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AVERSIVE CONTROL: A SEPARATE DOMAIN?

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Traditionally, aversive control has been viewed as a separate domain within behavior theory. Sometimes this separateness has been based upon a distinction between reinforcement and punishment, and sometimes upon a distinction between positive and negative reinforcement. The latter is regarded here as the more compelling basis, due to some inherent procedural asymmetries. An approach to the interpretation of negative reinforcement is presented, with indication of types of experiments that support it and that also point to promising directions for further work. However, most of the interpretive issues that arise here are relevant to positively reinforced behavior as well. These include: possible reformulation of the operant/respondent distinction; the place of emotional concepts in behavior analysis; the need for simultaneous, complementary analysis on differing time scales; the understanding of behavioral situations with rewarding or aversive properties that depend as much upon the contingencies that the situations involve as upon the primary rewarding or aversive stimuli that they include. Thus, an adequate understanding of this domain, which has been traditionally viewed as distinct, has implications for all domains of behavior-analytic theory.

Key words: negative reinforcement, punishment, emotion, aversive control, resonance, schedule-induced behavior, scales of analysis

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As categorized by textbooks and as indexed by the grouping of papers at professional meetings, aversive control has been a fairly distinct domain with its own procedures, its own phenomena, and especially with its own interpretations. Conceptually, the domain of aversive control is defined in terms of punishment and of negative reinforcement, for these are the bases for specifying what we mean by "aversive." Other phenomena, such as conditioned suppression and stimulus-induced aggression, have also been regarded as demonstrating aversive control when they involve events that commonly function as aversive in punishment or negative reinforcement procedures. In addressing these phenomena and what is to be made of them, I shall touch upon the conceptual history of their separate status, but shall also argue that these phenomena have helped to force some new ways of approaching all behavioral interpretation. My thesis is that the most promising directions for further research and conceptual development regarding aversive control will involve mainly issues that are not peculiar to that domain.

THE ORIGINS OF "AVERSIVE AS DIFFERENT"

Punishment

Even in the precursors of modern behavior theory there was a fundamental distinction between aversively based and appetitively based behavioral processes. Thorndike (1898) initially proposed two laws of effect, one positive and the other negative, corresponding to what we now call positive reinforcement and punishment. Although Thorndike eventually abandoned the negative law, the distinction was still implicit, for he asserted that whatever was going on in the production of "unsatisfying consequences" was less effective and thus different from the process described by the positive law.

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Within behavior analysis, Skinner has consistently advocated keeping punishment in a separate domain. Initially, the balance of data supported that view, as recounted by Michael (1975). However, Skinner has continued to argue—in the face of accumulating contrary data—that punishment procedures produce only indirect effects on behavior, and has emphasized temporary effects of punishment when punishment procedures are discontinued. Of course reinforcement effects are similarly temporary when reinforcement procedures are discontinued. Furthermore, Azrin and his colleagues showed long ago that behavior-analytic techniques readily apply to the analysis of punishment (for a review, see Azrin & Holz, 1966). Dinsmoor (1954, 1977) has rejected the notion of punishment as qualitatively different from reinforcement, but in a different way has kept aversive control as separate. In his view, punishment is a variant of negative reinforcement, to be understood in terms of avoidance theory. Rachlin and Herrnstein (1969) have pointed out weaknesses in that approach; my own reasons for rejecting it relate to the type of avoidance theory that Dinsmoor invokes—one that depends heavily on contiguous causation and has much in common with traditional, mediational theories.

There appears to be one pervasive asymmetry that differentiates the study of punishment from the study of reinforcement: If one is to study punishment, there must already be a substantial tendency for some specified pattern of behavior to occur. A single punishment procedure will have varying effects depending upon how the behavior is being maintained, thus giving apparent complexity to punishment effects. Of course this difference need not imply that punishment *as process* is more complex than reinforcement *as process*. Studying positive reinforcement of behavior patterns that normally occur at high operant levels could involve similar complexity. Azrin and Holz (1966) have shown how to reveal the order embedded in the complexity of punishment effects, and Johnston (1972) has documented literature showing that their analysis is valid for human behavior as well as for the behavior of animal species that provided the initial data for their account.

To be sure, there can be troubling byproducts of punishment, and there are many applied situations in which punishment procedures should not be the procedures of choice. These are the apparent bases for Skinner's continuing to deemphasize the role of punishment in a behavioral system. However, applications based upon positive reinforcement can also produce deleterious side effects (Balsam & Bondy, 1983). Furthermore, punishment is a frequent fact in human interactions, and in some circumstances punishment procedures even provide the most effective basis for humanely achieving social good. Such facts are to be understood in terms of analyses such as that offered by Azrin and Holz. I am not sure how to improve upon their approach so shall have little more to say about it here, except as relating to the ways in which we should characterize aversive events.

