*THE COMPLEAT ASSOCIATIONIST:
A REVIEW OF N. J. MACKINTOSH'S
THE PSYCHOLOGY OF ANIMAL LEARNING*¹

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For many years, Hilgard and Marquis' *Conditioning and learning* (1940) and the second edition by Kimble (1961) were, successively, among the well thumbed and frequently cited textbooks of young students of learning. Then, in the late sixties and early seventies, with the vast increase in research concerned with behavior change, it began to look as if the topic of animal learning might never again be covered in a single volume, other than an edited work. Thus, the appearance of a new comprehensive and integrated textbook of animal learning is unlikely to escape notice. It can hardly be incorrect to predict that Mackintosh's *Psychology of animal learning* (1974) will be cited and discussed extensively in JEAB. This impressively written and referenced book (which I have tagged by its obvious acronym, PAL) is already seen by some as the rightful successor to *Conditioning and learning*. And so it is.

Mackintosh's table of contents begins with six chapters concerned with the phenomenon and theories of classical conditioning and instrumental learning. These chapters can be seen as a book-within-a-book—the essence of the exposition. They are certainly the best chapters of the book and perhaps the best written modern survey these topics have yet received. The remaining chapters are concerned

with contrast effects, extinction, generalization, and discrimination learning, respectively. Although not as well organized or well integrated with the rest of the text as the preceding chapters, it is still possible to hold these chapters in reasonably high regard. Of course, a survey of research can be little better but much worse than the work upon which it is based. So perhaps we should share out the praise and blame between Mackintosh and the field for variability in the quality of chapters. Because of changes in the literature, I could not in fairness compare conclusions drawn by Mackintosh with those of Hilgard and Marquis or Kimble. But most JEAB readers will be pleased with how well Mackintosh's empirical generalizations match their own judgments in the field.

Given that PAL will be required reading for most of us, the question becomes who else should work through its over 600 pages of text and 2000 references. The first six chapters could be assigned in one term to bright fourth-year students. They might have hard going in places, but Keller-type personalized instruction could smooth the task out. For graduate students, one should assign the whole book, or better yet use it with current literature as the basis for a term or two of seminars.

Forcefully written, PAL is, as its author points out, unlikely to command universal assent. I was much impressed with PAL's summaries of research findings and empirical level conclusions and with its associationist theoretical framework. But I found PAL's attack on behaviorism and the inductive model of research insubstantial and quite out of place in a general textbook on learning. Consider the pattern of omission and inclusion. Research in motivation, interoceptive conditioning including cortical and subcortical conditioning, conditioned drug reactions, and nonassociationist empirical formulations of the

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reinforcement relation found no space in PAL, and yet a few admittedly confounded 45-yr-old latent learning studies (p. 207-208) got a figure and three paragraphs. The book is richer for its handling of latent learning but it must be poorer for its failure to consider alternatives to associationist positions. The book, of course, is not organized into three distinct bits: useful literature review and empirical generalization, interesting and convincing associative learning theory, and muddled acrimonious critique of inductive research strategy and behaviorism. It was useful, however, to arrange the review as if this were its abridged table of contents.

A SURVEY OF EMPIRICAL GENERALIZATIONS

This section of the review samples quite nonrandomly from PAL's store of literature review and empirical generalization. These samples are quoted directly to give the reader the flavor of Mackintosh's style. I will offer the opinion in advance that PAL contains many interesting and provocative literature reviews. Some were in fact so provocative that I have been unable to resist responding to them.

On the nature of the CR in classical conditioning: "In spite of almost universal rejection, stimulus-substitution theory is strikingly resilient: the variety of responses elicited by a CS closely matches the variety of responses elicited by the UCS" (p. 107-108). Mackintosh has made stimulus substitution theory more appealing by broadening the class of UCR's to include those that might occur long after UCS onset and perhaps at UCS intensities different from the one employed. Nevertheless, the best detailed predictor of the CR might be a sort of ethological lexicon of releasers and the species-characteristic responses they control in various situations.

