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INTERPRETING COGNITIVE PHENOMENA: REVIEW OF DONAHOE AND PALMER'S LEARNING AND COMPLEX BEHAVIOR[1]

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The thing that was exciting about Keller and Schoenfeld's *Principles of Psychology* (1950) and Skinner's *Science and Human Behavior* (1953) as textbooks was their systematic treatment. A few general, basic principles were introduced at the beginning, explained, and given empirical justification. Then those basic principles were used to interpret a wide range of interesting, significant, and complex behavioral phenomena such as conceptualizing, solving problems, remembering, talking, self-regulating, knowing oneself, and functioning as a member of a social group. Psychological phenomena that had seemed disconnected from each other could now be discussed in common terms. A student who studied one of these textbooks could come away with a broad but integrated view of psychology as a coherent science.

The approach, now known as behavior analysis, continues to be well integrated and broad in scope. But for some reason, this breadth is not always well represented in textbooks.[2] Understandably and appropriately, basic textbooks focus on fundamental principles and on the research designed to clarify those principles. That body of research has grown enormously over the years, and the issues have become more intricate and subtle. It is hard to cover the basic principles adequately in a course and still find time to show the wide-ranging applicability of those principles to the interpretation of complex human behavior. It may also be the case, as Dinsmoor (1989) has suggested, that textbook authors have become more conservative in their willingness to extrapolate. Whatever the reasons, the restriction in coverage can have unfortunate effects.

A particular problem is that students can develop a very mistaken impression about the division of labor within psychology. They may take a course entitled something like "cognitive psychology." There they will learn about research on perception, remembering, problem solving, and language. Most of this research will have been conducted with normal humans as subjects. The theoretical interpretations will likely be framed in the language of information-processing systems. And, indeed, there may be some effort to relate the concepts of human cognitive psychology to the modules of artificial intelligence (AI) computer models. Moreover, the students may hear about the spectacular growth of an interdisciplinary field called cognitive neuroscience that explicitly accepts the categories of cognitive theory and tries to identify their neurological underpinnings.

If students take a course on "basic learning processes," in contrast, they are likely to learn about research conducted mostly with nonhuman animals on Pavlovian and operant conditioning, and they will learn the terms and concepts that have evolved to describe those phenomena. Extrapolations to humans are likely to emphasize relatively simple phenomena such as child-rearing problems (e.g., temper tantrums and bed-wetting), teaching simple skills to developmentally delayed children, and treatment of phobias. Moreover, the students are unlikely to hear much about supposedly relevant neurological processes or computer models.

In other words, students encounter cognitive phenomena in one course and basic behavioral/learning phenomena in another, each with a distinctive set of terms and concepts. Understandably, then, students may conclude that psychologists who use terms like *reinforcement, stimulus control, conditioned reinforcement,* and *conditioned stimulus* are ei-

[1] Donahoe, J. W., & Palmer, D. C. (1994). *Learning and complex behavior.* Boston, MA: Allyn & Bacon.

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[2] There are, to be sure, some notable exceptions, such as Catania's (1992) coherent treatment of behavior analysis and cognitive phenomena.

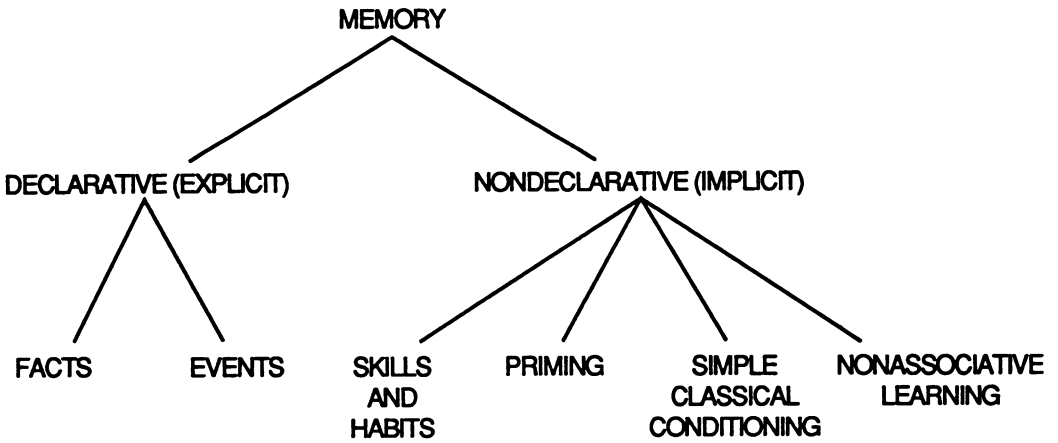

Fig. 1. A classification of memory systems. (Reprinted with permission from Squire, L. R., & Zola-Morgan, D. (1991). *Science, 253,* 1380–1386. Copyright 1991, American Association for the Advancement of Science.)

ther disinterested in or unable to deal with cognitive phenomena related to remembering, talking, solving problems, and so forth. Instead, the behavioral terms and concepts may appear to be applicable only within a narrow field—a field that appears to be isolated from developments in neuroscience and computer science.