Negative Reinforcement: Some Preliminary Considerations

Negative reinforcement is said to occur when some class of behavior is strengthened through its removing, reducing, or preventing some event or events. The events are defined as aversive through this relationship with behavior. One might challenge this as a basis for distinguishing positive from negative reinforcement, as Michael (1975) has done, by pointing out that the consequences of behavior are environmental changes the signs or directionalities of which are arbitrarily specified. The addition of one event is the removal of another, and vice versa: Adding heat is removing cold; adding food is decreasing deprivation; adding a smile removes a frown. However, there is a fundamental asymmetry, for if a stimulus or situation is to be reducible or removable by some response, that response must occur in its presence. In contrast, positively reinforced responses necessarily occur in the absence of the stimuli upon which reinforcement is based. As Catania (1973a) has noted, this may provide an unambiguous basis for distinguishing negative from positive reinforcement. In any case, it means that the to-be-reinforced behavior must occur in the face of a situation or stimulus that is likely to have direct influence on that behavior—which leads to a concern with patterns of behavior that are reliably produced by negative reinforcement procedures, but apparently are not directly attributable to reinforcement.

Bolles (1970) argued that such patterns, which are of likely phylogenetic origin, themselves constitute most of what we call avoidance. Characterizing such patterns within behavior-analytic interpretation has presented problems, for the patterns often are not simple, phasic reflexes (for example, freezing is not phasic; running is not simple), and hence are not concisely handled in terms of the operant-respondent distinction. Also, it has become evident that such patterns are not limited to aversive control, for we have come to recognize similar phenomena in appetitive situations. Nonoperant behavior (behavior understood in relation to its immediate antecedents rather than its consequences) has been prominent within biological traditions of study (e.g., see Hinde, 1966) and behavior analysts have been accused of ignoring it. Such behavior *can* be included within our interpretive approach (e.g., Skinner, 1981, 1984). However, if we are to make our special contribution to the understanding of it, we must account for the relevant details on a scale that lies within individual organisms' lifetimes, rather than merely appealing to phylogenetic origins. Such work has been attempted in recent years, as in the study of autoshaping (Brown & Jenkins, 1968) and schedule-induced behavior (Falk, 1966). The place of such work within behavior-analytic theory bears examination here, in a digression that will lead back to the topic of negative reinforcement.

BEHAVIOR CONTROLLED BY ANTECEDENT EVENTS

Elicitation, Induction, Modulation

Behavior-analytic theory is basically an attempt to efficiently describe the interplay between behavior and environmental events. It emphasizes the environmental side of that interplay, focusing on principles that translate directly into experimental procedures that produce robust, orderly patterns of behavior. Exemplifying the interplay, orderly environmental patterns interrelate with orderly behavior patterns. Response-consequence relationships constitute one major category of principles, defining operant behavior through reinforcement and punishment. The other major category traditionally has been defined in terms of behavior and its immediately antecedent events, the "respondent" side of the operant/respondent distinction. However, some of the behavior patterns in this category do not occur in discrete, phasic relations to discrete, antecedent events (Wetherington, 1982); they are not well described by "elicitation," which has been the principle translated into procedure. Yet such patterns are not controlled by their consequences and hence are not operant in nature. We have distinguished such patterns under various labels—adjunctive behavior, interim behavior, schedule-induced behavior—but we have not yet identified principles that efficiently characterize the interplay between the behavior patterns and the environmental events that appear to produce them. Wetherington (1982) reviewed characteristics that traditionally define a reflex. She pointed out complications in respondent behavior—sensitization, habituation, summation, and temporal conditioning—and questioned whether the features of adjunctive or schedule-induced behavior are distinguishable from these. The concept of elicitation, then, needs reexamining.

I have proposed elsewhere (Hineline, 1981b) that elicitation might be considered a special case of a more general process called "induction," a term that suggests "a certain indirection in causing something to happen" (Segal, 1972, p. 2) but which still is essentially antecedent rather than consequent to behavior. Thus construed, induction can involve effects on behavior of response-independent sequences of environmental events.

The relevant events are not confined to immediate antecedency; were we to animate them, they might be labeled "intrepid" or "incorrigible," to emphasize the fact of their proceeding independently of behavior. Many instances of induced behavior can then be characterized in terms of sensitivity to features of the sequences of environmental events—features such as frequency and degree of periodicity—rather than in terms of sensitivity to the individual events in the sequences. Such behavior also may be characterized in terms of sequences or groups of responses, focusing upon dynamic characteristics such as: (1) damping—the rate with which a discontinuation of the environmental sequence results in the discontinuation of a repetitive behavior pattern. Phasic reflexes are then viewed as analogous to critically damped or overdamped systems in the physical domain; they are affected mainly by individual environmental events. Critically damped systems precisely track environmental events irrespective of how frequently those events occur. Overdamped systems are relatively insensitive to events that repeat too quickly. (2) resonance—the property of being induced more easily at one frequency than at other frequencies of occurrence of environmental events. (3) tuning—the degree of selectivity to a particular resonant frequency. Schedule-induced behavior patterns are frequency-sensitive, as revealed by the bitonic function that typically is obtained when the frequency of inducing events is varied systematically (e.g., Allen & Kenshalo, 1976; Falk, 1966; Flory, 1969; Roper, 1980). Thus these behavior patterns have a basic feature of resonant systems. A next question to ask is whether these behavior patterns can be "driven at the harmonic," as is almost universally true of resonant physical sytems, and whether there is a reciprocal relation between degree of damping and degree of tuning. Should these characteristics be verfied, one could go further and ask whether features of coupling, phase-lag, and the like could be used to further characterize the interplay between behavior and environment.