On the nature of reinforcement in classical conditioning: ". . . there is good reason to conclude that classical CRs are not simply modified by their consequences in the manner required by the law of effect. In too many cases, omission schedules of appetitive reinforcement have resulted in the acquisition and maintenance of responses whose sole effect is to prevent the delivery of food or water; other studies employing aversive reinforcers have frequently found that the addition of an

avoidance contingency may decrease rather than increase the probability of a CR" (p. 115). To say that classical CRs are not simply modified by their consequences is not to say that they are not modified by their consequences at all. In omission schedules, response-reinforcer contingencies can have effects (Barrera, 1974; Wessells, 1974) but stimulus-reinforcer contingencies have much stronger effects.

On Pavlovian contingencies in instrumental learning experiments: "Instrumental experiments necessarily contain implicit classical contingencies and many supposedly instrumental responses may be Pavlovian in origin. It is probable that pigeons peck keys in large part because pecking is a classical CR elicited by stimuli associated with food" (p. 268). ". . . Pavlovian contingencies may be as important in studies of avoidance learning as in other operationally instrumental experiments. Avoidance learning may be easy if the required response is similar to that elicited by shock and difficult or impossible if the two responses are incompatible" (p. 347). Mackintosh makes it clear that his conclusions here have been influenced considerably by Moore (1973) and Bolles (1970). These investigators deserve much credit for redirecting research to the control of species-typical skeletal responding by Pavlovian processes, but surely too much has been made of a good thing. Consider that there are no data, only speculations, to persuade one that 'hand shaping' of pigeons' key pecks involves only Pavlovian contingencies. Moreover, the statement (Moore, 1973, p. 177) that the pigeon's simple instrumental peck is a Pavlovian conditioned response now seems to be only sometimes true. Pigeons do stop pecking keys on free operant food-omission schedules and they can be taught to peck keys for a mate or to avoid shock. Of course it is easy to speculate that shocks and mates elicit pecking (Moore, 1973, p. 163), but the data are less reassuring. It now appears that electric shock may not elicit agonistic pecks even toward a conspecific model (Rashotte, Dove, and Looney, 1974). Biological constraints are more usefully seen as channels for modification rather than as barricades against change (Waddington, 1953).

On an independent instrumental learning process: "Provided that the response specified

by the experimenter is one generating sufficient feedback . . . and provided that it is not incompatible with the behaviour elicited by the reinforcer, the subject's behaviour will be modified so as to satisfy the experimenter's requirements" (p. 139). "There . . . seem to be numerous situations where behaviour is altered by instrumental contingencies in a way which suggests that . . . specific learning about the contingency between responses and reinforcers must have occurred" (p. 206). One finds this a more temperate view of instrumental contingencies even for the pigeon's peck.

On punishment: "It is no longer possible to argue that punishment is an ineffectual procedure for suppressing responses, or that response-suppression is an artifactual consequence of noncontingent emotionally derived disruption" (p. 275). "As empirical generalizations . . . both positive and negative versions of the law [of effect] remain generally true and there are no grounds for arguing that the positive law is better substantiated than the negative" (p. 277). It seems confusing and a bit inconsistent that while finding the empirical laws of effect generally true Mackintosh can, elsewhere, find them accepted in only a loose and more or less trivial sense (p. 196). Later (*e.g.*, p. 346), he continues to use the term 'reinforced' in its usual technical sense. It would seem that Mackintosh wanted to expunge the empirical law of effect but could not get through the book without it. Readers of JEAB will not be very surprised by this difficulty.

On conditioned reinforcement: "By no means all studies of conditioned reinforcement have satisfied these requirements; nevertheless there remain a sufficient number of valid studies in which new responses or patterns of responding have been established solely because they have been followed by the presentation of a stimulus previously, or concurrently, associated with primary reinforcement. . . . A stimulus paired with food elicits some of the responses elicited by food: it also acquires another of the properties of food—that of reinforcing the instrumental behaviour of a hungry animal" (p. 269). With conditioned reinforcement as with punishment, good research (often published in JEAB) has made discussion much more straightforward for Mackintosh than it was for Kimble or Hilgard and Marquis.

