*VARIED APPROACHES TO AVERSION:
A REVIEW OF AVERSIVE CONDITIONING AND
LEARNING,¹ EDITED BY F. ROBERT BRUSH*

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The book is a collection of nine independently written chapters, organized within the general topics of "Aversive Conditioning", "Avoidance Learning", and "Problems of Punishment". Most chapters integrate data from both free-operant procedures in within-subject designs and discrete-trial procedures in between-group designs, often with healthy de-emphasis of the chapter authors' own experiments. The chapters represent a variety of theoretical viewpoints, some of which are directly relevant to the experimental analysis of behavior—notably A. H. Black's discussion of the operant-respondent distinction in light of data on autonomic aversive conditioning; Robert C. Bolles' account of conditioning problems and phenomena in terms of species-specific defense reactions; and James S. Myer's integrative treatment of noncontingent aversive stimulation. Other expressed viewpoints, while somewhat inimical to explanation solely within the realm of behavior and its controlling variables, do not stand in the way of this approach, for the theorizing is clearly separated from the description and organization of data. Among these are the chapter on "Unpredictable and Uncontrollable Aversive Events", by Martin E. P. Seligman, Steven F. Maier, and Richard L. Solomon; "Retention of Aversively Motivated Behavior", by Brush; and "Suppression and Facilitation by Response Contingent Shock", by Harry Fowler. In the remaining chapters, data and theory are much more intertwined. They will be of interest primarily to those concerned with the details and ramifications of the particular theories or experimental

procedures under consideration: Warren R. McAllister and Dorothy E. McAllister on "Behavioral Measurement of Conditioned Fear" (with conflicting data from four methods for assessing effects of pairing stimuli with shocks); John Theios on "Mathematical Models for Aversive Conditioning" (dealing only with discrete-trial shuttle procedures); and M. Ray Denny on "Relaxation Theory and Experiments" (using conditioning and extinction of a jump-out response, to test a theory in postulatory form).

While the variety and independence of the various chapters can be construed as a strength of the book, they preclude much discussion of the book as a whole. Hence, I shall discuss a few chapters separately, dealing first with those I found best. Later, I will deal with some issues of theory and theorizing that arise frequently throughout the book, and that have produced divisiveness and lack of communication among students of conditioning.

In the opening chapter, A. H. Black begins by drawing careful and potent distinctions and by establishing basic definitions that give broad implications to his topic, autonomic aversive conditioning. An optimistic tone is set, hinting that the whole respondent-operant issue will be laid out clearly and finally, even though the author professes a more modest goal. The chapter then proceeds in a thorough and systematic fashion, dealing with both operant and Pavlovian conditioning procedures. But alas, the elegant layout of distinctions and data does not result in neat systematic conclusions. Rather, the chapter ends in a mood of futility, asserting above all that "the facts of autonomic conditioning do not support a distinction between fundamental behavioral processes that correspond to the two classes of conditioning procedures." Black moves from a primary concern with theoretical issues to an

¹New York: Academic Press, 1971. Pp. xii + 626 \$25.00.

²Preparation of this manuscript was supported by PHS research grant 1 RO-1-MH-18432-01 to Philip N. Himeline. Reprints may be obtained from the author, Department of Psychology, Temple University, Philadelphia, Pennsylvania 19122.

advocacy of empirically oriented research on patterns of autonomic and skeletal responses during conditioning, rather than on tests of "theoretical notions concerning classical and operant conditioning which are either outmoded or better studied by some other means."

Along the way, Black asserts that our fundamental understanding of Pavlovian conditioning is inferior to our understanding of operant conditioning—mainly because of the lack of viable principles for response specification and the lack of adequate methods for eliminating possible operant artifacts in Pavlovian procedures. Consequently, Black questions the advisability of treating Pavlovian conditioning as a simpler, more basic process as is commonly done in two-factor theories. He also questions whether we ought to regard as a single phenomenon, all that has been called classical conditioning—noting especially the difference between modulating operant behavior (as in the Estes-Skinner paradigm) and eliciting a discrete response. He moves on to a criticism of two-factor avoidance theories on logical grounds:

"On the one hand, researchers look for a difference between autonomic and skeletal responses in order to justify the use of the autonomic response as an index of classical conditioning On the other hand, they look for a positive relation between autonomic responses and skeletal operant responses in order to justify the hypothesis that the autonomic response motivates the operant response. *Therefore, the more successful they are in demonstrating the independence of the autonomic and skeletal responses in order to justify using the autonomic response to measure classical conditioning, the more they provide evidence which contradicts two-factor theories.*" (pp. 92-93; Black's italics)

With this observation, Black points out a difficulty that besets most mediational theories of conditioning.

