THE OTHER PSYCHOLOGY OF ANIMAL LEARNING:
A REVIEW OF MACKINTOSH'S
CONDITIONING AND ASSOCIATIVE LEARNING¹

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Any student of animal learning, or more precisely of the behavior of animals subjected to conditioning procedures, quickly comes to appreciate that the knowledge on this subject has developed in what appear to be two separate domains. On the one hand are the Skinnerians, or as they recently have labeled themselves, behavior analysts. On the other are those descended from the heyday of learning theory, when influential figures like Hull, Guthrie, and Tolman debated over the nature of learning and the proper theoretical framework in which to analyze it. Numerous subdivisions make this latter set more difficult to characterize, but their common ingredient has been an interest in the *associative processes* underlying conditioning effects. Since at least the middle 1950s these two traditions have developed largely independently in terms of the methodologies employed, the experimental issues addressed, and the explanatory concepts used to interpret their empirical findings.

The reasons for this separation are varied and not easily understood. According to the folklore of behavior analysis, the separation was the result of discrimination by associative learning theorists against Skinner and his early disciples, whose dedication to positivism and use of single-subject methodologies made their analysis of learning phenomena clearly deviant from prevailing practices. Early advocates of behavior analysis thus labored with little reinforcement from the mainstream of experimental psychology, and it was not until the

founding of this Journal that a forum was provided that facilitated research in the new tradition without interference from reactionary elements. Such an account is of course simplistic. For example, it is apparent from the reception of Skinner's early research that he was highly regarded from the outset (see Skinner, 1979, for his account of his early career), as attested by his receiving the prestigious Warren Medal from the Society of Experimental Psychologists in 1942 (two years prior to Clark Hull receiving the same award) at the age of only 38. This was soon after the publication of his book *Behavior of Organisms* in 1938, which was widely cited shortly thereafter in the major textbook in the field of conditioning and learning of its period (Hilgard & Marquis, 1940). Moreover, Skinner's book received generally favorable reviews (Finan, 1940; Hilgard, 1939; Jones, 1939; Wolf, 1939), although the reviewers were clearly more pleased with his new findings and methods than with his positivistic approach and the limited attention given to the efforts of other investigators working on similar problems. The fact that Skinner was soon offered a position at Harvard is further evidence of his professional prominence. It seems, therefore, that Skinner's work received a great deal of attention even during its seminal stages; he and his disciples founding a separate "school" of animal learning cannot be ascribed simply to the lack of a receptive audience.

At least part of the separation was due to the rise of schedules of reinforcement as the primary topic of investigation during the 1950s coupled with direct attacks by Skinner and others during the same period on the hypothetico-deductive approach to experimentation. Together these fostered a philosophy of research that emphasized an inductive analysis of maintained behavior in which regularities in behavioral control by reinforcement contin-

¹ Mackintosh, N. J. (1983). *Conditioning and associative learning*. New York: Oxford University Press. vii + 316 pp.

Preparation of this paper was supported by NSF grant 84-00878. Thanks are due Michael Morgan for his helpful comments on an earlier version. Requests for reprints should be addressed to the author, Department of Psychology, C-009, University of California-San Diego, La Jolla, California 92093.

gencies were demonstrated without regard to preexisting theoretical notions, and then were analyzed for their determinants. In contrast, the associative learning tradition was concerned principally with testing various "theories" of behavior, usually with respect to variables determining acquisition. The two fields thus evolved quite different subject matters, which can be ascertained from a comparison of the content of *JEAB* and the *Journal of Experimental Psychology* during the period of the early years of *JEAB*'s existence. Differences in the type of experimental analysis (e.g., the use of cumulative records) are of course apparent, but these were secondary to the more substantive differences in the experimental phenomena being investigated.

The reasons for the initial bifurcation of the field of animal learning do not, however, necessarily explain the persistence of that separation for 30 years. There in fact have been numerous developments that have provided incentive for the reunification of the field. Investigators in the associative learning tradition have adopted many of the methodologies introduced by Skinnerians, including not only the "Skinner Box" but procedures such as conditioned suppression, variable-interval schedules as baselines, and Sidman's avoidance procedure. Perhaps more importantly, there has been a variety of experimental findings for which there have been converging interests. The studies of avoidance learning in the late 1960s and of autoshaping throughout the 1970s provide two examples where there has been substantial contact between the two sides of the field, and there is no question that both contributed significantly to understanding those areas of study. More recently there has even been a convergence on a common theoretical framework, as several investigators in the associative tradition have come to appreciate the wisdom of Skinner's "three-term contingency" as an alternative to "S-R" theory (e.g., Colwill & Rescorla, 1986; Holman & Mackintosh, 1981; Ross & LoLordo, 1987).