On forgetting: "A widely accepted generalization is that laboratory animals, like elephants, never forget. . . . As with many myths, this one rests on a small core of truth surrounded by a larger mass of casually reported and uncritically accepted data" (p. 472). A survey of animal research on forgetting is a welcome and important addition to the book.

On overshadowing: "Overshadowing of one cue by another is affected by not only their relative intensities, but also by . . . their relative validities—a more valid cue, *i.e.*, one which better predicts the occurrence of reinforcement, will overshadow a less valid one" (p. 47). Mackintosh's review of the overshadowing and blocking literature is another of PAL's valuable contributions. In particular, the discussion of the overshadowing of response-reinforcer correlations by stimulus-reinforcer correlations (p. 220-221) makes provocative reading.

In his treatment of sensory preconditioning and latent learning, Mackintosh is much nearer to Tolman than to Hull. On sensory preconditioning: "Sensory preconditioning experiments may not produce very large effects but they are undoubtedly reliable and their obvious interpretation is that animals can learn to associate motivationally neutral events" (p. 95). On latent learning: ". . . the results of experiments in which rats were exposed to a maze before receiving some such reinforcement as food in the goal box . . . suggest very strongly that animals are capable of learning that their behaviour has consequences" (p. 211). "In experiments on latent learning, the arrangement of a contingency between a response and a neutral stimulus results in a change in the probability of that response when the stimulus is subsequently associated with reinforcement" (p. 217). Mackintosh would like us to view sensory preconditioning and latent learning as reinforcerless homologies to classical conditioning and instrumental learning respectively. Just why he feels that he must insist that transfer occurs from associations between motivationally neutral events remains a mystery. It seems at least as plausible to suggest that lights, tones, and locomotion may produce reliable even if not very large effects as reinforcers (Kish, 1966; Premack, 1965) and the most obvious interpretation is that few if any events are motivationally neutral, at least initially.

THE ASSOCIATIONIST LEARNING THEORY

Interwoven into and around material similar to that already cited is a pervasive associationist theory of learning. Somehow it seems quite natural that Mackintosh should continue in Britain's longest established philosophical tradition. Important antecedents, and also a fine summary of the associationist stance of PAL, should be read from a slim symposium volume edited by Mackintosh and Honig (1969). The general associationist position is "... that animals may associate any set of events that happen to be correlated in time" (p. 140). Of course, this results in a clean uncluttered approach to the validity and importance of latent learning and sensory preconditioning. It also results in the view that classical conditioning and instrumental learning are specific instances of this very general associational process.

In particular, Mackintosh means that the neural correlates of events, stimuli, responses, and reinforcers are associated inside animals' heads (p. 87). Animals learn what leads to what. In classical conditioning, animals learn to associate stimuli with reinforcers; in instrumental learning they learn to associate responses with reinforcers (p. 139); in punishment they learn to associate responses with aversive reinforcers, while in avoidance they learn to associate responses with the omission of expected aversive reinforcers (p. 345); and so it goes. The outcome of this line of reasoning is that animal learning becomes a primitive form of the acquisition of knowledge. That is, Mackintosh would insist that as a result of instrumental learning procedures, animals know the consequences of their own behavior. Indeed, they may even learn that their behavior is without consequence with respect to a reinforcer, just as they might learn that particular stimuli and reinforcers are uncorrelated (pp. 40 and 218). In these quite clearly cognitive terms, animals know about events after having learned the correlations between them. And knowing is what cognitive psychology is all about. So the theory in PAL is not just associationist but cognitive as well.

It has often been said, quite mischievously mind you, that cognitive-associationist theorists leave rats lost in thought at the choice point or hopping about indecisively on the

jumping stand. By this it is meant that cognitive theories have no performance rules. But Mackintosh is well aware of this. He has proposed stimulus substitution and a kind of goal-expectancy response enhancement of responding (p. 267) as the performance rules for Pavlovian and operant contingencies respectively. Characteristically, however, the care lavished on these performance rules, especially the second one, is rather slight—cognitive psychologists can be seen to care less about what their subjects do than about what they know. Indeed, Mackintosh's operant performance rule seems a poor substitute for the empirical law of effect.