I am reluctant to criticize features of Black's chapter that result from its thoroughness, for it covers virtually all extant issues regarding autonomic *vs* skeletal conditioning, and their relation to the operant-respondent distinction. Still, it must be noted that some aspects of the

presentation made it difficult reading. First, although at the beginning the author clearly indicated how he was going to approach the material, it was difficult to keep track of the overall organization while dealing with details of specific issues. In short, this chapter, like most others in this book, needs a detailed outline at the beginning of the chapter to supplement the more general table of contents. Parts of the chapter are needlessly expanded and complicated by placement of extensive methodological and theoretical discussions in advance of the relevant data. This often results in elaborate concern with distinctions that turn out to be trivial. In addition, the author gives little indication of which questions or theoretical propositions he finds particularly plausible or significant. Only at one point does he comment that "one is always uneasy that the difficulties in interpretation raised are niggling and, perhaps, implausible, . . ." (p. 63). Since this chapter is not primarily a history, fine and subtle theoretical arguments could have been reserved for issues not resolved by data in hand.

Finally, the limitation of this chapter to aversive conditioning, as required by the defined area of the book as a whole, was at times unnatural (in fact, the limitation was not consistently observed). Most of the definitions, distinctions, and conclusions of this rigorous chapter apply equally well to all conditioning, rather than just to aversive conditioning.

Concealed behind a modest title, "Some Effects of Noncontingent Aversive Stimulation", is an outstanding contribution. The author, James S. Myer, begins by enumerating the variety of effects commonly attributed to aversive stimulation, noting that they are unlikely all to occur at once, and then methodically describing which effects occur when. He treads on some commonly held assumptions, and in so doing creates order out of potentially chaotic data. For example: it has been pointed out by some investigators that virtually all stimuli become aversive at high intensities, with the implied assumption that all intense stimuli are equivalent in their aversiveness. Myer shows, however, that these should not be considered interchangeable, for animals' reactions to different kinds of aversive stimulation vary systematically. He distinguishes two classes of stimuli: proximal and distal, and assembles data that support a guiding principle:

aversive stimuli on the surface of the body produce skeletal activity that tends to remove the organism from the source of stimulation, whereas aversive stimuli sensed through distance receptors produce a suppression of ongoing behavior. Hence, if a somesthetic stimulus is demonstrably aversive either in escape or in punishment situations, the unconditioned response to its presentation will be activation rather than suppression of skeletal activity. In contrast, the natural reaction to visual and auditory stimuli, whether conditioned or unconditioned, is a cessation of skeletal activity. This principle combines well with a later caution against extrapolating too readily from shock to all other aversive stimuli. There he cites taste-aversion data, showing that nausea is readily associated with tastes and odors but not with visual and auditory stimulation, while shock is readily associated with visual and auditory stimulation, but not with tastes and odors. In another neatly executed section, Myer provides a brief historical treatment of the concept of "fear", dovetailing with an argument against over-extension of that concept, which we shall consider later in this review.

Finally, Myer's chapter is enhanced by the inclusion of data, partly from his own laboratory, that interrelate the effects of contingent and noncontingent shocks, combining conditioning procedures with response measures derived from study of species-specific behavior. Such ecumenical work provides a refreshing contrast to the conceptual fragmentation that often characterizes our field.

The chapter by Robert C. Bolles, on species-specific defense reactions, is written with the lucid style and well-documented arguments that we have come to expect from its author. Still, if the reader is interested mainly in the rationale and main features of Bolle's formulation of conditioning experiments, I would recommend his recent review article (1970), which sets species-specific defense reactions in a biological, naturalistic context, applying them primarily to avoidance conditioning procedures. The present chapter is an expansion of the same basic idea, with less naturalistic argument and more detailed organization of experimental procedures, including but extending beyond the realm of avoidance conditioning. His arguments deal primarily with the acquisition of behavior; extension to the main-

tenance of behavior must be done only with care. Still, the chapter has considerable generality; several sections provide a different perspective on procedures that are also dealt with elsewhere in the book. The thorough literature review makes this an excellent reference chapter, whether or not one finds all of the accompanying arguments convincing.