But for every such example there are corresponding counterexamples in which separation has continued despite what appear to be common interests. Most recently, the resurgence during the 1970s of research on second-order conditioning within the associative framework (e.g., Rescorla, 1980) occurred largely independently of any contact with the

extensive earlier literature on conditioned reinforcement in the behavior analysis tradition. Overlapping interests clearly do not guarantee contact between the two branches of the field, and it remains unclear to what extent common interests will provide a meaningful bridge between them. In fact, the gulf between the two sides of the field seems to have increased rather than decreased over the past few years, as indicated by an increasing divergence between the contents of *JEAB* and the other major journals publishing conditioning research.

The preceding discussion provides a necessary context for evaluating the book under review, a book by one of the major figures in contemporary learning theory. Professor Mackintosh has provided a critical summary of the current standing of the major issues in associative learning theory. He is perhaps uniquely qualified to provide such an assessment, not only because he has been among the most prolific and insightful investigators of animal learning on a wide variety of issues, but also because of his previously demonstrated command of the enormous, and often conflicting, experimental and theoretical literature. Most readers will be familiar with his previous book, *The Psychology of Animal Learning* (published in 1974 and reviewed in *JEAB*: Weisman, 1975), an enormous effort that set a standard for scholarship and intelligent discussion that few other books on the subject can even approach.

Although the present book overlaps substantially with its 1974 predecessor, its content is quite different. Whereas the earlier book included a review of a large number of empirical findings, the book under review is focused much more tightly on theoretical issues and includes only those experimental findings that bear directly upon them. Absent are the explications of basic procedures and descriptions of parametric effects. The result is that the recent book is much less useful as a text, because it assumes that the reader has a considerable amount of basic information.

Also absent are entire topics to which the 1974 book gave extensive coverage. These omissions are instructive, especially as they bear upon the theoretical perspective around which the book is organized. They include all discussion of schedule-maintained behavior, conditioned reinforcement, contrast, and stimulus generalization. The reader familiar with the

1974 book will recognize that these topics provided the major contact with the literature of behavior analysis. Their exclusion from the present book thus reflects one of its most salient features: the book makes *no* effort to reconcile the different traditions of research on animal learning noted above. The limitations on the scope of the book are noted by Mackintosh in his Preface, where he suggests that the study of associative learning is a sufficiently broad field to be studied in its own right.

A good sense of what is included in the book is provided by its separate chapter topics. The book begins with a highly selective discussion of the historical origins of the field and of how the learning-performance distinction provides a necessary framework for any theoretical analysis. The most tightly organized are the next three chapters, which discuss, respectively, whether classical and instrumental conditioning are separate processes, the theoretical analysis of classical conditioning, and the theoretical analysis of instrumental conditioning. The remaining topics include the relation between appetitive and aversive reinforcement procedures, the nature of avoidance learning, the analysis of contingency effects and inhibitory conditioning, the laws of association, and discrimination learning. Under each of these topics there is clearly much of interest to any student of animal learning, regardless of theoretical orientation. But it is also the case that the approach to even those topics shared by the two branches of the field is not one with which the behavior analysis tradition will resonate. A common feature of many of them, and the dominant theme throughout the entire book, is a concern with specifying the "unit of learning." That is, when an animal is exposed to a conditioning procedure, what is the nature of the associative structure that results from that experience? With a few notable exceptions (e.g., Thompson & Zeiler, 1986) behavior analysts generally have disdained this issue, so it should not be surprising that Mackintosh has found little reason to present their perspective.

As will be argued below, the continued separation of the two traditions of animal learning exacts a substantial price. But a fair assessment of the book requires that we first consider the book within the framework of its stated goals. How good is it as an account of contemporary associative learning theory? The answer is very

good indeed. The discussion of each of the major issues that are addressed is provocative and lively. But at the same time it is also very incisive and evenhanded, and readers should have little quarrel with Mackintosh's selection or treatment of the available evidence. He is at his best in dissociating substantive questions from pseudo-problems, not only for the larger issues (e.g., the current status of stimulus substitution theory in classical conditioning) but for a host of subsidiary concerns as well (e.g., the determinants of the effects of the interval between the conditioned stimulus (CS) and unconditioned stimulus (US)). Pockets of genuine wisdom are scattered throughout the book, and any reader who teaches a course in animal learning can profit greatly from his trenchant distillation of what is critical evidence from what is not. Quite apart from one's attitude toward his theoretical perspective, the book is a highly valuable contribution to the body of knowledge in animal learning.

