*THE VOCABULARY OF REMEMBERING:
A REVIEW OF KENDRICK, RILLING, AND DENNY'S
THEORIES OF ANIMAL MEMORY¹*

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Theories of Animal Memory, edited by Kendrick, Rilling, and Denny, presents a state-of-the-art cognitive analysis of how animals remember. The book consists of nine chapters written by some of the leading researchers in the field of animal memory. Its three main sections are entitled "The Grand Approach," which includes chapters justifying, describing, and illustrating the cognitive approach to understanding animal behavior; "Memory Processes," which reviews specific theories and some data; and "Theoretical Issues," which offers some comprehensive cognitive models of animal memory and some thoughts regarding the future of animal memory research.

The book's title, and certainly its content, calls to mind Skinner's (1950) classic article, "Are Theories of Learning Necessary?" According to Skinner, mentalistic theories are not only unnecessary they can be detrimental to the proper conduct of scientific inquiry. When behavior is explained by appealing to underlying mental phenomena, an unwarranted sense of satisfaction may discourage further analysis in terms of manipulable variables. Even the highly touted heuristic properties of mentalistic theories are, in Skinner's view, of questionable worth: "That a theory generates research does not prove its value unless the research is valuable. Much useless experimentation results from theories, and much energy and skill are absorbed by them" (p. 194).

The volume under consideration provides a fitting opportunity to evaluate Skinner's reservations about theories. The "cognitive revolution" in animal learning has been underway for approximately 20 years, and during

that time a large number of animal memory theories have been proposed. As we shall see, some of the more elaborate models appear to exemplify the dangers enumerated by Skinner. On the other hand, most of the theories reviewed in this volume consist of little more than mentalistic translations of our everyday language of remembering. Before these terms evolve into more complex mental structures they occasionally suggest novel experimental tactics that yield intriguing results. Thus, we may wish to consider whether any of these terms can participate in the experimental analysis of remembering without eventually generating useless experimentation or diverting attention from the environmental determinants of behavior.

TERMINOLOGY

In the first chapter of the book, Roitblat and Weisman express their dissatisfaction with any analysis based upon functional principles and suggest that areas such as "learning, remembering, problem solving, rule and concept formation, perception, and recognition" (p. 3) are better analyzed by conceptually decomposing the animal mind into an extended series of hypothetical mental stages. As they describe it, "Each stage takes time and operates on the output of the previous stage, transforming the output, and perhaps combining it with information from other sources" (p. 7).

If terms such as *remembering* and *recognition* are, from the outset, taken to refer to events occurring "somewhere else, at some other level of observation" (Skinner, 1950, p. 193), then the stages-of-processing orientation certainly has its advantages over a functional analysis. Few strategies are available to the experimental psychologist interested in manipulating intangible mental processes. But it seems fair to question whether these terms are correctly interpreted when they are necessarily assumed to refer to something beyond the reach of our

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senses. Indeed, by examining how these terms are actually used by the verbal community, we may find that they refer to something altogether different. A logical place to begin such an inquiry is with an examination of the term *memory* itself (cf. Branch, 1977; Marr, 1984; Shimp, 1976; Wasserman, 1982).

From a cognitive standpoint, the term *memory* refers to an actual or metaphorical repository of past experience stored in the form of enduring mental representations. Under the right conditions, these representations may exert control over (i.e., explain) current behavior. Defined this way, one studies memory by investigating hypothetical processes that bridge the temporal gap between the presentation of a stimulus and the opportunity to respond. Alternatively, we may take note of the fact that *memory* and *remembering* are nontechnical terms born of our natural language and that we have learned to use these terms when certain discriminative conditions prevail (cf. Skinner, 1945). For example, we say that I have memorized the 50 states if I can name them without looking at a map. I remember how to play the piano if, after a long hiatus, I can still play the Moonlight Sonata without making any mistakes. And I am said to remember what was on the list of words you just read to me if I can correctly state them aloud or write them down. The latter use of the term, an example of what Tulving (1972) has labeled "episodic" memory, seems closest to the way in which the term is usually applied to animals. That is, we use the term *memory* to refer to situations in which behavior is a function of an episode no longer present (Branch, 1977; Catania, 1984; Jans & Catania, 1980).