The Editor's chapter is entitled "Retention of Aversively Motivated Behavior". In it, Robert Brush deals with a limited but puzzling set of phenomena characterized by non-monotonic retention functions. The reference experiment is one by Kamin, who found in 1957 that when shuttle avoidance training was interrupted for an hour, completion of training was slower than if the interruption was shorter, longer, or than if there was no interruption at all. The story told in this chapter is clearly a live and unfinished one, in which some of the requisite antecedent conditions for this U-shaped function are identified, and various explanations are attempted but found wanting. Along the way, some suggestive similarities are seen between the retention of avoidance and effects of other aversive conditioning procedures, and it is shown that some non-associative manipulations affect the U-shaped functions. This latter fact leads to Brush's speculative appeal to pituitary-adrenal activity as an underlying mechanism. Irrespective of theoretical views, this chapter contains behavioral data that will suggest fresh and different observations in many laboratories where aversive conditioning is under study.

Seligman, Maier, and Solomon begin their treatment of "Unpredictable and Uncontrollable Aversive Events" with an excellent exposition regarding the specification of predictability and unpredictability of stimuli in Pavlovian procedures, and of controllability and uncontrollability of reinforcers in instrumental procedures. In so doing, they prepare the way for dealing with predictability and controllability as independent variables, measurable and manipulable along continua. It is disappointing, in light of this, that they then deal with dichotomous groups of procedures, rather than with the continua which their initial specifications suggest.

In the case of controllability they use the dichotomy to justify using preconditioning procedures without examining the fundamental nature of the conditioning procedures

themselves (partial controllability would constitute conditioning rather than a preconditioning manipulation). However, I am left with the feeling that in so doing they have not finessed the problem, but have rather created the illusion of explanation where there is none. They translate their dichotomized variable into a theoretical-sounding assertion: "subjects can learn that an unconditioned stimulus is unpredictable or uncontrollable", describing the presumed nature of the engram rather than of the controlling variable. In repeatedly using the phrase "subjects *learn that* . . .," they gloss over the learning process. In a similar vein, in describing situations where animals tend to respond or not to respond in a particular way, these authors translate tendency to respond into "having an incentive to respond", thus replacing a measurable tendency with a concept that, as used, accomplishes only the illusion of explanation. Some of these issues regarding explanatory style will be considered in more detail below, in relation to the chapter on conditioned fear.

If one accepts the authors' labels only as approximate characterizations rather than as explanations, their main notion becomes more tenable. "Helpless" does seem to characterize some animals that have been exposed to uncontrollable shock. Further, the authors organize their data well, identifying a variety of effects of uncontrollable or unpredictable shocks on subsequent conditioning, and on preference, stress, and other dependent variables. Still, I do not agree with their assertion that "conditioned helplessness" is an adequate theoretical concept, or that it is likely to suggest more good experiments than would the mere use of unpredictability and uncontrollability as characterizations of independent variables.

Harry Fowler's chapter on "Suppression and Facilitation by Punishment" emphasizes the facilitative effects of punishment in order to examine the extent to which the more usual, suppressive punishment effects can be attributed to something other than a basic suppressive process. This emphasis also serves the useful function of gathering together a substantial number of these "paradoxical" data in one place. He begins by contrasting what he calls an operational (transituational) definition of punishment, with a functional definition. For his own part, referring to "facilitative effects of punishment", he has no choice but

to use the operational definition because a functional definition identifies facilitative effects with reinforcement, rather than with punishment. Using transituationality, he defines as punishment the response-contingent delivery of a stimulus that has been shown to be aversive in a different situation.

Moving on to data, the cue, or discriminative properties of shock delivery are clearly and systematically described, permitting one to summarize functions that shock shares with other stimuli: Punishment can provide discriminative stimuli for appetitively reinforced responding, for escape, or for avoidance. Shocks need not be contingent in order to function discriminatively; on the other hand, response-contingency itself can have a discriminative function. Fowler attempts to tease apart discriminative from reinforcing functions, and supports a "distinctive cue" function, although the precise properties of this function are not made clear.