Admittedly, these notions are speculative. What makes them attractive from a behavior-analytic viewpoint is, first, the fact that they portray reflexive and schedule-induced behavior on a single, quantitatively specified continuum. The classic reflex is a damped behavior pattern. Second, the proposed principles have great generality across domains of natural phenomena, applying equally well to such diverse events as swaying trees, slopping coffee, and oscillating electronic circuits. Third, these processes are not characterized as lying behind behavior; rather, they are posited as *in* or *of* behavior. For example, frequency sensitivity can be identified as intrinsic to a pattern of behavior without appealing to a mediating oscillator. Fourth, the processes translate easily as principles of orderly behavior-environment interaction, directly portrayable in experimental procedures. Hence I present these notions here without supporting data to illustrate a purely behavior-analytic type of theory addressing schedule-induced behavior, and to suggest new types of relationships that we might be looking for.

Emotional Effects: Not Just Classical Conditioning and Aversion

The topic of elicitation suggests respondent conditioning, which has figured prominently in the aversive domain in two ways: through conditioned suppression and through two-process avoidance theory. The procedure for conditioned suppression was devised by Estes and Skinner (1941), who presented it as a technique for studying emotion. They offered it as a measure of "conditioned anxiety"—emotional behavior to be identified and measured through its disruption of operant behavior. Subsequent behavior-analytic research with their procedure has focused upon both schedules of stimulus presentation and baseline schedules of reinforcement (for a review, see Blackman, 1977). But the procedure has been more prominent as a technique for research addressed to mediational, neobehavioristic theory—theory of associations between stimuli or between representations of stimuli.

The technique has been especially useful for detecting inhibitory as well as excitatory Pavlovian effects (e.g., Hammond, 1966). Following the tradition of "methodological behaviorism" rather than behavior analysis (Skinner, 1945), associative theorists have used conditioned suppression and other measurement techniques to operationally define the theoretical construct, "conditioned fear," which is then invoked to explain behavior (e.g., see McAllister & McAllister, 1971). However, "fear" as a unitary construct seems not to be supported by data, for various measures used to index it do not covary consistently (Black, 1971; Brady, Kelly, & Plumlee, 1969; Myer, 1971). An additional behavior-analytic objection to the "fear" construct is its taking a vernacular term as appropriately categorizing psychological process. As Schoenfeld (1969) so eloquently stated:

> Words from the layman's vocabulary . . . reflect social attitudes and beliefs about behavior; they are defined by social criteria; they group acts by their social outcomes; and, they are almost always mistaken in what they accept as their behavioral referents. (p. 669)

Behavior-analytic concepts are derived from experimental operations, rather than the converse (Hineline, in press). Thus, as a result of its having been researched and interpreted in a fundamentally different rubric, behavior-analytic theorists have tended to ignore much of the work done with conditioned-suppression procedures, some of which could inform the behavior-analytic enterprise.

We need to deal with a broader range of phenomena that are at issue when one speaks of emotion. (And the locution, "when we speak of," is deliberately chosen here since it does not take vernacular labels as defining the categories for analysis.) Emotional processes traditionally have been viewed as mainly Pavlovian, with emphasis on stimuli that accompany noxious or painful events and thus come to elicit autonomic reactions. But when speaking of emotion we often are not referring to effects of conventional eliciting stimuli, nor are the discriminanda provided mainly by autonomic reactions. For example, my basis for talking of anger or frustration, and my culture's basis for establishing and maintaining consistency in such talk, is not the degree to which my stomach agitates or to which my pulse rate and blood pressure are elevated. Rather, the basis is a set of environmental events, such as: "After I spent three hours working on this manuscript, the word processor wouldn't take a 'save' command, and now the printer is omitting all the semicolons." Thus when we speak of emotion we refer not only to elicited reactions nor only to products of painful stimulation.

> Your teenage daughter is pregnant, and you've just been elected chairman of the Department! Your fiancée is leaving for Alaska on Sunday night, and you have two lectures and a grant proposal due Monday? I just received in the mail the only copy of an 800-page manuscript that I had lost on the train to New York!

Somehow, "My stomach was tied in a knot," or the like, is not adequate to such occasions. The domain of emotional experience, however we are to characterize it, is to be understood substantially in terms of operant as well as respondent contingencies. Furthermore, intense emotional effects are not peculiar to aversive control.

