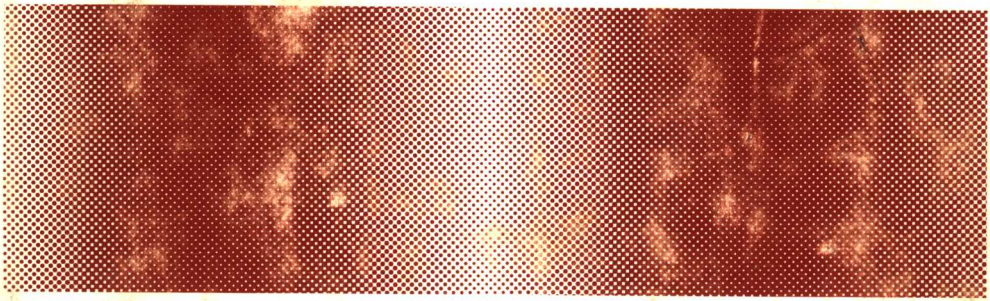
COGNITIVE PROCESSES IN ANIMAL BEHAVIOR



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Preface

At the meetings of the Psychonomic Society in Boston in November 1974, the three of us had the opportunity to discuss some current developments in the field of animal learning and behavior. Research in this province had long been characterized by a stimulus–response (S–R) tradition which had as its basis many historical and philosophical antecedents. These included an early emphasis on reflex physiology as a source of theoretical models for learning; a biological continuity suggesting that the apparently "simple" or mechanistic principles of animal learning could be applied to man; and a behavioristic metatheory which argued against introspection and the study of mentalistic processes—such as cognition—which could not be tied to direct observation and measurement. It was apparent to us, however, that many of the principles of animal learning had turned out not to be so simple after all, suggesting an important limitation to the strict application of an S–R approach. Furthermore, we found ourselves and those sharing our interests attacking problems which seemed to stem more and more from the general domain of cognition, such as higher-order association, memory, and concept formation. Clearly, we were using with increasing frequency some of the techniques and models of our colleagues in the field of human cognition and information processing. The time seemed proper for an organized discussion of matters, and so we undertook to bring together some fellow psychologists whose work reflected the state of affairs as we saw it. Accordingly, an appropriate group gathered at a conference in June 1976 at Dalhousie University in Halifax, Nova Scotia, Canada. This book is a collection of chapters based on the papers read at that conference.

Many of our friends in some other spheres of experimental psychology may find no small measure of irony in our endeavor. For years, those who worked on problems in human learning—memory is an excellent example—borrowed heavily from strict S–R models of behavior patterned after those

developed in animal learning. But with the advent of psycholinguistics and mathematical learning theory, and especially with the appearance of the computer both as a tool and as a ready-made model for human thought, memory, and attention, S–R approaches were largely discarded. Their contributions had been absorbed. Now, it may be the turn of us animal psychologists to reverse the principle of biological continuity and borrow ideas and methods from our neighbors in human cognitive psychology.

By such steps, progress takes place. Certainly the question of the limits of animal intelligence is very old, and our work may reflect a current renewal of interest in comparative cognition. Others have already made important contributions to this topic, broadly conceived, through such books as Wilson's *Sociobiology,* Thorpe's *Animal nature and human nature,* and Griffin's *The question of animal awareness.* Not everyone, including these authors (and some of the contributors to this volume) would agree on the defining properties of a field of comparative cognition, however, and some may question whether in fact it does—or should—exist. But we view the material in this book to be part of an approach, however loosely defined, that deems the study of the cognitive capacities of animals other than man to be a worthwhile enterprise. Perhaps our work will help to cross-fertilize the production of still newer and fresher views of the commonalities and differences among organisms.

