ON THE ROLE OF "MEMORY" IN THE ANALYSIS OF BEHAVIOR

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Shimp's (1976) recent paper on memory and the structure of behavior is discussed, as is the concept of memory itself. Radical behaviorism is distinguished from associationism, and behavioral accounts of experiments on short-term and long-term memory are provided. Unobservable theoretical quantities, such as rate of response, are distinguished from theoretical constructs such as memory. The logical inconsistency involved in the use of short-term memory as a "theoretical primitive" in the definition of units of behavior is explicated, and an alternative strategy for identifying units of behavior is presented. It is argued that the experimental analysis of behavior has ignored processes usually considered as memorial because many of the phenomena are irreversible. Methods for the study of transitions in single subjects should be developed more fully. Possible pitfalls associated with subscription to the term memory are listed, and it is concluded that the concept is neither required nor helpful in the analysis of behavior.

Key words: memory, short-term memory, operants, radical behaviorism, transition states

Shimp (1976) argued that to understand behavior it may be necessary to include a concept of memory. The aim of this paper is to argue that not only is a concept of memory unnecessary, but that universal subscription to such a concept may actually retard the analysis of behavior. I shall try to address the issues raised by Shimp from the perspective of a radical behaviorist and show how they can be dealt with without resorting to a concept such as memory. Shimp's paper is an important one that could serve to initiate a shift in perspective by those engaged in the experimental analysis of behavior. Before we embark on a new course, however, it may be worthwhile to consider simply making minor changes in present courses instead.

I shall try both to speak to several of his arguments and make some summary statements about changes in both theory and practice in the experimental analysis of behavior that will allow its proponents to deal more fully with the important issues raised by Shimp.

Structuralism, Functionalism, and Radical Behaviorism

Shimp begins his arguments by correctly pointing out that two underlying philosophies, structuralism and functionalism, have dominated psychology for most of this century. He then implicitly equates a functionalist position with what he calls "associationism", and in so doing sets up a straw man. Not surprisingly, the straw man is reduced to mere chaff in short order. If radical behaviorism is associationistic (an arguable point), it certainly bears little resemblance to the extremely rigid associationism, described by Anderson and Bower (1973), that Shimp assumes. Associationism as described by Anderson and Bower leaves no place for dynamic behavioral change or functional definition as does radical behaviorism. While describing a rigid, simplistic associationism, Shimp makes statements such as "associationism would focus on the increase over trials in the number of remembered words, the simple responses assumed at the outset to constitute the functional unit" (p. 116) and "associationism focuses on individual words as units of analysis" (p. 117). These statements imply that units of behavior may be identified in advance merely on the basis of their putative structure or topography. While such state-

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ments may be true of associationism, they certainly are *not* at present true of radical behaviorism. The radical behavioristic position continues to be that behavioral units are functionally defined. Units are not assumed in advance of observation (Catania, 1973; Skinner, 1935, 1938, 1953, 1957, 1966, 1974). The reliance on functional units has always been one of the distinguishing characteristics of radical behaviorism, and serves to differentiate it from associationistic or stimulus-response approaches to the study of behavior. Students of the experimental analysis of behavior almost always study units that are defined by their function (*i.e.*, what they do to the environment), and the old dictum "the organism is always right" (*cf.* Findley, 1962) emphasizes that the subject's behavior indicates whether a unit was properly specified. The crux of the issue is not whether units can have structure; they do. The important question is, to what is the structure due? A structural account points to structural aspects of the behavior such as memory, whereas a functional account will emphasize the role of manipulable variables in the *formation* of units.

Behavioral Accounts of Experiments on Memory

Shimp reviewed experiments on verbal memory to establish a base from which to argue that units of behavior may have significant "internal structure". Most radical behaviorists have no difficulty with the notion of significant structure in a unit, so Shimp's arguments that units may have such construction are really unnecessary. Radical behaviorism asserts that units are functional and that they may have considerable structure (Catania, 1973; Skinner, 1935, 1938, 1957), and this assertion in no way depends on an assumption of memory.