Skinner made some of the same points long ago (Skinner, 1953, chap. 10), but his discussion of emotion has not led to much systematic research other than that on conditioned suppression. One possible shortcoming is his having defined emotion as "a pattern of behavior" (p. 168), while the categorizing of emotional patterns must include environmental settings. If so, the definition should be modified to "a pattern of environment-behavior interaction." This need for explicitly including the environmental side of the interaction is supported by Skinner's own informal observations:

> In the search for what is happening "in emotion" the scientist has found himself at

> a peculiar disadvantage. Where the layman identifies and classifies emotions not only with ease but with considerable consistency, the scientist in focusing upon responses of glands and smooth muscles and upon expressive behavior has not been sure that he could tell the difference between even such relatively gross emotions as anger and fear. Some means of identification available to the layman appears to have been overlooked. (Skinner, 1953, pp. 161-162)

Indeed, what must be included in a scientific categorization of emotional phenomena are the sets of public events that participate in the occurrence of those phenomena. I doubt that an adequate behavior-analytic categorization in the emotional domain will correspond exactly to that of vernacular vocabulary, but I do predict that the two categorizations will share the characteristic of being founded as much upon the environmental as upon the behavioral side of the interaction.

Outside the domain of emotion, behavior analysts do need to attend more to stimulus-stimulus relations, especially to address phenomena such as those identified with the verbal behavior of labeling, and with the details of someone's quietly comprehending a lecture. Analyses of stimulus equivalence classes (e.g., Fields, Verhave, & Fath, 1984; Sidman, Rauzin, Lazar, Cunningham, Tailby, & Carrigan, 1982; Sidman & Tailby, 1982) appear to be providing some advances of that sort, but we still have a long way to go. Also, aspects of conditioned suppression that relate to "behavioral output," as distinguishable from stimulus-stimulus relations, may be understood in conjunction with other phenomena such as schedule-induced behavior. Other aspects may come to be understood in terms of stimulus functions other than those of simple associative relation. For example, rather than simply eliciting conditioned responses, the stimuli that accompany primary aversive events may provide boundaries, affecting the grouping of brief events distributed over time. This sort of function is illustrated by

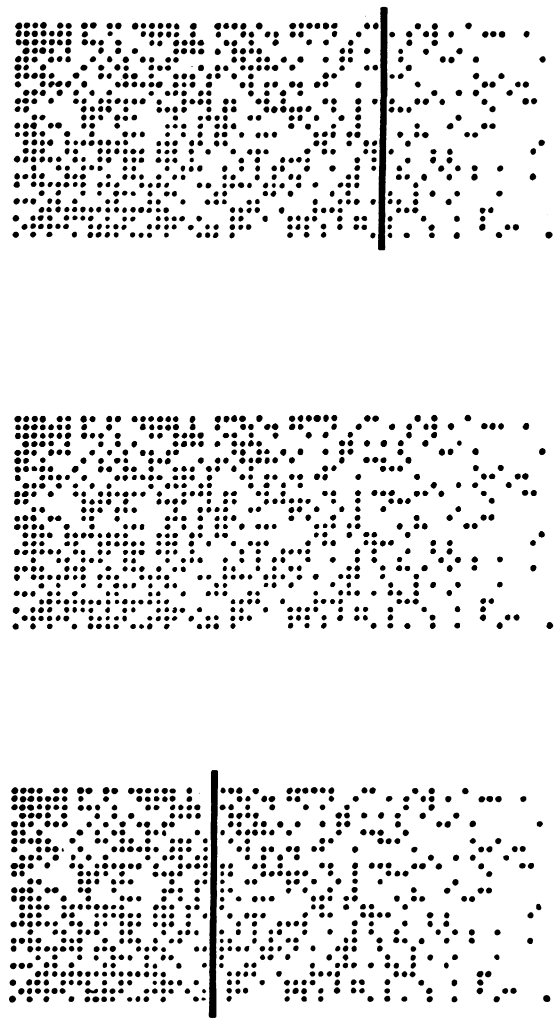

Fig. 1. Three identical arrays of dots, with gradually increased spacing from left to right. In the top and bottom arrays, vertical lines have been added to demonstrate boundary effects, resulting in the gradual density change being "regrouped" as dichotomous dense and sparse areas. (Reprinted from P. N. Hineline, The several roles of stimuli in negative reinforcement. In P. Harzem & M. D. Zeiler (Eds.), *Advances in the Experimental Analysis of Behavior: Vol. 2. Predictability, Correlation, and Contiguity*. Copyright 1981. Reprinted by permission of John Wiley & Sons.)

the visual analogy in Figure 1. Here small dots are arrayed with density decreasing from left to right. In the middle part of the figure this gradual change is readily distinguishable as such. However, when a vertical line is added, the array tends to be perceived as distinct dense and sparse areas. I have argued elsewhere that some of the phenomena of avoidance may be understood with respect to analogous boundary effects on the time continuum, with "warning stimuli" or other "neutral" intruded events determining the effective grouping of shocks (Hineline, 1981a). Such delineations of integration over time are a likely focus for future research; they may enable us to achieve complementarity between molar and molecular scales of analysis.

