MEMORY AND VERBAL LEARNING[1]

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The domain of psychological research known today under the bifurcated title of verbal learning and memory has suffered through a long and dull history. Ever since Aristotle, some 2300 years ago, popularized the notion that we remember things by virtue of their contiguity, similarity, and contrast, and ever since Ebbinghaus, less than 100 years ago, experimentally proved that Aristotle's claims about the importance of contiguity were not entirely unfounded, psychological study of how people learn and remember things has kept countless thinkers, scholars, researchers, teachers, and students gainfully occupied without yielding any dramatic new insights into the workings of the human mind.

Many inventions and discoveries in other fields of human intellectual endeavor would bewilder and baffle Aristotle, but the most spectacular or counter-intuitive finding from psychological studies of memory would cause him to raise his eyebrows only for an instant. At the time when man has walked on the moon, is busily transplanting vital organs from one living body into another, and has acquired the power to blow himself off the face of the earth by the push of a button, he still thinks about his own memory processes in terms readily translatable into ancient Greek.

Fortunately, all this will be changed in the not too distant future. Gordon Rattray Taylor, in his widely read book, *Biological Time Bomb* (158a), has predicted that selective erasure of memories will become possible soon, perhaps by 1975, and that the skills required for transmission of memories will take only a little longer to develop, being available perhaps by 2000. Unfortunately for the psychologists interested in problems of verbal learning and memory—and reading between Mr. Taylor's lines—these great changes that would truly amaze Aristotle will be effected without their help or contribution. Once man achieves the control over erasure and transmission of memory by means of biological and chemical methods, psychologists armed with memory drums, *F* tables, and even on-line computers will have become superfluous in the same sense as philosophers became superfluous with the advancement of modern science: they will be permitted to talk—about memory, if they wish—but nobody will take them seriously.

[1] This chapter covers the period from approximately 350 B.C. to 1969 A.D., with a strong emphasis on the literature published between January 1967 and March 1969. The writing of the chapter would have been impossible without the support from the National Research Council of Canada under Grant APA 39, and it was aided by the National Science Foundation Grant GB 3710.

The knowledge that we are rapidly approaching the twilight of the era of psychological study of memory—again if we believe Mr. Taylor—imparts to us a calm feeling of detachment and serenity as we contemplate the state of the art in verbal learning and memory at the end of the seventh decade of the twentieth century. It also relieves us of some anxiety about displaying our ignorance and biases to the public—ignorance reflected in our failure to appreciate the profound significance of many things our wiser colleagues do in their laboratories, and biases that are too many to enumerate. If we are all out of business in another few decades, our being utterly wrong does not matter. Besides, an excellent source of objective information about who did what, and what he thought he found, is available in the form of *Psychological Abstracts*.

Verbal Learning and Memory: Some General Impressions

Research in verbal learning and memory has as its goal the understanding of how human beings retain and use information about symbolically representable events and objects, as well as information about relations among them. Symbolically representable events and objects that have most frequently been used in research to date—although they need not be—are verbal items: letters, letter combinations, digits, numbers, words, sentences, and the like. The perceived relations—as they necessarily must be—are spatial-temporal: an event, a verbal item to be remembered, occurs simultaneously with, appears next to, or is preceded or followed by some other discretely specifiable aspect of the perceived environment. It is also possible to specify the co-occurrence of an item with some other item in terms of other characteristics of the time span in which the items occur.

Why the split title when we refer to the field of research directed at the goal just stated? The reasons are primarily historical and therefore not really determinable. But some purely descriptive comments may help the reader to prepare himself for the somewhat schizophrenic state of affairs discussed under the general title of the present chapter.

If a chapter such as this one had been written between 1885 and 1930, it would have been entitled "Memory," because the expression "verbal learning" did not come into use until the late 1920s. As far as we know, the first time it occurred in the title of a paper was in 1929 (158). The imaginary early chapter entitled "Memory" would have contained mostly information about research that today would be labeled "verbal learning." Between 1940 and 1955 the same chapter in the *Annual Review* would have been entitled "Verbal Learning," and the term "memory" would not have occurred anywhere in it.

In the first full-fledged review of "verbal learning and memory" in the *Annual Review*, Keppel (82) devoted seven pages to the "memory" part of the bipartite field. Research concerned with memory storage and that relevant to theories of forgetting was discussed in this section. The rest of bipartite title, was concerned with matters classified under associative repertoire, acquisition, response learning, associative learning, and different topics of transfer.

Our impression of the relation between verbal learning and memory can be summarized in the following set of propositions: (*a*) Two different subcultures—verbal learning and memory—are clearly identifiable in the broad field with which this chapter is concerned. (*b*) The two subcultures share a common goal, but they talk different languages, ask different questions, use different methods, and have sworn allegiance to different pretheoretical assumptions. (*c*) Members of the two subcultures can be readily recognized in terms of kinds of things they write on the pages of learned journals, although not in terms of things they say to each other in face-to-face situations. (*d*) The members of each of the two subcultures—we will refer to them as students of verbal learning and students of memory—can again be divided into two categories depending upon their reactions to the other subculture. They either tend to mind their own business and consequently are completely oblivious of what the other subculture does, or they spend their waking hours plotting and executing forays into the other's territory.

Let us next consider the two subcultures in greater detail.

Research in verbal learning.—Students of verbal learning talk the stimulus-response language. For them, research in verbal learning has to do with acquisition and retention of verbal responses to stimuli. Acquisition is frequently equated with "attachment of responses to stimuli," and forgetting is frequently referred to as the "loss of response availability." Acquisition of responses reflects the strengthening of associations between the responses and the stimuli to which the responses are "attached." The response can be "made," in the course of acquisition or at the time of a subsequent retention test, as long as the strength of the association exceeds a hypothetical evocation threshold.

Forgetting is conceptualized as the consequence of weakening of associations, but recall failure may also reflect competition of responses attached to one and the same stimulus. Weakening of associations, frequently referred to as "extinction" or "unlearning," can be brought about by various experimental manipulations, or it may occur "normally," outside the laboratory.

The emphasis on stimulus-response connections in verbal learning dictates a concern with explicit specification of stimuli. Use of experimental paradigms in which the stimuli could be specified at least nominally—serial anticipation and paired-associate methods—and, more recently, the search for functional stimuli in paradigms such as serial learning and free recall, reflect this concern.

Subjects in verbal learning experiments usually learn lists of materials under carefully paced conditions and over several trials. A strong motor element is thus involved in their performance. It is frequently possible for the subject to "know" the material and still fail to get credit for his performance in which he has to make the responses at a rapid pace. For instance, even though a person can write the letters of the alphabet in their reverse order, he may require many trials to "learn" a serial "list" consisting of all the letters of the alphabet in reverse order if he is tested under the typical 2-sec rate. Finally, students of verbal learning frequently derive both experimental and theoretical inspiration from the classical conditioning paradigm.

Research in memory.—Students of memory are concerned with the study of storage and retrieval of item and order information. Item information has been stored and retrieved if the subject can (*a*) recall that item *i* occurred in a specified set of previously presented items, or (*b*) can identify a test item as being identical with one that was presented earlier. Storage and retrieval of order information implies the subject's ability to reconstruct from memory a smaller or larger segment of an ordered set of items presented on an earlier occasion. Many experimental situations require the subject's use of both item and order information.

Students of memory get their experimental and theoretical ideas from watching electronic computers. Consequently, they talk the information-processing language. When a subject sees an object or a picture, or hears an isolated item or a sentence, appropriate information is entered into his memory store. Frequently a distinction is drawn between at least two kinds of memory stores—short-term and long-term—with the information being "transferred" from either store to the other. Stored material can be "utilized" by the subject in recall or recognition tasks. Utilization of stored information, or retrieval, is often likened to a search and decision process. Forgetting occurs when retrieval is unsuccessful, either as a consequence of some loss or deterioration of the stored information, or because of the failure to "find" the desired information in the store.

Students of memory are more eclectic than students of verbal learning in their choice of methods and experimental paradigms. They are quite willing to use traditional list-learning tasks, but they are equally happy to study the performance of subjects in tasks in which students of verbal learning would have difficulty specifying the stimulus to which the response is attached, such as free recall and recognition memory.