Mackintosh's authoritative assessment of associative learning theory provides an opportunity to summarize the major conclusions that have been gained from recent research in that tradition: (a) Classical and operant conditioning are indeed separate processes, involving different associative units and different rules of performance. However, much behavior is under "biconditional control," being under the influence of both conditioning processes. (b) Stimulus substitution theory still provides an adequate account of what is learned in classical conditioning, and of the nature of the conditioned response, especially when it is appreciated that a US is not a unitary event but a composite of elements, each of which may control different aspects of the animal's behavior. (c) The unit of learning in instrumental learning procedures is the response-reinforcer association, not the stimulus-response association, and the function of the discriminative stimulus is to serve as a conditional cue informing the animal of the response-reinforcer relation. However, under several conditions (e.g., extended training), the stimulus may come to control the response directly. (d) Punishment is the symmetric opposite of positive reinforcement, as the animal learns the association between the response and the consequent aversive event. (e) The effects of reinforcers involve two separate types of associations: One is between the response and the general he-

donic effects (positive or negative) of the consequent event; the second is between the response and the particular sensory properties of the particular reinforcer. As a corollary, the general hedonic effects of all reinforcers can be placed on a single bipolar scale, with the implication that conditioned inhibitors with respect to rewards are equivalent to conditioned excitors with respect to aversive stimuli, and vice-versa. (f) Avoidance learning requires an analysis in terms of the events immediately consequent upon the avoidance response, which in most cases will be a change between stimulus conditions that vary in their correlation with aversive stimulation. Although the Pavlovian stimulus contingencies embedded in an avoidance procedure may cause the stimulus to acquire conditioned aversive properties, "two-factor" theory is unnecessary, because response-produced cues will become conditioned inhibitors with respect to aversive stimulation and thus assume positive value in their own right. (g) Contingency effects are best understood not in terms of the animal actually learning the molar relationship between the CS and US (or response and reinforcer) but rather can be derived from the more molecular principles that excitatory conditioning occurs whenever an "unpredicted" reinforcer occurs and that inhibitory conditioning occurs whenever a "predicted" reinforcer fails to occur. (h) The determinants of the strength of an association depend not only on the degree of temporal proximity between the elements of the association but on a host of other factors as well, including relative predictiveness, spatial contiguity, similarity, and "relevance." (i) The degree to which a stimulus or response enters into an association also depends upon past experience, as previous exposures of the stimulus or response in conditions in which nothing of consequence is predicted by those elements will cause them to lose "associability" with respect to all subsequent learning experiences (i.e., the general "attention value" of a stimulus is decreased). (j) Discrimination learning is best analyzed in terms of the more elementary processes of conditioning and extinction, as in the tradition of Spence (1936), but with the recognition that the basic processes themselves are more complex than recognized by early learning theorists and that additional, more complex, processes (e.g., memory effects) may play

important roles in some discrimination procedures.

Several of these empirical generalizations are not without grounds for disagreement. For example, Mackintosh's postulation of the notion of stimulus-reinforcer "relevance" to account for the results of taste-aversion learning and related findings seemed unnecessary to this reader, especially because the domains of alternative associative variables such as novelty, spatial contiguity, and stimulus similarity have yet to be explored in any meaningful way. Still other of his conclusions are based on a limited empirical corpus, notably his provocative proposition that conditioned inhibitors for aversive stimuli are equivalent to conditioned excitors for appetitive stimuli, and vice-versa. Such a view is highly counterintuitive and will remain suspect until further empirical substantiation is provided. But such criticisms are little more than minor carping that do an injustice to the evenhanded treatment of the available evidence that Mackintosh provides. Even when he is arguing forcefully for a controversial position he is quite careful to note the contrary evidence and provides a reasonable, if not always convincing, interpretation of why his own position is preferable.