Nevertheless, the general associationist is likely to be with us for a long time. The position makes easier work of the facts of sensory preconditioning, latent learning, learned irrelevance and helplessness, and conditioned inhibition. These are phenomena about which S-R psychologists have been at best silent and at worst stubbornly obstinate and obtuse. Also, the general associationist position is quite compatible with the heavily cognitive zeitgeist seen in the study of memory and perception. All of this makes it particularly important to remind the general associationist and cognitive theorist at frequent intervals (*e.g.*, as in Spence, 1951, or Mackintosh, 1969, p. 192) that his is not a complete theory of psychology or even animal learning. It wants a seriously considered and well demonstrated array of performance rules, *viz.* behavioral laws, and these are not likely to be cognitive in nature.

Finally, there remain two almost stylistic comments on the theory. The first concerns the difference between a correlation and an association. As Skinner (1953; 1974) has pointed out, the locus of an association can be various: the environment, the mind, or the brain. One supposes that Mackintosh meant associations to occur only in the brain and correlations to occur only in the environment, at least in his book. But the distinction is not without its problems. Associations are completely dependent on the correlation in time of events existing elsewhere in nature. Moreover, real confusion between theory and description can result when, as in the statement cited on conditioned reinforcement, correlation blurs into association. The second point is probably less substantial and more a matter

of personal preference. It concerns Mackintosh's tendency to talk about rats as if they were furry little people, as when we are told that "... rats do not know they are in extinction" (p. 436) or that rats "... just remember that they have not been reinforced . . ." (p. 466). Finally, in characterizing a view that he rejects, Mackintosh presents what must be English upper-class public school educated rats "... calculating variations in the overall rate of shock per session, and correlating these with variations in their own pattern of responding" (p. 328). Cognitive psychologists sometimes talk as if subjects might only obey scientific laws if they understand them. Some of my colleagues even believe that. Of course if this is more than loose talk, psychology is indeed a science different from the others. But filling animals' heads with shadows will not put neural events in their rightful place in the causal chain of behavior control. Or as Mackintosh himself reminds us "Morgan argued that the apparent complexity of overt behaviour was not a reliable guide to the complexity of the underlying processes involved" (p. 1).

THE CRITIQUE OF BEHAVIORISM

There are two possibilities. The first is that in his haste to attack S-R behaviorism Mackintosh simply failed to distinguish that view from any other sorts of behaviorism. The second and more likely possibility is that he distinguished well enough but attacked all types of behaviorism anyway. Possibly no one now alive, will object seriously to most of the criticisms of S-R theory. In particular, serious support for Thorndike's theoretical law of effect had quietly died before Mackintosh began the attack. By choosing an S-R theory based on Thorndike's law of effect as the main adversary for his more cognitive theory, Mackintosh assured the latter's victory. Still, for a while during one discussion of the neural antecedents and correlates of events (p. 80) the outcome did seem uncertain.

Mackintosh's impatience with inductive generalizations concerned with relations between the environment and behavior is nearly complete. Surely it is not rash to characterize a book as unremittingly critical of the experimental analysis of behavior if the book finds

(a) the empirical law of effect loose and trivial (p. 196), (b) efforts to make the law more quantitative misleading (p. 191), (c) schedules of reinforcement of almost mystical importance to operant conditioners (p. 164) but frighteningly complex (p. 181). It might even be possible to convince you that the critique is often confusing and mistaken.