In his review of experiments by Tulving (1962, 1964) and Murdock (1961), Shimp chooses not to use explanations of their findings that do not require a concept of memory. In Murdock's experiment, in which a traditional short-term memory procedure was used, he reported that it made no difference whether three-consonant trigrams or three-word triads were used as stimuli. The probability of correct naming as a function of delay was about the same for both cases. From

this, Shimp justifiably argues that words were probably units in the second case and that letters were probably units in the first case. A radical behaviorist would most likely agree and then point to historical variables that may have been responsible for the apparent equivalence of words and letters in the circumstances of the experiment. Shimp, by contrast, assumes a structuralist position and points to no variables. Instead, he merely focuses on the structure of the stimuli when he says:

"Clearly, the behavior of emitting a single letter is not always functionally the same: in one case, subjects emitted three letters in a consonant trigram and in a second case, subjects emitted nine letters grouped in three patterns of three, three-letter words" (p. 115).

Note the shift in emphasis from behavior to stimuli. In each case, the subject emitted only three verbal responses per item. The subject either named three letters or said three words.

Near the end of his discussion of Murdock's work, Shimp rightly points out that "... behavior that is a functional unit in one context may not be a functional unit in another context . . ." (p. 115). Similarly, Skinner in *Verbal Behavior* (Skinner, 1957) argues the same thing when he says: "Although parts of these larger [units] have the same form as parts of other operants or even of whole units, there may be no functional interaction" (p. 21). The radical behaviorist then need not look to experiments on memory to find justification for the notion that units of behavior cannot be identified simply on the basis of their structure. Shimp goes on to argue, however, that because units may differ in size:

"... we shall consider the possibility that the functional significance of a key peck depends on the behavioral pattern of which it is a component and that results stated in terms of key pecks are *unlikely* to have the generality of laws stated in terms of *true* functional units that are organized patterns of behavior having a significant degree of internal structure" (p. 115, italics not in original).

Thus, Shimp implies that by using small behavioral units, we may be limiting the generality of our behavioral laws. Of course, this

is an empirical question that can be answered only by experimentation involving larger units of behavior. The data that are available, however, suggest that complex operants are subject to the same laws as are rapidly repeatable operants such as key pecks or lever presses (*e.g.*, Boren and Gollub, 1972; Ferster, 1960; Findley, 1962; Kelleher, 1966). Thus, the temporal economy afforded by the study of rapidly repeatable operants does not appear to have jeopardized the development of general behavioral laws. Also implied is that laws derived from what might be called "sub-units" of behavior could be invalid. Shimp argues that a single key peck may be part of a larger unit consisting of several pecks, and that we may be missing orderly relations at this higher level of analysis. Such might certainly be the case, but the fact that order at a higher level was not examined in no way invalidates the order that exists at the more microscopic level. It is quite possible that order exists at several levels.

Shimp also describes two experiments by Tulving (Tulving 1962, 1964) that are supposed to demonstrate that the size of a unit may change during experiments. In Tulving's experiments, over the course of several presentations of a list of items, the number of items correctly named increased and names tended to occur in the same order. This "chunking" is thought to reveal an increase in the size of a functional unit. Tulving's results are not particularly surprising to radical behaviorists, who have long assumed that previously emitted behavior may serve as part of a stimulus complex controlling subsequent behavior. That such can be the case has been demonstrated in experiments in which emitted behavior serves a discriminative function (*e.g.*, Hobson, 1975; Pliskoff and Goldiamond, 1966; Rilling and McDiarmid, 1965). A radical behaviorist, then, could argue that words, or groups of words, emitted correctly on a trial could form part of the functional stimulus-complex controlling the emission of subsequent words, and over repeated emissions the larger chunk becomes a unit in much the same way as an arpeggio progresses from a sequence of discrete responses to an integrated unit. Shimp apparently rejects such an account, and instead suggests that "... a subject established a small number of functional units which increased in size as new words

were integrated with them" (p. 117). The objection to such a statement is not over whether functional units change in size, but concerns the origin of the change. In Shimp's account, it is the subject that is the originator, whereas in a radical behaviorist's account the originators are environmental events. In a structural account, the subject is the locus from which behavior emerges, whereas in a functional account, the subject is the focus where independently manipulable variables meet and have their effect.