A more serious difficulty with Mackintosh's summary of the literature is his virtually complete disregard for anything that has evolved from the Skinnerian tradition. In some cases such omission leads to potentially serious errors. For example, a major piece of evidence invoked by Mackintosh for the functional similarities between stimulus-reinforcer and response-reinforcer associations is the reduction in operant response rate when a stimulus is presented just prior to the reinforcer. That is, the occurrence of the stimulus, which is more strongly correlated with the reward than is the response, is said to "block" or "overshadow" the response-reinforcer association in much the same way as it would block or overshadow another stimulus-reinforcer association. However, such a conclusion requires the assumption that changes in response rate correspond to changes in "associative strength." As argued elsewhere (Williams, 1982; Williams & Heyneman, 1982), an alternative explanation is that response rate may decrease because of changes in the response unit. The problem is of course a familiar one for operant condition-

ers, as the role of "response strength" versus "response selection" has been one of the dominant themes of the analysis of schedules of reinforcement. By ignoring those analyses, however convoluted they have become, those in the associative tradition run the risk of offering a simplistic account of what has been shown to be an extremely complex subject.

Other omissions from the behavior analysis literature, while more understandable, have the consequence that major disparities in the experimental literature are not confronted. For example, one of the major puzzles in the study of stimulus control is the relation between fading and blocking, each of which has generated an enormous experimental literature. Both procedures involve pretraining, in which stimulus control is established to one stimulus element, and a second conditioning phase in which the pretrained element is compounded with a second element which is later tested for control in its own right. But whereas research on fading has viewed the pretraining phase as a method of facilitating control by the added element during the compound conditioning phase, research on blocking has viewed it as a method for interfering with learning about the new element. There is clearly a contradiction here, the understanding of which could possibly lead to important theoretical advances. But such understanding is unlikely to occur when neither side of our field bothers to address the important concepts developed in the alternative domain.

Still other omissions are less understandable. Among the most notable is the evidence derived from the matching law regarding the nature of punishment effects (deVilliers, 1980; Farley, 1980). Those studies provide perhaps the strongest basis available for believing that punishment is the symmetric opposite of positive reinforcement, as they demonstrate through a quantitative analysis of choice that response-contingent shock must be understood in terms of "negative food-units." Mackintosh in fact endorses this view of the nature of punishment, but the evidence he invokes in its favor is much more equivocal. His omission of the more powerful studies derived from the matching law is representative of his apparent disdain for research from the behavior analysis tradition, even when that research is directly relevant to his discussion.

The omission of the quantitative studies of punishment is characteristic of a more general feature of Mackintosh's approach, which is to eschew quantitative models of any sort. This is perhaps justifiable given that his major topic of consideration is the acquisition of associations, because it is likely that these monotonically increase with training, thus allowing the use of dependent measures that are entirely ordinal in nature. Focusing on acquisition certainly eliminates a significant proportion of research pertaining to operant theory, which has been devoted in large measure to the analysis of steady-state maintained behavior. But the use of different modes of analysis should not be allowed to obscure the fact that a common goal is involved in both cases—the identification of the basic conditioning processes. To the extent that quantitative analysis of steady-state behavior sheds light on those basic processes, there is little justification for its systematic omission.

In fact, Mackintosh's disregard for quantitative issues has significant implications even for his analysis of acquisition. The major theoretical controversy during the past several years in the study of acquisition effects in classical conditioning has been the relative merits of the Rescorla-Wagner model versus the Gibbon-Balsam model. The former (Rescorla & Wagner, 1972) assumes that learning can be analyzed in terms of the incremental effects of individual excitatory and inhibitory events, but assumes that the determinants of those events are more complex than is acknowledged by previous learning theories because the effects of any individual reinforcement or extinction occurrence are determined by whether they are "expected" or "surprising." The latter is of more recent vintage (Gibbon, 1981; Gibbon & Balsam, 1981; Gibbon, Farrell, Locurto, Duncan, & Terrace, 1980), and assumes that responding is determined by the more molar relation of the rate of reinforcement associated with the CS versus that associated with the background. That is, the fundamental determinant of conditioned responses is a "comparator" process by which the animal assesses the "relative value" of the CS (see Miller & Schachtman, 1985, for a recent variation of the "comparator" concept). Most importantly for the present discussion, the evidence separating the two accounts is primarily of a quantitative

nature. Both models predict the qualitative effects of such variables as intertrial interval (ITI), free reinforcers added during the ITI, and CS duration, but only the Gibbon-Balsam model has provided a basis for the more exact quantitative properties as well (see Gibbon, 1981, pp. 298–307, for why the Rescorla-Wagner model has difficulty with the quantitative features of such data). Mackintosh's approach is insensitive to the quantitative differences between the models, which may explain why the theoretical disagreement was simply ignored. (In fairness, it should be noted that the Gibbon-Balsam model appeared only shortly before the completion of Mackintosh's book.)

