JOURNAL OF THE EXPERIMENTAL ANALYSIS OF BEHAVIOR 1976, 26, 113-130 NUMBER 1 (JULY)

ORGANIZATION IN MEMORY AND BEHAVIOR[1]

CHARLES P. SHIMP[2]

UNIVERSITY OF UTAH

Some common reinforcement contingencies make the delivery of a reinforcer depend on the occurrence of behavior lacking significant temporal structure: a reinforcer may be contingent on nearly instantaneous responses such as a pigeon's key peck, a rat's lever press, a human's button press or brief verbal utterance, and so on. Such a reinforcement contingency conforms much more closely to the functionalist tradition in experimental psychology than to the structuralist tradition. Until recently, the functionalist tradition, in the form of a kind of associationism, typified most research on human learning and memory. Recently, however, research on human memory has focused more on structural issues: now the basic unit of analysis often involves an organized temporal pattern of behavior. A focus on the interrelations between the function and structure of behavior identifies a set of independent and dependent variables different from those identified by certain common kinds of "molar" behavioral analyses. In so doing, such a focus redefines some of the significant issues in the experimental analysis of behavior.

TABLE OF CONTENTS

Introduction
Human memory experiments on the functional unit
  Short-term memory experiments
  Free-recall experiments
The status of a concept of memory in the experimental analysis of behavior
  The traditional view and its origins
  Behavior and unobservable theoretical quantities
  The vocabulary of the experimental analysis of behavior
Two different treatments of structure in the fundamental unit of analysis
Some unresolved theoretical issues corresponding to different treatments of structure in the fundamental unit of analysis
  The nature of independent and dependent variables
  Molecular *versus* molar analyses
  Free-operant behavior as baseline behavior
  Biological constraints on learning
  Mathematical theories
Conclusion
References

Two different traditions have long coexisted in experimental psychology, with the fortune of each waxing and waning over the decades. These traditions may be called the functionalist tradition (Angell, 1904; Thorndike, 1898; Watson, 1924) and the structuralist tradition (Titchener, 1898; Tolman, 1932; Köhler, 1947; Tulving, 1962; Mandler, 1967). Behaviorism is frequently perceived to be descended from, and more closely related to, functionalism than structuralism (for example, see Anderson and Bower, 1973). For several decades, the study of human learning and memory was un-

[1]This paper was begun in response to an invitation to review recent books on human memory and to describe their relevance for the experimental analysis of behavior. The scope of such a review gradually was seen to exceed every practical limitation of space, time, and the present writer's understanding. Therefore, this paper addresses the more restricted but still very general issue of the interrelations between the experimental analysis of behavior and recent work on certain aspects of organization in human memory. Reprints may be obtained from the author, Department of Psychology, University of Utah, Salt Lake City, Utah 84112.

[2]This paper could not have been written without the support given to the author by NIMH grants 16928, 19901, and 24537, and by a fellowship from the Center for Advanced Study in the Behavioral Sciences, where the author first began to explore the interrelations between recent work on organization in human memory and a science of behavior. I would like to thank my colleagues at the University of Utah, William Johnston, Raymond Kesner, William Prokasy, John Sullivan, and Charles Uhl for their stimulating and constructive comments on several of the issues dealt with here.

questionably dominated by the functionalist tradition in the form of a kind of associationism (McGeoch, 1942; Hull, Hovland, Ross, Hall, Perkins, and Fitch, 1940; Postman, 1961). Recent developments, alternately enthusiastically or disparagingly referred to as the "cognitive revolution", have raised to prominence in the analysis of human learning and memory some issues historically closer to the structuralist than to the functionalist tradition.

The present paper attempts to relate the recent functionalist-structuralist debate over issues in human learning and memory to various theories and methods in the experimental analysis of behavior. To this end, the paper is organized into four parts. First, a very small sample of the kinds of human memory data that force consideration of structural issues is reviewed. These human memory experiments deal primarily with the nature of the basic unit of analysis. Second, the general concept of memory is examined as it bears on some distinctions between behaviorism and cognitive psychology. In this second part, a rationale is developed for the constructive use of cognitive vocabulary and unobservable theoretical quantities in a way that is compatible with a science of behavior. Third, some examples are given to show how structural issues have arisen in the experimental analysis of behavior. These examples are contrasted with other behavioral analyses that do not give attention to these same structural issues. Finally, it is shown how this side-by-side coexistence of structural and nonstructural analyses corresponds to unresolved theoretical problems of the most fundamental importance. In this section, it is argued that an adequate science of behavior requires methods that acknowledge both structural and functional issues, and perhaps most important of all, their interactions. This paper argues for, if not a reconciliation or unification of structuralist and functionalist traditions, then at least for an appreciation of the methodological and theoretical implications of the interrelations between the structure and the function of behavior. This argument has much in common on an abstract level with views expressed by Tolman (1932), Bolles (1974, 1975), Catania (1973*b*), Estes (1973), Hearst (1975), and others concerning the development of mutually constructive relations between behavioral and cognitive analyses.

Human Memory Experiments on the Functional Unit

We face one brief preliminary before we can look at specific data. We need first to describe briefly the nature of the functional unit from an associationistic position. Anderson and Bower (1973) provided a most readable account of the role of associationism in the analysis of human learning and memory. In their review, they listed a few "metafeatures that seem to universally typify associationism". The third of these metafeatures of association theory was that "the simple ideas" that become associated "are to be identified with elementary, unstructured sensations . . ." . Along with the authors whose work is reviewed below, Anderson and Bower (1973) proposed and argued "for a radical shift from the associationist conceptions that have heretofore dominated theorizing on human memory". They maintained that "this shift is most apparent in the unit of analysis which we adopt. Unlike past associative theories, we will not focus on associations among single items such as letters, nonsense syllables, or words". They focused instead on basic units having a significant degree of internal structure. This shift in focus is the major concern of the present paper. Let us now consider some of the data that have compelled investigators to change their assumptions about the basic unit of analysis. First, we shall examine the nature of the functional unit in the light of human short-term memory experiments. Then, we shall briefly examine the functional unit in multi-trial free-recall experiments. The material described in this section can be, and often is, translated into an encoding vocabulary (Miller, 1956; Melton and Martin, 1972), but here it will be sufficient, and more convenient, to restrict ourselves to a vocabulary in terms of organization, structure, and patterning of behavior.

Short-term memory experiments. Two classic papers on short-term memory are those by Brown (1958) and Peterson and Peterson (1959). In the discrete-trials experiment by Peterson and Peterson, each trial consisted of three stages: a presentation stage, a delay stage, and a test stage. In the first, the experimenter spelled aloud a three-consonant trigram. In the second, a subject counted backwards from some number (varied over trials) by threes or fours. This delay in the second stage was varied over trials from 3 to 18 sec. In the third, a subject was required to speak aloud the consonant trigram from the first stage of that trial. Each subject was tested eight times after each of six delays. There were 48 consonant trigrams, with which each subject was tested once. The percentage of trials on which a subject gave the correct trigram (averaged over 24 subjects) fell from about 90% at the shortest delay to less than 10% at an 18-sec delay.

Consider now the issue of behavioral organization in short-term memory experiments such as the one just described. Murdock (1961) used the same procedure as Peterson and Peterson (1959) to obtain results that illustrate the general point to be made here. First consider his method. He employed three different types of stimuli in the first stage of a trial: single three-consonant trigrams, single monosyllabic words, or three monosyllabic words. With the first type of stimulus, he should have and did replicate the result obtained by Peterson and Peterson described above. With single words, he found that, for a given delay, the percentage of trials on which a subject gave the correct word was much higher than with three-consonant trigrams, even though both types of stimulus consisted of three letters and in that sense should be equally difficult to remember if the units of analysis were single letters. Lastly, with the third type of stimulus, a three-word triad, Murdock obtained results virtually identical to those obtained with the first type, a three-consonant trigram. This result defines a functional equivalence between the effects of a three-consonant trigram and a three-word triad. Clearly, the behavior of emitting a simple letter is not always functionally the same: in one case, subjects emitted three letters in a consonant trigram and in a second case, subjets emitted nine letters grouped in three patterns of three, three-letter words, and in each case, performance was the same. This suggests that the functional significance of a simple response such as a single letter depends on the way in which behavioral output is patterned, or organized. In particular, when three consonants are presented, each letter acts as a separate functional unit, but when nine letters grouped into three words are presented, each word, or integrated pattern of letters, acts as a separate functional unit. In the latter situation, a simple letter is not a functional unit. Thus, performance in the two cases is equal because each case contains the same number of functional units, *i.e.*, three. The invariant relationship, the general law, interrelating behavior and the retention interval in Murdock's and related experiments must be stated in terms of functional units, not letters, words, or any other arbitrary behavior (Miller, 1956; Norman, 1969). These data suggest that a given behavior that is a functional unit in one context may not be a functional unit in another context, and that a functional unit may not be a simple response and may indeed be quite structured and complex behavior.