Rationales for the Use of Memory

Shimp employs two main lines of argument to convince us that use of the term memory is legitimate. The first argument centers around the status of "unobservable theoretical quantities" (UTQ). As an example, Shimp describes a measure employed by Tulving to describe the patterning of responses in a free-recall paradigm. Specifically, the measure was defined as

$$\frac{\sum_{i,j} n_{ij} \log n_{ij}}{\sum_i n_i \log n_i},$$

where n_{ij} is the frequency with which the j^{th} word followed the i^{th} word, and n_i is the frequency of appearance of the i^{th} word. Shimp noted that the measure is a derived one and suggested that it therefore was a "kind of unobservable theoretical quantity" (p. 116). The measure refers to nothing directly, but is perceived by scientists in general as perfectly acceptable. Shimp then points out that the experimental analysis of behavior also uses such measures. Having laid this groundwork, he later says: "Indeed, unobservable theoretical quantities of a certain type have always been a part of the experimental analysis of behavior . . ." (p. 120), and then goes on to use rate of responding as an example of an UTQ. Although neither rate of responding nor Tulving's measure of patterning are directly observable, both are derived *directly* by mathematical operations from observables. No inferential leaps are made, and most observers would not refer to either as theoretical *constructs* (MacCorquodale and Meehl, 1948). Memory, by contrast, has often been accorded status as a construct, *i.e.*, something with actual physical status but, *due to limitations of technology*, not observable. Rate of response

and Tulving's measure, on the other hand, are not observable in principle.²

To his credit, Shimp argues that:

"We would have no historical precedent for excluding memory simply because it referred to a theoretical quantity not directly observed but mathematically derived from observables" (p. 120).

A problem is that no one has defined memory in such a fashion, and the likelihood of getting even a significant minority of psychologists to agree on a single mathematical definition of memory appears virtually nil. The term refers to too many different sets of phenomena.

A functional approach to acquisition and loss of stimulus control is at least as likely to be successful in discovering unobservable theoretical quantities as is a structurally based approach. Just as the universal gravitational constant G was derived from functional relations obtained from studying falling bodies, celestial motion and the like, so it is possible that the determination of functional relations related to the loss of stimulus control may also result in the derivation of "universal" behavioral constants. Note that in a functional approach, universal constants and laws, laws that will undoubtedly include quantities that will be thought of as theoretical, will be discovered in the data resulting from experiments on interesting behavioral phenomena. By postulating theoretical entities in advance (*viz* "memory"), we make it less likely that new, nonintuitive, formulations will appear.

As a second line of argument, Shimp points out that in the experimental analysis of behavior the word "choice" is often used when describing situations in which two or more responses are measured. Choice, usually expressed as a percentage measure, qualifies as an UTQ by Shimp's definition, and so he argues that if we can use "choice" why not also use "memory"? There are at least two an-

swers to the question. First, the term "choice" always refers to an *effect*, not a cause. Preferences are treated as a result of experimental manipulations, not as circular explanations. Could we do the same with memory? Quite possibly, but it does not seem likely. Rather, memory somehow always manages quickly to assume explanatory status. In fact, Shimp himself uses memory (STM in particular) as a "theoretical primitive" in his discussion of functional units (see below). Second, we may answer the question with a question. What has the use of the word "choice" done to advance the analyses of concurrent responses? As best I can see, nothing. It's major utility appears to lie in its use as a label for a research area. And use of the word must be carefully scrutinized by scientists everytime it appears, lest it suddenly become the "thing" that is responsible for observed preferences.

Memory, Measurement, and Units of Behavior

Shimp's discussion of a concept of memory and of early experiments on recall were conducted to establish a base for an attack on one of the long-standing problems in psychology in general, and in the experimental analysis of behavior in particular. The problem is that of identifying behavioral units. Now and again, the problem arises for discussion, and is usually not completely resolved (Catania, 1973; Schick, 1971; Skinner, 1935).