If students of verbal learning are preoccupied with time—temporal contiguity between stimulus and response is regarded as the most important necessary condition for the development of an association—then students of memory are preoccupied with space: information is placed or laid down in the memory store or stores, it can be transferred from one store to another, and retrieval in a search through the store. The division between space-oriented and time-oriented individuals is not new to psychology: only 20 years ago place-learning organisms, guided by cognitive maps in their head, successfully negotiated obstacle courses to food at Berkeley, while their response-learning counterparts, propelled by habits and drives, performed similar feats at Yale. No wonder then that the label of "cognitive psychologist" is frequently applied to a student of memory, while the equally complimentary term "behaviorist" is often used to describe a student of verbal learning.

In this chapter, we will speak both stimulus-response and information-processing languages, depending upon the type of work we happen to be describing. We would like to refrain from using the cumbersome label of "verbal learning and memory," but we know of no ready substitute free from undesirable connotations. Our own preferred name for the field of research covered by this chapter is "ecphoric[2] processes," to parallel terms such as "sensory processes," "perceptual processes," and "cognitive processes." We will try out the reaction to this new term in the present chapter. Whenever the term occurs in what follows, the traditionalist can avoid a feeling of discomfort by substituting "verbal learning and memory" for it.

Some general impressions.—In the course of preparation for this chapter, we selected a sample of 540 publications—slightly less than one half of all relevant publications that appeared during the main time-period under review here—and independently rated each paper in terms of its "contribution to knowledge." We agreed to a remarkable extent in classifying all papers into three categories. The first, containing approximately two thirds of all papers, could be labeled "utterly inconsequential." The primary function these papers serve is giving something to do to people who count papers instead of reading them. Future research and understanding of verbal learning and memory would not be affected at all if none of the papers in this category had seen the light of day.

The second category, containing approximately one quarter of all the papers in our test sample, fell into the "run-of-the-mill" category. These represent technically competent variations on well-known themes. Their main purpose lies in providing redundancy and assurance to those readers whose faith in the orderliness of nature with respect to ecphoric processes needs strengthening. Like the papers in the first category, these articles also do not add anything really new to knowledge, and they, too, will have fallen into oblivion 10 years from now.

Many papers in the first two categories simply demonstrate again something that is already well known. Many others offer one or more of the following conclusions: (*a*) variable X has an effect on variable Y; (*b*) the findings do not appear to be entirely inconsistent with the ABC theory; (*c*) the findings suggest a need for revising the ABC theory (although no inkling is provided as to how); (*d*) processes under study are extremely complex and cannot be readily understood; (*e*) the experiment clearly demonstrates the need for further research on this problem; (*f*) the experiment shows that the method used is useful for doing experiments of this type;

[2] According to the dictionary, "ecphory" means "activation of a latent engram." We suggest using the term in a broader sense on the tacit assumption that the understanding of the activation of the latent engram also requires the understanding of the characteristics and origin of the engram.

(*g*) the results do not support the hypothesis, but the experiment now appears to be an inadequate test of it. Apart from providing dull reading, papers with such conclusions share another feature: they contain an implicit promise of more along the same lines in the future. They make one wish that at least some writers, faced with the decision of whether to publish or perish, should have seriously considered the latter alternative.

The third category of papers in our sample, comprising less than 10 per cent of the total, was classified as "worthwhile," including a small group of real gems. The papers in this category carry the burden of continuous progress in our field, by clarifying existing problems, opening up new areas of investigation, and providing titillating glimpses into the unknown. In most cases, the contribution that each particular paper makes is of necessity most modest. Nevertheless, the papers in this category unmistakably stand out from the large mass of other publications.

A phenomenon that should prove to be of great interest to future historians of verbal learning and memory is one that we would like to call—with a nod of acknowledgment to Gordon Allport—the functional autonomy of methods: yesterday's methods have become today's objects of study. Early investigators used serial anticipation and paired-associate list-learning methods to study acquisition and retention of associations; for a long time now the study of serial and paired-associate learning for their own sake has been proceeding apace (9, 185). More recent instances of canonization of method show new areas of research growing up around verbal discrimination (about which we will have more to say later), Peterson and Peterson's short-term "distractor" technique, Sternberg's method of studying errorless retrieval (156), and the like. We wish that someone wiser than we would explain the functional autonomy of methods to us.

Without saying why, we will assume that the function of experiments is to allow the construction, elaboration, modification, and overthrow of theories. By this criterion, too, much of the current research into ecphoric processes fails to make any contribution. Very few new theoretical ideas are being distilled from a flood of data. Existing theoretical notions are hardly ever modified or abandoned. Whenever the results of an experiment clearly do not support the hypothesis derived from a theory, the last thing the typical experimenter does is to question the theory. He usually finds good reasons why the theory should emerge unscathed from contact with the data.

Association, Redintegration, and Organization

Association.—The concept of association is almost as important to a student of verbal learning as the concept of cell is to a biologist, molecule to a chemist, and atom to a nuclear physicist, but despite its much greater age, its usefulness has not yet been universally acknowledged. It has aided the thinking of many at the same time that it has confused some, and it has been both used and abused more often than any other concept we know.

In a scholarly and timely review of the concept, Postman (133) has gone a long way towards clarifying some of the misunderstandings. He distinguishes among different senses of "association" and points out that in most of these senses association represents a disposition or capacity. To say that an association exists between A and B does not mean that whenever A occurs B will follow. It only means that B will follow A if some other conditions are met. "In any given situation the probability of a particular response will be a function not only of associative strength but also of the conditions of performance" (133, p. 556).

So far so good. But difficulties arise in the specification of these "conditions of performance." Under "performance variables," Postman includes a consideration of differences between intentional and incidental learning, the effects of instructions on learning, and preference effects in acquisition. But these factors are clearly concerned with initial acquisition and hence must have relevance primarily for associative factors. Other matters Postman considers—transfer and retention tests—also fail to clarify the nature and role of nonassociative "performance" variables. These tests do tell us something about what the subject has learned, but they do not tell us what else, beside an intact association, is required for successful performance of a learned act. The vase on the mantel—an example Postman uses—has the capacity to fall if touched by the wind. What are the events that combine with the capacity of A to evoke B to actually produce B? The associative framework of learning and memory will not come of age until it comes fully to grips with this problem.

Contextual associations.—Of all the different "kinds" of associations discussed by Postman, the contextual association enjoys the greatest popularity with students of verbal learning today. It is an old concept, although its name is relatively new. It usually refers to the association between a response—corresponding to a given item in the list—and the general environmental stimuli in presence of which the response is learned and recalled.

The increasing fondness with which the concept of contextual association is regarded by students of verbal learning is attributable to several factors.

First, it represents "one half" of the two-stage theory of paired-associate learning (167) that continues to guide a lot of thinking. Second, it has permitted the inclusion of free-recall phenomena, previously a source of embarrassment to the associative tradition, within the associative framework (133, 134, 147, 148). Third, the concept of contextual association has played an important role in explanations of unlearning in retroaction paradigms (81, 101). Fourth, the concept can be invoked to explain certain phenomena in recognition memory, such as the identification as "old" of words associated with study-list items (186). Finally, accrual of frequency units to items in the course of acquisition of verbal discrimination tasks (59) represents a quantal analogue of the development of contextual associations and promises to bring about a true marriage of the associative view of learning and recognition memory.

We have no objection to the concept of contextual association as a descriptive term, but we doubt its theoretical usefulness. Contextual association is simply a shorthand expression for the fact that the subject can recall individual items or small subsets of items from larger sets in absence of any specific experimentally manipulated retrieval cues. It explains such recall behavior in the same sense as the soporific quality of sleeping pills explains the effectiveness of these pills in inducing sleep.

Redintegration.—Association is the relation between two parts which, by virtue of their relation, constitute a whole; redintegration, on the other hand, refers to the relation between a whole and any one of its constituent parts. The term "redintegration" was introduced by William Hamilton (68), who used it as a name for his 'supreme principle of association,' according to which "thoughts or mental activities, having once formed part of the same total thought or mental activity, tend ever after immediately to suggest each other" (68, p. 436). The principle of redintegration thus was an early alternative to the conception of thought and memory as a chain of ideas.