This interpretation of memory is certainly not new, though it does seem to require reiteration from time to time. Wasserman (1982), for example, reminded us that Tolman (1925/1961) viewed memory in precisely the manner described above:

To say that an animal remembers "such and such" is merely another way of saying that his present behavior can be shown to be causally dependent upon "such and such." The animal's going-to-or-from an object, not immediately present, does, to be sure, imply that his behavior is then and there a function of a now absent object. But this tempero-functional dependence of behavior is a purely objective fact and all

that need be meant by memory. (Tolman, p. 35)

Tolman's functional account of the word *memory* seems to be in agreement with Ryle's (1949) analysis of mentalistic terms in general (cf. Harzem & Miles, 1978). From one perspective, the term is taken to refer to an invisible happening that exerts control over (and explains) observed behavior. From another, the term is taken to refer to a particular type or category of behavior. Although we may never be able to settle the question of interpretation to everyone's satisfaction, it seems important to consider the differences between the two approaches; how one construes the language of memory may partially establish the direction of subsequent research and perhaps predetermine the character of more developed explanations.

The volume under consideration is replete with terms that may be given a dual interpretation of the sort suggested by Ryle (1949). In what follows, I shall consider some of these terms, describe their usual mentalistic interpretation, and inquire into whether they might instead be taken to refer to discriminative conditions relevant to the study of animal behavior. Following that, I shall briefly examine several elaborate models of animal memory that have evolved, perhaps inexorably, from the initial mentalistic translation of these terms.

Imagery

In chapter 3, Denny proposes a return to stimulus-response (S-R) conceptions of behavior, but with a twist. Specifically, he suggests that memory may be best analyzed in terms of stimulus classes and response classes that are mediated by "imagery." To make his case, Denny provides two arguments intended to justify the use of this term as an explanation for observed behavior. First, "... if the critical cue is clearly a percept, it stands to reason that the 'memory,' to function at all, must be as similar as possible to the percept, that is, a representative visual image" (p. 41). And second, "There is absolutely no theoretical reason why animals devoid of language should be without imagery" (pp. 41-42). Denny then shows how his appeal to imagery helps to resolve the problems posed by long-delay learning for S-R accounts of behavior (Garcia &

Koelling, 1966). In this procedure, an animal is allowed to eat, then, after some long delay, the animal is given an injection that causes illness. Despite the long delay, animals learn to avoid the food after as few as one trial. An S-R analysis has difficulty accounting for such learning because the interval separating the conditioned and unconditioned stimuli is far longer than the maximum learning interval in standard Pavlovian arrangements.

Denny suggests that the problem is resolved in a rather obvious way when the concept of imagery is introduced. That is, he assumes that when the animal is made ill, an image of the most recent gastronomical events is evoked. As a result, the conditioned stimulus (image of the food) and unconditioned stimulus (nausea) would be contiguous after all, and conditioning (albeit backward conditioning) would be expected to take place.

Of course, one can always simply posit the existence of a conscious or unconscious mental image, derive behavioral predictions, and subject those predictions to empirical test. This appears to be standard practice in cognitive science. But, to be useful in the functional analysis of behavior, one would like to be able to identify examples and counterexamples of imagery. Can we imagine a set of conditions and behavioral responses to which we might be prepared to apply the term *imagery*? I think that perhaps we can, though one would not want to defend the application of the term too passionately because it is assuredly inexact. If an adult man describes for us the location of all of the windows in his childhood home, we might be prepared to classify this as a case of imagery (because he is not, one would presume, reciting memorized verbal facts). If he then tells us the address of that home or the name of the city in which it is located, we would not continue to call this an example of imagery. In the one case, the man's behavior seems more or less similar to the behavior we would expect if he were viewing a model of the house (e.g., he is describing its three-dimensional visual properties). In the other case, he seems to be revealing a memorized fact unrelated to its visual properties. But, interpreted this way, it would be a mistake to ask if the first case *really* involves imagery while the other case really does not and to suggest that the methods of science will help us to decide. The

term does not apply to an entity but to an inexact partitioning of behavior. We call it imagery when behavior is more or less similar to behavior occasioned by a picture, image, or model, not when we believe that there is an actual entity in front of the mind's eye.