Let us proceed through Shimp's arguments. He does concede that experimental analysts of behavior have been interested in structure, but cites only the work of researchers who have delved into a very restricted area of structure, the temporal structure of response rate (Jenkins, 1970; Weiss, 1970; Williams, 1968). Strangely missing are any references to the considerable literature on response differentiation (*e.g.*, Dunham, Mariner, and Adams, 1964; Hawkes and Shimp, 1975; Lane, 1960; Margulies, 1961; Notterman and Mintz, 1962, 1965; Platt, Kuch, and Bitgood, 1973; Zeiler, 1970). Since response differentiation is the process most empirical theoreticians rely on to account for variations in structure or topography of behavior, it is strange that the process is not even mentioned. Apparently Shimp is concerned only with the temporal patterning of relatively brief responses, and not with factors such as posture, force, dura-

²Rate of responding and Tulving's measure may be considered to lie at different points on a continuum of observability. If one were to watch two organisms responding at different rates, it generally would not be difficult to identify the subject with the higher rate. It might be more difficult, however, to tell which of two subjects had a higher patterning value (Tulving's measure). For present purposes, this important difference will be overlooked.

tion, and other differentiable aspects of behavior. In fact, on several occasions he asserts that individual responses (*e.g.*, key pecks) normally studied by experimental analysts of behavior have virtually no structure at all. Such a claim is counter to considerable data that show that individual key pecks or lever presses are in fact complex organized responses that can be further analyzed into even smaller units (*e.g.*, Jenkins and Moore, 1973; Schwartz, 1977*a, b*; Smith, 1974).

Shimp next argues that we must try to isolate basic or "true" functional units of behavior, because that is what other sciences have done. In his list of other sciences, he includes biology, of which psychology is properly a part. Is there a fundamental unit of biology? Is it a fixed-action pattern? A muscle? A cell? An enzyme? A gene? The answer obviously is that it depends, that there is no "true" fundamental unit. Instead of arguing over which unit is "the" unit, biologists have proceeded on several levels of analysis and have discovered useful relationships at all of them. Is it not possible that the same can be true for psychology? Shimp himself has shown that relatively complex structures of behavior can act as units under certain circumstances (*e.g.*, Hawkes and Shimp, 1975). It is incorrect to conclude from such data however, that behavioral units in general have such structure. That is, demonstration that a complex sequence of responses can act as a unit does not imply that all units have such structure. The better question asks how such structures were *made to be* functional units.

In describing how one might identify fundamental units Shimp employs STM as a "theoretical primitive". A "theoretical primitive" is a basic process by which other results are explained. Shimp argues that: "... the structural properties of the fundamental unit in a given context will depend on how that context assigns roles to all the variables determining a subject's short-term memory for its own recent behavior" (p. 123). On the surface, this seems like a legitimate (and functional) statement. The problem, however, lies in determination of the short-term memory span in a given context. How is that to be accomplished? The logic quickly becomes circular and leads to the type of problem outlined by Craik and Lockhart (1972), who reviewed some of the literature on STM and

LTM. They noted that: "Besides constituting the basic phenomenon to be explained, they [STM and LTM] have also been used to generate the theoretical constructs in terms of which the explanation is formulated. The apparent circularity has been avoided by specification of additional properties of the stores (such as their capacity and coding characteristics) . . ." (p. 671). Thus, the theoretical viability of independent STM and LTM depends on demonstration of differences other than temporal ones. Proposed distinctions between STM and LTM include the capacities of the two types, the type of material to be remembered, as well as temporal characteristics. Capacity estimates for STM, however, range widely (*cf.* Craik and Masani, 1969; Crannell and Parrish, 1957; this discrepancy is usually attributed to "chunking", but it is apparently difficult to define "chunks" independently from their memorial consequences [Simon, 1974]) and it also appears that STM and LTM cannot be differentiated on the basis of the type of material to be remembered, nor on the basis of reliable temporal characteristics (Kintsch, 1970; Kroll, Parks, Parkinson, Bieber, and Johnson, 1970; Levy, 1971; Shulman, 1970, 1972; Waugh and Norman, 1965). The status of LTM and STM as independent entities, then, is questionable, making Shimp's reliance on STM as an important variable somewhat suspect. Thus, STM span is defined by the number of units remembered, and the units are in turn defined by the STM span. Neither is independent of the other, so neither can be considered more basic than the other. Successful determination of STM span requires prior, independent identification of units. This is the dilemma that has led many cognitive psychologists to abandon the concept of STM altogether and to begin to focus on a more functional analysis of forgetting, and the same dilemma is established by taking Shimp's position. It appears, therefore, that an appeal to a concept of memory is not necessary or helpful in the analysis of local control of response rate.