It is helpful at this point to look ahead a little to see what this result will later signify for us. This result encourages us to ask if a simple response, such as a pigeon's key peck, is always functionally the same behavior. It even suggests that we ask if such a simple response can by itself ever constitute a functional unit. Accordingly, 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.

Free-recall experiments. Let us now look at data interrelating organization in behavioral output with the effects of repetition. In this brief survey, we can look in detail only at two classic experiments by Tulving (1962, 1964). Tulving (1962) developed a measure, derived from information theory, of the extent of behavioral patterning or organization in a behavior sequence. Tulving's measure estimated the extent to which successive pairs of responses were the same on successive trials of a free-recall experiment. To understand completely what this means, we must first understand the multi-trial free-recall method used by Tulving. He took 16 English words, each a disyllabic noun, with 5, 6, or 7 letters. He then took these words and made 16 different sequences so arranged that there was no tendency, greater than that expected by chance, for any given pair, triplet, *etc.*, of words to occur together over the 16 different lists. Sixteen female college undergraduates then served as subjects in the following experiment. On the first trial, one sequence of the 16 words was presented visually at a rate of one word per second. After this presentation of the 16 words, a subject was required to write down as many of the 16 words as she could remember in 90 sec, after which the second trial began. All 16 sequences were presented to each subject. The subject thus saw the same 16 words on each trial, but in a different order. She was instructed that the order in which she wrote down the words on a trial did not matter: her only task was to write down as many as possible of the 16 words. The data were analyzed as follows. The frequency with which each word followed every other word was determined for each subject over a block of trials. A measure of behavioral patterning was then calculated on the basis of these frequencies. The measure of behavioral patterning was

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

where n_{ij} is the frequency with which the j^{th} word ($j = 1, \ldots 16$) followed the i^{th} word ($i = 1, \ldots 16$). The formula is the ratio obtained by dividing the pair-wise patterning in the data by the maximum possible pair-wise patterning. If the ratio is 0, then no pair-wise patterning is present. If the ratio is 1, then all pairs or words are emitted in the same sequence on every trial. Note that this measure is a derived measure: it is a number calculated from data according to a formula derived from a theory. It will be important later to have noted here that the number does not represent immediately observable behavior: the number is a kind of unobservable theoretical quantity.

Tulving (1962) found that this measure of patterning, averaged over the 16 subjects, increased over trials. That is, after several presentations of the 16 words, a subject tended to give various pairs of words in the same sequence, or pattern, even though no such patterning was present in the different sequences presented to the subject. This tendency for certain words to cluster together more with successive repetitions of the list was correlated, of course, with an increase in the number of words correctly written down: by the end of the 16 trials, the average number of words written down was over 15. Tulving (1962) conjectured that this increase in organization in behavioral output resulted from the establishment, with successive presentations of the list, of new functional units of analysis that consisted of more than one word. His results clearly agree with such a position: repetition apparently can induce an integration or unitization of simple responses, such as single words, into larger functional units.

Observe how earlier associationism and the newer view would offer different interpretations of the results of this experiment. 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 units. Patterning in the results would be viewed as a by-product of the increase in the strengths of the invariant units, the single words. A structural view would, on the other hand, focus on the increase over trials in the extent to which clusterings of words appeared, *i.e.*, it would focus on the emergence of new behavioral patterns. The increase in single words remembered would be viewed as a by-product of the emergence of new patterns, the true functional units.

A subsequent experiment by Tulving (1964) provided more direct evidence on the nature of the establishment of new, higher-order, functional units of behavioral analysis. A number of procedural details distinguished this experiment from that just described, but for present purposes it suffices to think of the method as the same multi-trial free-recall method described above. Tulving (1964) dichotomized the correct words given by a subject on a trial into those that were or were not correctly given on the preceding trial. The latter words were said to measure the number of new words learned on a trial (intratrial retention) and the former were said to measure the number of words learned on a previous trial and still remembered on the present trial (intertrial retention). Now, several lines of evidence suggest that the number of different functional units that can be remembered at a time is approximately seven, plus or minus two. In the present context, this implies that no more than about seven new words could be learned on a given trial, if all were independent units, as one might expect them to be by virtue of their not having been remembered on the previous trial. That is, a word not previously remembered would not yet be part of a higher-order unit, and would constitute a functional unit by itself. The number of new words remembered on a trial, averaged over subjects, was in fact approximately constant over trials, after taking into account the progressively fewer remaining new words with every repetition of the list. And this approximately constant number was within the range, seven plus or minus two. The number of remembered old words increased over trials. As in the previous experiment (Tulving, 1962) so too did the measure of behavioral patterning, clustering, or organization. Tulving's interpretation of these interrelated results was that the number of functional units remained approximately constant throughout the experiment, bounded by the limit on how many such units can be retained at one moment. The size of these units, however, systematically increased with successive repetitions of the list. A subject did not learn a list of, in this case 22, separate words. Instead, a subject established a small number of functional units which increased in size as new words were integrated with them. Miller (1956) described this phenomenon of the establishment of new, larger, functional units as a "chunking" of simple units into larger units. Note again that the traditional learning curve plotting number of words remembered on a trial as a function of the trial number now becomes a by-product of the chunking phenomenon. The traditional learning curve, counting only words, does not according to this view represent a general law, because such a law must be stated in terms of true functional units.

It is impossible to overemphasize the essential point, so let us summarize it again here. Associationism focuses on individual words as units of analysis in multi-trial free-recall experiments, and interprets the traditional learning curve to show that the number of units, or words, that a subject has learned increases over trials. The size of each unit remains the same, the size of a word. An alternative view espoused by Tulving (1962, 1964) focuses on the structure of behavioral output, and decomposes the traditional learning curve into two parts showing that the number of units a subject has learned stays approximately constant over trials, as required by the upper bound on the number of functional units a subject can retain at one time, but that the size of a unit increases. That is, new, higher-order units are established over trials.

Mandler (1962, 1967) also clearly demonstrated in many other recall experiments the same fundamental importance of organization. The present paper can only refer the interested reader to Mandler's work, as well as to important experiments by Bower and his colleagues (Bower, 1970; Bower, Clark, Lesgold, and Winzenz, 1969; Bower and Lesgold, 1969). For additional related empirical and theoretical developments a reader should consult Bousfield (1953), Estes (1972), Johnson (1972), Tulving and Donaldson (1972), and Tulving and Madigan (1970).

Several tentative conclusions have been drawn from free-recall data. Most important, a simple response, such as a single word, does not necessarily have the properties of a functional unit simply by virtue of its extreme simplicity and comparative lack of internal structure: a functional unit of behavioral analysis may change during the course of an experiment depending on the nature of the contingencies imposed on a subject's behavior, and some resulting units may be quite complex. Associationism tended not to consider this possibility. It can scarcely be said, however, that all the structural issues dealing with the functional unit have now been resolved by recent work on organization in memory. This work has not led to general methods to control the nature of the functional units established by an individual human, at least not with the precision traditionally required in the experimental analysis of behavior. Neither are methods available to allow an experimenter to control a human's preference for different functional units depending on their internal structure, their quantitative properties in general, or the experimental parameters associated with those units. There have been, with some notable exceptions (for example, see Nuttin, 1976; Weiner, 1974), few studies of interrelations between structural variables and motivational variables, reinforcement variables, or other variables essential to a thorough understanding of means to control behavior of a single organism. There has been, that is, rather less attention given to the interactions and interrelations between the structure and the function of behavior than to certain structural properties by themselves. In later sections of this paper it is argued that the experimental analysis of behavior is well suited to the study of these relatively neglected interactions and interrelations.