Following an introductory essay in which Honig discusses a metatheoretical and philosophical approach to the problem of cognition in animals, succeeding chapters are arranged, topically, from basic associative processes to higher mental operations. Thus, Rescorla, Hearst, Bolles,and Fowler discuss problems derived from models of association; Mackintosh, Wagner, Honig, and Riley and Roitblat describe their work on attention, memory, and the processing of stimulus information; whereas Church, Hulse, Olton, Menzel, and Premack deal with time, spatial, and serial organization of behavior, and concept formation. Because the topic at hand is hardly unidimensional, the order of chapters is somewhat arbitrary, and many could fit several locations in the book.

There are many who deserve the thanks of both the editors and the other contributors. First and foremost, we thank both the National Research Council of Canada and Dalhousie University for the financial support that made the conference possible. We are also greatly indebted to Dalhousie University, especially the faculty, students, and staff of the Psychology Department, for acting as such gracious hosts during the four days that we were their guests in Halifax. Finally, we owe a special debt of gratitude to Larry Erlbaum, who, with a welcome wry twist, helped shepherd our efforts into print.

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Contents

1 On the Conceptual Nature of Cognitive Terms: An Initial Essay

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In this book, cognitive terms are used in the description, analysis, and explanation of animal behavior. They do not describe behavior in the narrow sense of referring to specific observable events; they are not part of the "data language" of experimental psychology. To say that an animal chooses between two stimuli, runs to a particular location, or reduces its rate of responding in the presence of a specific signal is quite different from saying that it *remembers* one or another stimulus, *knows* the location of food, or *associates* two events. Much of the present essay is concerned with the difference between these two kinds of description. Terms of the latter kind are more and more being used in summary descriptions of behavior, but often they refer to states or processes that enter into the determination of behavior.

It is therefore important to clarify the conceptual status of cognitive terms within the psychology of animal behavior. In the first part of this essay, I will make such an attempt, particularly with respect to their role in concept formation, explanation, and theory construction. But even when the status of cognitive terms has been discussed, the nature of their "content" will not have been specified. Is it possible to distinguish cognitive concepts from others that also lie outside the data language of psychology? I will discuss this question in the second part of the chapter.

In the chapters that follow, many contributors argue in favor of the use of cognitive terms within the vocabulary appropriate to their particular areas of research. I do not want to anticipate their arguments, nor do I intend to review the empirical findings which they use to support them. The present discussion concerns conceptual rather than empirical aspects of the material in this book. It is neither an overview of the contents, nor an introduction to particular

chapters. I intend to raise general questions and issues that are within that domain of the philosophy of science which is relevant to a cognitive description and explanation of behavior. An evaluation of empirical material in cognitive terms requires an understanding of the conceptual issues that are raised by the use of such terms. Conceptual issues facing psychologists who want to employ cognitive concepts should be understood before the empirical material on which these concepts are based can be evaluated.

I. SOME CATEGORIES OF PSYCHOLOGICAL TERMS

Terms whose function is other than to provide an immediate account of observed behavior and the conditions under which it occurs, are generally concepts. Such concepts may have different functions and attributes; they may be *descriptive, explanatory, theoretical*, or *mental*. These functions or attributes are not mutually exclusive, although a rough set of distinctions will be useful. A concept may simply be used to summarize or to provide a category for a set of related primary accounts of behavior. Cognitive concepts often play this *descriptive* role. The events of interest are often observed under critical test conditions, and the concepts derived from such observations are *dispositional* concepts (Hempel, 1952). Closely related or parallel test conditions can be used as a set of convergent operations (Garner, Hake, & Eriksen, 1956) to provide the empirical basis of particular concepts. It can be argued that most or all cognitive concepts are dispositional terms, but a defense of this point would take us too far afield.

A concept can be used within the paradigm for scientific explanation that is generally accepted in the philosophy of science (Hempel & Oppenheim, 1948). No concept by itself comprises an explanation; *explanatory* concepts are used in explanatory statements. In an explanation one or more statements must be general and law-like; at least one other, more specific statement describes a set of specific conditions. From such a set of statements, others can be deduced which will describe or predict particular observations. In the absence of such a deductive procedure, the observation of interest has not been explained.