Nowhere in Shimp's analysis of the unit problem is the really fundamental question addressed. That question is the one of measurement. How are we to measure these units? We must be able to identify independently the reproducible units of analysis. If units

have as much structure as Shimp suggests (and they may well have), how are we to identify them? The problem is apparently yet to be solved, and Shimp is far from the first to encounter it (*cf.* Catania, 1973; Schick, 1971; Skinner, 1935). Sometimes the fact that a question goes unanswered for a long period of time is an indication that it is a bad question. Clear progress has been made in the analysis of behavior in spite of the fact that the problem of how to identify units reliably has yet to be solved. How has this been possible? In practice, experimental analysts have never directly measured the basic unit of analysis, the operant. Instead, the most common practice has been to measure "end points" of sequences of behavior. For example, lever pressing is generally measured by a switch closure at the end of a sequence of actions. The sequence of actions is not directly measured. Rather, a defining property (or set of defining properties) is established to tell us whether or not an end point has been reached. By merely focusing on end points, or behavioral products, considerable progress has been made. It is only in the theoretical realm that a problem arises.

That truly unobservable theoretical quantity, an operant, presumably consists either of all the topographical variants that can result in the behavioral product or of all those topographical variants that are affected by arranging special consequences for the product (Catania, 1973). Complete topographical measurement of behaviors preceding the end point never occurs, and the assertion here is that it should not occur in such circumstances. If not impossible it certainly is almost totally unfeasible to measure *all* aspects of topography. It seems unreasonable to have to measure, for example, the position of each of a rat's toes during emission of a lever press. Yet, that is what must be done if we are to identify *all* the topographical variants that change as a result of experimental operations.

We must rephrase the question so that instead of asking what the units are, we ask how we can define units and still observe order. Prediction and control of behavior are enhanced by research on the latter question, whereas dealing with the former leads us into a philosophical and metrical morass. For too long, those engaged in the experimental analysis of behavior have claimed to be func-

tionalists, but when faced with the problem of identifying behavioral units have become "closet structuralists". Units of behavior cannot be identified by merely observing their structure. One cannot necessarily "see" a unit in a cumulative record. Units can be defined as such only by demonstration of functional relations between the supposed units and certain environmental events or arrangements, and the functional relations must bear a resemblance to those described for other proposed units. By employing a tactic whereby characteristics of units are specified in advance, the discovery of orderly relationships will lead to the formulation of general rules or laws of *specification* of units.

Behavioral Analysis of Transient Processes

Those in the experimental analysis of behavior have spent little effort on the subject of memory. Behaviorists as a group have not dealt extensively with the variables responsible for diminished stimulus control (*i.e.*, forgetting). That is a cause for concern, but not sufficient reason to adopt a concept of memory with all its potential extra meaning. The dangers associated with postulating the existence of a memory or several memories are clear. It is much too easy to assign properties to memory as a structure to "explain" behavioral phenomena. The ease with which such assignments may be made can easily retard or prevent investigations that could lead to the discovery and analysis of manipulable variables that truly "cause" the observed phenomena. A concept of memory may serve as a sort of heuristic crutch for some, but inquisitive functional analysts surely need no such support in the generation of experimental ideas.

The behavioral processes involved in determining loss of stimulus control can be analyzed without resorting to notions such as "memory", "storage", "retrieval", "encoding", *etc.* Just because there are changes in the probability of certain stimulus-control relationships as a function of time, there is no need to say that because the probability is zero that "storage" did not take place, nor to say that there was a "retrieval" failure because the probability of correctly responding was zero in one circumstance and nonzero in another. What is needed is an analysis of variables responsible for these kinds of outcomes.

Merely labelling them "encoding error" or "retrieval error" in no way leads us any closer to an explanation.