I have already had occasion to point out inconsistencies in Mackintosh's treatment of the empirical law of effect. The treatment of the quantification of the law and its extension to choice is sadly incomplete, providing a clear example of the bias against behavior analysis. Mackintosh (pp. 191-195) suggests that matching between relative rates of responding and relative rates of reinforcement is explained by a rule which states that animals respond to maximize the momentary absolute probability of reinforcement. In particular, matching is said to result from shifts in responding between the alternative schedules to maximize the absolute rate of reinforcement (p. 193). One should recognize this maximizing rule as a moment-by-moment description of the empirical law of effect and concur that it can be an important determinant of choice. But it is easy to show that the tendency for animals to maximize the absolute rate of reinforcement is an incomplete account of responding on concurrent schedules. As Nevin (1973, p. 222) pointed out, subjects can obtain all available reinforcements with nearly any distribution of responses between alternative schedules. In an experiment Mackintosh cited, but the conclusive role of which he denied in this argument, Catania (1963) made the momentary probability of reinforcement on one schedule irrelevant to choice and all but eliminated responding to that schedule. He signalled reinforcements as they became available on one schedule and held the rate of reinforcement constant on the other schedule. In another experiment, reinforcements on the first schedule were unsignalled. If the momentary maximizing rule explained choice in concurrent schedules, then Catania might have obtained one relation between the rate of responding in the constant schedule and the rate of reinforcement in the second schedule when reinforcements were signalled and quite another when they were not. Of course, Catania found the same inverse proportional relation in both experiments. The

important implications of Catania's experiment have been taken up elsewhere (Herrnstein, 1970; Nevin, 1973) and it is these sources one must consult for a review of the development of a quantitative empirical law of effect. Mackintosh's review of concurrent operants is incomplete because it does not account for obvious exceptions and because it omits the quantitative formulation that does.

Although research employing schedules forms the basis for much of the book, Mackintosh protests any expansion of research on the analysis of schedules. He writes and quotes approvingly of Jenkins' two objections (1970) to schedule analysis but stresses the latter one. These objections are that experimental analysis of schedules is not necessary, as might be the case for reproduction in biology, and is in any event exceedingly complex.

First, "neither men or animals are found in nature responding repeatedly in an unchanging environment for occasional reinforcement" (Jenkins, 1970, p. 107). The nearest I can come to refuting this argument on the spot is to recall literary descriptions of the sexual adventures of young people in rear seats of automobiles in the 1950s. Jenkins means his statement to be itself a refutation of the view that schedules are, in one way or another, a given in psychology. But it is clearly not a logical argument at all. Neither Jenkins nor Mackintosh mean to advise us to vary the environment more during our experiments or even to reinforce our subjects' behavior more frequently. What they mean, I think, is that reinforcement schedules are simply not an account of the circumstances people and other animals face, and therefore that what animals do, especially about the arrangements of events called schedules, is not of general interest. Another view is that schedules, especially those of consequences for responding, are among the most powerful yet common determinants of behavior in people and other animals. Moreover, it is this second view, not that of Jenkins and Mackintosh, which has the broader base in evidence. One might see all of this as part of the continuing debate between behaviorists and cognitive psychologists.

The second point raised by Jenkins and reiterated by Mackintosh is that schedules are far from simple to analyze, meaning that they may not be well suited to the elucidation of

the simple associative processes said to underlie instrumental behavior. To put it bluntly, when faced with a choice between simple principles, (a theory) and orderly behavior changes, we should eschew the latter. As if spending our time considering complex behavioral sequences controlled by reinforcers in the laboratory and beyond might disenchant us with elementary associative principles and with claims concerning their generality; "Oh ye of little faith!"

Of course one must concede that it is misleading to call most of the standard schedules "simple" and that some schedule research is less orderly than one might hope. Indeed, Mackintosh's task was hardly made easier by the dearth of recent research seeking to investigate cohesive general explanations of the effects of schedules. For his part, Mackintosh has attempted the difficult task of summarizing the work with the standard schedules. And he, at least implicitly, grants with Jenkins that schedules are very useful contrivances.

CONCLUSION

Important books are written to convince as well as inform their readers. One might as well admit that PAL deserves good marks on both counts. Mackintosh's associationistic position provides a comfortable framework around which to organize the facts of animal learning. Nevertheless, opinion may well be divided concerning the usefulness of a cognitive-associationist theory of learning. Of course, the alternative to accepting the position is to provide a translation or alternative position that organizes the facts at least as well. And that is a tall order.

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