In the subsequent section, on response-eliciting properties of punishment, the data fall roughly into place with a Guthrie interpretation: ". . . how the animal learns to respond to shock, both in manner and magnitude, constitutes a major determinant of the punishment effect." (p. 573). In an attempt to go beyond this, Fowler invokes the "fear" concept, which ultimately leads only to a vague concluding sentence about how the manner in which fear is produced, and the manner in which it elicits, maintains, and intensifies behavior must be used to account for enhancement of punished behavior. The futility of this is illustrated elsewhere in the book: an appeal to stimulus substitution for the establishment of fear is discredited in Black's chapter, and an argument for summation of fear with appetitive drive is found by Myer to be too tricky to be useful in explanation, for it is difficult to predict when the summation will or will not occur. Further, the McAllisters' chapter, which is substantially devoted to these issues, falls far short of the clarification that Fowler requests.

Discussion of the "escape-permitting function" of shock provides a convenient occasion to assemble experiments on the "vicious-circle phenomenon", which are followed by final, theoretical summaries, set apart from data. Clearly, some of the suppressive effects of punishment derive from discriminative and other non-suppressive processes. Fowler interprets

any residual effects of punishment as escape conditioning. However, this interpretation has been of little use for predicting the functional properties of punishment.

"FEAR" vs "CONDITIONED AVERSIVE STIMULI"

Chapter 2, by Wallace R. and Dorothy E. McAllister, is entitled "Behavioral Measurement of Conditioned Fear". The basic assumptions of the chapter and its authors' preferences for style of explanation are shared by several other contributors to the book. The same assumptions and preferences are questioned or challenged by other contributors to the book, and by the editorial practice of this Journal, raising some issues regarding theory and explanation of aversive behavior. Although other terms are also sometimes involved, the issues are well characterized by arguments regarding technical use of the term, "fear". Hence, the manner in which fear is defined, the difficulties it produces, and the arguments for and against its use are examined here.

McAllister and McAllister define fear, typically used, as a classically conditioned, internal, unobservable response, accessible only through indirect measurement. Unflinchingly, they state the conditions for dealing with this concept:

"Any change in an observable response which follows the presentation of the CS and which is the result of the appropriate pairing of the CS and the noxious UCS can potentially be used to measure fear. . . .

"The assumption underlying the indirect measurement of fear is that a correlation exists between the magnitude of the measurable response and the magnitude of fear. This relationship may not be linear and it may differ for the various response measures. The measures may not, therefore, be highly correlated with one another, which is consistent with recent research showing that one measure may reflect fear, or differences in fear, when another does not. . . . No single measure can be considered to be superior to the others in all respects." (pp. 105-106)

In spite of the foregoing, fear is tenaciously regarded as a single, unitary entity:

"The specific responses involved (*e.g.*, bar-pressing, hurdle jumping, changes in skin resistance, *etc.*) are not uniquely associated with fear; all occur under a wide variety of other conditions and can be affected by variables unrelated to fear. Therefore, fear cannot be defined in terms of a particular response measure. The only characteristic common to all the occasions in which the presence of fear is asserted seems to be the prior appropriate pairing of neutral and noxious stimuli (classical conditioning). For this reason, fear is best defined in terms of these antecedent conditions. With this definition, any of the various response measures may then be used as an index of the relative strength of fear." (p. 106)

Recapitulating their statements, we see that a single defining operation, pairing of a noxious stimulus with another stimulus, is used to determine whether fear should be relevant to a given situation. However, following accepted psychophysical practice, the operation for producing fear is not accepted as a basis for specifying the magnitude of fear. For this, the authors require independent measures, based on the organism's reaction to the defining operation.

The viability of fear as an explanatory concept depends heavily on the reliability and validity of these independent measures. The McAllisters' acknowledgement that fear is not linearly related to its measures, and that even the assumed monotonic relation may be obscured by "other variables" in any specific situation leads one to question the feasibility of achieving adequate reliability and validity. Fear may well have a more ambiguous relation to its presumed measures than to the operations that produced it. Despite these difficulties, the McAllisters proceed, using four measures as indices of fear: (a) learning to escape from a stimulus that has been paired with shock, (b) changes in performance resulting from conditioned punishment, (c) changes in rate of an ongoing response (either suppression, as in the Estes-Skinner (1941) procedure, or acceleration, as in the procedure used by Sidman, Herrnstein, and Conrad, 1957), (d) change in magnitude of an unlearned response

as a result of presenting a stimulus that has been paired with shock. These measures share an advantageous feature: the pairing of the stimuli with shock can be accomplished in a situation independent from the situation in which the effects of the pairing are assessed. A disadvantage for the McAllisters' stated purpose, as they acknowledge, is that the various measures do not correlate well with each other. Interestingly, these measures identify phenomena that are commonly used to justify the use of fear as an explanatory concept; they, along with the effects of various avoidance conditioning procedures, constitute most of the phenomena that the concept of fear is used to explain.