The storage models of memory also pose an additional problem. They encourage investigators to search for "representations" of events. People eagerly search out memorial representations to find out how "information" is "encoded". Is it not more logical to consider behavior as biological rather than computer-like, to consider a behaving organism as a biological unit, rather than as a machine in which representations may be stored? When an egg is exposed to heat, we say that the egg is cooked, not that it has "stored" the "information" that it was heated.³ Similarly, experiences are not "stored" in or by an organism; instead they change the organism, surely behaviorally and almost assuredly physiologically. Careful analyses of how experience changes an organism's behavior will provide better guides for physiological enquiry than will speculations and theorizing about structural representations.

The question remains as to why those committed to an experimental analysis of behavior have spent so little time studying processes normally thought of as memorial. At least part of the answer lies in the nature of the phenomena. They are transient. Experimental analysts of behavior have predominantly adopted the steady-state methodology espoused by Sidman (1960). Consequently, we have spent relatively little effort on developing methods for the study of transient behavioral phenomena, and instead have continued to use designs that allow for tests of reversibility. Also, relatively little experimental effort has gone to the analysis of the familiar processes of extinction and acquisition because they, too, are transient phenomena. Skinner's recent lament (Skinner, 1976) reflects his disappointment with this fact. He asked "What has happened to the curves?" (p. 218), and most surely he was referring not just to cumulative records, but to cumulative records of behavior in transition.

Shimp correctly points out that experimental analysts of behavior have contributed little to the development of methods to examine transient processes. (See, however, Anger and Anger, 1976; Boren and Devine,

1968.) Guidelines for the analysis of behavior in transition, however, are provided by Sidman (1960). He described rational methods for identifying when transitions begin and suggests possibilities for identifying their ends as well, so the problem is not one that is a result of a complete lack of technology. Two of the most widely cited pieces of work in operant conditioning are Skinner's *Behavior of Organisms* (1938) and Ferster and Skinner's *Schedules of Reinforcement* (1957), and a majority of data in both are from irreversible behavioral transitions. Also, much of the recent research on autoshaping deals with irreversible transitions (see Hearst and Jenkins, 1974). Thus, although the study of irreversible behavioral transitions in individual subjects is more hazardous than the study of steady-state phenomena, because the test of reversibility is lost, such investigations are far from impossible.

SUMMARY AND CONCLUSIONS

I have briefly reviewed recent arguments by Shimp that experimental analysts of behavior need to become more interested in the structure of behavior in general and in a structural concept of "memory" in particular. It was argued that postulation of structures still leaves unanswered the question of what the structure is a function, and such postulation also can retard further functional analysis because it is easier to assign new properties to a hypothetical structure to "explain" experimental findings than it is to engage in a functional analysis. In addition, postulation of unobservable constructs tends to move the locus of causality to the subject and away from the manipulable variables in the environment.

Specifically, it was argued that a concept of memory is not necessary for progress in the analysis of behavior. The problem of "behavioral units" can be dealt with without resorting to concepts such as STM. It was argued however, that phenomena often subsumed under the rubrics STM or LTM have been studied far too little, largely due to the fact that those engaged in the experimental analysis of behavior have generally avoided dealing with transient behavioral processes.

It may appear that my opposition to the use of the term "memory" has a large semantic

³Thanks to M. J. Marr for this analogy.

component. That is true. I suggest that more be learned about the processes involved in acquisition and loss of stimulus control. Why not call research on these topics research on "memory"? While the argument may largely be one of semantics, I believe that it is important. The pitfalls associated with the use of terms that lead easily to the postulation of inner, hypothetical structures have been outlined here and elsewhere (Skinner, 1953, 1969, 1974). If we are to increase our control and understanding of behavior, including its structural aspects, we must discover and analyze the variables of which it is a function. Shimp says it well when he states that:

"The primary criterion by means of which one measures the stage of development of the experimental analysis of behavior is the precision and generality of available methods with which one can control the behavior of an individual organism" (p. 119).

On this score, the experimental analysis of behavior certainly does much better than more mentalistic approaches. The technology that has developed out of the functional point of view far exceeds that emanating from cognitive psychology. Techniques derived from the experimental analysis of behavior have been applied effectively by teachers, therapists, businessmen, physicians, lawyers, pharmacologists, and a host of others. All this has been accomplished without resort to "memory". I see no reason to begin now.