Mackintosh may well be correct in his implicit endorsement that the principles embodied in the Rescorla-Wagner model reflect the most fundamental mechanisms of conditioning, and that our understanding of those principles represents the most significant insight about animal learning during the past several decades. But it is worthwhile to consider how different the perspective on associative learning theory would be if the comparator notion of conditioning proved to be correct. Whereas the Rescorla-Wagner model, in the tradition of incremental learning theories, views conditioning to be a composite of individual episodes of reinforcement and extinction that somehow sum together, the comparator concept rejects this "molecular" analysis in favor of one that relies on the molar concept of "rate" of reinforcement (or the inverse, "average time" to reinforcement). The appeal of the latter is that it allows a direct translation of real-time experimental operations into theoretical concepts. The more molecular "episodic" notions have substantial difficulty in making such a translation because the units of the molecular analysis are not easily defined, a difficulty that becomes especially pronounced when variables with temporal dimensions are manipulated, such as intertrial interval or stimulus duration. For such operations the concept of rate of reinforcement at least has the advantage of parsimony, although it remains to be seen whether it can deal effectively with more basic experimental phenomena such as blocking.

One reason for dwelling upon "comparator" learning models as an alternative to the incremental associative models favored by Mackintosh is that the comparator models share the

same conceptual structure as that dominant in the analysis of free-operant, steady-state behavior. A dominant concept in the latter domain of research during the past 25 years has been "relative rate of reinforcement" (and related notions such as relative temporal proximity to food) as an independent variable. Studies of choice, of response rate maintained by simple schedules, of contrast, and of temporal patterning with fixed-interval schedules have invoked that concept as a primordial explanation of many basic effects. Whether such analyses will ultimately prove to be the most appropriate explanation of those phenomena is still an open question. The point for the present discussion, however, is that the analysis of behavior in terms of such molar concepts is not simply the parochial concern of the esoteric subject matter of schedules of reinforcement. The issue is among the most fundamental in animal learning: What is the basic independent variable? Those who ignore this issue in favor of an unquestioning acceptance of associative learning principles do so at substantial peril for their own theoretical enterprise.

An example of how our conceptual framework would be changed by an appreciation of nonmolecular independent variables is the concept of inhibition. Its status is fundamental to Mackintosh's position because he uses inhibitory conditioning as a pivotal concept at several junctures in his analysis. Not only is it among the most important pieces of evidence in favor of the learning-performance distinction, it lies at the heart of his treatment of the basic principles of association and such ancillary subjects as the relation between appetitive and aversive reinforcement procedures. But according to comparator models of conditioning, the concept of inhibition, at least in its usual sense, should be abandoned. The dissection of rate of reinforcement into the two separate processes of excitation and inhibition is unnecessary, because a single unitary independent variable will do the work of both. Moreover, as combined with the comparator concept, the notion of rate of reinforcement implies that the value of a stimulus does not depend simply upon the sum of reinforcement and nonreinforcement experiences but rather is always relative to its context. Thus, a given history of pairings and nonpairings with reinforcement may produce either "excitation" or "inhibi-

tion" depending upon the value of the context to which the stimulus is compared.

Difficulties with the concept of inhibition are especially evident with the study of steady-state behavior. As a result, the analysis of such behavior has ignored the concept in favor of collapsing the notions of excitation and inhibition onto a single dimension—rate of reinforcement. The justification of this position is not simply parsimony; rather, it emerges readily from extensive investigation of intermittent reinforcement schedules. It is more plausible to view such schedules in terms of major dimensions such as the rate of food delivery than to consider how individual reinforcement and nonreinforcement episodes summate to produce stable behavior. Indeed, some versions of the relation between excitation and inhibition, including that endorsed by Mackintosh, would lead to declaring the latter task impossible, because they view the two processes as largely independent. That is, extinction episodes are not viewed as causing the loss of excitation but rather an increase of inhibition, with the two processes then summing to produce their effects. An important result is that the effects of any given reinforcement or extinction episode is not "path-independent," in that a given level of "associative strength" may reflect very different combinations of the two conditioning processes. Given such a possibility, the analysis of reinforcement schedules in terms of the molar variable of rate of reinforcement would seem entirely foolhardy. From that perspective it is not surprising that Mackintosh ignores the analysis of maintained behavior, except for the occasional comment that schedules of reinforcement are not amenable to experimental analysis because they confound too many variables.