The Status of a Concept of Memory in the Experimental Analysis of Behavior

We have reviewed a small part of the literature on organization in human behavior that has played an important role in the shift away from functionalist toward structuralist issues that has been so important in the cognitive revolution. Cognitive psychologists tend to look at the structure of behavior as only a means to the end of understanding the structure of the mind. More generally, we find the view expressed that general laws and principles will not be found in immediately observable behavior so much as in more abstract expressions (Anderson and Bower, 1973; Estes, 1975 *a, b*). Yet, at the same time we can see that a patient author could rephrase the methods and results of cognitive experiments so that no cognitive word or phrase ever appeared. I performed only part of such a translation for the first part of this paper. Why did I not complete the translation? In my opinion, such a translation would *not* achieve greater objectivity or parsimony of description. Consider Tulving's formula for organization in the data from multi-trial free-recall experiments. We noted that this number is not "an immediate observation". It is a number calculated from immediate observations. A question is, what shall we call such a number? Tulving (1962, 1964) called it a measure of "subjective organization", a measure of organization in memory. I chose instead, in the first part of this paper, to refer to it as a measure of organization in behavior, but chose not to try to continue the translation and to decompose the formula further into its observable components. The reason for this choice is that I agree with the view of Anderson and Bower (1973), Estes (1975*a, b*), and others, that such a decomposition of the formula into what ultimately is immediately observable would not only fail to serve a constructive purpose, it would indeed substantially reduce the parsimony and descriptive power of the formula. We shall return later to this issue of the role played in a science of behavior by certain kinds of unobservable theoretical quantities.

Let us now broadly consider some possible virtues in the use of cognitive vocabulary in discussions of behavior. Our first consideration will have to be a description of the origin of, and nature of the strong tendency to exclude words such as "memory" from the experimental analysis of behavior.

The traditional view and its origins. Watson helped to define the traditional view of Radical Behaviorism on the concept of memory.

> "The behaviorist since he never uses the term 'memory' is under no compulsion to attempt to define it. So many individuals getting their first orientation in behaviorism seemed to be troubled by the omission of the term that it seems best to use some illustrations and analogies" (Watson, 1924, p. 220).

Watson summarized his ensuing illustrations and analogies as follows.

> "By 'memory,' then, we mean nothing except the fact that when we meet a stimulus again after an absence, we do the old historical thing . . . that we learned to do when we were in the presence of that stimulus in the first place" (Watson, 1924, p. 237).

Consider also the following:

> "So far as scientific method is concerned, the system set up in the preceding chapter may be characterized as follows. . . . Its concepts are defined in terms of immediate observations . . ." (Skinner, 1938, p. 44).

It can safely be said that Radical Behaviorism encourages an analysis in terms of immediate observables and heavily discourages an analysis in terms of theoretical quantities that are not immediately observable. Furthermore, it has been held that terms that do not refer to immediate observables, such as memory, hunger (Skinner, 1932), attention (Skinner, 1971), and the like, can be translated without loss of scientifically useful meaning, into behavioral terms that are immediately observable.

This view can easily be seen to fit into a logical niche in the continuing evolution of ideas about a science of behavior. The continuity in mental processes across the phylogenetic scale that seemed required by Darwin's theory of evolution led at the close of the nineteenth century to a spate of scientifically unsupported claims of quite complex mental abilities in animals (Romanes, 1884; Miller, 1962). This required continuity was achieved, that is, by dramatically increasing man's opinion of the intelligence of animals. But so scientifically unacceptable were the anecdotal methods supporting these claims that a strong counter-reaction understandably set in. Morgan (1909), in the forefront of this counter-reaction, advocated the view that no greater intelligence should be attributed to an organism in an explanation of its behavior than is sufficient for the purpose. Subsequently, Watson only carried this cogent advice to its logical extreme. He achieved the required continuity in animal and human intelligence in a manner precisely opposite from the way in which Romanes had earlier achieved the same continuity. Rather than attribute greater mental complexity to animals, Watson attributed less to humans, and indeed he did so to the greatest possible logical extreme. In short, he denied that memory and the like, interpreted as an unobservable, had any value for the scientific study of behavior in animal or man. This then, is the traditional view of Radical Behaviorism toward memory.

Behavior and unobservable theoretical quantities. Skinner (1974) draws an important distinction between Radical Behaviorism and the experimental analysis of behavior. Radical Behaviorism is a philosophy of a science of behavior, while the experimental analysis of behavior is an emerging science of behavior. What we have seen thus far is that Radical Behaviorism has traditionally rejected the possibility that a concept of memory, something not immediately observable, has a rightful place in a science of behavior. Radical Behaviorism seems to have done so on behalf of efficiency and parsimony: it has tried to ensure that the emerging science of behavior, the experimental analysis of behavior, will waste as little effort as possible chasing mentalistic will-o'-the-wisps and will not become confused by a metaphorical usage of cognitive vocabulary. Radical Behaviorism split the domain of psychological concepts in two parts: those that deal with immediately observable behavior and play a role in a science of behavior, and those that deal with unobservable mentalistic notions and do not play a role in a science of behavior. The objective in this paper requires us to dwell for a moment on this dichotomy, to examine its nature, and to consider whether it may not have excluded, for reasons no longer valid, some general classes of variables that ultimately may be essential to the development of a science of behavior.

Radical Behaviorism's position on the concept of memory is both a prediction of what a mature science of behavior ultimately will look like and a prescription advising behavioral scientists on the significant issues at present confronting them and on how to attack them. One cannot over-emphasize that this view on memory was derived from a philosophy of science, Radical Behaviorism, and is not a view that was forced by data obtained by a science of behavior. The science now has the task of evaluating the philosophy's claim. How could the experimental analysis of behavior evaluate this claim that it does not need a concept of memory? How does the experimental analysis of behavior in general evaluate any such claim? Clearly, what is needed to answer this question is a criterion to evaluate the progress that has been made toward developing a science of behavior. Fortunately, there is a criterion agreed upon by nearly all those engaged in the experimental analysis of behavior. Indeed, it is strongly implied on the inside front cover of this journal. 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. All other criteria, including those derived from philosophical predictions of the ultimate nature of the experimental analysis of behavior, should be subservient to this one criterion. If one is to reject memory from the experimental analysis of behavior, one must, therefore, justify doing so on the grounds that one's capability of controlling the behavior of an individual organism is not thereby diminished. There are reasons to believe, unfortunately, that the development of this general capability has sometimes been diminished by the way in which memory has been excluded. No one could deny that impressive advances in behavioral control have taken place in recent decades. But there are contexts about which little is known. Consider the fact that we have scarcely more than the Delayed-Matching-To-Sample paradigm as a generally accepted method with which to study behavorial phenomena in settings resembling those that have revolutionized the analysis of human learning and memory. This is not a very impressive record. In fact, it is only a slight oversimplification to say that the experimental analysis of behavior simply has not studied behavior in settings resembling those for which a man in the street, a beginning student of psychology, a cognitive psychologist, or a behaviorist using ordinary language, would find the concept of memory relevant. By its own criterion of scientific progress based on precise and general behavioral control, we accordingly have made little progress in 40 years in extending a science of behavior to these settings (Kantor, 1970).

Let us tentatively accept the position that the experimental analysis of behavior is seriously restricted by its manner of excluding memory from its domain. How might a science of behavior expand its scope into the domain of what generally would be regarded as memory phenomena? The answer does not seem to require an abandonment of the position on mentalism recommended by Radical Behaviorism, but it probably does require a revision of our view that immediate observations are sufficient for the expression of all important theoretical variables. Indeed, unobservable theoretical quantities of a certain type have always been a part of the experimental analysis of behavior, although they admittedly have not always been regarded as such. Consider the rate of a free operant. To be specific, consider the frequency of a pigeon's key pecks per unit time. One does not observe a frequency of key pecks per unit time. This datum is a type of unobservable theoretical quantity. It is a number derived by totalling up the observed key pecks in some measured duration and then dividing the total by the duration. In this case, the mathematical derivation of the quantity of theoretical interest happens to be an exceedingly simple one, and everyone's mathematical education is sufficient to provide him not only with the necessary mathematical tools but also with an intuitive understanding of the mathematics. Different theories typically require one to compute different numbers, however, and there is no guarantee that all quantities of theoretical interest may be either derived so easily from the actual observations or as intuitively meaningful. Indeed, most theories require one to deal with numbers with significantly more complex mathematical heritages than a simple ratio. But there can be nothing inherently objectionable to the experimental analysis of behavior in any of these derived numbers simply by virtue of their being derived numbers. 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. Suppose now that we had such a quantity, and suppose further that the number were shown to be a function of one or more experimental parameters in a situation that in ordinary language would invite use of the word "memory". Tulving's measure of organization is an example of such a number. Clearly, there is nothing incompatible in principle between such a number and a science of behavior. A great deal of the contemporary mathematical and computer-simulation work in cognitive psychology and mental processes is of this type (Bower, 1975; Falmagne, 1974) and therefore is compatible in this specific sense with a science of behavior.