The concept has recently been resurrected by Horowitz & Prytulak (74) to explain certain memory phenomena which cannot be accounted for by traditional principles of association. Horowitz & Prytulak define redintegrative memory in terms of a high conditional probability of recall of a "whole unit" given that a part of the unit has been recalled. They also introduce and illustrate the "principle of redintegrative power": the part of a unit that is best remembered in free recall is also the best cue for eliciting the whole unit. It remains to be seen whether the surplus meaning the concept of redintegration possesses over and above its basic definition as a strong association between a part and a whole will provide any fresh insights into the nature of memory and learning, but the effort is certainly worthwhile.

Organization.—Organization was just a synonym for association in McGeoch's (100) evaluation, but McGeoch was a temperate man. Many contemporary students of ecphoric processes are willing to die defending the proposition that organization, as a concept in psychological study of memory, has no more in common with association than a nobleman has with a beggar.

Mandler is the chief proponent of the position that phenomena of memory cannot be understood without recourse to the concept of organization, a concept that is very different from that of association. He has recently written extensively on the subject (92–95). Mandler's basic position is that organization is a necessary condition for memory.

What is organization and why is it such an indispensable concept in the study of memory? Mandler defines organization explicitly in several places, both generally, following Garner's (61) definition of structure, and more specifically, when referring to organization of words in lists to be learned. These words, he says, are organized "when the functional aspects of a word, specifically its meaning, depend at least in part on the set of words of which it is a member, and the relation of the members of the set to each other" (95 p. 102). Organization can take several forms: a set of words can be organized hierarchically, categorically (in unordered sets), or serially (in ordered sets). According to the definition of organization, the meaning of a word depends at least partly upon its position in the hierarchy, in the category, or in the series.

The evidence Mandler presents in support of his theoretical position comes mostly from experiments in which free recall of words is shown to be a linear function of the number of subject-determined categories into which words are sorted prior to their recall. In these experiments, the correlation between number of words recalled and number of sorting trials taken by the subject, and hence amount of exposure to the words to be recalled, is essentially zero. Thus the conclusion follows that the classification of words into groups on the basis of their meaning is a sufficient condition for recall above the limits set by immediate memory, while repetition—strengthening of contextual associations—is not a necessary condition. The same principle is also neatly demonstrated in another experiment: subjects who were simply asked to categorize a set of words into a convenient number of categories, in absence of any instructions to memorize the words, recalled as many words as did subjects who were explicitly instructed to commit all items to memory (92).

Mandler views the effect of organization on memory in terms of the format of storage of to-be-remembered materials—multiple hierarchies, subjectively determined categories, or series. Recent experiments by Bower and his associates (17) have complemented this view by emphasizing retrieval processes. Bower et al. had subjects learn sets of words presented for study either in familiar or randomly arranged hierarchies. The results were dramatic: in one experiment, after a single trial, subjects recalled 73 words out of 112 when the words were presented in hierarchical organizations, but only 21 words out of 112 when the same words were presented in random sets. Bower et al. concluded that hierarchical organization was used by subjects as a retrieval plan for cuing recall.

Both association and organization refer to relations between and among verbal units. The associative relation has its origin in the spatial-temporal proximity of the associated items, while organization is based on the intrinsic characteristics of organized items. Association has a single property—strength—while organization has many facets. Whether we need one set of laws governing the learning and recall of associated items and another set to handle memory of organized items remains an interesting and important question. Available evidence, although scant and indirect (149, 161), suggests the answer to the question is affirmative.

Acquisition and Storage

We discuss six topics under the general heading of acquisition and storage: repetition and memory, learning of paired associates, memory and mental imagery, memory for serial order, verbal discrimination, and sensory modalities and memory. Research that can be fitted into these rubrics is concerned primarily with the "what" and "how" of establishment of associations, or placement of mnemonic information into the store.

Repetition and memory.—Rather simple and straightforward classical frequency theories have held for a long time that repeated presentations of nominally identical test items result in the strengthening of the trace of that item, or strengthening of the association of that item with some general contextual features present during all presentations. Alternative views are (*a*) that each occurrence of a test item creates an independent trace, and (*b*) that each repetition creates many replicas of the trace.

This distinction between strengthening of a single trace versus multiplexing a trace as a consequence of presentation of a nominally identical event is an old one. Ward (171), for instance, referred to it as the functional vs. atomistic view. The functional view held that repetition produces further growth or some other such change in the old trace, while according to the atomistic view, each repeated event calls into being a new trace, thus resulting in a large number of traces "qualitatively alike but numerically distinct."

The stimulus-response theory of acquisition ascribes a mechanical "stamping-in" function to repetition, and sometimes assumes a continuity of repetition and "reinforcement" effects (1). Such a point of view encounters difficulty when confronted with the results from experiments such as that of Glanzer & Meinzer (65): overt repetition of items in free recall added less to the memory store by way of information capable of supporting recall than did "silent rehearsal." Such data suggest that a normal subject does not use processing time for mechanical repetition, and that repetition per se is not a sufficient condition for trace strengthening (cf. also 52).

Other data suggest the need for a much finer-grained analysis of repetition effects than that offered by the classical trace-strengthening view. Melton (104) has found the effect of repeated presentation of items in single-trial free recall to depend critically on the interval between repetitions: recall increases as the interval between two occurrences of a repeated item increases. Melton has suggested that at least a part of this distribution phenomenon is attributable to differences in contextual encoding of two nominally identical items.

Melton's distribution effect, replicated by Glanzer (62), is fascinating because of the apparent paradox it demonstrates: as the repetition interval increases, it becomes less likely that the subject will recognize the second presentation of an item as being a repetition; yet recall of repeated items is better as the interval is greater. The paradox is the same as the one posed by Peterson (126) for the short-term recall of paired-associates (cf. 87).

The intriguing nature of Melton's data is enhanced by Waugh's (172,

173) repeated failure to find any trace of the distribution effect, and by Underwood's failure to find an interval effect beyond a difference between massed and distributed presentations (159). The discrepancy between Melton and Glanzer on the one hand and Waugh and Underwood on the other may be attributable, at least in part, to differences in input modalities: the former two investigators presented their material visually and the latter two auditorily. But even if this hypothesis turns out to be tenable, it would only deepen the mystery of the Melton effect by an order of magnitude.

The more experiments are done on repetition effects, the more modifications must be added to the classical trace or association strengthening view. Bower & Lesgold (19) have replicated and clarified the finding that the inclusion of previously learned items in free-recall lists retards the acquisition of these lists, in comparison to control conditions in which subjects learned lists composed of new "weak" or "unlearned" items, and similar findings have been reported by others (121a, 182).

The negative transfer effects found in these experiments point to the importance of meaningful specific interitem associations, or subjective organization, in free-recall learning. They also should dampen the enthusiasm of those who believe in the importance of contextual associations in learning tasks such as serial learning and free recall. It is rather difficult to see why contextual associations count so little in learning of second lists in these transfer studies. Items in the first list should be reasonably strongly associated with the context when the subject starts learning the second list. Yet they end up being recalled less readily than items the subject has not seen before the second list.

All current theories and almost all extant data tell us that the best way to learn verbal material beyond the immediate memory span is to allow its repetition—exactly the same item presented trial after trial. It is therefore interesting to note conditions under which something other than straight repetition might lead to superior retention. The two relevant papers are by Bevan and his associates (13, 14).

In one experiment (13), subjects were tested for free recall of lists varying in the similarity of successive presentations of to-be-remembered material. For example, a list might contain four presentations of one member of a conceptual category, or one presentation of each of four different members of that category. Recall of categories improved as more "variation" in category instances was provided. In another study (14), the same kind of effect occurred when cues for to-be-remembered items were changed from one presentation to the next instead of being simply repeated.

These data illustrate one of the problems in studying repetition effects within lists (62, 172), and raise some interesting questions. With free recall tests, there is no way of knowing which of two or more presentations of an item is being recalled. This problem does not arise in the context of a trace-strengthening theory, but if we assume that two temporally distinct presentations of one item constitute two different "events," and produce similar but not identical traces, the problem becomes meaningful and its implications quite exciting.

A new method of investigating repetition effects in typical list-learning experiments was described by Jung (77). His subjects learned paired-associate lists, starting with a single item and seeing one additional new item on each successive "mini-trial." This method, for which we would like to suggest the simple name of "adit method," is being adopted in different versions. We can distinguish between "redundant" and "nonredundant" adit methods. In the former, on successive mini-trials the input from the preceding trial is repeated along with the added item, while in the latter, the subject is given just a single item on each mini-trial, with instructions to "carry" the rest of the set in memory and to reproduce the whole set on each mini-trial.