There is, to be sure, more than a bit of irony in this perception of narrowness, given the broad scope of the classic textbooks in the field of behavior analysis. But the problem is an old one. In the 1930s and 1940s, psychologists who were influenced by Gestalt ideas (e.g., Tolman, 1932) carried out experiments designed to demonstrate that animals as simple as the rat were capable of remarkably complex adaptive adjustments when confronted with novel variations of training tasks. The performances were aptly characterized in cognitive terms. That is, the performances were described as "insightful," "purposive," and "expectant." At the same time psychologists in the stimulus–response (S-R) tradition conducted experiments designed to reveal and clarify elementary relationships—for example, between the ability of a stimulus to evoke a response and the number of times the response had been reinforced in the presence of that stimulus. The purpose of these detailed parametric studies was to generate the basic principles that would, it was hoped, explain complex cognitive phenomena. Yet critics of the S-R tradition saw in these simple experimental preparations evidence of disinterest in the more complex cognitive phenomena, prompting Hull (1943) to reply in some exasperation:

> The present approach does not deny the molar reality of purposive acts (as opposed to movement), of intelligence, of insight, of goals, of intents, of strivings, or of value; on the contrary, we insist upon the genuineness of these forms of behavior. We hope ultimately to show the logical right to the use of such concepts by deducing them as secondary principles from more elementary objective primary principles. Once they have been derived we shall not only understand them better but be able to use them with more detailed effectiveness. . . . (pp. 25–26)

The issue is not, of course, only a matter of misperception about narrowness of interests. Indeed, many psychologists believe that attempts to understand complex cognitive phenomena in terms of basic learning processes are, frankly, misguided (e.g., Gallistel, 1990; Köhler, 1947; Squire, 1987; Tolman, 1932). They may grant that the processes of Pavlovian and operant conditioning are real enough, but they doubt that such processes are usefully regarded as basic or foundational to higher cognitive phenomena. Instead, they prefer to account for higher cognitive phenomena in terms of correspondingly high-level (i.e., molar) cognitive processes, or systems, that operate in addition to and on a par with the basic processes of Pavlovian and operant conditioning. Figure 1, taken from Squire and Zola-Morgan (1991), illustrates

one such organizational scheme. Because the higher level processes are viewed as fundamental in their own right rather than derivable from the basic learning processes, distinctive research and theoretical traditions are seen as appropriate. Tulving (1991), expressed the view clearly:

> There exist a number of different brain/behavior/cognition systems and processes that through cooperation and interaction with one another, make it possible for their possessor to benefit from past experience and thereby promote survival. The known and as yet unknown memory systems deal with and operate on different aspects of the organism's environment, they function according to different principles, and they follow their own specialized laws of processing. (p. 25)

In sharp opposition to this "distinctive domains" approach, Donahoe and Palmer's *Learning and Complex Behavior* presents a carefully reasoned case in favor of hierarchical integration based on a single, small set of organizing principles. The book gives serious attention to research findings that have been generated in the traditions of cognitive psychology and psycholinguistics. In doing this, however, it attempts to show explicitly how those findings can be interpreted as products of learning histories involving relatively simple and general processes of behavioral selection. Moreover, the book attempts to show how these basic learning processes can be derived from even more fundamental principles that have been established through neurophysiological research. These derivations are made, in part, through a kind of computer model known as adaptive networks, instances of which are built from simple, undifferentiated elements rather than from the highly modularized units typical of traditional AI models. The book thus promises an integrated account of complex human behavior that is strongly grounded in biobehavioral science and connected with recent developments in computer modeling.

A number of noteworthy themes are developed in the book. I will comment on just four of them: selectionism and historical science, interpretation, the process of reinforcement, and the treatment of memory phenomena. (It should be noted that the authors have discussed many of these themes previously, in a series of papers: Donahoe, 1977; Donahoe, Burgos, & Palmer, 1993; Donahoe & Palmer, 1989; Palmer, 1991; Palmer & Donahoe, 1991, 1992.)