Two-Process Avoidance Theory

As I noted earlier, two-process avoidance theory is the second major involvement of respondent conditioning and emotional concepts in the aversive domain. "Avoidance" has been taken as a special problem of explanation; even "purists" among behavior analysts have seen it as requiring special theorizing in addition to functional analyses. The apparently crucial, defining feature of avoidance is the absence of contiguous consequences of behavior—of behavior that is clearly maintained by its remote consequences. And the task taken on by most avoidance theorists has been to supply plausible contiguous consequences that could be maintaining the behavior. Thus, the Pavlovian relationships that are embedded in virtually any procedure (response-consequence relationships are always accompanied by stimulus-stimulus relationships) have been portrayed as producing respondent behavior with a special mediating role. I shall not recount the details of two-process theory here, nor shall I repeat the arguments that have echoed through the history of theorizing about avoidance. Suffice it to say that two-process, mediational theory has persisted in modern, advanced, even behavior-analytic textbooks in spite of the fact that data seriously compromising it have been accumulating since 1957: (a) independent indices of Pavlovian conditioning embedded in avoidance do not correlate well with avoidance performance (e.g., Kamin, Brimer, & Black, 1963); (b) attempts at independent validation of two-process mediation are intrinsically susceptible to artifact (Black, 1971); (c) Pavlovian conditioning with aversive stimuli does not produce unitary effects that can be given a predictive mediating role in the control of other behavior, for different indices of such conditioning are completely dissociable (Brady et al., 1969); and (d) the theory is silent regarding whole categories of variables that strongly affect performance—variables that relate to response requirements, and that suggest situations in which one commonly speaks of avoidance, and that might contribute to its definition (e.g., Krasnegor, Brady & Findley, 1971; Sidman, 1957; Sidman & Boren, 1957a, 1957b).

Anger's (1963) account should be acknowledged as the most thorough two-process interpretation of avoidance that has appeared within the behavior-analytic literature. It is consistent with behavior-analytic tenets partly in its finessing the suggestion of any peculiarly emotional or experiential characteristics of the respondent conditioning that it proposes as mediating operant reinforcement. But in my view it is less characteristically behavior-analytic in its distinct appeal to contiguous causation in the interpretation of behavior. This adherence to contiguous causation also figures prominently in Dinsmoor's "avoidance theory of punishment" (1954), which I mentioned earlier. I belabor this point because a major innovation of behavior-analytic theory (Skinner, 1931, 1935) was its departure from mediational, connectionistic interpretation when dealing with the simple reflex. Definition and analysis of the operant can be treated analogously and can be kept free of dependence on contiguous causation (Catania, 1973b). Interpretive stances with respect to causation and noncontiguous events are currently important as fundamental bases of difference between cognitive and behavior-analytic theory (e.g., Lacy & Rachlin, 1978; Marr, 1983; Morris, Higgins, & Bickel, 1982). The issue is especially relevant in the present context, because assumptions of necessarily contiguous causation appear to contribute to the persistent adherence to two-process theory. Yet this feature that appears to have justified two-process theory is also a feature that, if adequately confronted, points the way for conceptual development in behavior-analytic interpretation in all domains, not just the aversive one. In addressing this type of development, I shall also sketch an alternative accounting of the facts that are involved when we speak of avoidance (Hineline, 1977, 1981a), which prompts a return to the more basic topic of negative reinforcement.

NEGATIVE REINFORCEMENT AND SCALES OF ANALYSIS

"Avoidance" and "escape" have been taken as standard defining categories within the domain of negative reinforcement; they are easily understood through their similarity to vernacular usage. However, when one speaks of escape and of avoidance, one speaks of two parts of a continuum of events distributed more or less densely over time. "Escape" refers to response-produced termination of some continuously occurring aversive stimulation. But if instead of being continuous, that stimulation is pulsed once per second, or once per 2 s, we still speak of escaping from it. But what about once per 4 s, or 8 s, or once per minute? At some point we tend to speak of avoiding events rather than of escaping from them. However, "avoidance," too, is a label that appears in a variety of situations. We avoid getting colds, we avoid particular people, and we avoid particular places. Although some of this variation can be characterized in terms of the ancillary cues that are involved, a major dimension is the degree to which the relevant events are arrayed over time.

Morris et al. (1982) have neatly characterized a way in which this time continuum can be viewed in a functional account, unencumbered by assumptions of contiguous, mediational causation:

> Just as the power of a microscope must be adjusted as a function of the phenomenon under study, so too does the level of behavior analysis need to be adjusted to the functional unit of behavior-environment interaction. To be specific, when order is not apparent at a molar level, a more molecular analysis may be necessary. . . . Conversely, if one fails to find an immediate stimulus that controls a response, perhaps the response is only an element of a larger functional unit which is controlled by currently operating variables not immediately attendant to that element. (pp. 119-120)

In the study of negative reinforcement, this approach to analysis was forced by the apparent sensitivity of behavior to noncontiguous consequences, as indicated by the following sketch of recent history.