It is generally accepted within psychology that many concepts are *theoretical* in nature because they refer to entities which are not observable. Such entities are assumed to possess particular attributes which make then useful within a system of theoretical terms, often of a deductive nature. Such terms are often known as constructs. They are "constructed" on the basis of a set of data. They derive support from the predictions that are confirmed within the deductive system in which they participate.

Mental, or subjective terms are presumably part of the particular domain of psychology. Basically, they are descriptive of "private" experience, rather than "public" events. Mental terms have been used both as explanatory and theoretical terms, although this route toward explanation in psychology has not been

accepted by behaviorists. Cognitive terms can be mental terms if they refer to private experience, but clearly they need not be limited to such a reference.

To which of these categories of concepts do cognitive terms belong? I believe that they can and do belong to all of them, depending on their function. We shall see that they can be used to *describe* sets of related observations in the realm of animal learning and behavior. If cognitive terms possess attributes beyond those specified in the defining observations, they can be *theoretical* terms as well. I will also suggest uses of cognitive terms which endow them with *explanatory* capacity. And finally it is reasonable, and perhaps even obvious, to argue that many cognitive terms were originally adopted from our vocabulary of *mental* experience.

It runs counter to the behavioristic tradition to suppose that any conceptual term could play such a variety of roles. According to the philosophy of science that until recently dominated experimental psychology, terms either described behavior as part of the "data language" of psychology, or they entered deductive systems as theoretical terms used for the explanation of behavior. Cognitive terms were suspect as part of the data language because they did not provide an immediate description of behavior, and because their mental or subjective connotations were contrary to behavioristic principles. The explanation of behavior could presumably be accomplished with other, more "behavioral" terms, which rendered cognitive concepts unnecessary.

In my view, the functions of terms cannot always be so neatly divided between the empirical and the theoretical. Even a descriptive vocabulary involves classification and abstraction to some degree. A particular term will play quite a different role depending on its relationship to the data on which it is based, and on its function within the laws or principles that encompass the generalities of scientific discourse. In an ideal science, these roles can perhaps be clearly separated. But the fact that we cannot do so with cognitive terms in psychology is not a sufficient reason to reject them.

II. SOME FUNCTIONS OF COGNITIVE TERMS

A. Description and Conceptualization

Some descriptions of behavior deal only with the characteristics of observed behavior, while others provide a conceptualization, which is based upon complex behavioral interactions and contingencies. Descriptions of the latter kind emerge from the observation of behavior, but they are at least partly theoretical in nature, because they refer to some state or process which is not directly observable. It may be useful to identify a continuum that extends from the "more observable" and thus "less theoretical" to the more theoretical and thus less observable.

1. At one level, we can observe a performance that is not under the immediate control of stimuli presented by the experimenter, and which is not exemplified by a simple, specific behavior pattern, but which is appropriate to the experimental contingencies. For example, Olton and I have both (rather independently) proposed the notion of a "working memory." In some learning situations, animals need to remember an event, or a set of events, to perform efficiently during a given trial. They also need to terminate the memory of these events in order to perform well on a later trial. In Olton's work, rats are placed on a central platform of an eight-arm radial maze. Each arm is baited at the end with a bit of food. Rats readily learn to obtain this food in an exhaustive fashion; they run down every arm before returning to any. On subsequent trials the rat shows little or no interference from previous trials; the working memory has presumably been canceled, or reset. The concept of working memory provides a framework for the description of behavior that is systematic but not under the control of a sensory cue, such as a scent mark. However, it does not *explain* the behavior; on the contrary, it summarizes in a more abstract way those observations which now stand in need of explanation.

Other examples similar in kind are the "internal" clock of the rat described by Church in Chapter 10 and the division of attention between the elements of a sample in a matching-to-sample task noted for pigeons by Riley and Roitblat in Chapter 9. These authors would, I think, agree that these concepts are not explanations, as such, of "timing behavior" or of the differences in performance on trials involving elements and compounds as stimuli. In fact, Church's chapter is largely devoted to experiments which specify the operation of the cognitive device. They elucidate the workings of the clock rather than citing them to explain behavior. Certainly one may eventually propose theoretical mechanisms for the operation of the clock, for the division of attention between stimuli, or for the registration of events in a working memory, but these concepts are not in themselves meant to accomplish this task.