But the status of the concept of inhibition is ultimately an empirical issue. This is not the place to review the relevant evidence, but it should be noted that despite substantial evidence in its favor (see Rescorla, 1969, and Hearst, Besley, & Farthing, 1970, for early reviews), the concept is under renewed attack (e.g., Fowler, Kleiman, & Lysle, 1985; Miller & Schachtman, 1985). As an example, consider some of the difficulties posed by a neglected learning phenomenon that appears to show that inhibitory learning does not occur, at least in the sense that it has been commonly conceptualized. The phenomenon has been la-

beled "invariance in reinforcements to acquisition" (Williams, 1981). When a subject is exposed to either an autoshaping procedure (Gibbon et al., 1980), a successive discrimination (Eckerman, 1969), or a simultaneous discrimination procedure (Williams, 1981), and S+ presentations are followed by reinforcement on only a percentage of the trials, the finding has been that the number of reinforcers required to attain some learning criterion is approximately constant across all percentages presented. Thus, for example, the number of reinforcers required for a 25% reinforcement schedule is the same as for a 100% reinforcement schedule. The effect appears to be quite general, and Gibbon et al. (1980) have argued on the basis of a review of the experimental literature that it occurs in all classical conditioning procedures in which it has been investigated, in addition to the examples cited above. Its implication is that the nonreinforced S+ presentations have no effect. In other words, whereas the concept of inhibition implies that each nonreinforced S+ should subtract from, or counteract, the growth of excitatory associative strength to the S+, the findings are that no such effect occurs. Gibbon et al. (1980) showed that the lack of effect is consistent with their comparator model of acquisition and thus view it as important evidence against incremental learning models such as that of Rescorla and Wagner (1972). The puzzle is how such effects are to be reconciled with other experimental findings that seem clearly to demonstrate inhibitory learning. However that puzzle is resolved, it is clear that the status of inhibitory learning is sufficiently uncertain to provide a shaky foundation for any general approach to learning theory.

The nature of the fundamental independent variable for conditioning procedures is especially crucial for studying choice, which Mackintosh subsumes under the more general topic of discrimination learning. He recognizes that there must be some choice rule by which behavior is distributed even after the associative strengths for the individual alternatives have been established. The two possibilities that he considers are the maximization rule, whereby the animal chooses whichever alternative has the greater strength, and the "matching" rule, whereby the behavior is emitted probabilistically in proportion to its relative strength. Rather than give careful scrutiny to how the

existing evidence (most of which is quantitative) bears upon this issue, he dismisses the previous research on the topic as being confounded by too many different issues, overly complex, and "tedious" (p. 258). He certainly is correct that the problem is a complicated one, and that sorting through the contradictory claims in the literature is not easy. But his cursory dismissal of the problem as an ancillary issue shows that he has not appreciated Tolman's insight of many years ago that "all behavior is choice." That is, in "deciding" whether to respond or not respond, the animal's choice presumably is always described by some choice rule, so that any conditioned responding will be a manifestation of the quantitative properties of that rule. This of course is the fundamental insight of Herrnstein's (1970) extension of the matching law to free-operant behavior in single-schedule situations, and the rationale behind relative rate of reinforcement as a fundamental independent variable for all instrumental conditioning situations.

While we can only speculate about why Mackintosh does not share this appreciation for the importance of choice, it is notable that most of his conceptual analysis of instrumental learning is taken directly from that of Pavlovian conditioning. Indeed, one of the major themes of the book is that the rules of learning the association between a response and reinforcer correspond directly to those governing the association between a CS and US in a Pavlovian procedure. The importance of the concept of choice is much less apparent with the Pavlovian procedure, because the conditioned response has been assumed to be a direction function not of its relative value, but rather of its strength of association with the CS. Even that assumption is now suspect, however, as shown by the continued viability of the comparator models noted above. If a Pavlovian conditioned response always depends on its associative strength *relative* to that between the context and reinforcer, then the notion of "choice" becomes applicable to Pavlovian procedures as well.