On the basis of some data from a concurrent, two-operant procedure, Sidman (1962) provided the initial suggestion that a molar variable, shock-frequency reduction, was the effective variable in avoidance. Herrnstein and Hineline (1966) translated his suggestion into a procedure that explicitly manipulated shock frequency without the usual attendant moment-to-moment regularities in the occurrence of shock. That is, the subject could receive either of two sequences of brief shocks; each was of constant overall frequency (being based on the sampling of a probability distribution every 2 s), but the two frequencies differed (being based on distributions with differing probabilities). Assessed from moment to moment, shocks occurred irregularly but with constant probability over each respective sequence. The subjects' responding changed the sampling from one distribution to the other, and this could produce shifts from a higher to a lower frequency, but responses could still be followed immediately by shocks. This procedure was extremely effective for the production and maintenance of rats' lever pressing.

Although this was presented as evidence that shock-frequency reduction per se can constitute reinforcement, some theorists have interpreted the experiment in terms of smaller-scale variables—time between shocks and time from response to shock (e.g., Dinsmoor, 1977). To be sure, this provides orderly relationships and is perhaps easier to discuss in terms of mediating mechanisms. But those time intervals vary and thus must be averaged: The resulting account is merely a translation of shock frequencies into their reciprocals. The molar/molecular issue, then, could be taken purely as a matter of theoretical taste if the above were the only experiment at issue.

Fortunately, there are additional relevant experiments. I interpreted the above experiment in terms of shock frequencies; however, it seemed that there could be circumstances in which short-term consequences would be operative. The question, then, was not which account was correct, but rather which level of the time variable was more effective in which circumstances. In an initial experiment dissociating short-term postponement of shock from changes in overall shock frequency (Hineline, 1970), short-term postponement maintained responding when the overall frequency remained constant, but it did not maintain responding when the cost of short-term postponement was an increase in overall shock frequency. Gardner and Lewis (1976) developed a more flexible set of procedures for dissociating short-term postponement from overall shock frequency. Their techniques were refined further in a pair of experiments by Lewis, Gardner, and Hutton (1976), which nicely portray a direction for future research.

In their procedures, all shocks were preceded by a 4.5-s tone, and responses that occurred within 0.5 s following a shock were ineffective. These two features served to rule out elicitation and preference for signaled versus unsignaled shock as bases for observed responding. In the absence of responding, brief shocks occurred at regular 30-s intervals in what was characterized as the "imposed situation." An effective response produced an immediate transition to a 5-min "alternative situation," accompanied by a light and clicker. Responses were ineffective during the alternative situation and 10 shocks were delivered during the 5-min period, thus holding the overall shock frequency constant at two per minute. But while overall shock frequency remained constant, shocks were redistributed within the 5-min alternative situations according to two different procedures. In one, the first shock in the alternative situation was delivered just as if no response had occurred. The remaining nine shocks were postponed until near the end of the alternative period, where they were delivered at a rate of one per 5 s. Stable responding was established and maintained in all four naive rats placed on this procedure.

In the second procedure, the first *two* shocks of the alternative condition occurred just as if no response had occurred. The remaining eight shocks were postponed until near the end of the 5-min period. Thus in both procedures, responding affected the short-term distribution of shocks; however, in one, the change of distribution occurred after one postresponse shock, and in the other, the change occurred after two postresponse shocks that were delivered by a timing cycle identical to that of the imposed situation that the response had eliminated. In the first experiment, responding was produced and maintained through consequent changes in distributions of shock, but without reductions in overall shock frequency, and without immediate postponement of shock. The second experiment indicated limitations on the degree to which consequences could be separated from behavior and still be effective.

However, Mellitz, Hineline, Whitehouse, and Laurence (1983) recently reported an experiment that substantially increases the evident time scale of potential interaction between behavior and aversive events. Rats were given initial training on Sidman's (1953) shock-postponement procedure, but with two levers each feeding equivalently into the single control circuit. Access to the two levers was manipulated to ensure some distributing of responses between them, but for each animal a greater proportion of responses consistently occurred on one lever or the other. Then, each response on the previously nonpreferred lever postponed shock as before, but also subtracted 1 min from the duration of the session. Responses on the previously preferred lever merely postponed shocks as before. The added contingency was disabled during the final 2 min of the session, ensuring that the session-shortening response could not differentially produce immediate end of session. Responding systematically shifted to the lever that could shorten the sessions; the effect was verified through repeated reversals.