What is it, then, that distinguishes cognitive psychology from the experimental analysis of behavior? What distinguishes a mentalist from a behaviorist? To a significant degree, it is how we interpret these theoretical quantities. Many cognitive psychologists maintain that a theoretical quantity has value for a science of psychology to the extent to which the quantity has "psychological relevance", *i.e.*, relates to what intuitively seems to be psychologically important (Falmagne, 1974; Pribram, 1973). It is this *interpretation* of unobservable theoretical quantities that appears to separate a radical behaviorist from a radical cognitivist. But one is not compelled to provide mentalistic interpretations for these theoretical quantities, and when one refrains from doing so, the gulf between behavior and cognition narrows appreciably, and perhaps disappears. Recall that a number of investigators anticipate that truly general laws will emerge in terms of variables more abstract than immediately observed behavior (Anderson and Bower, 1973; Estes, 1975). Such an opinion has been thought by many theorists to be incompatible with the encouragement provided by Radical Behaviorism to analyses in terms of immediate observables. This apparent incompatibility in views on the nature of acceptable theoretical variables has contributed significantly to an unnecessary and unproductive gulf between scientific communities oriented toward behavioral or cognitive analyses. It is therefore important for social as well as for scientific reasons that we explicitly acknowledge that important theoretical variables may not be expressible as immediately observable behaviors, and that this acknowledgment is compatible with the methods and objectives of a science of behavior.

The vocabulary of the experimental analysis of behavior. The use of the word "choice" in the experimental analysis of behavior illustrates the point to be made in this section. The percentage of a pigeon's pecks on one of two keys in a concurrent schedule is an unobservable theoretical quantity, or derived measure, that is obtained in a situation where ordinary language invites the use of the word "choice". The experimental analysis of behavior uses the word "choice" but by doing so implies no mentalistic interpretations of the theoretical quantity. Furthermore, the word "choice" is liberally sprinkled throughout the literature on concurrent operants and the experimental analysis of concurrent operants has flourished. The word "memory" has seldom appeared in this journal and the experimental analysis of delayed stimulus control has, comparatively speaking, languished. This evidence, admittedly correlational rather than experimental in nature, suggests, nevertheless, that words having mentalistic interpretations might have heuristic value for a science of behavior. This is not a new idea.

> "In sum, the fact that the social scientist, unlike the student of inanimate nature, is able to project himself by sympathetic imagination into the phenomena he is attempting to understand, is pertinent to questions concerning the *origins* of his explanatory hypotheses but not to questions concerning their validity. His ability to enter into relations of empathy with the human actors in some social process may indeed by heuristically important in his efforts to *invent* suitable hypotheses which will explain the process. Nevertheless, his empathic identification with those individuals does not, by itself, constitute *knowledge*" (Nagel, 1961).

Even physicists, who presumably have no recourse to "sympathetic imagination" when dealing with sub-atomic particles, find it useful to speak in this sense of a particle "feeling" an interaction, and so on.

The experimental analysis of behavior has tended to refrain from applying cognitive labels to certain kinds of theoretical quantities on the advice of Radical Behaviorism. The essential component of this advice seems, however, to deal with mentalistic interpretations of variables, rather than with unobservable theoretical quantities in general. In addition, it was suggested that we need not confuse the objective interpretation of "memory", a derived measure in a specified experimental context and having heuristic value, with its mentalistic interpretation. On these grounds, it is suggested that the word "memory", appropriately interpreted, need not be excluded from the vocabulary of the experimental analysis of behavior. The word "memory", then, refers to some number or derived measure having significance in terms of some theory, and calculated from data obtained in a context where a speaker using ordinary language would find the word "memory" relevant. We shall have to confront the possibility that a useful concept of memory will not necessarily refer to immediately observed behavior.

Two Different Treatments of Structure in the Fundamental Unit of Analysis

Let us turn now to implications of the literature previously reviewed on organization in memory and behavior for the experimental analysis of behavior. In this section are described two different theoretical perspectives on the nature of the significant issues in the study of behavior. These two perspectives handle the notion of structure rather differently. We shall begin by considering a common view on the theoretical substance of behavioral methodology.

> "Acceptance of behaviorism as a methodological approach in no way necessi-

tates the acceptance of any substantive theory" (Nagel, 1961).

". . . the presuppositions of this approach are so simple that it is essentially atheoretical" (Mowrer, 1973).

A preference for inductive methodology has not encouraged experimenters to inquire into the nature of theoretical assumptions embedded in the experimental methodologies used to collect data. Views such as these of Nagel, Mowrer, and many others, may or may not be correct in a philosophical sense, that is, as descriptions of the theoretical content implied by Radical Behaviorism. But such a view is demonstrably false as a description of the theoretical content implied by some of the classical methodology in the experimental analysis of behavior. It should be noted in passing that scholars in the philosophy and history of science are by no means unanimous in the view that a science can be so thoroughly inductive that it can bypass a stage of premature theorizing by means of an extended stage of objective data collection (Hanson, 1958; Kuhn, 1970; Gillespie, 1960, but also see Quine, 1963).

Let us therefore continue on the assumption that an empirical methodology implies some kind of theoretical commitment. Let us specifically consider how different behavioral analyses commit an investigation in specific ways to different positions on the issue of temporal structure in behavior.

The structure of behavior has been for a long time and continues to be an important consideration in many behavioral analyses (Jenkins, 1970; Weiss, 1970; Williams, 1968; and many others). Consider in particular the cumulative record. One of its major virtues is its graphic portrayal of behavioral patterning (Skinner, 1938; Ferster and Skinner, 1957). While extremely useful in providing a nonquantitative picture of molar structure, the cumulative record is now appearing in this journal with a density noticeably less than in former years (Skinner, 1976). One problem with the cumulative record is precisely the complex structural picture it can reveal: despite some success in quantifying a few relatively simple structural features in certain special cases (for example, see Gollub, 1964), no general, elegant, quantitative system has emerged to categorize cumulative records in terms of basic principles. Thus, reading and interpreting complex patterns in cumulative records retains an element of art.

The cumulative record is but one of many signs of interest in structure in behavioral analyses. Structural concerns have played central roles in behavioral analyses of language (Skinner, 1957; Catania, 1972; Salzinger, 1973). These concerns with structure in language admittedly have been on a more programmatic and philosophical than empirical level, but behaviorally oriented empirical studies of language reasonably can be expected to appear more frequently in the future (Premack, 1971).

Some of the earliest behavioral studies of organization and structure were those of Hunter (1920, 1928) on double alternation and on temporal mazes in general. A later study by Schlosberg and Katz (1943) on the same topic introduced an idea basic to the present discussion. This idea deals with the interrelations between a subject's short-term memory for its own recent behavior, the chunking phenomenon, and the nature of the fundamental behavioral unit. In their own words,

". . . in the present situation the behavior sequence is so condensed in time and content that the effects of all stimuli and responses can be fused into a continuous behavior pattern. This would account for the relative ease with which the rat can acquire double-alternation lever pressing."

What structural properties might such a continuous behavior pattern, or unit, have? Recall Anderson and Bower's (1973) assertion that it was the nature of the unit of analysis that most sharply discriminated between the functionalist or associationistic view and their own structural view. The nature of the fundamental unit, particle, or atom in a science has historically been a significant issue in the older sciences, as well as in the behavioral sciences. An exposure to the philosophical history of this issue in chemistry, physics, biology, and psycholinguistics, provides a useful background for our present task, but unfortunately this material is far beyond the scope of the present paper (Clagett, 1959; Gillespie, 1960; Hanson, 1958, 1972; Kuhn, 1970). Our next task is to discuss some ways in which different behavioral analyses have handled this issue.