Selectionism and Historical Science

Donahoe and Palmer contrast two different approaches to understanding the origin of complex phenomena. One approach is to assume that specialized, complex processes are necessary to account for complex phenomena. The other approach is to assume that the processes are relatively simple and general; the complexity arises from the recurrent selection of small advantages (Catania, 1987; Dawkins, 1986).

Skinner's approach was strongly selectionistic (see Catania, 1987; Catania & Harnad, 1988). In this he was allied with other relatively molecular behavior theorists such as Thorndike, Guthrie, and Hull in opposition to Gestalt-oriented behavior theorists like Tolman and Köhler, who tended to invoke emergent, higher order processes to account for complex cognitive functioning.

Donahoe and Palmer provide a useful and extended discussion of the centrality of the selectionistic style of explanation to Skinner's approach. And, like Skinner, they emphasize that the selectionist style of explanation applies very broadly—to extraordinarily diverse phenomena such as the structure of the universe, biological evolution, and the evolution of cultural practices. The details about what gets selected and what does the selecting differ, of course, from field to field. But the basic explanatory framework is the same: There is some means of generating variability, some basis for selecting some variants over others, and some means of retaining and thus accumulating the effects of past selections. A common theme across the various fields is that selection-based explanations often come to replace explanations based on inferred, specialized, emergent, complex processes.

At the same time, Donahoe and Palmer urge a sense of humility about what a selection-based account can accomplish. Such accounts are historical, so they are constrained by the quality of the historical record.

> There are . . . limits to how deeply we can understand complex behavior, which is necessarily the product of an extensive history of selection in the natural environment that cannot be subjected to experimental analysis.

> Our incomplete knowledge of the initial conditions and the full history of selection limits the accuracy of our accounts of the present and our predictions of the future. . . . [Yet to] us, no other approach offers more genuine insight into the origins of organized complexity while simultaneously avoiding an appeal to gratuitous order-imposing principles that postulate the very organization that we seek to understand. (p. 26)

Interpretation

Although Donahoe and Palmer are unenthusiastic about inferred "order-imposing principles" and the like, they do see a critically important role for a kind of interpretation that they call *scientific interpretation.* And they provide a very useful discussion in favor of this technique. Their discussion is useful, in part, because the role of interpretation has been a source of some tension within behavior analysis. By arguing so clearly and explicitly in favor of interpretation, they invite readers to examine the issues carefully.

It is apparent, as Donahoe and Palmer suggest, that researchers never observe more than a small fraction of the total ongoing biobehavioral activity, much of which is inside the skin. Moreover, some of the unobserved biobehavioral activity surely has an effect on the behavior that is observed, either as part of a causal sequence or as additional elements of a larger pattern of somato-autonomic activity that includes the observed behavior (Hefferline, 1962; Kuo, 1967; Mechner, 1992; Schoenfeld, 1971; Smith, 1954/1967). Because biobehavioral events, whether measured or not, are natural events, their influential effects should, in principle, be verifiable by experimental analysis. But matters of convenience or of limitation in current technology often conspire to make such measurement impractical. In such cases there is a choice: One can simply refuse to incorporate in one's account any events that are not observed and, instead, include only those relationships that can be demonstrated with the techniques at hand, or one can try to make reasoned inferences about the existence and functioning of unobserved events.

The tension comes from the fact that behavior analysts have presented strong arguments on both sides of the question. Some, like Donahoe and Palmer, favor the disciplined use of inferred events in causal explanations; others oppose such constructions (see Schnaitter, 1978, for an insightful discussion of this tension). Interestingly, both sides can find support in the tradition of pragmatism (Baum, 1994, chap. 2; Zuriff, 1985). According to this tradition, the goals of scientific work are to enhance our ability to predict and control phenomena of interest and to achieve a greater degree of conceptual integration so that diverse phenomena can be seen as variations of a common pattern. Conceptual integration promotes what Mach called *economy of thought*: With conceptual integration we become more efficient, and thus more effective, in dealing with aspects of natural phenomena (Marr, 1985).

Often a scientific strategy that promotes the goal of conceptual integration will also advance the goals of prediction and control. But sometimes it will not (at least not immediately), and then it matters which of the pragmatic goals one most values—prediction and control, on the one hand, or conceptual integration, on the other.