Mandler & Dean (96) have used both versions of the adit method for fine-grained analyses of recall in tasks requiring "seriation" and free recall. These experiments have shown no consistent difference in the acquisition of lists depending upon whether traditional or adit methods were used. Since the subject spends very much less time examining the input under the adit conditions, the finding of no difference constitutes a departure from the total-time hypothesis which has been shown to hold under many conditions (46).

Dalrymple-Alford (52) compared subjects' immediate memory spans for digits obtained by means of the classical method with those obtained under the nonredundant adit method. Digits were presented visually at the rate of one per second, and subjects' verbal recall was paced at the same rate. Two methods yielded identical measures of memory span. Apparently, repeated reproduction of systematically increasing parts of the span series under the adit conditions are totally ineffective in producing learning that would enable the subject to recall more material than he could after a single presentation of the whole series.

Repetition seems to be effective, however, under conditions investigated by Buschke & Hinrichs (35). These investigators used the redundant adit method in presenting to their subjects series of numbers selected from the set of 1 to 20. Numbers were presented auditorily at the rate of one per second, and subjects' recall was obtained under different recall instructions. Recall was higher when the subject recalled the items in the ascending natural serial order (given the input of 12, 8, 17, 2, recall in the order of 2, 8, 12 and 17) than in the order in which the items were presented, despite the fact that in the former case a continuous rearrangement of items in the store would appear to be necessary, while in the latter case it is not. This latter finding is consistent with the format of what Buschke has called a "marker" storage system (33, 34).

The reasons for the apparent discrepancy between the Dalrymple-Alford findings on the one hand, and those of Buschke & Hinrichs on the other, with respect to the effect of repetition or rehearsal, are not clear. The two experiments differed from one another in many ways. Could it be that the difference is related to inputs in different sensory modalities?

Learning of paired associates.—No reviewer can possibly ignore research in paired-associate learning primarily because of its great quantity in the contemporary literature. Why does the enthusiasm for it continue unabated?

It is possible to identify at least four different uses to which the paired-associates method has been put. First, it has been regarded as a close facsimile of conditioning procedures, and used to extend animal behavior principles to "complex" learning tasks. Second, it has served as a type of cued recall test, one of many different ways of indicating to a subject which item he is to attempt to recall. Third, it has been the main tool for investigating the interference theory of forgetting, that is, a means and not an end. Fourth, paired-associates learning has become a topic of study in its own right, that is, an end and not a means.

This fourth category finds its most vocal advocate in Battig (9). He views paired-associate learning as a very complex task requiring multiprocess models to explain it, and quite unlike a simple stimulus-response situation as it was previously thought to be. It is in fact so complex that what is needed to advance our understanding of it is, according to Battig, a gigantic factorial experiment, "involving the simultaneous orthogonal manipulation of all known task and procedural variables known or suspected to have any effect whatever within any kind of PA learning task" (9, p. 167). Battig adds that this would probably require the "cooperation of a large number of PA researchers," although he has taken a modest step in this direction on his own (10). We hope that when the day of the ICBM (Intercontinental Battig Method) arrives, we shall be able to fill one of the cells.

Battig's hopes for the future define one kind of theory about paired-associates phenomena. Presumably, once his giant factorial is done (and replicated ?), one would have a listing of all the effects of all the variables. This would then tell us everything about paired-associate learning and would thus obviate the need for a theory, at least until such time as someone thought up a new variable.

Until such time that a complete table of facts about paired-associate learning becomes available, we have to manage with old-fashioned theorizing. The theoretical light that has guided a good deal of experimental work was provided by Underwood & Schulz in 1960 (167). By far the most impressive thing about their two-stage theory is its longevity. After 10 years of steady use, it shows absolutely no signs of wear and tear, and it looks as if it is going to be around in the same form for a long time to come.

The theory as stated makes some interesting predictions. The one we like is a straightforward deduction from the basic principles: given that paired-associate learning is a matter of establishing response availability and specific intrapair associations, if responses are fully available and the strength of associations between each stimulus and its corresponding response is the maximum possible, the list requires no learning, or should be learned in one trial at the most.

A list consisting of, say, 24 common nouns as stimulus terms, each accompanied by the same noun, either in the singular or plural form, as the response member would constitute such a list. We can call it the A-A list. All response terms would be readily available if subjects were informed about the construction of the list, and an association between two identical words is presumably stronger than any other association. The singular-plural wrinkle is introduced in order to convert the task from a reading task into one requiring the learning of specific associations, as in any other paired-associate list.

We suspect the list cannot be learned too readily, even though the two-stage theory as it is known today says it can. We also suspect that the list of this kind would be learned in the all-or-none fashion. Analysis of the data in the way suggested by Bower & Trabasso (20) should prove that. Although the issue of all-or-none vs. the incremental learning of association was successfully pronounced dead by Restle (139), it may be worth reviving for the purpose of the demonstration that a list entailing highly available responses and extremely strong intrapair associations is learned in the all-or-none fashion.

A major step in the conceptual analysis of classical paired-associate phenomena has been provided by Martin (99). His is the most important theoretical statement in the area since Underwood & Schulz's two-stage model.

The central concept of Martin's thesis is "encoding variability": a nominal (experimenter-defined) stimulus may evoke a different perceptual "encoding" response from the subject on each of its successive appearances. In simple paired-associate learning tasks, subjects must recognize each stimulus term as "old" before they can respond to it. Martin's (98) and Bernbach's (12) experiments have made it quite clear that the recall of the response term is no better than chance if the subject fails to recognize the stimulus as an "old" item.

Success or failure of recognition of the stimulus term as "old" at the time of the attempted recall of its corresponding response term, according to the theory, depends upon the similarity of the encoding responses made to the stimulus term on the test trial and on the original learning trial. Since the associative bond supporting the response recall is built up between the encoded version of the stimulus and the response term, the subject's failure to encode the stimulus term identically on study and recall trials leads to the consequence that the associative bond, even if intact, cannot be activated.

The power of the encoding variability hypothesis can be seen in Martin's account of the role of stimulus meaningfulness (m) in paired-associate learning and transfer. For example, the hypothesis predicts that while List 1 learning may be slower for low m stimuli than for high m stimuli, transfer to an A-Br list will be positive for low m, negative for high m. This follows from the theory since low *m* stimuli can be encoded differently during List 2 learning, while high *m* material cannot. Thus, encoding variability effectively reduces interlist similarity for low-*m* stimuli.

Most paired-associate work continues to be done in the context of multi-trial situations, and most theory is couched in learning terms. Despite the early start made by Calkins in 1896 (36), followed a little later by Murdock (110), little attention has been paid to short-term processes inherent in any trial-by-trial procedure. The subject always knows the pair he has just seen, if the pair is composed of two well-integrated items, but the pair is quickly "forgotten" as other pairs are presented and recalled (3). The effect of repetition, therefore, can be viewed as the production of increasing resistance to forgetting by individual pairs (11). Increasing resistance to forgetting and strengthening of associations may turn out to be nothing more than two different ways of talking about the same process, but different labels frequently suggest different further thoughts, and in that sense one name may be better than another.

An experiment by Bregman (21) on massed and distributed practice in paired-associate learning illustrates some of the differences between short-term memory and traditional methods and analyses. Bregman defined massing and distribution of practice in terms of interitem intervals, rather than in terms of interlist intervals. The conclusions he reached were the exact opposite to those accepted earlier in traditional experiments: distributed practice facilitated retention, not acquisition. In addition, the effects Bregman obtained were large ones, in contrast with some of the tiny differences that usually result from massing and distribution in list-learning experiments (160).

Paired-associate research has been dominated by stimulus-response concepts since its early days. It is high time, therefore, that someone posed the question about the function of stimulus terms in paired-associate tasks. What exactly do stimulus terms do in such tasks?

Plainly, stimulus terms are not necessary to produce response recall. The fact that subjects store information about stimulus items and can recall them (53, 168), the fact that free recall tests or MMFR (modified method of free recall) tests may reveal only the smallest of differences in amount recalled (83), and the fact that either item in a pair can function as a cue for recall of the other (90), make it clear that the labels "stimulus" and "response" have been somewhat inappropriate. They are replete with surplus meaning, and have apparently escaped the notice of the soul-searching that Postman (133) says is a mark of the associationist's use of theoretical terms.