The general question that these experiments raise is that of the principles whereby and the scales over which events are effectively integrated over time in the control of behavior. The results sketched above for rats, and similar experiments with pigeons (Gardner & Lewis, 1977), indicate tradeoff between short-term postponement and overall shock frequency over time periods of a few minutes' duration. The experiment by Mellitz et al. (1983) extends the range of effective time scales. Figure 1, and its attendant discussion, suggest one basis for predicting the scale that will be appropriate to particular circumstances.

BEHAVIORAL SITUATIONS AS AVERSIVE

In addition to considering varied time scales whereby negative reinforcement is effective, we need also to examine the range of events that function as aversive. Traditionally, this issue has been raised in questioning the use of electric shock as the typical form of stimulation used in experiments on aversive control. Other, perhaps more biologically valid, types of stimulation have been considered and occasionally tried (see Hineline, 1977, p. 367), but most have been difficult to manipulate effectively within experimental situations. Thus, shock remains the typical form of stimulation used. We should continue to look for alternatives to shock, but the issue also needs to be addressed by asking more broadly what ranges of events—and particularly, what arrays of or relations between events—may function as aversive. This leads to a consideration not only of discrete aversive stimuli, but also of aversive situations. Baum (1973) introduced the notion of "behavioral situations" as including sets of events, often with correlated, delineating stimuli, but also as defined partly by contingent relations between behavior and those same events. Strong evidence supporting such an analysis of aversive control is found in experiments by Sidman and Boren (1957a, 1957b), Sidman (1957), and by Krasnegor et al. (1971). Each of these revealed orderly, dynamic interactions between multiple contingencies whereby behavior was affected not only by its postponing or preventing shocks within a situation, but also by contingencies relating to transitions between situations.

For example, Krasnegor et al. (1971) exposed rhesus monkeys to a recycling sequence whereby in the absence of responding, a 30-s blue light was followed by a 30-s green light, followed by a 3-s red light accompanied by three brief shocks. This in turn was followed by a 27-s blackout and then a return to the beginning of the 90-s cycle. Fixed-ratio (FR) schedules were operative during the blue and green periods; during initial training 30 lever presses during either the blue or green period (with the count starting over at any stimulus change) turned off the lights for the remainder of the 90-s cycle and prevented the shocks. The two monkeys in the experiment received very few shocks once initial training was achieved. The main parts of the experiment involved manipulating the numbers of responses required for producing blackout. When the FR requirement was varied in the blue situation, with the FR in the green situation constant at 30, responding in the blue situation (measured as number of FR completions) varied inversely with ratio size, while amount of responding in the alternative (green) situation varied as a direct function of that FR value. In complementary fashion, when the FR value was manipulated in the green situation, responding in *that* situation varied inversely with ratio size, while responding in the alternative (blue) situation varied directly. When ratios were equal in the two situations, responding occurred primarily in the green situation, which was the one proximal to red (in which shocks occurred). Thus, the effects of FR manipulation were seen as much in the alternative situation in which the FR value was held constant, as in the situation in which the ratio requirement was manipulated.

This experiment, along with the more extensive set of experiments by Sidman and Boren noted earlier, permits a statement of a set of summarizing principles that could replace traditional avoidance theory:

(1) Negative reinforcement is to be understood in terms of transitions between situations as well as by postponement or prevention of events within a situation. In some cases, a continuously present aversive stimulus defines such a situation; in other cases, the situations will be partly defined by additional, delineating stimuli and by operant contingencies that are in effect only during the situation.
(2) Relative aversiveness of a situation (the degree to which transitions away from it will reinforce behavior) depends only partly upon primary aversive stimuli that occur within the situation. Even when those stimuli do contribute to aversiveness, a relevant feature is the relation between their short-term versus longer-term distributions over time.
(3) Relative aversiveness of a situation depends substantially upon: (a) contingencies ("work requirements") in that situation, but also (b) contingencies ("work requirements") in alternative situations.
(4) Most importantly, the role of the alternative situation(s) depends upon contingencies regarding change of situation (i.e., upon "what is involved in getting from one situation to the other").
(5) All things being equal, performance tends to allow persistence of the situation closer to primary aversive events.

This account deals with a substantial range of relationships upon which traditional avoidance theory is silent. Added stimuli surely will have respondent conditioning effects of the sort to which two-process theory appeals, but these need not be viewed as having mediating functions of the sort posited by that theory. Rather, they simply occur concurrently. The added or superimposed stimuli often can be viewed as discriminative stimuli, but as noted earlier with respect to Figure 1, they have additional, delineating roles, sharpening the boundaries between behavioral situations. They help to delineate changes in the frequency of some event, changes in the contingent relation between behavior and events, or changes of opportunity for some particular type of behavior to occur. In this role, added cues may help to determine the effective integration of events, and thus the appropriate scale of analysis.