Despite a very different conceptual framework from that dominant in the free-operant literature, Mackintosh's analysis of instrumental learning leads him in a similar direction. He clearly recognizes that operant behavior cannot be accounted for solely in terms

of the degree of associative strength between the response and reinforcer. It also must depend upon the "value" of the reinforcer, as shown by numerous studies in which instrumental behavior was greatly reduced when its contingent reinforcer was devalued outside of the training context (e.g., by taste-aversion conditioning). While he considers various versions of "two-factor" theory to account for such effects of "value," his choice for the dynamic principle behind operant behavior is a version of expectancy theory. In his own words,

The association between instrumental response and reinforcer should be regarded as an expectation or proposition about certain relationships, which, when combined with other premises, can be used to derive instructions to perform or withhold that response. Exposure to a positive contingency between lever-pressing and food will establish an association between them, or the proposition that lever-pressing produces food; if the rat is hungry, and the food pleasant, this premise will be combined with the further premise that food is to be sought; from these two premises can be derived the instruction to press the lever. (p. 111)

While those in the behavior analysis tradition are unlikely to sympathize with such an account, much of the analysis of choice in terms of the concept of relative value seems implicitly to accept it. Accordingly, some comparison of the response consequences correlated with the different alternatives must occur, and choice is then determined not by the absolute level of "response strength" of the two alternatives but by the relative value of the possible behavioral outcomes. In contrast to Mackintosh's hypotheses about the subject's mental machinery, such an analysis adopts a descriptive framework in which choice is quantitatively described, and from which the relative values of the response alternatives can be inferred. Accounts such as Herrnstein's (1970) are the next step beyond Mackintosh's, after it has been shorn of the mentalistic deadwood. Oddly, however, he seems to regard such accounts as needlessly esoteric and fails to see their relevance to an analysis of instrumental conditioning.

A thesis of the preceding discussion is that the study of instrumental learning in the associative tradition has developed to a point at which it now faces many of the same problems that have occupied those in the behavior analysis tradition for some time. The relations be-

tween response rate and "associative strength," and between single-response and choice procedures, are just two of many issues that have been confronted by students of free-operant behavior, and those in the associative tradition could easily profit from taking cognizance of the answers that have been provided. But the need for an appreciation of the other domain of animal learning research is a two-sided affair. Despite recent developments in the associative learning tradition that clearly impinge upon some of its most important concepts, the behavior analysis tradition has given no acknowledgment of their potential importance. For purposes of illustration we will consider two of the most fundamental concepts in behavior analysis, "response strength" and the "discriminative stimulus," to show how they may be altered substantially by recent findings.

According to most textbook accounts of operant behavior, the strength of behavior can be understood as the result of its previous history of reinforcement contingencies. For example, the reason a child asks for a drink when thirsty is that previous instances in which behavior has occurred under the appropriate stimulus and motivational conditions have resulted in reinforcing consequences. The critical feature of the analysis is that the probability of behavior is viewed as the result of the "response strength" generated by the previous history, in contrast to it being a result of the subject "anticipating" the immediate effects of his or her behavior. This view is strongly inculcated in students reared in the Skinnerian tradition, because it has been considered one of the cardinal principles of how "mentalism" is to be avoided.

Results from the associative analysis of instrumental learning are relevant to the concept of response strength because they clearly demonstrate that the past history of reinforcement with respect to a particular response is insufficient to predict its subsequent occurrence. The most powerful such evidence is when two separate responses are trained, each with a separate reinforcer, and then only one of the two reinforcers is devalued by poisoning without the operant response being available. Then during an extinction test both responses are again available with the result that the response that led to the previously devalued reinforcer is much less likely to occur (e.g., Colwill & Rescorla, 1985; also see Kriekhaus & Wolf,

1968, for an earlier demonstration of the converse effect). Such findings (see Colwill & Rescorla, 1986, for a review) show that the "strength" of a response is a direct function of the immediate value of its reinforcing consequence, quite apart from whether the animal has actually experienced the new response contingency in which the response is followed by the devalued reinforcer.

Such findings seem to demonstrate that the effects of a reinforcement history cannot be summarized in terms of the attribute of "strength" of the behavior. Instead, the specific response-reinforcer relationship has been learned, and the animal responds according to the current value of the contingent reinforcer. It is of course possible that such effects can be incorporated by a descriptive analysis in which the concept of response strength is completely abandoned. That is, the occurrence of behavior can be viewed as the result of its previous reinforcement history *and* of those operations affecting the value of the reinforcer. The devaluation procedure would then be analogous to a motivational operation such as hours of deprivation. The cost of such an approach is that it abandons the search for the basic processes that provide unifying principles that cut across a diverse set of procedures. It is also unnecessary, since the determinants of "response strength" seem straightforward: it is a joint function of the strength of the association between the response and reinforcer and of the current value of the reinforcer. Whatever may be the drawbacks of "association" as a theoretical construct, in the present case it coherently organizes a large body of knowledge.