Those who most value prediction and control as goals of scientific work are likely to be suspicious of explanations of behavior that rely on inferred events. For those goals the most useful kind of account is one that specifies how measurable events are related to each other. Ohm's law provides a simple example. It specifies that the level of the current in an electrical circuit (I) is directly proportional to the voltage (E) and inversely proportional to the resistance of the circuit (R); that is, $I = E/R$. Thus, if one were interested in predicting the current before actually testing a circuit, one could do so first by measuring the resistance of the circuit and the voltage of the power source and then applying the formula to calculate the predicted current. Likewise, one could alter the current by changing the resistance or the voltage. There are no inferred events in Ohm's law, and no "free parameters" that must be extracted after the fact from the data that one hopes to predict.

Explanations based on inferred events do not contribute to the goals of prediction and control, at least not so directly. Indeed, efforts to construct the inferential steps may distract from efforts to discover relationships between measurable events (e.g., between the

likelihood of behavior and aspects of the environment, both current and remote). Skinner made the point frequently that for the practical prediction and control of behavior, the causal sequence must be taken back to the point at which the most relevant variables can be measured and manipulated. Once that point has been reached, the intervening events can be ignored as far as their practical contribution is concerned (e.g., Catania & Harnad, 1988, p. 184; Hineline, 1990, 1992; Skinner, 1974, p. 231, 1980, pp. 77–78).

There are, however, some potential benefits of a different sort from including inferred events in accounts of behavior. Sometimes their inclusion can help to achieve a kind of parsimony by making it possible to conceptualize a poorly understood behavioral phenomenon as an instance of a more general, familiar type. For example, it may be possible to conceptualize some behavior as occurring due to the presentation of a discriminative stimulus or a conditioned aversive stimulus, even though no such stimulus can be found in the environment. Internal events may be inferred to play those roles. Without the inferred events, the occurrence of the behavior may be traced to observable antecedents, but the demonstrated molar relationships can often have an ad hoc character instead of being expressions of familiar behavioral processes. For someone who highly values conceptual integration as a scientific goal, then, accounts that rely on inferred events may hold some attraction.

Skinner, of course, made extensive use of these sorts of inferred events in his interpretations of complex phenomena such as "self-awareness," "self-control," "problem solving," and verbal behavior (e.g., Skinner, 1953, 1957, 1969). Autoclitic verbal behavior and editing, for example, were conceptualized as being evoked by stimulation (sometimes conditioned aversive stimulation) arising from the incipient stages of other verbal behavior. Other examples of behavior-analytic accounts based on inferred response and stimulus events include Schoenfeld and Cumming's (1963) analysis of perceptual phenomena, Hefferline's (1962) and Sidman's (1989) analysis of "defense mechanisms" such as repression, Sidman's (1989) interpretation of "conscience," Keller's (1958) analysis of skill learning, and Dinsmoor's (1985) interpretation of attentional phenomena.

It is well known too that S-R behaviorists such as Hull (1930, 1937; see Amsel & Rashotte, 1984; Wearden, 1989), Guthrie (1935, 1959), and Miller (1959) made skillful use of inferred internal responses and stimuli to construct parsimonious accounts of complex behavior in terms of acquired S-R relationships. Interestingly, Skinner's approach has sometimes been distinguished from those of the S-R theorists on the grounds that Skinner was unwilling to infer internal stimulus and response events to mediate temporal gaps between environmental cause and behavioral effect. It is indeed true that Skinner was often critical of such practices, especially when he was in his "prediction and control" mood. But he sometimes displayed a "conceptual integration" mood as well, and his interpretations, at least in their general style and purpose, would be hard to distinguish from those of Hull, Guthrie, Miller, and kindred behavioral theorists (see the interchange between Zuriff and Skinner in Catania & Harnad, 1988, pp. 216–217).

On the question of inference, then, Donahoe and Palmer have aligned themselves with the interpretive Skinner in a tradition that behavior analysis shares with S-R behaviorists who have sought to interpret complex behavior in terms of a limited set of basic learning processes. Accordingly, readers who especially value conceptual integration will probably find the book more appealing than will readers who especially value immediate, practical prediction and control.

And yet the kind of conceptual integration that interpretation fosters can, in fact, have practical implications, although those implications might not be apparent immediately. When the unfamiliar is conceptualized in terms of the familiar, whatever practical understanding we have of the latter can then be applied to the former. The new conceptualization can thus point the way to potentially effective variables that might otherwise go unnoticed.

Donahoe and Palmer are well aware that there is a slippery slope separating disciplined interpretation from unbridled speculation; consequently, they offer a thorough discussion clarifying what they consider to be the important differences between the two.