Since fear is an illusive entity at best, some theorists have argued that the conditioning phenomena described above are best handled differently. A common alternative, articulated by Dinsmoor (1954), Schoenfeld (1950), and Anger (1963) accounts for (a) and (b) above in terms of conditioned aversive stimuli, rather than in terms of fear. According to this approach, a version of two-factor theory, a stimulus paired with shock becomes aversive. That is, removal of such a stimulus strengthens behavior, as illustrated in (a) above, and presentation of the stimulus weaken behavior, as in (b) above. Hence, instead of being measures of an internal entity, (a) and (b) become operations that define properties of stimuli that have been paired with shock. Presumably, (c) becomes a separately defined property as well. Each of the three operations describes a kind of effect that a stimulus paired with shock has on behavior. No one effect necessarily bears functional relation to another, although in some situations more than one may occur simultaneously. (The phenomena represented by (d) are usually ignored in this approach, and will be considered below.) The advantage of this approach, then, is to account for behavior in terms of manipulated environmental events, without having to be concerned with inferred internal events.

Several proponents of the fear concept, including the McAllisters and Fowler in the present volume, object to this alternative formulation by noting that in becoming aversive the stimulus itself does not change through its pairing with shock; what changes is the way the animal behaves in relation to that stimulus. Fowler attributes the argument to Mowrer, whom he paraphrases:

"... *conditioned* aversiveness is not a property gained by a neutral stimulus in its own right, but rather a property acquired by the subject; that is to say, the physical features of a neutral stimulus are not altered by its association with a primary aversive event like shock, but only the responses of the animal that are elicited by the stimulus." (footnote p. 575)

While the above distinction is valid, it is irrelevant to the present issue. Those making the distinction ignore the fact that even primary aversive stimuli have no aversive properties definable independently of the response characteristics of the receiving organism. In fact, the definition of stimulus itself, of whatever kind, is subject to the same distinction: when we specify a "red light", our adjective does not refer to a property of electromagnetic radiation; rather, it refers to responses—mainly learned responses—of human organisms to the radiation. Similarly, whether one speaks of conditioned aversiveness of a stimulus, or of fear evoked by that stimulus, one refers to properties of an organism's reaction to the stimulus. However, there are valid distinctions to be drawn between fear and the reactions that define an aversive stimulus. As Anger (1963) explains:

"... When a noise stimulus preceding an electric shock is said to have acquired an aversive property, this certainly does not refer to any change in the acoustic events in the environment of the animal. Instead, the 'acquisition of an aversive property' refers to a change in the reaction of the animal to the noise. Then why not call this new reaction a 'fear reaction'? Because 'aversive property' refers to just one part of a complex reaction, a precisely defined part. The whole reaction to a conditioned aversive stimulus is an extraordinarily complex reaction with many different components . . . , many of which are difficult to measure." (p. 480)

Anger goes on to assert that when dealing with the aversive properties of the stimulus that has been paired with shock, one can consider these properties apart from the others. Including the others would merely impede under-

standing of the aversion and its effects. An added argument against the "fear" formulation is one of parsimony. If a fear-eliciting stimulus is required to produce fear, and removal of that stimulus is required to remove fear, why not simply deal with directly measurable properties of that stimulus's effect on behavior? In the formulation using conditioned aversive stimuli, fear is superfluous.

Surely, many of those who adhere to the fear formulation have read Anger's rebuttal; they are no doubt aware of Dinsmoor's similar arguments as well. Yet, they still assert that the concept of fear is desirable, even necessary. They assert a different parsimony. Rejecting the formulation of the operations (a), (b), and (c) as independent phenomena delineating different properties of a stimulus once paired with shock, they claim that referring to a single entity, fear, accounts for all of these phenomena when given a few functional properties as in the Miller-Mowrer paradigm (Mowrer, 1939; Miller, 1948, 1951). Fear is said to be an acquired drive that motivates behavior; its reduction reinforces behavior. Further, fear is said to be a response that produces additional stimuli, to which other responses may be learned or which may elicit innately associated responses. (p. 106).

Assessing their application of this approach, I find that McAllister and McAllister do account for the above-described measures of fear, but only in a general and imprecise way. Their treatment produces an impression of parsimony, but, upon close examination, each application makes additional assumptions that belie this impression.