Recent analyses of the associative relations underlying changes in behavior are similarly important with respect to the concept of the discriminative stimulus. Its usage within behavior analysis has been entirely descriptive: a discriminative stimulus is said to "set the occasion" on which the response is reinforced. The natural interpretation of this phrase is that the stimulus serves as a conditional cue: in its presence the response is followed by the reinforcer; in its absence the contingency does not apply. In other words, the effect of the discriminative stimulus is to signal the response contingency. But what this conditional relationship implies about the nature of the associative relationship that has actually been learned has not been addressed.

In fact, the concept of stimulus control within behavior analysis often seems to be used in quite different ways. In contrast to the "informational" function of a discriminative stimulus noted above, many extrapolations of the concept of stimulus control to nonlaboratory situations implicitly assume that the stimulus controls the response directly. For example, Skinner's use of the notion of multiple causation in the analysis of verbal behavior and problem-solving assumes that the functional stimulus can be regarded as a constellation of stimulus elements (much like those invoked in Estes' stimulus sampling theory), each of which makes an independent contribution to "response strength" such that their joint effects may summate to bring an otherwise weak response above the threshold for emission. It is not obvious how such a conception of stimulus control can be reconciled with the notion that discriminative stimuli do not themselves directly evoke the response but serve only to signal the response contingencies. For most examples involving multiple causation, in fact, the reinforcement contingencies are usually not specifiable. Consider as an example attempting to facilitate the recall of someone's name by going through the letters of the alphabet. The one letter that begins the name will aid recall presumably because of its past association. But that individual letter would signal nothing about the reinforcement contingencies correlated with producing the name at that moment, nor is there any clear relationship between the initial letter and the reinforcement contingencies that have occurred in the past. To the extent that the concept of multiple causation is meaningful (and it is surely one of Skinner's most insightful contributions) it may be that there are different types of stimulus control, and considering them as a single descriptive category may lead to conceptual confusion.

The issue is not merely semantic. Given that stimuli compete for control in a variety of ways, and that there may be qualitatively different forms of stimulus control, a significant experimental question is whether "associative competition" occurs only within a particular form, or across forms. Recent evidence (Ross & LoLordo, 1987) strongly supports the reality of such distinctions, as blocking effects have been shown to occur with respect to some combinations of stimulus-control procedures but

not others. The functional dissociation of such effects portends a major advance in our analysis of the different ways that stimuli control behavior. The likely result is that the use of "stimulus control" as a purely descriptive category will be replaced with a more detailed explication of different types of associative relationships. Mackintosh's attempt to define "what is learned" in instrumental learning is representative of such efforts, which may lead ultimately to a deeper understanding of the different processes underlying our descriptive categories (see Williams, 1984, for further discussion of these issues).

It should be apparent from the preceding discussion that the division between behavior analysis and the associative learning tradition is not ascribable simply to differences in subject matter. Although the topics of primary concern continue to be substantially separate, the major obstacle to significant interaction between the two approaches to animal learning stems from a different source. The most pervasive difference has been in their respective philosophies of science, as associative learning theory, like cognitive psychology generally, has attempted to understand the nature of the associative structures mediating conditioning effects. Behavior analysis, in contrast, has embraced a descriptive approach that has attempted to avoid such constructs altogether. The result is that the two sides of the field have very different goals and often share little of the same language. For that reason many in behavior analysis will ignore Mackintosh's efforts on the grounds that it is fundamentally misguided (a sentiment that Mackintosh apparently reciprocates). My own view (see Williams, 1986) is that behavior analysts are mistaken in believing that hypothetical constructs can be avoided, because they already are widely used and are the inevitable result of an advancing science. What is at issue instead is whether the particular concepts that are employed provide a meaningful organization to a body of different observations, and hence are productive. Mackintosh's summary of associative learning theory is implicitly an argument in favor of associative concepts, which must be judged by the coherence they bring to our current knowledge about the effects of conditioning procedures. In that token he is at least partially successful, as no one who pretends to be a student of animal learning can afford to

ignore his analysis. It is unfortunate that his acumen was not also applied to the traditional problems of operant conditioning, nor to the nexus between steady-state procedures and simple acquisition effects, for it is there that the most severe test of associative concepts will occur (see Williams, 1983, for further discussion). But regardless of how those issues are eventually addressed, Mackintosh's book shows that associative learning theory is alive and well. The vigor of his discussion of the associative basis of learning conveys a sense of excitement about the issues involved and about the power of experimental analysis to resolve them. During the past 20 years the associative learning approach has enjoyed its most productive period, and those in the behavior analysis tradition are well advised to take heed of its progress and of the explanatory concepts that have been distilled from its research.

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