Memory and mental imagery.—The use of mental imagery as a mnemonic device is an ancient and venerable practice, but it failed to interest the followers of Ebbinghaus. In 1963, Noble (119) suggested that the claims of professional mnemonists be subjected to impartial scientific evaluation, presumably by the hard-nosed, no-nonsense techniques of rote learning research. We can report that this evaluation is under way, and it is indicating that the efficacy of imagery in memorization does not always vanish in the laboratory.

Modern researchers have approached the study of imagery and memory in two ways. The first revolves around the use of to-be-remembered materials that are presumed to differ in their ease of representation as visual images. Paivio (122), in an extensive research program, has found that pictures of objects are more readily recalled than their labels, and that concrete words are superior to abstract words in almost any task: paired-associate learning, free recall, serial recall (123), and recognition. In addition, the rated concreteness or imagery value of words appears to be a much better predictor of paired-associate learning than other more traditional measures of word attributes such as meaningfulness and frequency (124).

The second approach has been taken through the use of instructions: subjects coached in the use of special imagery techniques can perform at levels that are stunning in comparison to those common to rote learning situations. The effects of imagery instructions such as those reported by Bugelski (32) and Bower et al. (18) qualify as something more than amusing demonstrations of a mnemonic device. Imagery recommends itself as an extremely powerful means of producing strong associations without the use of laborious trial-by-trial procedures or norms of pre-experimental relatedness.

The study of imagery and memory has just begun, and consequently, there has been little by way of explanation of why imagery "works." Paivio (122) suggested that independent verbal and visual "codes" could be stored for certain kinds of materials (concrete words and pictures) while only verbal codes could be employed for abstract words. This position identifies imagery effects with amount of information stored, and agrees with the interesting hypothesis that verbal and visual components of a trace can be forgotten independently (7). The basis of imagery effects may lie in the "distinctiveness" of information stored as images (90), but this hypothesis offers little by way of explanation for an effect that appears in so many different kinds of tasks.

The revival of interest in imagery and the dividends it is paying in imaginative experiments such as those by Brooks (27) and Huttenlocher (76) may indicate the end of dominance of the field of verbal learning and memory by theories concerned exclusively with verbal associative processes. Just 10 years ago, Bousfield (15) could say that the concept of word meaning made sense only if it referred to verbal associations, and many were apparently willing to agree. Unobservable verbal processes were acceptable in this formulation; they were called "mediators." Unobservable visual images at that time were, of course, nonexistent and therefore could not be investigated.

The main problem facing imagery research now is to provide some integration of the data with memory theory in general. To say that some material is remembered because some images are remembered, or because natural language mediators are remembered (1), is to say nothing about the workings of memory.

Memory for serial order.—The acquisition and retention of information about the temporal and serial order of items in memory tasks continue to be heavily researched topics. Major interest focuses on three areas: identification of the effective stimulus for recall of individual elements of a series; explanation of the serial position effect; and short-term retention of order information.

Both the stimulus-response chaining and position-learning hypotheses and their various combinations, have continued to inspire experimenters (185). Some rather peculiar approaches have been taken to demonstrate the superiority of one theoretical position over the others. Foremost among these we find transfer tests—serial to paired-associate learning, as well as vice versa. The rationale of these tests has been discussed by Postman (133). But whatever the reasons for the use of such tests as measures of cue functions in serial learning, whatever the apparent degree of clarity of predictions—e.g., formation of item-to-item associations producing positive transfer—we think that attempts to understand serial learning by studying a subject's subsequent performance on another task which is itself not understood will only serve to confuse the issues further.

If transfer tests do not tell us what the functional stimulus is in serial learning, is it possible that some other method will? Woodward & Murdock (183) showed their subjects a list of 10 words and then probed for the recall of a specific word by giving as the retrieval cue (*a*) the immediately preceding word, (*b*) the ordinal number of the position of the target word, or (*c*) both of these cues (double cue). They concluded, on basis of their data, that sequential and positional cues "may be equally effective" in serial learning.

The most interesting finding, however, of the Woodward & Murdock studies, in our opinion, was the failure of the double cues to yield any higher recall than single cues. The lack of additivity of cues means that the immediately preceding word and the position of an item represent completely redundant information to the subject and that the contrast between them is meaningless.

Nothing is as futile, even silly, in our opinion, as the search for the stimulus in serial or any other type of learning. One can think of scores of queries or "stimuli" that would produce various proportions of correct responses in different retention tests of a series of items (22). We can call all these queries "functional stimuli" and thereby express the realization that the human mind is an extremely fine and powerful information processing device. This insight, however, requires no experimental evidence; Aristotle was well aware of it.

At this point we cannot resist the temptation to concur in Cofer's judgment (42) about the snail's pace of progress in our field. In 1912, Franz

Nagel (116) reported some observations relevant to the question of sequential associations. A single well-trained subject learned series of 12 nonsense syllables. After reaching the criterion of one perfect recitation on a given series, the subject was given 24 overlearning trials for the same series, and then tested by means of the sequential probe method (Trefferversuche). The subject was explicitly instructed to refrain from thinking about the whole series, and to produce the desired syllable solely "out of" the probe syllable. The probability of correct responses, over six such series, was 0.30. This and similar findings should have led investigators to be wary of (*a*) the proposition that one item is a stimulus for the recall of the next item in a series, (b) the usefulness of the sequential probe method for the purpose of testing the hypothesis, or (*c*) both the hypothesis and the method. The hypothesis and the method, however, are still being put together by indomitable students of verbal learning and memory more than 50 years later.

A refreshingly novel approach to the problem of "the stimulus" in serial learning has been taken by Voss (169, 170). His experimental findings have revealed the inadequacy of both chaining and position theories, and his methodological innovations promise to give new life to research in serial learning.

The serial position curve.—The difference in recall at the ends and middle of a list maintains its reputation as the Chinese puzzle of verbal learning. Despite a great deal of imagination shown in applying the word-magic treatment to the phenomenon, frequently under the guise of "theory," many still believe that our understanding of the phenomenon has advanced little since Ebbinghaus first described it.

We do not understand why serial position effect is frequently investigated in multitrial experiments. The largest difference between recall of items from the ends and from the middle of a serially ordered collection occurs on the first trial, thus clearly suggesting that the explanation for the phenomenon must be sought in the psychological events occurring on such a single trial, rather than in the processes underlying acquisition of the list over multiple trials.

The fact that serial position effects are obtained on a single trial, with fast input rates (183), and in general show marked similarities to curves produced by tachistoscopic presentation (69), suggests that it may be worth-while to consider the effects as a perceptual-memory phenomenon rather than a learning phenomenon. Morin et al. (108) found serial position effects for latencies of responding in a task in which the subjects "scanned" the memory trace of a serially presented set of four digits—a situation involving the barest minimum by way of "learning."

The similarity of one "serial position curve" to another, of course, is no guarantee that both are consequences of one and the same set of underlying processes. Serial position curves in a task requiring recall of a briefly flashed series of items, for instance, need not be produced by the same factors as those responsible for the serial position curve demonstrated in an interesting experiment by Slamecka (151). In Slamecka's study the effect was produced by output order rather than by input order: the first and last items recalled showed fewest errors, and their point of presentation did not matter. At least in some situations (47), the output order seems to be determined independently of the organization of the items in the store, reflecting characteristics of output processes as such. On the other hand, serial position curves can be demonstrated under conditions where the subject's retention of the material is tested by the recognition method (41), thus clearly implicating the properties of the stored information.

It is clear, to use the most popular phrase in the field of verbal learning and memory, that further research is needed on this problem.

A spate of studies concerned with order information in short-term memory has recently appeared. We would classify many of them as studies in perceptual-motor learning, to the probable consternation of their authors. The most conspicuous example is provided by studies that investigate the effect of acoustic and visual similarity of items on their ordered recall (72, 88). The effects of acoustic similarity, of interest to students of memory since Conrad's (44) influential paper, are thought to be relevant to the continuity-discontinuity problem: are the laws governing short-term memory the same as, or different from, those that describe long-term memory?

To the extent that experimental data generated in these short-term perceptual-motor experiments can be variably interpreted, thus creating "theoretical" issues, they may turn out to have some usefulness. The problem of whether acoustic similarity has its effects via audition or kinaesthesia (72, 88, 115, 177), for instance, appears to be one of those local skirmishes that will provide some excitement to the participants, even if it is not going to teach us much about basic issues in memory.