We can easily imagine situations like this involving animals. For example, consider a hypothetical experiment in which a rat is presented briefly with a map (or image) of a maze and then allowed to traverse the maze to find food. If the rat negotiates the maze successfully on the first trial, we might be prepared to say that the animal remembered the image of the maze. And we would mean that the animal's responses are consistent with an overall picture of the maze rather than being consistent with trial-by-trial conditioning of turn-point choices. But that is all we would mean. Which is to say that we would not mean that the animal is guided by a subjective inner picture, or that we have in any sense explained its successful performance, or that the appropriate next step is to map out the properties of that image. The utility of the term (if any) is to differentiate situations in which responding is more or less similar to human behavior occasioned by an image, picture, or model of something from those in which responding is not especially similar to that kind of behavior. A pigeon choosing the correct response on a delayed matching-to-sample (DMTS) task, for example, is not a clear case of imagery, because pecking a red key following a red sample is not at all like following a map or rotating a three-dimensional object. Unlike the case of the rat running the maze or the man describing the windows of his house, there is nothing in the behavior of the pigeon that is like what humans do when they are guided by a picture of something.

Prospection and Retrospection

Wasserman (chapter 4) offers an overview of animal memory research concerned with the distinction between *prospection* and *retrospection*, giving credit to Konorski (1967) for initiating empirical inquiry into the subject. As typically defined, a prospective strategy involves remembering "what to do," whereas retrospective strategy involves remembering "what was presented." Konorski distinguished between procedures that seem to be conducive

to establishment of prospective codes (e.g., the delayed response) and those that seem more conducive to the establishment of retrospective codes (e.g., DMTS). For example, in the delayed-response paradigm, an animal observes the illumination of one of several food wells and, after some delay, is released and permitted to approach one of the wells. In this case, the animal need only remember what to do upon release (e.g., go to the middle food well). In DMTS, however, the choice response is less predictable because the matching stimulus can appear on either of two keys. Such conditions presumably favor a retrospective memory code (i.e., remembering what was presented).

It is, of course, a simple matter to hypothesize on purely theoretical grounds the existence of a prospective code or a retrospective code (independent of what kind of memory procedure is employed) and test some derivative predictions. Indeed, Wasserman's chapter contains a review of many studies that have adopted precisely this strategy. But the results of this review suggest that it is exceedingly difficult to resolve the issue one way or the other. For each of several procedures considered, some studies are consistent with an analysis in terms of prospective codes, whereas others are consistent with an analysis based on retrospective codes. Moreover, in many cases the predictions tested do not seem to be especially compelling in the first place. For example, Honig and Wasserman (1981) compared pigeons' performance on a delayed simple discrimination task with performance on a delayed conditional discrimination task. Their results showed that lengthening the duration of the sample stimulus had a beneficial effect only on the delayed conditional discrimination problem. From their perspective, this result suggested a retrospective strategy because "retrospection involves perseverative sensory traces" (p. 59) that are presumably strengthened while the sample is present. But it seems just as reasonable to suppose that the longer the sample was presented, the more likely the pigeons were to adopt some sort of prospective strategy (e.g., "peck if the test stimulus is red, don't peck if it is green"). Indeed, one reason why this area of research produces such inconclusive findings might be traced to the way in which the terms *prospective* and *retrospective* are interpreted to begin with.

To what do the terms *prospection* and *retrospection* refer if not to an invisible memory

code? Probably they refer to nothing exactly, but instead inexactly map onto certain kinds of memory situations. With humans, examples of prospective and retrospective memory are easy to identify. Upon leaving in the morning, a father may say to his son "Don't forget to take this letter to the post office this afternoon." If the son complies, this is clearly a case of prospective memory as he has remembered what to do. If a colleague later asks the father, "What did you have for dinner last Tuesday?" and the father supplies the correct answer, we would perhaps be prepared to classify this as a case of retrospection. His job was to remember what took place, not what to do.