Presented in support of the "fear formulation" are a few phenomena related to effects of stimuli that have been paired with shock that are not easily handled by the alternatives described above. One of these is the McAllisters' fourth measure of fear—the effect of a stimulus paired with shock in enhancing other elicited responses. For example, a light previously paired with shock will, when simultaneously presented with loud noise, enhance the startle response to the noise. The "fear as drive" interpretation handles this simply as drive summation. However, the effects of such an added stimulus may be facilitative in one situation and disruptive in another. Those favoring the "fear" formulation find this no serious difficulty, as when G. A. Kimble stated in com-

paring enhancement effects to the suppressive effect discovered by Estes and Skinner: "The fact that conditioned fears have different, and opposite effects upon behavior poses no particular problem of interpretation. We have seen in connection with motives in general that their effects cannot be predicted without taking into account the responses elicited by the drive-establishing operation and the relationship of these responses to the responses they influence." (Kimble, 1961, p. 429). On the other hand, James S. Myer, in the present book, when actually taking these additional factors into account, finds the fear concept to be superfluous. Since one must always consider the specific properties of the stimuli involved, one may as well use these to account for the resulting behavior:

"There seems to be little reason . . . to maintain the deception that after administration of aversive stimuli there exists some state of 'pure emotionality' which is independent of the stimuli in the test situation and which can multiply with other existing drives to facilitate any arbitrarily chosen behavior." (p. 495)

In additional defense of the necessity for "fear", the McAllisters cite a study by Solomon and Turner (1962) as "a compelling argument for the use of the fear concept." In this study, dogs were first trained to press a panel, using an avoidance procedure, with a light as warning stimulus. Following administration of curare to eliminate skeletal responses and their proprioceptive feedback, tone was paired with shock and another tone was presented in the absence of shock. When tested later, performance to the light was comparable to that before curarization. In five of the six dogs, a panel-pressing response occurred upon the very first presentation of the tone that had been paired with shock under curare. Much less responding occurred when the other tone was presented. McAllister and McAllister account for this experiment as follows:

"Presumably, during the initial learning of panel pressing, fear would become conditioned to the original CS, and the response-produced cue from fear would become associated with the instrumental

panel-pressing response. Under curare, fear would become conditioned to the tone which was paired with shock, but not to the other tone. During the test phase, the former tone should elicit fear with its response-produced cue, which in turn should mediate panel pressing at the outset of training because of the association learned initially between the fear-produced cue and the instrumental response. It is difficult to see how these results could be accounted for merely in terms of the reinforcement provided by the termination of a conditioned aversive CS, in accordance with Schoenfeld's (1950) theory, since the instrumental response occurred on the first trial before reinforcement could have had an opportunity to affect performance." (p. 127)

Contrary to the McAllisters' assertion, Solomon and Turner's results can be handled without appeal to the concept of fear. One could appeal to aversiveness as a property providing the basis for generalization, arguing that one conditioned aversive stimulus will tend to be generalized with another. This argument to some degree parallels that put forward by the McAllisters except that it does not postulate a covert stimulus-response chain. Its strength is that all the parts of the argument use observable, directly manipulable stimuli. Its weakness is the fact that this generalization is hypothesized, not directly demonstrated, although supporting data could doubtless be marshalled.

More important to the controversy, theorists using conditioned aversive stimuli instead of fear tend not to deal with these data that are claimed as support for the concept of fear. To some degree, this is incidental to the fact that the two styles of explanation have been correlated with two different styles of experimentation—between-group *vs* within-subject designs. But even to the extent that they do deal with the same data, alternative treatments tend to go unnoticed by the fear theorists, for in the absence of the organizing concept of fear, the various studies that the fear theorists group as a single set are dealt with separately, in a variety of contexts. Multiple effects of stimulus-shock pairing are still considered, but only when simultaneously operative, as when presentation of the shock-paired stimulus is

made contingent upon avoidance responding. There are advantages to starting with these as separate effects of the stimulus-shock pairing: one is led to examine closely the conditions under which these effects should be considered in combination. The fear formulation has a corresponding weakness: when one begins by lumping all of the possible effects of pairing a stimulus with shock as manifestations of a single entity with a familiar name (whose familiarity derives from vernacular English rather than from understanding of the conditioning phenomena), the principles or conditions for predicting which effects will occur or be relevant in a particular situation tend to be added, almost unnoticed, on an *ad hoc* basis. Nevertheless, those who are impatient with the persisting and often *ad hoc* use of the fear formulation must acknowledge that fear theorists are probably equally impatient with conditioned aversion theorists' failure to emphasize or at least acknowledge certain types of data.