REFERENCES

- Anderson, J. R. and Bower, G. H. *Human associative memory*. Washington: V. H. Winston & Sons, 1973.
- Anger, D. and Anger, K. Behavior changes during repeated eight-day extinctions. *Journal of the Experimental Analysis of Behavior*, 1976, **26**, 181-190.
- Boren, J. J. and Devine, D. D. The repeated acquisition of behavioral chains. *Journal of the Experimental Analysis of Behavior*, 1968, **11**, 651-660.
- Boren, M. C. P. and Gollub, L. R. Accuracy of performance on a matching-to-sample procedure under interval schedules. *Journal of the Experimental Analysis of Behavior*, 1972, **18**, 65-77.
- Catania, A. C. The concept of the operant in the analysis of behavior. *Behaviorism*, 1973, **1**, 103-115.
- Craik, F. I. M. and Lockhart, R. S. Levels of processing: a framework for memory research. *Journal of Verbal Learning and Verbal Behavior*, 1972, **11**, 671-684.
- Craik, F. I. M. and Masani, P. A. Age and intelligence differences in coding and retrieval of word lists. *British Journal of Psychology*, 1969, **60**, 315-319.
- Crannell, C. W. and Parrish, J. M. A comparison of immediate memory span for digits, letters, and words. *Journal of Psychology*, 1957, **44**, 319-327.
- Dunham, P. J., Mariner, A., and Adams, H. Enhancement of off-key pecking by on-key punishment. *Journal of the Experimental Analysis of Behavior*, 1972, **12**, 789-797.
- Ferster, C. B. Intermittent reinforcement of matching to sample in the pigeon. *Journal of the Experimental Analysis of Behavior*, 1960, **3**, 259-272.
- Ferster, C. B. and Skinner, B. F. *Schedules of reinforcement*. New York: Appleton-Century-Crofts, 1957.
- Findley, J. D. An experimental outline for exploring multi-operant behavior repertoires. *Journal of the Experimental Analysis of Behavior*, 1962, **5**, 113-166.
- Hawkes, L. and Shimp, C. P. Reinforcement of behavioral patterns: shaping a scallop. *Journal of the Experimental Analysis of Behavior*, 1975, **23**, 3-16.
- Hearst, E. and Jenkins, H. M. *Sign-tracking: the stimulus-reinforcer relation and directed action*. Austin, Texas: The Psychonomic Society, 1974.
- Hobson, S. L. Discriminability of fixed-ratio schedules for pigeons: effects of absolute ratio size. *Journal of the Experimental Analysis of Behavior*, 1975, **23**, 25-35.
- Jenkins, H. M. Sequential organization in schedules of reinforcement. In W. N. Schoenfeld (Ed), *The theory of reinforcement schedules*. New York: Appleton-Century-Crofts, 1970. Pp. 63-109.
- Jenkins, H. M. and Moore, B. R. The form of auto-shaped response with food or water reinforcers. *Journal of the Experimental Analysis of Behavior*, 1973, **20**, 163-181.
- Kelleher, R. T. Chaining and conditioned reinforcement. In W. K. Honig (Ed), *Operant behavior: areas of research and application*. New York: Appleton-Century-Crofts, 1966. Pp. 160-212.
- Kintsch, W. *Learning, memory, and conceptual processes*. New York: Wiley, 1970.
- Kroll, N. E. A., Parks, T., Parkinson, S. R., Bieber, S. L., and Johnson, A. L. Short-term memory while shadowing. Recall of visually and aurally presented letters. *Journal of Experimental Psychology*, 1970, **85**, 220-224.
- Lane, H. Temporal and intensive properties of human vocal responding under a schedule of reinforcement. *Journal of the Experimental Analysis of Behavior*, 1960, **3**, 183-192.
- Levy, B. A. Role of articulation in auditory and visual short-term memory. *Journal of Verbal Learning and Verbal Behavior*, 1971, **10**, 123-132.
- MacCorquodale, K. and Meehl, P. E. On a distinction between hypothetical constructs and intervening variables. *Psychological Review*, 1948, **55**, 95-107.
- Margulies, S. Response duration in operant level, regular reinforcement, and extinction. *Journal of the Experimental Analysis of Behavior*, 1961, **4**, 317-321.
- Murdock, B. B. The retention of individual items. *Journal of Experimental Psychology*, 1961, **62**, 618-625.
- Notterman, J. M. and Mintz, D. E. Exteroceptive cueing of response force. *Science*, 1962, **135**, 1070-1071.