2. Concepts like the above classify and characterize a particular set of behavioral observations. We proceed to a second level of conceptualization, at which terms of this kind are more theoretical, because the observed behavior is more widely separated from its presumed determining conditions. The behavior is used as an indicator of a process that has already taken place. A good example is the concept of association. In Chapter 3, Hearst describes a study by Browne in which pigeons were exposed to pairing of a key light with food but without the opportunity to peck at either. When these restrictions were removed, the pigeons pecked at the key more often during the course of autoshaping (which again involved pairings of these stimuli) than did other birds who had earlier experienced random or negative correlations between key light and food. Clearly, something not obvious to the experimenter happened to the birds during the

initial pairings, and this influenced the later criterion behavior. This process can be called the formation of an association. However, this too serves as a descriptive conceptualization rather than an explanation of the process.

Another example of this sort is the "cognitive map" proposed by Menzel in Chapter 13; see also Menzel, 1973. A young chimpanzee is carried around a field by an experimenter, and is able to view another person hiding food in clumps of grass, under leaves, etc., in a number of different places. The chimpanzee is then released from his cage after an interval, and with remarkable accuracy he visits the places where the food has been hidden. He does not retrace the experimenter's route during the hiding of the food, but generally follows a shorter, more direct path. Clearly, the chimpanzee must have learned a great deal from his initial observations. This learning is manifested in the absence of the experimenter, and while the chimp is moving on his own, rather than clinging to the experimenter. Clearly a process is at work which cannot be encompassed by a description of the chimpanzee's behavior during the test phase. That behavior is the result of such a process. But this process – the development or use of a cognitive map – summarizes a set of observations rather than explaining them. If we learn enough about the formation of association, the use of cognitive maps, etc., such concepts may become useful for the explanation of behavior in other situations. I will return to this issue.

3. At a third level of conceptualization, the concepts are again inferred from the observation of behavior, but in addition the concepts are endowed with active processes. This is the most "theoretical" level of conceptualization. In his chapter, Wagner provides a number of good examples; let us take the "rehearsal" of a prior event by the rabbit. In one study (Wagner, Rudy, & Whitlow, 1973), rabbits received discriminative eyelid conditioning with stimulus *A* (which was a CS+) and stimulus *B* (which was a CS−). Then they were started on simple acquisiton with a third, independent stimulus *C*. A few seconds after each reinforced *C* trial, a further trial with *A* or *B* was presented. For some animals, this trial was congruent with previous training – *A* was followed by the US and *B* was not. For others, the event was incongruent, or "surprising"; the US was omitted after *A* and presented after *B*. The rabbits that received incongruent trials conditioned much more slowly with stimulus *C*. Since the critical events *followed* each *C* trial, Wagner argues that they must have interfered with some rehearsal or consolidation process which followed the paired presentations of *C* and the US. In this case, the cognitive process is postulated very indirectly – through the relative rates of the acquisition of a response to which it contributes. But "active" characteristics are ascribed to the process because it appears to be necessary for learning, and it is subject to disruption by events which are not expected. The conceptualization in this case is both rich and complex. Nonetheless, this experiment identifies a particular process that can be subjected to

further analysis at the hand of appropriate further experiments. However, the concept of rehearsal does not *in itself* explain the differential speeds of learning which comprise its empirical basis.

B. Conceptualization and Explanation

I have stressed the descriptive aspects of concepts which are based on sets of particular experimental procedures and results. Such concepts incorporate processes or states which are not directly observable, but which are inferred from the data at hand. If they are not observable, such processes must be theoretical, and this in turn suggests that the concept into which they are incorporated should play some role in the explanation and not just the description of behavior. The philosophy of science adopted by many experimental psychologists ascribes explanatory functions to theoretical terms. But I have already suggested that some of the theoretical concepts put forward in this book don't "explain anything." Rather, they characterize complex relationships between behavior and its governing circumstances. How can cognitive concepts serve an explanatory function as well?

