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

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

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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,

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

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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