Verbal discrimination learning.—A special type of recognition memory task that has become increasingly popular over the past few years and has all the earmarks of a new "hot" area of research in verbal learning is verbal discrimination. Pairs of items are presented to the subject, one pair at a time; the subject pays attention or responds to both members of the pair, and then is told that one of the two items is "right." After all pairs of a list have been presented, subsequent trials follow the typical anticipation procedure: a pair of items is presented, the subject says which item was "right" and then receives informative feedback from the experimenter. Many variations on this general paradigm are possible.

The verbal discrimination paradigm has been around for some time. It was initially used as a method or technique for a variety of purposes. Recently, the means has become the end: the method for studying other phenomena was elevated to the status of something to be studied in its own right. An important impetus for this development was provided by the Frequency Theory of verbal-discrimination learning by Ekstrand, Wallace & Underwood (59).

The Frequency Theory of verbal discrimination is wrong, in the same sense that all extant theories in our field are wrong: ten years from now, or a hundred years, or a thousand years, students of memory will look at it in the same way as we regard the attempts of the ancient Greeks to explain the composition of the universe in terms of four basic elements. But at the present time, the Frequency Theory must be counted among the few genuine theories we have. It does explain data from a number of experiments, it does make specific predictions about outcomes of as yet undone experiments, it does deal with important fundamental processes in learning and memory, and it is specific enough so that it is capable of being proved wrong. Because of these outstanding characteristics that distinguish it from many other collections of speculations referred to as "theories," we predict that it will receive a lot of attention, will generate a lot of research, and will be hotly debated at least over the foreseeable future.

The basic propositions of the Frequency Theory are simple enough: (*a*) in the course of trial-by-trial events, each item in the list acquires a certain number of "frequency units"; (*b*) frequency units are added to each item when (i) it is perceived in the study phase of the trial, (ii) when it is pronounced in anticipation of the feedback, (iii) when it is rehearsed after the feedback, and (iv) when some other item that elicits the item in question as implicit association occurs in the list and is perceived, pronounced, or rehearsed; (*c*) the subject discriminates between "right" and "wrong" items in a given pair on the basis of their respective frequency units.

A number of predictions and postdictions fall neatly out of the small set of basic assumptions of the Frequency Theory:

1. When some items occur as members of two different pairs in the same list, the list can be learned faster if the repeated items are all "right" than if they are all "wrong." If a repeated item is correct in one pair and incorrect in the other pair, learning is most difficult. This prediction has clearly been confirmed (59). The findings are particularly interesting in that from the point of view of common sense, subjects should have no greater difficulty learning to say "wrong" to repeated items than they should in learning to say "right" to each repeated item. Thus, the prediction and related findings are counter-intuitive. It makes this type of research interesting and greatly limits the number of alternative applicable explanations.

2. If a high frequency associate of a "right" item appears in the list as the "right" item in another pair, discrimination learning of both is facilitated; if it occurs as the "wrong" item of the other pair, learning of both pairs suffers interference. Experimental evidence in support of these predictions has been described by Ekstrand, Wallace & Underwood (59).

3. Pronunciation of "right" items facilitates discrimination, while pronunciation of "wrong" items interferes with it. The first half of the prediction has been demonstrated as an empirical fact by Carmean & Weir (37) in an experiment designed independently, and submitted for publication prior to the appearance, of the Frequency Theory. Other evidence relevant to this prediction has been reported by Kausler & Sardello (80) and by Un-

derwood & Freund (164). The sheer magnitude of some of the effects of pronunciation should delight those researchers who want to work with large effects.

4. Verbal discrimination performance is facilitated if "right" items have acquired frequency units prior to the experimental task proper, and retarded if frequency of "wrong" items has been built up prior to the task. Prior frequency of "right" and "wrong" items can be manipulated in another verbal discrimination task or in the context of another paradigm such as free recall. In either case, the effect of the manipulation is exactly what the Frequency Theory predicts (164, 165).

Some findings slightly embarrassing to the theory, however, should be mentioned.

First, in transfer experiments in which incorrect items initially start with a larger number of frequency units than correct items—having accumulated them in a previous task—the performance curve should drop to chance level at the point where frequency units for the two classes of items are balanced in the transfer task. This should be observable at least in the learning curves for the transfer list of individual subjects, but it is not (164, 165).

Second, according to the original version of the Frequency Theory, subjects should not be able to master a double-function discrimination list: a list in which each item serves as a correct item in one pair and as an incorrect item in another pair. While learning of such a double function discrimination list is very difficult and proceeds extremely slowly, subjects do learn under these conditions (79). As long as one assumes that frequency units accrue to a word regardless of its context (that is, the other member of the pair), a mechanism other than frequency discrimination must be postulated to account for such discrimination of double function lists, however slow such learning is.

Third, the Frequency Theory has difficulty with data showing transfer effects for sets of "right" and "wrong" responses (125).

These kinds of findings may imply that mechanisms other than frequency discrimination of nominal list items may be involved, or they may mean that the theory is incomplete or inadequate. But it is safe to predict that as long as no alternative theory is developed to account for verbal discrimination phenomena, the Frequency Theory will rule supreme, accounting for most of the findings, running into difficulty with some data, and in general keeping a large number of researchers fully occupied.

Ekstrand, Wallace & Underwood (59) have pointed out that any alternative theory will above all have to be able to handle the critical finding of the large difference in learning of lists with repeated items which are both "right" or which are both "wrong," a finding which we discussed above. The next few years should show whether any theorist dares to pick up the gauntlet.

Sensory modalities and memory.—Early students of memory were quite aware of the possibly important influences exerted by sensory modality in which material was presented upon recall. They also knew that these comparisons would be inconclusive owing to the lack of experimental control over the subject's handling of the material. McGeoch pointed out that a subject "may straightaway translate material presented to one sense organ into terms of other modalities . . . The receptor is the starting point of the practiced response, but it is by no means its sole determiner" (100, p. 169).

McGeoch examined the available evidence and concluded that differences between modalities are usually small and irregular in direction, and that the sensory character of the material is one of the "unimportant determiners" of rate of learning. Such a pronouncement by the master must have discouraged his disciples and their descendants from showing any interest in possible differences in retention and recall of visually and auditorily presented materials. For a long time, college sophomores serving time in verbal learning laboratories were exposed to the material to be learned in whatever modality happened to be more convenient to the experimenter.

Thus it has come to pass that the variable of input modality has been largely ignored in recent and contemporary theoretical conceptions of learning and memory. Theories postulate a sensory buffer or a perceptual store through which incoming material passes into the short-term memory (6, 50), but once the relevant information is safely inside, its origin in the eyes or in the ears is assumed to make no difference in what happens to it thereafter. Nevertheless, several reliable observations have recently alerted psychologists to the importance of the distinction between auditory and visual inputs, and a small but growing body of knowledge now exists about modality effects in short-term and "very short-term" memory.

A series of experiments by Murdock (111–113) has demonstrated pervasive modality effects in a variety of short-term memory tasks. Auditory presentation produces performance superior to visual presentation for paired-associate and serial recall, recognition, and single-trial free recall. Some of the differences are enormous. In one experiment (111) involving serial recall of mixed modality lists, probing for an item presented auditorily with another auditory item yielded recall four times as high as did any other combination of probe and target modality.

Serial position analyses suggest that the auditory superiority tends to be limited to the last few items in a list—the last two or three pairs in paired-associate lists (111) or the last six or seven items in free recall. Serial recall with the probe technique has sometimes shown auditory superiority over all serial positions (111). Auditory presentation results in higher recall even if to-be-remembered items are spoken by spatially distributed speakers, when there is no systematic relation between speaker location and temporal order of presentation, and when the probe is the spatial location of the speaker (113). Thus the superiority of auditory presentation is not necessarily related to the fact that the auditory modality is temporally distributed.

If one assumes, as Murdock (112) has done, that recognition tests obviate the need for the "search" phase of retrieval, then the superior recall of auditory material in tests of recognition of order and list membership implicates storage differences as the locus of the modality effect. These recognition data are of additional interest in that they indicate that auditory and visual inputs are equally well encoded or registered: modality had no effect on recognition of the last three items in a list. A similar lack of differences can be found for the last pair in paired-associate recall as well.