Interpretive and speculative accounts are similar in that both may include inferred events and processes. But they differ in the kinds of inferred events or processes that are legitimate to include. Scientific interpretations, as Donahoe and Palmer describe them, are limited to including only events or processes that are of a kind whose existence has been established through experimental analysis. Thus, interpretations may include events or processes that are hidden from view, but they contain no hidden *kinds* of events or processes. Speculative accounts are not so constrained.

> When we try to account for behavior outside the laboratory *solely in terms of the principles derived from an experimental analysis,* we are engaged in interpretation. To the extent that these principles prove insufficient and we are driven to invent supplementary principles, we are engaged merely in speculation. The interpretation of phenomena outside of the laboratory is a part of the scientific enterprise; speculation may suggest lines of inquiry, but it should not be confused with interpretation, as it is not constrained by empirically derived principles. (p. 325)

Behavior-analytic interpretations, such as Skinner's, are usually constrained in this way (cf. Schnaitter, 1978), as are those of Hull, Guthrie, and Miller. An advantage of such constraint is that disconfirmations may be interpretable:

> Unless a principle is the result of an independent experimental analysis, it is difficult to identify the origins of any problems that arise in interpreting the complex phenomenon. Do the problems result from a failure to trace all of the implications of the principles, a failure to identify all of the relevant principles, or an inadequacy in the principles themselves? When the principles arise from an independent experimental analysis, their adequacy is knowable apart from the complex behavior they are used to interpret. (p. 127)

An additional way to impose discipline on interpretations is to make each step in the inferential sequence as explicit and precise as possible. That way, one can be sure that the processes identified actually lead from the initial conditions to the complex result. Interpretations within psychology, including behavior analysis, are usually presented in narrative form—in a technical version of plain English, supplemented, perhaps, with some diagrams. Donahoe and Palmer acknowledge the value of this kind of interpretation, which they call *verbal interpretation,* but note an important weakness:

> Although verbal interpretation provides a very useful method for understanding complex phenomena, it has distinct disadvantages. Very often so many processes are involved—many of them acting simultaneously—that a purely verbal account cannot keep track of them all. Too much is happening at once for a sequence of words to faithfully describe the interrelations of the processes. . . . Verbal interpretation will always play an important role in understanding complex phenomena, particularly in the early stages of inquiry, but biobehavioral science ultimately seeks more precise means of interpretation. (p. 127)

In contrast, a method that Donahoe and Palmer call *formal interpretation* permits precise derivations.

> Computer simulation is the most common type of formal interpretation in modern biobehavioral science. In computer simulation, a computer is given a set of instructions, called a *program,* that embodies principles established by experimental analysis. As expressed in the program instructions, the principles are repeatedly applied to determine if they are sufficient to generate the complex behavioral phenomena observed in nature. . . . In comparison to purely verbal interpretations, computer simulations have the advantage of being precisely stated in the program's instructions and of being able to keep track of many simultaneously acting processes. (pp. 128–129)

From their view of interpretation, it is essential that the computer program include no processes beyond those that are analogous to processes demonstrated through experimental analysis.

> As with other forms of interpretation, no new principles may be introduced; the principles informing the program are restricted to those that are derived from research on the relevant biobehavioral processes. This use of computer simulation should be distinguished from that in which the goal is simply to devise a program whose output mimics some aspect of complex behavior, but whose instructions are not constrained by biobehavioral principles. Efforts of this second type fall within the field of *artificial intelligence.* (p. 128)

Students will encounter many examples

throughout the book of computer simulations of complex behavior that Donahoe and Palmer think meet these requirements. The programs are versions of a type called adaptive network, or neural net, models. Donahoe and Palmer describe the simulations clearly and in enough detail that students should be able to appreciate their broad purpose and the basic logic of their operation. But students will not be sufficiently prepared to generate their own simulations or to appreciate the extent to which the outcomes of the simulations might depend on the selection of initial conditions and the parameters governing how experiences change the strength of connections among the elements of the model.

Most of the interpretations in the book are verbal or mixtures of verbal and formal types. Students will encounter many detailed and elegant examples demonstrating that a few basic principles (e.g., conditioned reinforcement and elementary discriminative control) are sufficient to account for a variety of complex behavioral phenomena, including concept formation (chap. 5), stimulus equivalence (chap. 5), cross-modal generalization (chap. 5), and aspects of attention (chap. 6), verbal behavior (chap. 11), memory (chaps. 8 and 12), and problem solving (chap. 10). Many of the events critical to the accounts are inferred rather than observed. But their properties are constrained in the ways just described. Thus, students who follow the derivations should come to see how one can engage in disciplined interpretation while avoiding fanciful speculation.