RELATED PHENOMENA AND PRIORITIES FOR FURTHER RESEARCH

There are strong commonalities between the research and interpretations sketched above, and comparable study of positive reinforcement. For example, behavior and situations described under the rubric of "self-control" are characterized by simultaneous immediate versus long-term consequences of behavior (Ainslie, 1974; Rachlin & Green, 1972). The procedures for studying these are formally similar to those sketched earlier for dissociating short-term delay of shock from overall shock frequency. Additional work by Shull, Spear, and Bryson (1981) also has examined analogous appetitive effects. On the interpretive side, Fantino's (1981) "delay-reduction hypothesis" is essentially an attempt to assess short-term effects in the context of longer-term aggregates of events. The contributions of "work requirements" to reinforcing properties of behavioral situations have been assessed in the appetitive domain through use of concurrent-chain procedures. Although frequency and immediacy of primary reinforcers usually have been found to be crucial, there have been some instances in which contingencies, per se, in the second links of concurrent chains have contributed to choices of those second-link situations (Moore, in press; Moore & Fantino, 1975). Most saliently, the issue of scales of analysis—which identifies a major basis whereby "avoidance" has been treated as a distinct domain—has emerged as a point of interpretive contention in relation to positively reinforced behavior. Thus, study of aversive control should not be viewed as the examination of phenomena peculiar to that area, but rather as the examination of processes possibly common to all behavior.

In summarizing my suggestions for continued work along the lines I have indicated, I would add some additional points regarding future directions:

(1) A key focus for further research should be the analysis of integration of events over time. "Integration" here is to be understood in the sense of integral calculus. If a class of behavior, B, is partly a function of events, E, arrayed over time, this can be expressed as the definite integral,

$$B = \int_{t_1}^{t_2} f(E)\, dt.$$

Evaluating the integral for different values of t_1 and t_2 can reveal the range of times over which the particular class of behavior is sensitive. Experimentally, this translates into procedures that identify specific values of t_1 and t_2 between which the specified events affect the behavior in question, and outside of which the behavior is unaffected. In conjunction with the quantitative evaluation, we need techniques for efficiently identifying the most effective scales of analysis for particular situations, thus establishing the complementarity of molar and molecular analyses.

(2) Appropriate complementarity can be illustrated through the metaphor of a steel bridge. The properties of the bridge can be understood partly in terms of features of each piece of steel—its tensile strength, resistance to abrasion, shearing strength, expansion with increased temperature, and the like. Small-scale analyses might involve the shapes of pieces (I-beams, angle pieces, flat plates), focusing on their functional properties, along with those of welded and bolted joints. These "molecular features" tell us little about the relationships whereby the bridge spans the river—cantilevers, trusses, structurally rigid triangles, and the like, which correspond to more molar analyses. Finally, to understand the behavior of the bridge during violent weather, we need to include airfoil effects and the resonant properties of the bridge oscillating as a whole—the extreme of molar analyses. The molecular analyses inform us regarding the strengths of component units, but leave us ignorant of what stresses will test those strengths, or when those stresses will occur. The molar analyses can predict some stresses, and the intermediate analyses can predict others. The intermediate and molecular analyses will combine in predicting outcomes resulting from stresses, whether of molar or intermediate origin. Only a consideration that uses these various scales of analysis in complementary fashion can assess adequately the functioning of the bridge.

(3) We need further analyses of "situational aversiveness" based upon features other than frequency of primary rewarding or aversive events. For example, as my colleague Timothy Hackenberg has pointed out (personal communication, 1984), Skinner (1969) has asserted that assembly-line workers' performance is not controlled appreciably by positive reinforcement but rather by "preventing the loss of a standard of living" (p. 18). Although the assertion is plausible, I know of no data that convincingly support it.

(4) We sorely need to reformulate our interpretation of what traditionally has been called "respondent behavior." The concept of elicitation is no longer adequate for characterizing the dynamics of environment/behavior interaction in nonoperant behavior. I would hope that this reformulation could

include phenomena such as adjunctive and schedule-induced behavior, as well as simple reflexes.

(5) The reciprocity between behavior and environment should be made a more salient feature of our analyses. Some types of behavior are well construed as selecting environments, even while those environments select behavior. For example, a current experiment in my laboratory involves operant conditioning of a computer's behavior (by means of an "adjusting schedule"), while that behavior is modulating schedule-induced behavior of an albino rat.

(6) Although I have not mentioned it above, we need more emphasis on the analysis of transient phenomena. Behavior analysts have provided some examples showing how to address such phenomena—as in Azrin's (1960) study of transient effects of punishment, and in the work on "behavioral momentum" by Nevin, Mandell, and Atak (1983)—but most of our experiments stress steady-state performance. This forms marked contrast with the early experiments that documented the importance of the reinforcement principle through demonstrations of rapid and reliable shaping of behavior.

(7) Finally, we need to continue working on "meta-theory," clarifying for ourselves and for others the constructive features of our viewpoint. We are too often known mainly through our rejections of commonly held views. Identifying legitimate commonalities with viewpoints may enable us to talk and write more effectively about our own experiments, interpretations, and applied analyses. Besides making those endeavors more effective, a crucial priority should be that of communicating them more effectively to those who come from other traditions.

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