Auditory inputs seem more resistant to output interference than do visual inputs. The marked recency effect in digit-span tests under conditions when the subject pronounces visually presented items during input all but disappears under conditions of no pronunciation (45). This pronounced-visual-input task has also been shown to produce an instance of visual superiority: Ellis (60) found that recall from the last few positions in a list was superior when subjects read items aloud during presentation, but inferior to "silent" conditions when probes were for items in earlier parts of lists.

The disappearance or reversal in direction of modality effects when recall is from secondary memory constitutes a stumbling block for a number of possible explanations of modality effects. If auditory presentation is more effective than visual presentation because the short-term store holds only auditory information, or because auditory presentation ensures entry into a short-term processing buffer (23), then there should be modality differences in secondary memory as well. In a task such as single-trial free recall, all items "pass through" the short-term store, and if they reside in that store longer when presentation is auditory, or if the rehearsal buffer is more capacious for auditory inputs, then more information should be transferred to the long-term store for auditory items.

The notion that short-term memory might be an "acoustic store" has received quite an airing in recent work. While theorists are willing to entertain the possibility that initial storage of information can be either auditory, visual, or both, it has become most fashionable to assume that "the next level of processing"—in short-term memory proper—is exclusively auditory in nature.

Sperling was first to advocate the view that visual input is translated into an auditory analogue (154, 155). The idea received a great boost from observations that short-term memory for a series of acoustically similar items was impaired when compared with acoustically dissimilar items, regardless of whether the input modality was auditory or visual (72, 114).

Since students of memory are as quick on the draw when it comes to generalizing the findings as are any other psychologists, no one has worried too much about the boundary conditions under which this type of apparent translation takes place, and consequently it is very easy to talk oneself into believing that the initial encoding of verbal input is always auditory. The natural thing to do next is to analyze the auditory mode of presentation of material into finer components, such as acoustic and articulatory features (72, 177), and to extend the translation one more step. Despite this peculiar type of reductionism, we know that much of the mnemonic information human beings possess must be stored in some form analogous to visual images. Recognition of faces and familiar scenes proceeds without any apparent verbal mediation (51, 73, 145), and the storage of spatial information in short-term memory tasks (107, 142) could not possibly be handled by synesthesia alone. More generally, we fail to appreciate the logic—or perhaps intuition—involved in equating experimentally demonstrable modality effects with the properties of stored information.

Very little has been said or done about the modality effects in terms of the characteristics of auditory and visual signals per se, or about non-memorial aspects of processing of the two kinds of information. An outstanding exception is Savin's (143) examination of processing simultaneous auditory stimuli. We like this study because it is one that should have been done by those concerned with sensory channels and attention very soon after Broadbent's book (26) appeared, but was apparently overlooked. What Savin found was that the tendency to report one message and then the other has nothing particularly to do with sensory channels and attention-switching; he obtained the "channel-by-channel" reporting of simultaneous inputs when there was no channel distinction possible. His conclusion may prove to be important for modality effects in memory: the auditory system groups successive rather than simultaneous stimuli.

Why have we devoted so much space to the modality effects? The main reason is that they necessitate the rethinking of many problems and the rewriting of many theories. Auditory and visual information may be processed quite differently, and some phenomena may be entirely peculiar to one or the other modality. Understanding these differences seems to be a partial prerequisite to understanding human memory.

Storage and Retrieval

In the same way as storage of presented information depends on factors other than those prevailing at the time of the acquisition, retrieval of information from the memory store depends on things other than the contents of the store. In this section we will consider matters that are at least to some extent relevant to the interrelation between storage and retrieval, or to the ecphoric processes proper.

In order to understand retrieval processes, some basic principles must first be accepted. One of the most important of these was formulated by St. Augustine more than 1500 years ago: we cannot seek in our memory for anything of which we have no sort of recollection; by seeking something in our memory, "we declare, by that very act, that we have not altogether forgotten it; we still hold of it, as it were, a part, and by this part, which we hold, we seek that which we do not hold" (68, p. 442). Another principle, also known to the scholars of antiquity, says that the failure of retrieval does not necessarily imply the lack of appropriate information in the memory store.

Both principles are being continually overlooked by some researchers and theorists. A naive observer of the contemporary scene might assume that 1500 years is long enough to try to figure out exactly what it is that one has not forgotten about some previous experience, such as a word or some other verbal item in a memory task, and exactly what are the characteristics of the "part which we hold and by which we seek that which we do not hold." The successful evasion of these apparently fundamental problems by students of memory and verbal learning must be considered a ringing testimony to their ingenuity.

Part of the blame for this peculiar state of affairs must be laid to the chaining notion of associations. Associationistic pretheoretical assumptions preclude raising questions about recall mechanisms: after all, is it not a self-evident truth that recall is a function of a stimulus with which the response to be recalled is associated?

The changing conceptions of memory have not remedied the shortcoming. Instead of taking the knowledge of the "part which we hold and by which we seek that which we do not hold" for granted, as the traditional associationists do, most students of memory simply ignore the necessity of postulating such a "part." Instead, retrieval is thought of as consisting of two basic subprocesses, search and decision. A search and scanning process somehow finds access to stored information of all kinds, and a decision mechanism compares some property of the "found" information, such as its "familiarity" or its "list tag," with a hypothetical entity called the "criterion." Depending upon the results of such a comparison, the information is used to produce a corresponding response or is rejected, whereupon the search process continues until the desired information is found and identified as such, or when it grinds to a halt for as yet unknown reasons.

Despite the rather discouraging picture reflected by the inadequate handling of retrieval processes, some research seems to be gradually zeroing in on relevant issues. The impetus for it has come from the study of phenomena conveniently subsumed under the category of "coding processes."

Coding of individual items.—Underwood & Erlebacher define coding as "the changes, transformations, additions, subtractions, adumbrations, and so on which occur to and between verbal units as presented and which are, we assume, reflected in what is stored as memory" (162, p. 1). The definition is quite broad, and a reader who fixates too long on "subtractions" may become confused, but it is obviously better than no attempt at definition at all, a practice that characterizes the work of many authors who otherwise use the term. Underwood & Erlebacher's definition implies that a difference may exist between a physical stimulus presented to the subject for memorization and the trace, engram, information, or association that is laid down in the memory store. Differential covert responses that subjects are assumed to make to verbal items, implicit associative responses, mediating responses, and other like terms are synonyms for "coding operations."

Two different types of coding must be distinguished. We will refer to these as substitution and elaboration coding. Substitution coding refers to the replacement of the input stimulus by another symbol, the code, together with a general "decoding" rule—general in the sense that it might apply to many other symbols as well. The encoded version of the stimulus and the decoding rule are completely sufficient for the reproduction of the original stimulus. Translation of octal into binary digits, the unitization of "dits" and "dahs" of a telegraphic message into letters, words, or even longer phrases, and various mnemonic systems specifying a fixed correspondence between digits and letters represent examples of coding operations in which the to-be-remembered stimulus is completely redundant with its substitute code. Replacement of anagrams on the study sheet with their word-solutions in memory also qualifies as an instance of substitution coding, although the success of such coding in facilitating the performance on the memory task depends on how well various decoding rules are remembered (162).

Elaboration coding refers to the storage of additional nonredundant information with the verbal unit to be remembered. Such auxiliary information, if present at the time of attempted retrieval, may facilitate the reproduction of the verbal item to be remembered, but its exact copy in absence of original encoding is not sufficient for such production. Nor is it usually possible to state a general rule which permits the decoding of the coded information into the originally presented stimulus. Elaboration coding thus differs from substitution coding in important ways.

When subjects remember the serial position of an item in the free-recall task (187), when they use initial letters of words to retrieve these words (58), when their behavior at the time of recall suggests that implicit associative responses were elicited by input items at the time of presentation (67), or when the subject remembers that a word such as CLEVER was paired with a trigram VEC in the paired-associate list, to give but a few examples of an infinitely large number of possibilities, we have evidence for elaboration coding.

Wickens (178) has summarized many experiments that have convincingly demonstrated the complexity and variety of coding operations that take place in apparently straightforward memory tasks. In these experiments, subjects are first tested with a series of trigrams from some single class of materials, under the conditions of the short-term memory distracter task originally used by the Petersons. Investigators partial to the interference theory assume that over successive tests proactive inhibition is "built up," since recall of trigrams drops over the first few trials before reaching a stable asymptote. If on a given trial the type of material is changed, recall of this new "class" of material abruptly rises. This finding is referred to as "release from proactive inhibition."

