COMMENTARY

ARE RESPONSES IN AVOIDANCE PROCEDURES “SAFETY” SIGNALS?

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Dinsmoor’s (2001) position has the advantage of parsimony in that it relies on well-established principles rather than a separate process—shock-frequency reduction—to account for avoidance. Other advantages are that it blends well with what is known about the effectiveness of momentary contiguities in the study of positive reinforcement and that it might provide an account of why different response forms seem to condition at different rates. Despite these advantages, the view needs elaboration about the temporal characteristics of response-associated stimuli, the functions that “warning” stimuli may have, and especially about how “safety” is established.

Key words: avoidance, two-factor theory, shock-frequency reduction, safety signals, proprioceptive stimuli

It is to Dinsmoor’s (2001) credit that he has opened this discussion of sources of reinforcement in procedures said to examine avoidance. He champions the two-factor theory over the one-factor (shock-frequency reduction) approach, and there is one good rationale for doing so that he does not mention explicitly. The reason is that the two-factor approach can be viewed as, in fact, more parsimonious than the one-factor view. The two-factor view is based on two well-established sets of phenomena, Pavlovian conditioning and immediate negative reinforcement (escape). If those are not sufficient to account for what is called avoidance, then at least one additional process is needed. If one-factor theory is needed to account for avoidance, then it represents a new, third, process. There is good reason, therefore, to hope that already-established principles will provide a satisfactory explanation.

A second strength of Dinsmoor’s (2001) view, one that he rightfully emphasizes, is that the two-factor account is more easily compared to, and perhaps integrated with, what is known about the role of temporal contiguities in behavior maintained by positive reinforcement. That is, currently effective accounts of the temporal patterns and rates of free-operant behavior established by positive reinforcement rely heavily on factors occurring “at the moment of reinforcement.” Two-factor theory identifies moments of reinforcement.

Despite the metatheoretical reasons for preferring the two-factor account, Dinsmoor’s (2001) case is not as compelling as one might hope. He admirably goes right to the key experiments used by proponents of shock-frequency reduction to support their view, but I do not believe that Dinsmoor’s analyses of these studies are convincing. Perhaps that is because I do not fully understand his views, so I welcome his having the opportunity to set me straight in his reply to the commentaries.

Dinsmoor (2001) first takes on the important paper by Sidman (1962), in which it was discovered that concurrent free-operant avoidance schedules yielded an unexpected result. Sidman arranged that each of two levers was associated with a schedule according to which the response—shock (RS) interval was the same as the shock—shock (SS) interval. The surprising result was that when the RS–SS intervals on the two levers were not the same, rats preferred the response associated with the shorter interval, despite the fact that each lever press on the lever associated with the longer RS–SS interval produced a longer period of no shock. Sidman noted that this pattern of behavior resulted in greater overall reduction in shock frequency than would a pattern of preference for the lever associated with the longer RS–SS interval. Dinsmoor attributes these results to the fact that responses to the lever associated with the shorter interval resulted in more reinforcement because the schedule on that lever resulted in more frequent
shock in the absence of responding on that lever. That analysis, however, hinges crucially on the assumption that the rat’s behavior was sensitive to the scheduled source of shocks (i.e., the chamber, that the rat could tell from which schedule the shocks came). In fact, Dinsmoor states, “[a rat] distinguishes between those shocks that are delivered on an RS schedule and those delivered on an SS schedule” (p. 319). I see no reason to assume that was the case in Sidman’s experiment because all shocks were the same, and they could come from schedules associated with either lever. Rats often allocated a large proportion of their responses to one of the levers, with the result that shocks were often delivered on the basis of the schedule associated with the nonpreferred lever. As Sidman noted, high rates of response on the preferred lever increased the probability that a shock (from the schedule for the nonpreferred lever) would closely follow a lever press in time (and presumably act as punishment). That is, a high rate of pressing on the lever with the short RS–SS interval made it likely that some presses of that lever would be followed immediately by shock. All shocks in the experiment were the same, so how could a rat “assign” any given shock to one lever or the other? Dinsmoor needs to provide a more detailed analysis of how the situation (i.e., the chamber plus the passage of time without a press on a particular lever) gained negative reinforcing effectiveness (or, alternatively, provided greater relief to be associated with response-generated stimuli). Essentially, the question is, how does a response (or stimuli arising from that response) that is occasionally followed immediately by shock become highly effective as a safety signal? If the answer is that the response is followed on average by more safety, then it becomes more difficult to distinguish that view from one based on shock-frequency reduction.