To the extent that animal memory procedures approximate or represent these situations, we may be prepared to call them retrospective or prospective. But we will also recognize that many situations exist in which the connection to *prospection* and *retrospection* is not clear, and in those cases science may not help us to decide between them. As indicated above, Konorski has already classified as prospective those situations in which, upon presentation of the memory cue, the correct response, though delayed, is immediately apparent (e.g., to peck or not to peck, to peck left or to peck right, to choose the middle well, etc.). And we might just as easily classify as retrospective any memory task in which the correct response is completely unpredictable until after the delay elapses (e.g., as when the retention test is unexpected). We might even usefully extend the range of use in the following way. If we prepare a memory task in which, looking in the forward direction, the correct response is more or less predictable while, looking in the backward direction, many possible cues could have been presented, we might be prepared to classify a correct response as an instance of *prospection*. For example, we might train a pigeon to choose the red key following any of a set of 10 stimuli and to respond to a green key following any of a different set of 10 stimuli (cf. Zentall, Urcuioli, Jagielo, & Jackson-Smith, 1989). But if we arrange the task the other way around (following red, choose whichever of the 10 stimuli associated with red is presented; following green, choose whichever of the 10 stimuli associated with green is presented), we might be prepared to call a correct response an instance of *retrospection*. And if pigeons perform better on the first arrangement than on the second,

we might even be prepared to agree with Wasserman that prospective tasks are easier for the pigeon than retrospective tasks.

Obviously, we can imagine a host of experimental procedures within the field of memory that are not very easily regarded as prospective or retrospective. And, according to the translation outlined above, it would be a mistake to ask whether these procedures actually involve retrospection or prospection. The terms do not apply and thus may do more harm than good in our struggle to force a good fit. But this limitation does not necessarily render the terms useless. What they offer is a workable division of the domain of memory, not an account of inner workings or an explanation of the behavior we observe.

Expectancy and Anticipation

Honig and Dodd (chapter 5) review a number of fascinating results from the differential outcome literature that are explained on the basis of expectancy. The basic procedure is extremely well known, but a brief review is important in order to explore the use of the term *expectancy*. In a typical differential outcome experiment, a correct response following one stimulus combination (e.g., choosing the red stimulus following a red sample) is reinforced with water, whereas a correct response following another stimulus combination (e.g., choosing the green stimulus following a green sample) is reinforced with food. DMTS performance is markedly improved using this procedure compared to when the reinforcement outcomes are nondifferential. In addition, other stimuli separately associated with food or water can be substituted as comparison stimuli without necessarily disrupting performance. The most widely accepted explanation for these findings is that stimuli become associated with expectations that (a) are more durable than a sample memory trace and (b) guide responding to other stimuli associated with the same expectation (Peterson & Trapold, 1980; Peterson, Wheeler, & Trapold, 1980).

We may quibble with the proclivity of cognitive scientists to imagine an expectancy as an ontological entity (represented in theoretical accounts as "E") and to assume that the data are explained only after appeal to it. Nevertheless, the translation of an expectancy analysis into functional language is relatively straightforward. Even in an ordinary DMTS experiment, the assertion that a pigeon expects

to receive food is presumably one of the least controversial observations that can be made. And by that statement we may mean nothing more than that the pigeon is paying attention, engaging in responses that produce food, peering into the food hopper, and perhaps reacting with fluster when the hopper breaks down and food is not delivered on schedule. Furthermore, one may assert with confidence that the reliable presentation of food under particular discriminative conditions was responsible for generating the behavior we summarize by the term *expectancy*.

Extending the use of this term, one may wish to inquire into what would happen if the procedure were altered such that the pigeon's expectations differed depending upon which sample was presented. That is, following one sample, Event A would be arranged to happen reliably, and following the other sample, Event B would be arranged to happen reliably. The subsequent events could be almost anything (food outcomes, memory intervals, interval distributions, houselights, hopper lights, hopper durations, reinforcer delays, etc.). If all of these events improve performance, surely even a dedicated functional psychologist would be prepared to defend the claim that associating samples with different expectations facilitates performance compared to when they are associated with the same expectations. But the same psychologist would not be prepared to assert further that the expectancy is a real-time entity that exerts control over (and explains) observed behavior. The value of the term *expectancy* is that it carves out a particular subdomain of memory replete with experimental possibilities. Of course, this Rylean analysis of the term represents only a point of departure. A behavior analyst interested in furthering our understanding of these procedures would attempt to refine the expectancy principle by investigating what other properties (i.e., other than "differentness" of outcome) are involved in influencing behavior. A cognitive scientist, by contrast, might begin to investigate the nature of the "expectancy code" and the processes that operate upon it throughout the retention interval.