CONTROLLING VARIABLES *vs* "HOW THE SUBJECT DOES IT"

The formulations in terms of conditioned fear and in terms of conditioned aversive stimuli share a preoccupation with explanation in terms of "how the subject does it" instead of dealing directly with the prior questions: "what does the subject do, and under what conditions?" Much of this emphasis comes from the tradition of avoidance theory, where the "how he does it" question is especially tempting, since on avoidance procedures responding produces an absence of supporting external events. Anger's (1963) avoidance theory can be characterized as a statement that the animal "does it" by presenting and removing internal stimuli. Schoenfeld's (1950) early theory of aversion followed a similar vein, with dependence on proprioceptive and tactual stimuli. All of these internal stimuli share with "fear" the problems of indirect measurement, with plausibility their best defense. Theorizing in terms of "how the subject does it" is not necessarily counterproductive, and it can be construed as a method for summarizing data. However, as Herrnstein (1969) pointed out, it appears to have produced a premature limitation of the range of experimental procedures used to study aversive conditioning.

Only in breaking away from this approach and directly examining the controlling variables have we uncovered new and unsuspected ways in which a creature interacts with aversive stimuli. Schoenfeld's (1969) statement on aversive conditioning argues forcibly for this latter approach, probing toward principles for dealing with both appetitively and aversively maintained behavior.

LABELS AND EXCESS MEANING

The remaining and most divisive issue related to style of explanation concerns the inclusion of emotional labels in the technical vocabulary. The well-known experiment by Estes and Skinner (1941) provides a prestigious precedent for such inclusion, for they presented their conditioned suppression procedure as a means for measuring anxiety. However, as Schoenfeld (1969) argued, "anxiety" and even less controversial words from English vernacular—words such as learning, motivation, pain, and memory—are likely to lead to unproductive categorizations of behavior:

They 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. More often than not, they only obstruct a proper analysis of behavior by clouding issues, generating profitless debates, and side-tracking research. (p. 669)

In counterargument, it can be stated that using words from everyday English provides an intuitive advantage for understanding behavior. We often obtain insights by looking at an experiment from the subject's point of view, in relation to everyday experience. However, this does not mean that the source of our intuition should be included in our theory. The layman's terms change meanings over time, and mean different things to different people at a given time. For example, as Myer points out in providing a brief history of the term "fear", prior to Mowrer, fear denoted primarily a suppression or disruption of ongoing behavior. It was only in the service of a learning theory that fear was given an energizing

function as well, controverting the everyday meaning. Simultaneous variability in use of the term is exemplified throughout the book under review here. Some chapters use fear as explanation; some use it only as the label for a procedure in which stimuli have been paired with shocks. In some chapters, it has formal status as an intervening variable; in others, it seems to be just a word from everyday English, encouraging the reader to accept the plausibility of an explanation. Since the various meanings are not always made explicit, one can anticipate confusion arising even as these authors read each other's chapters. Encountering the emotional or everyday term, each reader supplies his own idiosyncratic definition; the writer loses control over communication.

More treacherous than the imprecision of vernacular terms is the tendency to turn description prematurely into explanation when using them, and to overrate the adequacy of that explanation. To say that a result is produced by fear has a more convincing sound of truth and importance than to say that the result was produced by a set of manipulations. To say that difficulty of conditioning is produced by helplessness sounds more explanatory than to say that it is produced by prior exposure to noncontingent events. But these words provide only illusion of explanation. Even accepting their referents, one knows less about them, or about what produced them, than about what produced the facts that they are said to explain.

Most important, argument or indignation over the use or non-use of emotional or other vernacular terminology should not divert us from realizing that, regardless of form of terminology, most of the well-worn explanatory concepts of aversive conditioning have been pushed beyond their usefulness. The present book suggests a few fairly new and perhaps viable organizing principles, and provides an impressive assemblage of experimental data. It does not provide a new and better rubric for integrating the full range of data. The few notions with generality seem to apply as well to appetitive as to aversively maintained behavior. Quite likely, in a new and more adequate order of explanation, the concept of aversiveness will disappear. Still, those building the new and more adequate order may find useful raw materials within this book.

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