Dinsmoor (2001) also offers an interpretation of the Feild and Boren (1963) study, which is often cited by proponents of shock-frequency reduction to illustrate that stimuli in avoidance experiments might be better thought of as discriminative rather than conditional. His analysis, however, seems not to be consistent with other positions he takes in the paper. Dinsmoor’s view is that the stimuli that the rats terminated must have been aversive conditional stimuli, that is, that they served the standard role that two-factor theory proposes for stimuli in discriminated avoidance conditioning. That view seems inconsistent with his opinion about the likelihood of third-order Pavlovian conditioning, which he describes as “very weak” (p. 325). The rats in the Feild and Boren study generally turned off a stimulus that was three stimuli and 30 s removed from electric shock, and they often turned off stimuli that were even more removed temporally and sequentially from shock. For two-factor theory to be a plausible account of these results, higher order Pavlovian conditioning seems to be required.

A major issue concerning the role played by warning stimuli added to free-operant avoidance procedures is that of why animals do not come to postpone them like they do the shock itself when no warning stimuli are presented. Instead, animals come to wait until the “warning” comes on before responding (e.g., Sidman, 1955). Dinsmoor (2001) is not entirely clear about why this is so. When discussing chained schedules of positive reinforcement, he has no reservation about imputing conditioned reinforcing efficacy to some of the stimuli in the chain (i.e., those that overlap with primary positive reinforcement), but why then should stimuli that overlap with primary negative reinforcement (e.g., electric shock) not gain conditioned negative reinforcing efficacy to the extent that animals will make a response that postpones them? His view about this is that “such responses [i.e., those that postpone an exteroceptive warning stimulus] do not produce a change in the exteroceptive stimuli from ones that are positively correlated (warning) to ones that are negatively correlated (safety) with the shock” (p. 325). But cannot the same be said about responses under a simple Sidman avoidance procedure? Why do stimuli arising from the designated response come to have a safety function in Sidman avoidance without a warning stimulus but do not when there is an exteroceptive warning stimulus (i.e., in lay terms, why does the animal not seek safety from the warning stimulus)? If the reply is that the presence of the exteroceptive warning stimulus makes
its absence a safety signal, then does that
not define the warning stimulus as aversive
and therefore something that will support
its avoidance? Or is the argument here that
conditioned aversive stimuli will only sup-
port escape and not avoidance?

The discussion of the Mellitz, Hineline,
Whitehouse, and Laurence (1983) paper
also seems somewhat strained, or at least
underdeveloped. Apparently, the claim is
that the reported effect (i.e., that rats pre-
ferred a lever on which presses shortened
the length of the session) was due to acci-
dental contiguities between presses on that
lever and the end of the session. The con-
tiguities had to be accidental because the
contingency between session shortening
and lever pressing was suspended for the
last 2 min of each session. Examination of
the data from Mellitz et al. reveals that le-
ver-pressing rates on the lever that was as-
associated with session shortening ranged
from 2.7 to about 11.3 responses per mi-
minute, and that in many cases the ratio of
rates on the two levers was about 2 to 1.
That means that in some instances the pref-
ference had to be controlled by an adventi-
tious relation between a lever press and an
event that occurred 20 s later, and which
occurred only in two out of three sessions,
on average. All this is presumed to occur in
a context in which the lever presses also
postponed shock. I think data supporting
that possibility need to be presented.