Interference

The term *interference* may sometimes be avoided in a functional analysis of behavior because it seems to suggest some sort of collision of or competition between nonmaterial

entities. Indeed, the term is often used in just this way in the animal memory literature. But Wright, Urcuioli, and Sands (chapter 6) approach the subject of proactive interference in a way that is already decidedly functional. That is, the term is used in such a way as to allow us easily to distinguish a proactive interference (PI) situation from a nonproactive interference situation. PI is said to exist when preceding stimulus conditions exert a detrimental effect on memory performance. Such interference is absent when the preceding conditions do not appear to affect performance.

Typically, PI is thought to derive from other stimuli presented either within a single trial (e.g., an interfering stimulus presented immediately prior to the sample) or on the immediately preceding trial (e.g., the sample from the preceding trial governs responding on the current trial). Wright et al. (chapter 6) review a convincing set of results that suggest yet another, more powerful, source of PI. Specifically, the repetition of a small set of sample stimuli throughout a session may itself impair performance through a buildup of PI, a phenomenon they term the *repeated-item PI effect*. For example, in a typical DMTS experiment, only two stimuli (e.g., red and green) are employed on every trial. These conditions maximize repeated-item PI because every preceding trial involves the same stimuli as the current trial. This interference can be attenuated greatly by sampling from a much larger stimulus pool such that stimuli are rarely, if ever, repeated. Indeed, the results of many experiments, especially those using the procedure of serial-probe recognition, confirm this source of PI. When list items are drawn from a small sample pool, performance typically falls within the range of 50% to 60% correct (e.g., MacPhail, 1980). When the list items are drawn from a large sample pool (such that they are rarely repeated within a session), performance typically falls within the range of 80% to 90% correct (e.g., Santiago & Wright, 1984).

As with the words considered earlier, the somewhat more specialized term *interference* is employed when certain discriminative conditions are satisfied. If the presentation of preceding stimuli disrupts matching performance on DMTS, then we are studying a case of proactive interference. Similarly, if the presentation of stimuli interposed between sample presentation and the opportunity to respond

has a similar detrimental effect on performance, then we are studying a case of retroactive interference. But acknowledging this is not to suggest that the preceding stimuli disrupt a memory trace or rehearsal process that bridges the gap between stimulus offset and the opportunity to respond. Instead, the term identifies a potentially useful strategy to exploit in the experimental analysis of remembering.

Rehearsal

In an effort to define comparative cognition, Rilling and Neiworth (chapter 2) essentially argue that confidence in the reality of a hypothetical cognitive construct is increased when the results of several different experimental preparations are all consistent with that construct. To illustrate this approach, they consider the question "Do animals rehearse?" The authors find that they are able to answer this provocative question confidently in the affirmative because the results of at least four distinct experimental preparations are consistent with the idea. An example from the directed forgetting literature best illustrates the putative rehearsal phenomenon. In this procedure, the DMTS sample stimulus is occasionally followed by a "forget cue" (F cue) that signals cancellation of the choice phase. Presumably, this cue instructs the pigeon to terminate rehearsal of the sample memory. After extended training on this procedure, the pigeons are exposed to F-cued probe trials in which the choice stimuli are presented unexpectedly. If the presentation of an F cue aborts a rehearsal process, then the pigeon should be unable to remember which sample was presented on these probe trials, resulting in poor performance. Indeed, several studies have supported this prediction (e.g., Grant, 1981; Stonebraker & Rilling, 1981).

Recall that the cognitive scientist is comfortable postulating the existence of a wholly theoretical entity (in this case rehearsal) and testing derivative predictions. If those predictions are confirmed, the term then serves as an explanation. But for the term to be useful in a functional analysis of behavior, we would like to be able to distinguish a rehearsal situation from a nonrehearsal situation. In humans, there is indeed a set of conditions that warrant the use of the term *rehearsal*. For example, when someone is trying to remember a phone number and repeats the number con-

tinuously prior to dialing, the term certainly applies. They might even do so silently. But if we are convinced that they are trying to remember the number, and they are in general truthful, and they report "yes, I repeated the number until I dialed," we might be prepared to say that they rehearsed. With regard to animals, I have been unable to convince myself that there is any set of conditions or behavior that would warrant the application of the term *rehearsal*. Perhaps the term would offer a useful distinction between those procedures that tend to produce differential delay interval behavior from those that do not. For example, if following the red sample the pigeon dips and raises its head rapidly and after the green sample the pigeon pecks the side wall continuously, perhaps we would be prepared to say that the bird is rehearsing. But, in this case at least, the application of the term does not seem to offer anything new, because delay interval response chains were recognized long before anyone adopted the belief that animals rehearse (Blough, 1959).