Suppose that the smallest meaningful behavioral unit of analysis, the fundamental unit, possessed a significant degree of internal structure. What would be the properties of such a unit? How might it be established? First of all, "structure" in the present context refers to systematic or stereotyped changes in behavioral topography over time, often but not necessarily over fairly short, but not extremely short, time intervals. (In fact, we shall identify this interval roughly with the short-term memory span.) Thus, structure here refers simply to temporal patterns of behavior. But not all patterns are, of course, basic functional units, so that it becomes necessary to distinguish between two different kinds of behavioral patterns: a behavioral pattern that defines the internal structure of a basic unit and the longer patterns generated over time by a succession of different units. How can one tell the difference between these two kinds of patterns? This difference clearly requires us to know the within-unit structure of a basic behavioral unit. But the determination of a unit of analysis is a bootstrap operation: one determines a unit of analysis by looking at the behaviors in terms of which general laws are stated, but one can frame general laws only by knowing the true units of analysis (Schick, 1971). (This unfortunate bootstrap feature of the search for the fundamental unit is certainly not unique to the present context. For example, it resembles problems encountered in the search for a criterion in epistemological studies; Chisholm and Swartz, 1973.) One method that has proved fruitful so far requires one simply to control experimentally features of the behavioral patterns that precede the delivery of a reinforcer. One may thereby determine functions relating those controlled features to the behavior such a method establishes and maintains. That is, one can thereby study the interrelations between the function and the structure of a behavioral unit. This method has proved successful in establishing and maintaining quantitative properties of the internal structure of behavioral units consisting of short sequences of simple responses patterned in time, such as interresponse times (Shimp, 1973*a*), sequences of interresponse times (Shimp, 1973*b*), and somewhat more complex distributions of simple responses (Hawkes and Shimp, 1975). This method allows one also to study preference among different units of analysis in terms of their various structural properties and of the reinforcement parameters experimentally associated with them (Shimp, 1968, 1969, 1970, 1973*a*, 1974; Hawkes and Shimp, 1974; Staddon, 1968). These molecular analyses of behavioral structure provide quantitative functional relations apparently unobtainable with the older, molar analyses of structure involving cumulative records. Perhaps the most successful attempt to date in developing a general quantitative system for molar structure is that by Killeen (1975). It will be most interesting in the future to see if this model is capable of accommodating the available literature on interrelations between the function and structure of behavioral units.

Human memory experiments imply that a fundamental unit of behavioral analysis can have structure, and the preceding evidence implies structure in fundamental units in the context of the experimental analysis of behavior. Additional research is now needed on the conditions necessary to establish units having specified structural properties. Research might well be directed to the proposal of Schlosberg and Katz (1943) and Hawkes and Shimp (1975) that these conditions require an understanding of the relations between short-term memory, behavioral patterning, and the fundamental unit. According to this view, 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. (For related literature on short-term memory, see Roberts and Grant, 1976; D'Amato, 1973; Devine and Jones, 1975; Murdock, 1974; Deutsch and Deutsch, 1975; Shimp, 1976).

We have now sufficiently characterized a structural analysis of the basic unit so that we may turn for comparison to an alternative approach. The most important difference between the two approaches deals with the assumed nature of the basic unit of analysis. Recall that one metafeature of associationism denies significant internal structure in the basic unit (Ebbinghaus, 1885; Pavlov, 1927; Hull, 1943; Estes, 1959). Associationism never denied structure in general, of course, but tended to regard the issue of structure as one that is to be treated most appropriately after one has grappled with the relations involving simple, unstructured, elementary units. Accordingly, associationists never devoted great time or effort to empirical investigations devoted to the discovery of the structural properties of the basic unit: such an enterprise did not appear especially appropriate or meaningful. Nonsense syllables, words, and the like, were treated as basic units more because those responses intuitively seemed to satisfy the metafeature of associationism than because any coherent set of empirical investigations into the properties of basic units demonstrated those responses to have those properties. Similarly, these rather arbitrarily selected units were assumed to retain their defining characteristics over varying situations where no cogent evidence showed them to do so: the identity of a simple response as a meaningful unit simply was assumed not to change during the course of many standard experimental settings. Behavioral patterning that was seen to develop was likely to be interpreted as a linear chain each link in which was supposed to contain a simple, elementary response.

Does this view on the nature of the basic unit that is characteristic of associationism ever arise in the analysis of reinforcement contingencies? It does indeed. It is especially important for us to acknowledge that this is so, in order that this view can undergo the empirical scrutiny that the experimental analysis of behavior usually requires of a theoretical position. This metafeature of associationism is likely to characterize a reinforcement contingency that makes the delivery of a reinforcer depend on extremely simple behavior. In addition, the same metafeature is likely to characterize a method of data analysis in which one counts the frequency of occurrence of a simple response. This metafeature is in fact sufficiently prominent in this journal that no statistical analysis is required to demonstrate the fact. Any survey of the experimental papers published in this journal will reveal that a great many, indeed a preponderance, of the schedules of reinforcement described here share the property that the behavior on which reinforcement is contingent, and the behavior in terms of which the results are analyzed, is as nearly without structure as it is practical to make it. After all, the mean rate of occurrence of a simple response has on occasion been said to be nearly the defining property of operant conditioning (Blough and Millward, 1965). For example, a pigeon's key peck requires only a small fraction of a second, and therefore has minimal temporal structure. The key peck has recently been seen to have some internal structure (Jenkins and Moore, 1973; Smith, 1974), but it undeniably has sufficiently little internal structure to qualify as an example of the kind of unit required by what Anderson and Bower called a metafeature of associationism. The same can be said of a rat's lever press, a human's simple verbal response, and so on. Interesting questions worthy of historical analysis but beyond the scope of the present paper are the origins of this commitment to a metafeature of associationism in reflexology (Pavlov, 1927; Skniner, 1938) and the maintenance of this commitment by various theoretical (Guthrie, 1959; Estes, 1959) and experimental (Grice, 1948; Skinner, 1948) developments.

Much of the interesting and constructive approach to behavioral structure exemplified by work on complex operants and higher-order schedules represents a compromise between the associationistic metafeature and a more consistently structural approach. A complex operant is one in which "the presentation of a reinforcer is made contingent upon sequences of responses as a unit" and a higher-order schedule is one in which "the behavior specified by a schedule contingency is treated as a unitary response that is itself reinforced according to some schedule" (Morse, 1966). Consider the following example. A reinforcer may be made contingent on a key peck that terminates, let us say, the third fixed-interval 30-sec schedule. In such a case, one speaks of a second-order fixed-ratio 3 schedule for the complex operant consisting of the pattern produced by a fixed-interval 30-sec schedule. By means of higher-order schedules of this general type, investigators have attempted to develop a system of relations involving units having significant internal structure (Findley, 1962; Kelleher, 1966; Marr, 1971).

Admirable as is the objective of this research on higher-order schedules, such research may be beset by a basic difficulty so long as the lowest-order reinforcement contingency hinges on the occurrence of an exceedingly simple response, such as a key peck. A fixed-interval schedule, to continue our previous example, does not very well control the pattern of behavior treated as a unit or complex operant, since the contingency involves behavior as simple as a key peck. One therefore can expect with such a contingency a considerably greater between-subjects variation in behavior than with a lowest-order contingency explicitly defined in terms of a behavioral pattern (Hawkes and Shimp, 1975; Shimp, 1975). Thus, experiments on higher-order schedules address in principle the same issue as that which the present paper also addresses. But in practice, these experiments have often examined molar structure of behavior with local structure being relatively uncontrolled, and therefore presumably less susceptible to our understanding. It is the local structure of behavior on which the present paper focuses as the most likely source of the true basic functional units. Let us put this in perspective by considering the notion of the "order" of a reinforcement contingency. Our previous example, a fixed-ratio 3 for fixed-interval 30-sec schedules is by tradition called a second-order contingency. The first-order contingency is said to be the fixed-interval schedule for a key peck that is taken to be the basic unit. But, if we consider local structure, the fixed-interval schedule is itself a second-order schedule, with the first-order contingency being that which establishes the basic unit or units. In the case of a fixed-interval schedule, the local behavioral patterns preceding reinforcement are uncontrolled, and therefore one could expect more than one unit to be adventitiously established (Shimp, 1975). Thus, an admission of local structure into our terminology redefines our example as a third-order, not second-order, contingency. And Morse's definition of a complex operant may be seen as an expression of the associationistic metafeature, because it implies that many of the responses we study, such as a key peck, are not complex, that is, that they lack significant internal structure, yet are meaningful units, *i.e.*, operants. A characteristic of an approach more consistently in tune with structural issues is an implication that such elementary responses do not have meaning or theoretical significance apart from whatever true functional unit of which they form a part (Anderson and Bower, 1973; Shimp, 1975).

Finally, let us summarize the difference between the two kinds of structural analyses we have examined. The central issue can be simply stated for a special case. Does the repeated delivery of food to a food-deprived pigeon that has just pecked a key simply increase the subsequent probability of a key peck, with the nature of the basic unit remaining invariant and equal to a key peck? Or, does it induce or establish new behavioral units related to whatever local behavioral patterns precede the delivery of the reinforcer, and therefore possessing significant internal structure?

Some Unresolved Theoretical Issues Corresponding to Different Treatments of Structure in the Fundamental Unit of Analysis

The unanswered questions with which we ended the previous discussion permit different views on a variety of fundamental theoretical issues.