Yet some readers may still feel some uneasiness about a presentation that so strongly endorses interpretation based on inferred internal events as a scientific strategy. They may remember that even the most plausible, disciplined, and internally consistent inferential accounts sometimes turn out to be wrong and, on hindsight, distracting. Such readers may prefer to develop accounts of complex behavior in terms of observed environment–behavior relationships, recognizing that such relationships will certainly be relatively molar. Conceptual integration may be achieved by developing an effective taxonomy of the relevant molar environmental events, such as contingencies, and their effects (e.g., Sidman, 1986).

From the other side, alert students may wonder if the line between inference and experimental demonstration is always so sharply drawn. They may go on to wonder if interpretation always can be so confidently distinguished from other kinds of inferential accounts. Whatever their position on the proper role of inference, however, students and teachers surely will benefit from the thoughtful consideration of these issues that Donahoe and Palmer's treatment invites.

Principle of Selection (Reinforcement)

Reinforcement, of course, is at the core of behavior-analytic accounts, and, as one would expect, there is an extensive discussion of the topic early in the book (chap. 3). But Donahoe and Palmer's treatment is unconventional in some interesting respects. First, they present a *unified principle of reinforcement,* one that applies equally to operant and respondent (i.e., Pavlovian) conditioning. It is a discrepancy-based principle. Second, they attempt to identify the neural processes that underlie the behavioral effects of the reinforcement operation. And third, the fundamental effect of reinforcement is to select an environment–behavior relationship rather than to increase the emission rate of the reinforced response. "The outcome of selection by reinforcement is a change in the environmental guidance of behavior. That is, what is selected is always an environment-behavior relation, never a response alone" (p. 68).

In this respect, then, Donahoe and Palmer's conception of reinforcement is very much in the tradition of S-R theory. Reinforcement either strengthened (e.g., Thorndike, 1911; Hull, 1943) or protected (Guthrie, 1935) the stimulus–response relationships that prevailed immediately before or coincident with reinforcement. In those accounts, the stimulus and response terms (i.e., the S and R) were understood to represent patterns rather than the isolated events characteristic of simple reflexes. Thus, what was selected was the ability of a particular stimulus pattern to evoke a particular response pattern.

An implication of the S-R conception is that the proper measure of the behavioral effect of operations like reinforcement and deprivation is some measure of the stimulus's

ability to evoke its response—for example, a latency or a probability of the response given the stimulus. Rate of response would make little sense as a measure, under the S-R conception, because the rate of a response would depend critically on the rate at which its evocative stimulus occurs.

There is a very basic problem, however, with measures of stimulus–response relationships: Operant behavior occurs in a stimulus context, but there often is no identifiable stimulus change that precedes each occurrence of the response that is at issue. Consequently, it is often impossible to measure the very relationship that is construed as fundamental to the reinforcement process according to the S-R conception. Faced with this dilemma, some theorists concluded that the "free-operant" preparation is a poor one for revealing the fundamental effects of reinforcement; instead, they focused on discrete-trials procedures in which a one-to-one relation between stimulus and response was enforced. Such theorists were inclined to infer the existence of stimuli that precede each response occurrence when such stimuli are not apparent, as in free-operant procedures.

Although Skinner initially treated operant behavior as being elicited by a stimulus, he soon took a different tack, arguing that operant behavior is "essentially an emissive phenomenon" (Skinner, 1950, p. 198; cf. Hineline, 1990). The proper measure for operant conditioning, then, is the likelihood of the behavior's occurrence (more precisely, its rate of occurrence) in the presence of a homogeneous stimulus context. Skinner's emphasis on emission rate as the fundamental measure (dependent variable) seemed liberating because it meant that fundamental relationships could be established in procedures that allowed the response to occur repeatedly over long periods of time without the constraints of trial onset and offset.

As discussed earlier, Skinner was not averse generally to inferring internal stimulus and response events in his interpretations. But he was unwilling to infer, as a matter of first principle, that there must be an evocative stimulus preceding each instance of an operant and that reinforcement was fundamentally a matter of strengthening S-R relations.