As I suggested earlier, the theoretical nature of a concept and its potential explanatory function are not necessarily linked. Explanations can, for example, be devised without the use of theoretical terms. Dallenbach and his associates (e.g., Supa, Cotzin, & Dallenbach, 1944) showed that blind people avoid obstacles by using auditory cues produced by their own movements. This explanation of "facial vision" replaced that theoretical concept, which had been invoked to characterize the ability of the blind to avoid obstacles. On the other hand, we have seen in the previous section that theoretical concepts can be formulated independently of any explanatory function.

Psychologists who have tried to link theory to explanation have at times introduced theoretical terms for the exclusive purpose of explaining a set of data, but without reference to any defining observations. These terms tend to be empirically rather vacuous, since no independent assessment of their validity is immediately available. I tried to show in the previous section that the cognitive concepts in question are not of this kind. The main effort has generally been the determination of the functional characteristics of the states or processes in question.

Church uses most of his chapter to delineate the characteristics of the internal clock. Riley and Roitblat (Chapter 9) ask whether the selective attention that is the topic of their chapter can be "cued" by a priming stimulus in advance of each trial. Olton has tested the capacity of working memory in the rat by increasing the number of arms in a radial maze. These characteristics are studied "in their own right," as it were, and not as part of an effort to provide the concepts with explanatory power. The usefulness of such concepts will, of course, be enhanced if they can participate in the explanation of behavior, but the

enterprise of formulating them on the basis of the kinds of data reviewed in the last section need not be abandoned if they do not do so immediately.

Under what conditions, then, does a concept possess explanatory power? An explanation is a set of general and specific statements from which particular observations can be deduced and/or predicted. If this set of statements accomplishes no more than the "prediction" of the data which led to the formulation of the theoretical terms contained in those statements in the first place, then the explanation is rather trivial. If, however, the theoretical terms permit the deduction or prediction of observations outside the set of the observations which led to their original formulation, then I suggest that the terms have true explanatory potential. In other words, the attributes ascribed to the states or processes represented by a concept should be useful in the prediction of data outside the definition of the concept. Such attributes may be suggested by the defining observations themselves. But they may also arise by analogy with concepts from other fields of research. For example, the similarity of cognitive concepts formulated independently on the basis of work with humans and animals may make it possible to extrapolate attributes from one set to the other. Such attributes may also emerge from a set of convergent operations, which suggest that a concept is richer than the observations based on any particular operation would imply. Attributes could also be suggested by work at a different empirical level. If the process in question is thought to reflect neurophysiological correlates, then those correlates may possess attributes which can be "translated" into a set of further behavioral observations.

Can the concepts offered in this book generate predictions in any of these ways? The pursuit of a cognitive approach would be a sorry undertaking if they could not. Wagner ascribes particular characteristics to the process of rehearsal in the rabbit; it is short term, as it tends to be in humans. From this it is predicted that if incongruent trials follow by a fair length of time the conditioning trials that provide the critical data, interference from the incongruent trials will be eliminated. This prediction has, in fact, been confirmed (Wagner et al., 1973). But the brief nature of the rehearsal was not inherent in the concept of rehearsal as a process that was originally identified with a separate set of observations.

In Chapter 5 on cognitive associations in instrumental conditioning, Fowler identifies a dimension of "discrepancy" between an outcome signalled by a stimulus (e.g., no food, delivery of shock), and the outcome produced by the response (e.g., food, absence of shock). This dimension is identified on the basis of one set of studies. He then predicts that if there is little discrepancy between the signalled outcome and the real outcome, the signal will block the effect of the outcome, even though the two may involve different motivational systems. Thus, Fowler predicts that the suppressing effect of a conditioned aversive stimulus upon drinking in the rat will be reduced (through blocking) when the shock reinforcer is preceded by a signal for the absence of food, but will be enhanced when the shock is preceded by a signal for the delivery of food. Fowler's