The nature of independent and dependent variables. A shift in one's view of the nature of a basic unit of analysis forces a change in view of appropriate independent and dependent variables. Many classical schedules of reinforcement require one to accept as dependent and independent variables the frequency of simple responses and of the occurrences of pairings between those responses and other events, such as reinforcers, respectively. Alternatively, we might try to determine what the functional units are in the first place. We have already noted above that one way to do this is to control specified structural features of the behavioral pattern or patterns preceding the delivery of a reinforcer. One can in this fashion establish a behavioral unit related to a pattern preceding a reinforcer, *i.e.*, having known, quantitative properties. In many cases, however, one could not expect such a behavioral unit to have all the properties of the patterns preceding a reinforcer (see also Catania, 1973*c*). This limitation can yet have the heuristic value of focusing our attention on what is a significant issue for behavioral analyses. The notion that a fundamental behavior unit extends over time and has temporal structure suggests relations between behavior in short-term memory experiments and the nature of fundamental units of behavioral analysis. That is, in these terms it makes sense to ask what relations obtain between what a subject can remember of its own recent behavior in short-term memory experiments, on the one hand, and what behavioral patterns can be established as behavioral units when those patterns systematically precede a reinforcer, on the other hand (Schlosberg and Katz, 1943; Hawkes and Shimp, 1975; Shimp, 1975, 1976). Note that this question makes sense only after one has rejected the "no short-term memory" assumption implicit when one takes the basic behavioral unit to be nearly instantaneous and without significant temporal structure.

Molecular versus *molar analyses.* Another general issue related to those above pertains to the distinction between molecular and molar analyses in the experimental analysis of behavior. Standard usage now defines a molar analysis in two ways: either as a presentation of large-scale structural features of behavior visible in a cumulative record, or, as is increasingly common, as the calculation of only a single number, the rate of a simple, virtually instantaneous and unstructured behavior averaged over the many different local reinforcement contingencies that prevail in standard schedules (Herrnstein, 1970). In this latter sense, the term "molar" is associated with the smallest possible basic units of analysis, those compatible with the metafeature of associationism we have discussed earlier. Standard usage defines a molecular analysis in terms of behavior such as interresponse times, invisible in a cumulative record, and presumably dependent on the local reinforcement contingencies of little concern to a molar analysis. Thus, molecular analyses deal with larger basic units of analysis than some molar analyses. This peculiar feature of standard terminology may be too deeply entrenched to change, but it would be well at least to keep in mind that the standard terminology implies a difference in the size of basic behavioral units that in some cases is backwards from standard practice.

Free-operant behavior as baseline behavior. Some common baseline procedures involve responses without significant temporal structure, such as key pecks, lever presses, and so on (Sidman, 1960). According to a view interrelating the short-term memory span and the structure of the smallest meaningful units of analysis, or operants, it is doubtful whether such elementary behaviors can satisfy the theoretical requirements of an operant (Anderson and Bower, 1973; Shimp, 1975, 1976). Thus, a baseline reinforcement contingency involving such an elementary response may leave the true functional units to be established and controlled by chance contiguous occurrences of various behavioral patterns and a reinforcer. Such reinforcement contingencies involve a lack of precise control over the true functional units. This lack of control may in some circumstances be a virtue, but it is important to distinguish these circumstances from those arising out of an effort to develop general laws of reinforcement contingencies. Consider when it might be regarded as a virtue that certain reinforcement contingencies only loosely determine the behavior they maintain: a response may be considered to be a sensitive measure of the effects of some experimental variable when the reinforcement contingency maintaining that response is sufficiently loose so that behavior may vary widely as a function of that variable. Such a baseline contingency is clearly required for many purposes and the experimental analysis of behavior has skillfully exploited such contingencies as are at present available. However, one must carefully distinguish between the value a contingency might possess by virtue of its desirable properties as a baseline schedule, and the value a contingency has through its ability to shed light on the fundamental nature of reinforcement contingencies themselves. These two properties of schedules on occasion may be mutually incompatible. The structural considerations we have discussed in this paper suggest that a contingency useful as a baseline schedule by virtue of the loose control it exerts over structure may have little use as a device to reveal fundamental laws of contingencies that may necessarily involve structure. Let us take just one of innumerable examples one can find. Consider some of the literature on conditioned suppression that has been so useful in helping to understand basic principles of conditioning (for example, see Kamin, 1969; Rescorla, 1972). It is not uncommon in this important literature to find a variable-interval schedule used as a baseline, yet such a schedule may prove nearly fruitless as a vehicle to reveal basic laws involving reinforcement contingencies because it fails to deal with structural considerations (Shimp, 1975; also see Jenkins, 1970). Note also that this same failure ultimately places an upper bound on how much we can learn about conditioned suppression using variable-interval baselines, since the measure of conditioned suppression may not be expressed in terms of true functional units.

Biological constraints on learning. The structure of behavior is a significant issue from the perspective of ethology (Shettleworth, 1974; Staddon and Simmelhaag, 1971). So too, of course, is an analysis of behavioral patterns that function as units. Thus, we face a challenging question: how can one distinguish between, on the one hand, a functional unit having structure and established by operant principles and, on the other hand, a functional unit having structure but established by the other means emphasized in ethology? The attempt to answer this question might generate some highly profitable interactions between behavioral analyses and ethology. Even now, it obviously offers alternative ways to interpret behavioral patterns observed in situations such as the "superstition" experiment (Skinner, 1948; Staddon and Simmelhaag, 1971; Shimp, 1975).

Consider the following example of how the structural nature of the basic functional unit can have important implications for our views on the relations between behavioral and biological issues. A distinction has been made between operant key pecks and autoshaped key pecks (Jenkins and Moore, 1973; Schwartz, Hamilton, and Silberberg, 1975). Part of this distinction is based on topographical differences between the two classes of behavior. But these differences are to be observed over time intervals far shorter than the short-term memory span. We earlier went so far as to suggest that there may be no meaningful unit of behavior having such short temporal duration as either an autoshaped or "operant key peck". Thus, our previous discussions would suggest that structural differences between autoshaped key pecks and operant key pecks are not likely to reveal the whole story, are not likely to be sufficient to distinguish between two classes of true basic units. In addition to looking for structural differences involving temporal durations so short as a small fraction of a second, we might look for differences in the temporal spacing of key pecks and for how those structural differences are perhaps differentially susceptible to control by reinforcement contingencies involving the temporal spacing of key pecks. To the extent to which the organization of behavioral output conforms to that required by a structural reinforcement contingency, no doubts need remain that we are seeing operant behavior, rather than more directly biologically determined behavior.

Mathematical theories. The trend in theory building in the experimental analysis of behavior has been to try to interrelate empirical functions of a particular type, specifically, functions showing how the average rate of a free operant depends on various experimental parameters (Baum, 1973; Catania, 1973*a;* Herrnstein, 1970; Rachlin, 1973). These theories have not yet accepted structure in behavioral output as a significant issue with which they should deal. A few free-operant theories are beginning to deal with the structure of behavior, but as yet they deal only with rather large-scale structural features and not with interrelations between behavioral structures and their functions (Killeen, 1975; Staddon, 1974). Formal theories of operant behavior almost uniformly accept the essentially structure-free operant as the unit of analysis. But if we take structural considerations seriously, the attempt to try to develop a theory for such facts is not a particularly worthwhile enterprise: these structural considerations imply that the frequency of occurrence of responses, such as key pecks, averaged over different local reinforcement contingencies and averaged over different local patterns of behavior, is not a derived measure, fact, or number, that has theoretical significance. Such a number assumes instead the same minor role assumed by the traditional learning curve: any number based on the assumption that a behavioral unit is a fixed, extremely simple and nearly unstructured behavior, assumes the status of a by-product having little general significance.

CONCLUSION

We have contrasted a metafeature of associationism and a structural viewpoint to arrive at different and seemingly incompatible sets of significant issues for a science of behavior. Structural considerations encourage an experimental analysis of behavior in settings where the word "memory" is both relevant in an ordinary language sense and valuable to the scientific community in an heuristic sense. They also encourage both an analysis of interrelations among such memory experiments, more traditional reinforcement contingencies, and an analysis of how local reinforcement contingencies establish and maintain the smallest, meaningful, behavioral units. It appears likely that a successful behavioral analysis of these issues will require certain kinds of theoretical variables that are not immediately observable, and therefore that a science of behavior may emerge that is somewhat different from that sometimes encouraged by Radical Behaviorism: as Wheeler (1973) correctly noted, there is no necessary logical relation between Radical Behaviorism and a science of behavior. In any event, the experimental analysis of behavior seems well suited for a variety of reasons to contribute toward significant progress in the analysis of interactions between the structure and the function of behavior.