After reading these chapters, one may be left with the impression that the animal mind is now a rather densely postulated place. Even in the most straightforward DMTS arrangement, we may have a mixture of memory traces, expectancies, prospective and retrospective codes, rehearsal, and imagery. According to the present view, however, efforts to determine if we are *really* witnessing imagery, or expectations, or rehearsal, or prospective codes, or whatever, by means of experimentation are unlikely to succeed. Such efforts demand precise separation of imprecise terms. From a functional perspective, the value of these terms lies in their potential heuristic properties; they help to subdivide the field and orient our investigative efforts. But, in borderline cases, asking whether the memory code is really prospective or really retrospective is akin to asking whether a motorcycle is really a car or really a bike.

MODELS OF MEMORY

In many cases, the question of whether or not a term is taken to refer to some sort of mental residue may not be especially important. What is important is that the terms may be put to good use in differentiating, say, an expectancy procedure from a nonexpectancy procedure. Nevertheless, it is important to ask

where the mentalistic strategy is likely to lead. In the later chapters of the book, we encounter some comprehensive cognitive models of animal memory that may help to supply the answer. Although these models would surely not be accepted by all animal memory theorists, they might be accepted as the kinds of model the field is working toward. More importantly, they may exemplify the kind of explanation that evolves naturally when the ill-defined terms of memory are interpreted initially as referring to entities of unknown ontological status.

In chapter 7, Kendrick and Rilling propose AIM, a theory of Active and Inactive Memory. A brief quotation will serve to capture the flavor of this account:

Recent events generate new memories that are added to the "conveyor belt" memory buffer so that temporal information is retained by the ordinal position of the memory in the buffer. Memories are encoded one at a time and stored in temporal order. Retrieval of a memory requires a backward scanning, self-terminating, search process. Memories selected by such a search process are "projected" into an active memory state. (p. 131)

Currently active stimulus representations control behavior. When environmental conditions change, new response decisions are required and the active stimulus representations reflect the changed environmental conditions. It is assumed that changes in stimulus input generate stimulus representations of the new stimuli, which are active. Newly generated representations are favored for activation. These new representations, once active, effect automatic scanning of the inactive memory buffer. The scanning process is responsible for selecting previously encoded information for activation. Thus, current stimulus representations and past representations may be simultaneously active. (p. 132)

The model is shown to apply to a variety of memory phenomena including delayed matching-to-sample, directed forgetting, and serial-probe recognition. The authors acknowledge that their model is not a finished product, but point out that it provides testable predictions for future research.

In the next chapter, Grant proposes a theory of delayed alternation in the rat. In this procedure, a rat is forced to travel down one arm of a T maze on the first trial and then allowed to enter either arm on the second trial. Only

the arm opposite to that entered on the first trial leads to reinforcement. The model designed to account for rats' ability to perform this task bears some resemblance to AIM:

The first process, involving memory retrieval, operates on inactive memories only, whereas the other four processes—rehearsal, tagging, updating, and discrimination—influence only active memories. . . . Only currently active memories can exert control over behavior. (p. 161)

At the time of target attribute activation, one or more additional attributes may be appended temporarily to the active representation; a process referred to as tagging. In contrast to target attributes, these additional attributes, called memory tags, are not represented permanently and therefore cannot be retrieved. Memory tags code information which is to some extent unique to a particular occurrence of a to-be-remembered episode. One type of tag postulated commonly is a time tag which is held to represent the temporal properties associated with the occurrence of a to-be-remembered episode. (p. 163)

The models described in these chapters will probably not appeal to psychologists interested in the identification of empirical principles of memory. The terms employed (e.g., *tags*) do not easily lend themselves to an interpretation in terms of measurable or manipulable variables. Furthermore, valued or not, these models might be viewed as a consequence of how one interprets the vocabulary of remembering to begin with. When memory is first viewed as an entity, speculation about its properties and activities is only natural. Although the eventual results (as illustrated by these models) may be in line with the aspirations of cognitive psychology, to the behavior analyst they represent what Skinner (1950) warned about well before the advent of the cognitive revolution: Attention to manipulable variables has apparently given way to in-depth speculation concerning the covert activities of the animal mind.