- Notterman, J. M. and Mintz, D. E. *Dynamics of response*. New York: Wiley, 1965.
- Platt, J. R., Kuch, D. O., and Bitgood, S. C. Rats' lever-press durations as psychophysical judgements of time. *Journal of the Experimental Analysis of Behavior*, 1973, 19, 239-250.
- Pliskoff, S. S. and Goldiamond, I. Some discriminative properties of fixed ratio performance in the pigeon. *Journal of the Experimental Analysis of Behavior*, 1966, 9, 1-9.
- Rilling, M. and McDiarmid, C. Signal detection in fixed-ratio schedules. *Science*, 1965, 148, 526-527.
- Schick, K. Operants. *Journal of the Experimental Analysis of Behavior*, 1971, 15, 413-423.
- Schwartz, B. Studies of operant and reflexive key pecks in the pigeon. *Journal of the Experimental Analysis of Behavior*, 1977, 27, 301-313. (a)
- Schwartz, B. Two types of pigeon key pecking: suppression of long- but not short-duration key pecks by duration-dependent shock. *Journal of the Experimental Analysis of Behavior*, 1977, 27, 393-398. (b)
- Shimp, C. P. Organization in memory and behavior. *Journal of the Experimental Analysis of Behavior*, 1976, 26, 113-130.
- Shulman, H. G. Encoding and retention of semantic and phonetic information in short-term memory. *Journal of Verbal Learning and Verbal Behavior*, 1970, 9, 499-508.
- Shulman, H. G. Semantic confusion errors in short-term memory. *Journal of Verbal Learning and Verbal Behavior*, 1972, 11, 221-227.
- Sidman, M. *Tactics of scientific research*. New York: Basic Books, 1960.
- Simon, H. How big is a chunk? *Science*, 1974, 183, 482-488.
- Skinner, B. F. The generic nature of the concepts of stimulus and response. *The Journal of General Psychology*, 1935, 12, 40-65.
- Skinner, B. F. *The behavior of organisms: an experimental analysis*. New York: Appleton-Century-Crofts, 1938.
- Skinner, B. F. *Science and human behavior*. New York: The Free Press, 1953.
- Skinner, B. F. *Verbal behavior*. New York: Appleton-Century-Crofts, 1957.
- Skinner, B. F. *Contingencies of reinforcement: a theoretical analysis*. New York: Appleton-Century-Crofts, 1969.
- Skinner, B. F. *About behaviorism*. New York: Alfred A. Knopf, 1974.
- Skinner, B. F. Farewell, My LOVELY! *Journal of the Experimental Analysis of Behavior*, 1976, 25, 218.
- Smith, R. F. Topography of the food-reinforced key peck and the source of 30-millisecond interresponse times. *Journal of the Experimental Analysis of Behavior*, 1974, 21, 541-551.
- Tulving, E. Subjective organization in free recall of "unrelated" words. *Psychological Review*, 1962, 69, 344-354.
- Tulving, E. Intrial and intertrial retention: notes towards a theory of recall verbal learning. *Psychological Review*, 1964, 71, 219-237.
- Waugh, N. C. and Norman, D. A. Primary memory. *Psychological Review*, 1965, 72, 89-104.
- Weiss, B. The fine structure of operant behavior during transition states. In W. N. Schoenfeld (Ed), *The theory of reinforcement schedules*. New York: Appleton-Century-Crofts, 1970. Pp. 277-311.
- Williams, D. R. The structure of response rate. *Journal of the Experimental Analysis of Behavior*, 1968, 11, 251-258.
- Zeiler, M. D. Time limits for completing fixed ratios. *Journal of the Experimental Analysis of Behavior*, 1970, 14, 275-286.

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