REFERENCES

Anderson, J. R. and Bower, G. H. *Human associative memory*. Washington, D.C.: Winston, 1973.

Angell, J. R. *Psychology: an introductory study of the structure and function of the human consciousness*. New York: Holt, 1904.

Baum, W. M. The correlation-based law of effect. *Journal of the Experimental Analysis of Behavior*, 1973, **20**, 137-153.

Blough, D. S. and Millward, R. B. Operant conditioning and verbal learning. In P. R. Farnsworth (Ed.), *Annual review of psychology;* Vol. 16. Palo Alto: Annual Reviews, 1965. Pp. 63-94.

Bolles, R. C. Cognition and motivation: some historical trends. In B. Weiner (Ed.), *Cognitive views of human motivation*. New York: Academic Press, 1974. Pp. 1-20.

Bolles, R. C. Learning, motivation, and cognition. In W. K. Estes (Ed.), *Handbook of learning and cognitive psychology*, Vol. 1. Hillsdale, New Jersey: Lawrence Erlbaum Associates, 1975. Pp. 249-280.

Bower, G. H. Organizational factors in memory. *Cognitive Psychology*, 1970, **1**, 18-46.

Bower, G. H. Cognitive psychology: an introduction. In W. K. Estes (Ed.), *Handbook of learning and cognitive processes*, Vol. 1. Hillsdale, New Jersey: Lawrence Erlbaum Associates, 1975. Pp. 25-80.

Bower, G. H., Clark, M. C., Lesgold, A. M., and Winzenz, D. Hierarchical retrieval schemes in recall of categorized word lists. *Journal of Verbal Learning and Verbal Behavior*, 1969, **8**, 323-343.

Bower, G. H. and Lesgold, A. M. Organization as a determinant of part-to-whole transfer in free recall. *Journal of Verbal Learning and Verbal Behavior*, 1969, **8**, 481-493.

Brown, J. Some tests of the decay theory of immediate memory. *Quarterly Journal of Experimental Psychology*, 1958, **10**, 12-21.

Bousfield, W. A. The occurrence of clustering in the recall of randomly arranged associates. *Journal of General Psychology*, 1953, **49**, 229-240.

Catania, A. C. Chomsky's formal analysis of natural languages: a behavioral translation. *Behaviorism*, 1972, **1**, 1-15.

Catania, A. C. Self-inhibiting effects of reinforcement. *Journal of the Experimental Analysis of Behavior*, 1973, 19, 517-526. (*a*)

Catania, A. C. The psychologies of structure, function, and development. *American Psychologist*, 1973, **28**, 434-443. (*b*)

Catania, A. C. The concept of the operant in the analysis of behavior. *Behaviorism*, 1973, **1**, 103-115. (*c*)

Chisholm, R. M. and Swartz, R. J. *Empirical knowledge*. Englewood Cliffs: Prentice-Hall, 1973.

Clagett, M. *Critical problems in the history of science*. Madison: University of Wisconsin Press, 1959.

D'Amato, M. R. Delayed matching and short-term memory in monkeys. In G. H. Bower (Ed.), *The psychology of learning and motivation*, Vol. 7. New York: Academic Press, 1973. Pp. 227-269.

Deutsch, D. and Deutsch, J. A. *Short-term memory*. New York: Academic Press, 1975.

Devine, J. V. and Jones, L. C. Matching to successive samples: a multiple unit memory task with rhesus monkeys. *Behavioral Research Methods and Instrumentation*, 1975, **7**, 438-440.

Ebbinghaus, H. *On memory*. Leipzig: 1885. (Rev. ed., translated by H. Ruger and C. Bussenius. New York: Teacher's College, Columbia University, 1913.)

Estes, W. K. The statistical approach to learning theory. In S. Koch (Ed.), *Psychology: A study of a science*, Vol. 2. New York: McGraw-Hill, 1959. Pp. 380-491.

Estes, W. K. An associative basis for coding and organization in memory. In A. W. Melton and E. Martin (Eds.), *Coding processes in human memory*. New York: John Wiley and Sons, 1972. Pp. 161-190.

Estes, W. K. Memory and conditioning. In F. J. McGuigan and D. B. Lumsden (Eds.), *Contemporary approaches to conditioning and learning*. New York: Wiley, 1973. Pp. 265-286.

Estes, W. K. The state of the field: general problems and issues of theory and metatheory. In W. K. Estes (Ed.), *Handbook of learning and cognitive processes*, Vol. 1. Hillsdale, New Jersey: Lawrence Erlbaum Associates, 1975. Pp. 1-24. (*a*)

Estes, W. K. Introduction to Volume 2. In W. K. Estes (Ed.), *Handbook of learning and cognitive processes*, Vol. 2. Hillsdale, New Jersey: Lawrence Erlbaum Associates, 1975. Pp. 1-6. (*b*)

Falmagne, R. J. Mathematical psychology and cognitive phenomena: comments on preceding chapters. In D. H. Krantz, R. C. Atkinson, R. D. Luce, and P. Suppes (Eds.), *Contemporary developments in mathematical psychology*, Vol. 1. San Francisco: W. H. Freeman and Co., 1974. Pp. 145-161.

Ferster, C. B. and Skinner, B. F. *Schedules of reinforcement*. New York: Appleton-Century-Crofts, 1957.

Findley, J. D. An experimental outline for building and exploring multioperant behavior repertoires.

Journal of the Experimental Analysis of Behavior, 1962, **5**, 113-166.

Gillespie, C. C. *The edge of objectivity*. Princeton: Princeton University Press, 1960.

Gollub, L. R. The relations among measures of performance on fixed-interval schedules. *Journal of the Experimental Analysis of Behavior*, 1964, **7**, 337-343.

Grice, G. R. The relation of secondary reinforcement to delayed reward in visual discrimination learning. *Journal of Experimental Psychology*, 1948, **38**, 1-16.

Guthrie, E. R. Association by contiguity. In S. Koch (Ed.), *Psychology: A study of a science*, Vol. 2. New York: McGraw-Hill, 1959. Pp. 158-195.

Hanson, N. R. *Patterns of discovery*. Cambridge, England: Cambridge University Press, 1958.

Hanson, N. R. *Observation and explanation: A guide to philosophy of science*. London: George Allen and Unwin Lts., 1972.

Hawkes, L. and Shimp, C. P. Choice between response rates. *Journal of the Experimental Analysis of Behavior*, 1974, **21**, 109-115.

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. The classical-instrumental distinction: reflexes, voluntary behavior, and categories of associative learning. In W. K. Estes (Ed.), *Handbook of learning and cognitive processes*, Vol. 2. Hillsdale, New Jersey: Lawrence Erlbaum Associates, 1975. Pp. 181-223.

Herrnstein, R. J. On the law of effect. *Journal of the Experimental Analysis of Behavior*, 1970, **13**, 243-266.

Hull, C. L. *Principles of behavior: An introduction to behavior theory*. New York: Appleton, 1943.

Hull, C. L., Hovland, C. D., Ross, R. T., Hall, M., Perkins, B. T., and Fitch, F. B. *Mathematico-deductive theory of rote learning*. New Haven, Connecticut: Yale University Press, 1940.

Hunter, W. S. The temporal maze and kinaesthetic sensory processes in the white rat. *Psychobiology*, 1920, **2**, 1-17.

Hunter, W. S. The behavior of raccoons in a double alternation temporal maze. *Journal of Genetic Psychology*, 1928, **35**, 374-388.

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 the auto-shaped response with food or water reinforcers. *Journal of the Experimental Analysis of Behavior*, 1973, **20**, 163-181.

Johnson, N. R. Organization and the concept of a memory code. In A. W. Melton and E. Martin (Eds.), *Coding processes in human memory*. New York: John Wiley and Sons, 1972. Pp. 125-159.

Kamin, L. J. Selective association and conditioning. In N. J. Mackintosh and W. K. Honig (Eds.), *Fundamental issues in associative learning*. Halifax: Dalhousie University Press, 1969. Pp. 42-64.

Kantor, J. R. An analysis of the experimental analysis of behavior (TEAB). *Journal of the Experimental Analysis of Behavior*, 1970, **13**, 101-108.

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.

Killeen, P. On the temporal control of behavior. *Psychological Review*, 1975, **82**, 89-115.

Köhler, W. *Gestalt psychology: an introduction to new concepts in modern psychology*. New York: Liveright, 1947.

Kuhn, T. S. *The structure of scientific revolutions*. 2d ed.; Chicago: University of Chicago Press, 1970.

Mandler, G. From association to structure. *Psychological Review*, 1962, **69**, 415-427.