THE ROLE OF LANGUAGE

If the mentalistic interpretation of terms such as *memory*, *expectancy*, and *retrospection* naturally leads to the sort of models reviewed

above, then perhaps a science of behavior would do well to avoid them altogether. But to do so would be to suggest that nothing of value has come from the study of these subjects, and that, I think, is an untenable position. According to the arguments presented earlier, the problem is that we have sometimes learned to use the vocabulary of remembering to our advantage while at the same time overinterpreting it. We can devise expectancy experiments and produce very interesting results, but we go too far and abolish the utility of our natural language when we attempt to study the expectancy "codes and processes" that must be in there somewhere. Perhaps it is not too much to hope, however, that we can somehow make use of these terms without introducing the dangers that Skinner (1950) cautioned us about.

In practice, how might the natural language of memory be brought to bear on the functional analysis of remembering? The answer to that question requires some consideration of the potential pitfalls. First, it is important to accept the possibility that certain terms that are applicable to human memory simply do not assist us in our efforts to classify and study animal behavior. The term *rehearsal* is a good example. Perhaps the day will come when a clever psychologist arranges an experiment in such a way that it compels us to suggest that animals rehearse. Until that time, the claim that animals rehearse requires a considerable leap of faith. Second, it is advisable to avoid the introduction of lay terms that might otherwise have some heuristic value without any consideration for their use in our natural language. In this regard, Denny's appeal to the term *imagery* seems especially objectionable. Our everyday use of that term has nothing whatsoever to do with a Pavlovian conditioning arrangement. On the other hand, experimental procedures for animals that reflect the use of that term with humans are certainly within the scope of imagination (cf. Neiwirth & Rilling, 1987). Third, it is especially important to recognize that this terminology is far too inexact to serve as the foundation for a scientific vocabulary of behavior, regardless of one's theoretical orientation. Wittgenstein (1953) convincingly demonstrated that words born of our natural language have vague and flexible boundaries. In everyday use, great precision is not a prerequisite to functional utility,

but a science of behavior must be precise. Cognitive and behavioral psychologists alike would do well to develop a different, more exact, vocabulary once our everyday terms have served their purpose (cf. Schnaitter, 1983).

With these qualifications in mind, the experimental utility of our natural language can be considered more carefully. The approach suggested here is that we first consider the conditions under which those terms are used in our everyday language. In other words, we should identify the discriminative conditions that occasion the use of these terms in everyday life. For each of the terms considered earlier, I have tried to suggest what some of those conditions might be. Once some measure of clarity on this point is achieved, then, if possible, we may proceed to arrange experimental procedures using animals that reflect those conditions. We may find that this approach yields interesting results that challenge our preconceived notions of animal behavior, or even that they fit right in with established functional principles. In either case, the results may help us in our efforts to understand animal behavior and, perhaps just as important, to clarify for the larger community of psychologists the connection between somewhat esoteric empirical principles of behavior and the familiar language of memory.

To some extent, the process described above already occurs. That is, from a methodological standpoint researchers have devised prospective versus retrospective experimental procedures (e.g., Honig & Wasserman, 1981). As currently practiced, however, any benefits accruing from the use of such terms may be partially canceled by the unfortunate side effects of assuming that these terms refer essentially to something that occurs inside the head of the organism. By ignoring the way in which these terms are actually used, floundering models can be rescued magically by introducing an "expectancy" or an "image" or whatever it takes. There are seemingly no constraints. The more evolved product of this tactic is an elaborate mental theory that seems very far removed from the world of behavior. If one wishes to take advantage of the common terminology of remembering, and I believe that to be a good idea, then more careful attention to the ordinary use of those terms seems to be an important precondition.

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