Some of Dinsmoor’s (2001) criticisms are
based on experiments that report condition-
ing with no decrease in shock frequency (e.g.,
These criticisms may be based on an erro-
nous assumption: that frequency should be
computed as the arithmetic average of the in-
verse of the interevent intervals. The litera-
ture on choice between delays to positive re-
inforcement makes it abundantly clear that
the arithmetic average can be inappropriate
(cf. Fantino, 1977). In the positive-reinforce-
ment domain, short delays are disproport-
ionately weighted, a finding that suggests that
relatively long delays may be important in the
realm of negative reinforcement.

I remain puzzled by Dinsmoor’s (2001)
claim that the stimuli associated with respon-
ses are observable. One might assume that
they are observable to the animal that emits
the behavior, but how are they observable to
an experimenter? I can, as an observer, see
the response, and I might even assume that
stimuli discriminable to the animal are pres-
ent at that time. But how would I know if the
stimuli are “fading” (and perhaps more im-
portantly, how much they have faded, or
when they have disappeared altogether), as
Dinsmoor suggests they are in his analysis of
the Feild and Boren (1963) study?

One of the more provocative suggestions
offered by Dinsmoor (2001) is that his for-
mulation, being based on discriminability of
one’s own actions, might offer a way to pre-
dict how rapidly avoidance learning might
proceed. As I understand his argument, ac-
tions that are more discriminable or more
distinctive to an organism would be predicted
to condition more rapidly. What is not clear
is what would make particular acts more dis-
criminable. Would more lengthy sequences
of action be more conditionable, or would
brief actions, because they represent distinct
deviations from ongoing other behavior, be
more conditionable? The question is impor-
tant because Bolles’ (1970) suggestions con-
cerning phylogenetic contributions to condi-
tionability of avoidance might be subsumed
by Dinsmoor’s view, but for that to happen
Dinsmoor must be able to specify in advance
(as Bolles could not) patterns that will pro-
duce conditioning more readily.

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A FEW MINOR SUGGESTIONS

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We agree with almost all of the analysis in this excellent presentation of the molecular view of avoidance behavior. A few suggestions are made as follows: Referring to response-generated stimuli as “readily observable” seems not quite right for the kinesthetic components of such stimuli, although their scientific legitimacy is not questioned. Interpreting response-generated stimuli as a form of positive reinforcement is contested, and an alternative interpretation is offered. A possibly simpler interpretation of the Sidman (1962) two-lever experiment is suggested. We question Dinsmoor’s (2001) explanation for warning stimuli not being avoided, except for the reference to the weakness of third-order conditioning effects. A final question is raised regarding the nature of the variables that are responsible for the momentary evocation of the avoidance response.

Key words: avoidance, response-generated stimuli, stimulus-change decrement, stimulus transition, evocative effect

Dinsmoor (2001) provides a persuasive and richly detailed analysis of avoidance theory and some of its experimental literature in terms of the role of response-produced “safety signals.” We found ourselves in agreement with most of the analysis, and our comments consist of minor terminological suggestions and slightly different interpretations of some of the experimental results.

Scientific Legitimacy of Response-Dependent Stimuli

This is a very useful treatment that directly challenges an often-made criticism, and we have only a slight disagreement with respect to the observability of the relevant stimuli. Dinsmoor states that “The occurrence of a physically defined response is just as material, just as observable, just as specifiable a source of stimulation as the presentation of a light or a tone” (2001, p. 315). Here a *response* is being described as observable, which is certainly true. But on page 316 the response-generated *stimuli* are referred to as “readily observable.” This seems not quite correct if kinesthetic response-generated stimuli are included. As observers we can make direct contact with exteroceptive stimuli that affect another organism in ways that we have strong reasons to believe are very similar to the contact made by the experimental organism, but we only make indirect contact with kinesthetic stimuli that affect that organism. Strictly speaking, they do not seem just as readily observable as a light or a tone. This does not detract from the scientific legitimacy of such stimuli, however, but only makes the issue a little more complex.

Response-Generated Positive Reinforcement for the Avoidance Response

Dinsmoor (2001) considers the possibility that response-generated stimuli that are “in-