Mandler, G. Organization and memory. In K. W. Spence and J. T. Spence (Eds.), *The psychology of learning and motivation*, Vol. 1. New York: Academic Press, 1967. Pp. 327-372.

Marr, M. J. Sequence schedules of reinforcement. *Journal of the Experimental Analysis of Behavior*, 1971, **15**, 41-48.

McGeoch, J. A. *The psychology of human learning*. New York: Longmans, Green, 1942.

Melton, A. W. and Martin, E. *Coding processes in human memory*. New York: John Wiley and Sons, 1972.

Miller, G. A. The magical number seven, plus or minus two: some limits on our capacity for processing information. *Psychological Review*, 1956, **63**, 81-96.

Miller, G. A. *Psychology: The science of mental life*. New York: Harper and Row, 1962.

Morgan, C. L. *An introduction to comparative psychology*. New York: Scribner's 1909.

Morse, W. H. Intermittent reinforcement. In W. K. Honig (Ed.), *Operant behavior: areas of research and application*. New York: Appleton-Century-Crofts, 1966. Pp. 52-108.

Mowrer, O. H. Preface to 1973 reprint of *Learning theory and behavior*. Huntington, New York: Robert E. Krieger Publishing Co., 1973.

Murdock, B. B. Jr. The retention of individual items. *Journal of Experimental Psychology*, 1961, **62**, 618-625.

Murdock, B. B. Jr. *Human memory: theory and data*. Potomac, Maryland: Lawrence Erlbaum Associates, 1974.

Nagel, E. *The structure of science: problems in the logic of scientific explanation*. New York: Harcourt, Brace and World, 1961.

Norman, D. A. *Memory and attention: an introduction to human information processing*. New York: John Wiley and Sons, 1969.

Nuttin, J. R. Motivation and reward in human learning: A cognitive approach. In W. K. Estes (Ed.), *Handbook of learning and cognitive processes*, Vol. 3. Hillsdale, New Jersey: Lawrence Erlbaum Associates, 1976. (*in press*)

Pavlov, I. P. *Conditioned reflexes*. Translated by C. V. Anrep. London: Oxford, 1927.

Peterson, L. R. and Peterson, M. J. Short-term retention of individual verbal items. *Journal of Experimental Psychology*, 1959, **58**, 193-198.

Postman, L. The present status of interference theory. In C. N. Cofer (Ed.), *Verbal learning and verbal behavior*. New York: McGraw-Hill, 1961.

Premack, D. A functional analysis of language. *Jour-*

nal of the Experimental Analysis of Behavior, 1971, **14,** 107-125.

Pribram, K. H. Operant behaviorism: fad, fact-ory, and fantasy? In H. Wheeler (Ed.), *Beyond the punitive society.* San Francisco: W. H. Freeman, 1973. Pp. 101-112.

Quine, W. V. Epistemology naturalized. Reprinted in R. M. Chisholm and R. J. Swartz (Eds.), *Empirical knowledge.* Englewood Cliffs: Prentice-Hall, 1973.

Rachlin, H. C. Matching and contrast. *Psychological Review,* 1973, **80,** 217-234.

Rescorla, R. A. Informative variables in Pavlovian conditioning. In G. H. Bower (Ed.), *The psychology of learning and motivation,* Vol. 6. New York: Academic Press, 1972. Pp. 1-46.

Roberts, W. A. and Grant, D. S. Studies of short-term memory in the pigeon using the delayed matching-to-sample procedure. In D. L. Medin, W. A. Roberts, and R. T. Davis (Eds.), *Processes of animal memory.* Hillsdale, New Jersey: Erlbaum Associates, 1976. (*in press*)

Romanes, G. J. *Mental evolution in animals.* New York: Appleton, 1884.

Salzinger, K. Inside the black box, with apologies to Pandora. A review of Ulric Neisser's Cognitive Psychology. *Journal of the Experimental Analysis of Behavior,* 1973, **19,** 369-378.

Schick, K. Operants. *Journal of the Experimental Analysis of Behavior,* 1971, **15,** 413-423.

Schlosberg, H. and Katz, A. Double-alternation lever-pressing in the white rat. *American Journal of Psychology,* 1943, **56,** 274-282.

Schwartz, B., Hamilton, B., and Silberberg, A. Behavioral contrast in the pigeon: a study of the duration of key pecking maintained on multiple schedules of reinforcement. *Journal of the Experimental Analysis of Behavior,* 1975, **24,** 199-206.

Shettleworth, S. J. Function, causation, evolution, and development of behavior: a review of *The animal in its world,* by N. Tinbergen. *Journal of the Experimental Analysis of Behavior,* 1974, **22,** 581-590.

Shimp, C. P. Magnitude and frequency of reinforcement and frequency of interresponse times. *Journal of the Experimental Analysis of Behavior,* 1968, **11,** 525-535.

Shimp, C. P. The concurrent reinforcement of two interresponse times: the relative frequency of an interresponse time equals its relative harmonic length. *Journal of the Experimental Analysis of Behavior,* 1969, **12,** 403-411.

Shimp, C. P. The concurrent reinforcement of two interresponse times: absolute rate of reinforcement. *Journal of the Experimental Analysis of Behavior,* 1970, **13,** 1-8.

Shimp, C. P. Synthetic variable-interval schedules of reinforcement. *Journal of the Experimental Analysis of Behavior,* 1973, **19,** 311-330. (*a*)

Shimp, C. P. Sequential dependencies in free-responding. *Journal of the Experimental Analysis of Behavior,* 1973, **19,** 491-497. (*b*)

Shimp, C. P. Time allocation and response rate. *Journal of the Experimental Analysis of Behavior,* 1974, **21,** 491-499.

Shimp, C. P. Perspectives on the behavioral unit: choice behavior in animals. In W. K. Estes (Ed.), *Handbook of learning and cognitive processes,* Vol. 2. Hillsdale, New Jersey: Lawrence Erlbaum Associates, 1975. Pp. 225-268.

Shimp, C. P. Short-term memory in the pigeon: relative recency. *Journal of the Experimental Analysis of Behavior,* 1976, **25,** 55-61.

Sidman, M. *Tactics of scientific research.* New York: Basic Books, 1960.

Skinner, B. F. Drive and reflex strength. *Journal of Genetic Psychology,* 1932, **6,** 22-37.

Skinner, B. F. *The behavior of organisms.* New York: Appleton-Century, 1938.

Skinner, B. F. "Superstition" in the pigeon. *Journal of Experimental Psychology,* 1948, **38,** 168-172.

Skinner, B. F. *Verbal behavior.* New York: Appleton-Century-Crofts, 1957.

Skinner, B. F. *About behaviorism.* New York: Alfred A. Knopf, 1974.

Skinner, B. F. *Beyond freedom and dignity.* New York: Alfred A. Knopf, 1971.

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.

Staddon, J. E. R. Spaced responding and choice: a preliminary analysis. *Journal of the Experimental Analysis of Behavior,* 1968, **11,** 669-682.

Staddon, J. E. R. Temporal control, attention, and memory. *Psychological Review,* 1974, **81,** 375-391.

Staddon, J. E. R. and Simmelhaag, V. L. The "superstition" experiment. *Psychological Review,* 1971, **78,** 3-43.

Thorndike, E. L. Animal intelligence. *Psychological Review Monograph Supplement,* 1898.

Titchener, E. B. *A primer of psychology.* New York: Macmillan, 1898.

Tolman, E. C. *Purposive behavior in animals and man.* New York: Century, 1932.

Tulving, E. Subjective organization in free recall of "unrelated" words. *Psychological Review,* 1962, **69,** 344-354.

Tulving, E. Intratrial and intertrial retention: notes towards a theory of free recall verbal learning. *Psychological Review,* 1964, **71,** 219-237.

Tulving, E. and Donaldson, W. *Organization of memory.* New York: Academic Press, 1972.

Tulving, E. and Madigan, S. A. Memory and verbal learning. In P. R. Farnsworth (Ed.), *Annual review of psychology.* Palto Alto, California: Annual Reviews, 1970. Pp. 437-484.

Watson, J. B. *Behaviorism.* New York: Norton, 1924.

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.

Weiner, B. *Cognitive views of human motivation.* New York: Academic Press, 1974.

Wheeler, H. Introduction: a nonpunitive world? In H. Wheeler (Ed.), *Beyond the punitive society.* San Francisco: W. H. Freeman, 1973. Pp. 1-21.

Williams, D. R. The structure of response rate. *Journal of the Experimental Analysis of Behavior,* 1968, **11,** 251-258.

Received 8 March 1976.
(Final Acceptance 5 April 1976.)