Thus, Donahoe and Palmer's treatment of reinforcement may be a bit jarring to some behavior analysts. An interesting question to consider, however, is what practical differences might arise depending on whether one takes an S-R strengthening view of reinforcement or an emission-rate-enhancing view. A reader might ask, for example, whether the interpretations of verbal behavior, remembering, and problem solving given in the later chapters of the book would have been different if the fundamental process of reinforcement had been expressed differently in chapter 3. And if so, how? If there are not many practical differences, then the sense of commonality between behavior analysts and the S-R behaviorists would be strengthened (see also Williams, 1994a, 1994b).

There is a practical consequence for Donahoe and Palmer's theoretical treatment. Their adaptive network models are constructed from elementary connections intended as analogues of stimulus–response relations. The program reacts to certain kinds of discrepancies (analogous to reinforcement) by changing certain stimulus–response connection weightings. They justify these program features partly on the basis of their interpretation of neurological evidence. Thus, they take the success of the computer simulations in generating analogues of complex behavioral phenomena as support for an S-R based principle of reinforcement rooted in and integrated with neurological processes that have been established through experimental analysis.

It probably should be noted, however, that their interpretation of one set of neurological research seems to be questionable.[3] They cite the work of Stein and colleagues as showing support at the level of neurons for an S-R-like conception of reinforcement. But what this body of research seems to have demonstrated is that the spontaneous firing rate of single neural cells can be increased by following spontaneous firings by injection of a neurochemical (e.g., Stein, Xue, & Belluzzi, 1993, 1994). Stein et al. described these results as a neural analogue of an emission-rate-enhancing effect of reinforcement rather than an S-R-connection strengthening effect.

Yet, regardless of the eventual fate of their particular integrative conception of rein-

[3] I thank Steven Kemp for pointing out this discrepancy.

forcement, Donahoe and Palmer certainly have performed a great service by making accessible to students the possibility of an integrative account and pointing the way. The possibilities are exciting, and students should benefit from seeing a carefully reasoned example.

Remembering and Problem Solving

The last five chapters (chaps. 8 through 12) focus on complex cognitive phenomena, including problem solving, verbal behavior, and remembering. Many of the data cited in these chapters are from research conducted outside the tradition of behavior analysis, but the interpretations are behavior analytic.

The treatment of memory is particularly useful because of the common impression that behavior analysts have little interest in the topic and little to say about it. This impression is incorrect, but it is easy to see how it might have arisen, because behavior analysts have tended not to talk much about human memory phenomena (at least not explicitly) nor have they contributed much to the empirical literature on the topic. Moreover, behavior analysts (e.g., Branch, 1977; Marr, 1983; Wixted, 1989b) have been highly critical of many of the theoretical constructs used commonly to interpret memory phenomena (e.g., encoding, storage, and retrieval processes). But rejection of a theoretical framework does not at all signal disinterest in the relevant empirical subject matter.

Donahoe and Palmer devote two chapters to this topic. The first is entitled "Memory: Reminding," and the second is entitled "Remembering." Interestingly, these two chapters do not occur consecutively but are, instead, separated by three other chapters entitled "Functioning of the Experienced Learner," "Problem Solving," and "Verbal Behavior." This sequencing of material may strike some readers as odd, but there is an important and interesting basis for it. The implication seems to be that *reminding* is somehow simpler than *remembering*. And that, indeed, is the authors' point. When we speak of memory, they argue, we often are speaking about instances of fairly straightforward stimulus control. Through conditioning, a stimulus becomes capable of evoking a response. If that stimulus (or some aspect of it) reappears, the response may be reevoked. The particulars of the stimulus-control relations may be remarkably adaptable, subtle, and complex due to the organism's complex learning history. But such complexities do not alter the basic point that the phenomenon is essentially environmental control of response occurrence. Thus, the term *reminding* seems appropriate. Much of the research on stimulus control conducted within the behavior-analytic tradition could be construed as research on reminding and thus on one class of memory phenomena.

There are important cases of human memory, however, that do not seem to fit the straightforward stimulus-control interpretation. The environment may not provide a stimulus sufficient to evoke the response, so the individual has to "do something else" in order for the response to occur. For example, if someone asked you what you had for breakfast, the current environmental stimulus may be insufficient to evoke the appropriate response such as, "I had fried eggs, toast, and strawberry jam." You may need to engage in some additional behavior that increases the likelihood that you will come into contact with stimuli that can evoke the response. If you are home, you might go to the sink and examine the dirty dishes for breakfast traces. The effect of this behavior may be to produce discriminative stimuli that evoke the appropriate target response. If you are not at home, you may do the same sort of thing imaginally: "Let's see, I can see myself rinsing crumbs off the plate, so that means I must have had toast; oh yes. . . ." An essential feature of this second kind of memory phenomenon is that the organism must *do* something before the target response can occur. The term *remembering* suggests this more active role for the organism.

