like that of humans, sometimes is controlled by local relations and sometimes by extended relations (e.g., Rachlin & Green, 1972). Research is beginning to help us to understand why (Rachlin, 2000).

Likewise, the molar view casts the effects of delay into questions for research. The passage of time affects behavior in at least two ways. Stimuli and responses affect present behavior less and less as they recede into the past, and events that will occur sometime in the future, even when their delay is signaled, affect present behavior less and less the more remote they are. These effects are probably related. Research on timing, delayed discrimination, and temporal discounting all come together to focus on this problem, which may be called the problem of time horizon—that is, the problem of discovering the factors that determine loss and gain of effectiveness with the passage of time.

Dinsmoor (2001) seems to think that the problem of time horizon is somehow fatal for the molar view, because he thinks that when one calculates a response rate, the time period in the denominator is arbitrary. His not recognizing that the question of time horizon is an empirical question arises from his molecular view. To him, response rate is a convenient summary, a derived measure that indicates response strength or probability. In contrast, the molar view takes response rate as a real entity, an aspect of a pattern of behavior that is extended in time. Extended patterns are composed of more local patterns, and every extended pattern is part of some still more extended pattern (Baum, 1995, 1997). A single key peck may be part of a pattern of pecking at a key—say, variable-interval performance—that in turn may be part of a pattern of choice between two keys, that in turn may be part of a pattern of behavioral allocation between key pecking and other food-related behavior, and so on.

The idea that behavioral patterns are necessarily extended in time, on which the molar view is based, raises an insoluble problem for the molecular view (Baum, 1997). Dinsmoor (2001) takes the measurement of the response for granted. He overlooks that, when a lever is depressed, one knows if it was a ''real'' lever press only if one eventually (i.e., after a while) comes to see it as part of an extended pattern. The rat might have stepped on the lever while exploring or fallen on the lever while sniffing at the ceiling. The notion of operant level was an attempt to deal with this point, but it fails, because more is involved. For example, in Schneider's (1969) classic study of fixed-interval performance, he had to devise a way to assign some responses to an initial low-rate period and other responses to the later high-rate period. He did this by analyzing the pattern of responding as a whole.

In closing, I would like to set out some questions for Dinsmoor. Not that I think he will lack answers, but rather that I think his answers will be revealing. First, if the response-produced proprioceptive stimuli are reinforcing, then they reinforce responses continuously. Why does free-operant avoidance responding occur only at moderate rates, instead of at high rates characteristic of continuous reinforcement?

Next, how does the molecular view explain the differences between ratio and interval response rates? This is really a two-part question. I expect that Dinsmoor would say that rate on interval schedules is lower than on ratio schedules because of differential reinforcement of long interresponse times on interval schedules. Two problems arise. First, differential reinforcement of long interresponse times should eventually reduce response rate to the point at which the average interresponse time would be long enough that probability of reinforcement would be about 1.0. Response rate should be low enough that the schedule would approximate continuous reinforcement. Response rates maintained by interval schedules are always much higher than this. Why is response rate on interval schedules so high? Second, why is response rate on ratio schedules so high? Here, differential reinforcement of interresponse times cannot apply, because all interresponse times are reinforced with equal probability. One suggestion was that ratio schedules reinforce bursts of responses, but that raises the question of how one defines *burst* without referring to response rate; for what is a burst but a period of unusually high rate? How will Dinsmoor construct an explanation that differs from the molar explanation: that ratio schedules differentially reinforce high response rates with high rates of reinforcement?

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