Such behavior may be called *precurrent*. Precurrent behavior alters the variables that make other behavior (e.g., the target response) more or less likely to occur, for example, by providing supplementary stimulation through prompts and probes. Skinner (1953, chaps. 15 through 18) discussed the role of such precurrent behavior in "self-control," "decision making," and "problem solving," and he briefly touched on its role in remembering. Skinner is, in effect, agreeing with nonbehavioral theorists that "something more" is going on in these phenomena than

merely the observed target response. But for Skinner that "something more" is not a different kind of thing from behavior; it is additional operant behavior. Part of our behavior can alter the variables that control other parts.

Donahoe and Palmer's treatment of remembering is consistent with Skinner's but is vastly more extensive and thoroughly developed. Their reasons for placing the chapter on remembering last, after the ones on problem solving and verbal behavior, become clear: The roles of precurrent behavior are easier to grasp in connection with problem solving, and much of the significant precurrent behavior in human remembering is verbal.

> As with problem solving in general, remembering is a process of marshaling supplementary stimuli to augment the effects of the current context so that the target response becomes stronger than competing responses. (p. 331)

An implication of this view is that effective remembering (like problem solving in general) is an acquired skill. Precurrent behavior is shaped like any other operant behavior and will vary in effectiveness across situations and among individuals.

> We must not forget that recall procedures do not lead us inexorably to the target response. They are more like blind groping than directed searches: Of necessity, we don't know what we're looking for. We use those mnemonic procedures that have been most strongly reinforced in the past under similar circumstances, and there is no guarantee that they will be the most effective ones for the task at hand. Moreover, recall procedures are likely to prime responses related to the target response as well as the target response itself. These responses will compete with the target response and may be prepotent. (pp. 341–342)

Moreover,

> If our account is correct, children will be unable to remember the past until they acquire behavior that generates supplementary stimuli. They will be skilled at remembering the past only when they have acquired a full range of acquisition and recall mnemonic procedures. . . . The child can be reminded of the past, but cannot remember it when asked about it. (pp. 342–343)

Although Skinner laid the groundwork over 40 years ago for interpreting human remembering as analogous to problem solving, behavior analysts seem not to have given the idea much attention since. Instead, when behavior analysts have discussed memory phenomena, they have tended to focus on stimulus-control relations, that is, on reminding (e.g., Branch, 1977; Catania, 1992; Wixted, 1989a).

Such focus may have encouraged the view that behavior analysis is fundamentally limited as an approach to human memory phenomena. Indeed, some experienced human memory researchers, who are otherwise quite sympathetic to behavior-analytic criticisms of the style of theorizing prevalent within cognitive psychology, nevertheless see a wide gulf between behavior-analytic interpretations of memory phenomena and their own (e.g., Roediger, 1980; Watkins, 1990). As they see it, behavior analysts properly and usefully understand the role of stimulus control but fail to grasp the essential role of the rememberer as a purposive, active participant. After arguing that memory researchers should give more attention to stimulus control, Watkins (1990) put the matter this way:

> Stimulus control has, of course, long been of central concern to the behaviorists, but if mediationists have failed to do justice to the stimulus world the rememberer inhabits, behaviorists have ignored as a matter of policy the role of the rememberer's own willful control. A more balanced perspective is sorely needed. Memory serves a biological function, and to understand it adequately requires consideration of both the needs of the rememberer and the nature of the real world in which the rememberer evolved. . . . (p. 331)

It may be that behavior analysts have indeed ignored the "the rememberer's own willful control." But if they have done so, it has been as a matter of practice, not as a matter of policy. Many behavior analysts will see a similarity between what is meant by "the rememberer's own willful control" and the kind of precurrent behavior discussed by Skinner and by Donahoe and Palmer. Such behavior is operant, and "operant behavior is the very field of purpose and intention" (Skinner, 1974, p. 61).

Thus, a potentially important contribution of Donahoe and Palmer's book might be to

remind behavior analysts of the broad roles of precurrent behavior in behavior-analytic interpretations of complex human cognitive phenomena. A conceivable result could be more substantial contributions by behavior analysts to the empirical literature on these topics and, perhaps, even some forging of productive alliances with cognitive psychologists who have become disenchanted with the excessively speculative theorizing that sometimes characterizes their field.

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