The class changes sufficient for the production of the "release" effect are many and varied. They include changes of conceptual category, connotative meaning, input modality, physical characteristics of items, mode of presentation, and the like. As yet, no theory can predict what kind of changes will produce release, or how much release. Grammatical class change, for instance, is ineffective (179) while changes in word length do produce the release effect (178). Data of this kind, as Wickens points out, make it quite clear that in many short-term memory tasks subjects store much more information about verbal materials than is apparent in standard methods of assessment, and that they do so quickly and efficiently without any prodding by the experimenter.

Temporal coding.—Almost any experiment in verbal learning and memory requires the presentation of a series of events distributed over some temporal interval. Given the complex nature of coding operations indicated by many studies, it would be most surprising if coding of items to be remembered did not include information about the temporal date of the occurrence of an item. Storage of information about this "temporal" date—or the "time tag" of an item (184)—is referred to as temporal coding. We devote a separate short section to it since we believe that understanding of temporal coding processes is an extremely important step towards understanding of memory.

Research on temporal coding is just beginning. The first experiments are understandably directed at the question of whether such temporal coding in fact occurs. In search of the answer to this question, Peterson (127, 129), Hinrichs & Buschke (71), Morton (109), and Lockhart (89) have studied judgments of recency by forced-choice and absolute judgment procedures. In these experiments, the subject is shown a series of items (words, pictures, numbers) and at given points in the presentation sequence is required to estimate how recently a particular item occurred in the preceding series, or to choose the more recent of two previously shown items.

What is the mechanism of recency judgments of this type? The only serious explanation offered so far is the "strength hypothesis": recency of an item is judged on the basis of its trace strength (109, 129). In our opinion, the strength hypothesis is a product of desperation. It is entirely possible that in absence of any other relevant information the subject may, correctly or incorrectly, reason that of the two items the one appearing more familiar may look so because it occurred more recently, but this does not mean that the subject has no access to more direct information about the temporal code of an item in many other situations.

We suggest the following simple experiment to those who believe in the strength hypothesis. Present to the subject a series of items, one of which is the subject's own name or some other very conspicuous item. Test many subjects for recall of all the items from such series. All subjects will remember seeing their own name. Hence, the trace strength of the name must be very high, possibly higher than that of any other item. Then ask the subjects to estimate the recency of various items, including the name. If the subjects say that the name was the last item in the series, please write to us, and to *Science* or *Nature*. If the subjects are reasonably accurate in their recency judgments about the name, do some more thinking about the strength hypothesis.

We have not done such an experiment nor do we plan to: the outcome appears to be too obvious. Other variations on this theme may be more interesting. Memory for the temporal order of two highly conspicuous adjacent or closely separated items—say the subject's own name and that of his or her sweetheart—might be investigated with profit.

There are additional difficulties with the strength hypothesis, some of which may stem from the lack of clarity of the trace strength notion itself. For example, the flat portion of the serial position curve in single-trial free recall can in some sense be taken as indicative of equal trace strength for all items in these positions. According to the simple strength hypothesis, the subject should not be able to discriminate the temporal recency of these items, but we already know that discrimination of recency varies positively with the intraserial distance between the two items (71, 184).

Temporal coding appears to be relevant to the question of how subjects keep apart successive lists of similar items. Winograd (180, 181) has suggested that the list differentiation process might proceed through the storage of "time tags along with relevant item information." This interpretation of list discrimination is a step in the development of an explanation of the processes described—and only described—by the "selector mechanism" (167) and the classical concept of list discrimination.

Bower (16) has suggested that "time tags" allow a subject to select only the most recently presented items from a crowded store of items otherwise tagged as having been presented in the context of the experiment. Temporal coding and temporal distinctiveness have also been invoked to account for the reduction in proactive inhibition with spaced acquisition trials on the interfering material (163), and the notion may be useful in understanding the dissipation of proactive inhibition with increasing intertrial intervals in short-term memory tasks (91).

Zimmerman & Underwood (187) found that subjects could recall temporal features of input in free recall even when they were not specifically instructed to do so. Subjects instructed to remember temporal information in the experiment recalled as many items as those instructed to remember items only. This finding suggests that temporal information about items is processed without any apparent "cost" to the system: a temporal code about each item is part and parcel of what the subject stores about the material to be remembered even under instructions to process only item information.

The issue of temporal coding is closely related to the whole question of memory for serial order. Tasks requiring subjects to make recency judgments about items appear to be the short-term discrete analogues of the much-neglected method of reconstruction. Bryden (31) has proposed a model of serial recall which is independent of item-to-item associations and according to which recall is guided solely by stored temporal information about items. There is also an obvious relationship between absolute judgments of recency (127) and the "reverse probe" technique (5): given an item from a short series, the subject must indicate its serial position.

Systematic study of temporal coding in all its aspects and in its relation to many problems of memory deserves much more emphasis than it has received so far. At the present time, experimental studies are hard to find in the literature, theoretical concepts are still ingenuous, and the whole approach to the issue is characterized by hesitation and reluctance. It is as if no one knew exactly what to do next. And yet, as shown by Peterson (127) among others, the literature on verbal learning and memory is replete with case histories demonstrating the profound importance that temporal discrimination and temporal coding of events and their order plays in determining subjects' performance in retention and forgetting of verbal material.

Cued and noncued recall.—The Latin-speaking sage who first proposed that *"ex nihilo nihil fit"* probably made the observation when pondering about retrieval processes. He must have realized, as did St. Augustine much later, that if a person is not, in some as yet unspecified sense, "aware" of certain characteristics of an event or item stored earlier, he possibly could not retrieve that item from the store. These characteristics, when present in the subject's active memory at the time of attempted retrieval, constitute the retrieval cues.

All retrieval of information from the memory store is cued. The distinction between cued and noncued recall (8, 48, 56, 57, 75, 78), strictly speaking, refers to the specificity of cues that are present at the time of attempted retrieval, unless the terms are used to describe experimental operations. Free recall, for instance, can be thought of as "noncued." But this usage of the term only illustrates our present ignorance as to what constitute the cues for retrieval in this situation. Luckily for the experimenter, the subject usually understands what the experimenter wants him to do when he is told to recall the words from the "most recent" or "the first" list.

Effectiveness of retrieval cues.—While there are many questions one can pose about retrieval processes, one which deserves highest priority at the present time, in our opinion, has to do with the effectiveness of retrieval cues. One need not do any experiments to know that (*a*) many kinds of cues are effective in getting the subject to recall the desired item or items, and (*b*) that large variations exist in the effectiveness of different kinds of retrieval cues. Exactly what determines the effectiveness of retrieval cues?

One thing with which few theorists would probably disagree is the belief that efficiency of retrieval cues depends strongly upon the nature of coding operations that have taken place at the time of the storage of the material to be remembered. A necessary condition of the effectiveness of a retrieval cue seems to be the correspondence between it and some part of the auxiliary information stored with the stimulus item. Another factor determining the efficiency of a retrieval cue is the number of items for which it serves as the cue (57). A third variable shown to be highly relevant is the strength of the pre-experimental association between the cue and the stored item (8). But apart from these rather obvious facts, relatively little is known about the conditions determining the nature and efficiency of retrieval cues.

Slamecka seems to be the only theorist so far who believes that retrieval processes are at least partially independent of the format of stored information. On the basis of an experiment showing that part of a free-recall list does not facilitate the recall of the other part—reminiscent of a similar finding under somewhat different conditions by Brown (28)—Slamecka proposed a dual-component hypothesis according to which "perception of the general list structure forms the basis for a retrieval plan which then operates upon the independently stored traces" (152, p. 504). This notion appears implausible to us, for reasons into which we cannot go in this chapter, but it does represent a novel way of thinking about the relation between storage and retrieval.

Temporal coding in retrieval.—An extremely important question, in our opinion, concerns the role that temporal coding plays in effecting retrieval. Intuitively it is obvious that availability of information about the temporal date of the occurrence of an item is an absolutely necessary condition for the recall or recognition of the item as a member of a particular collection of other items, but few experiments have been directed at this question. Brelsford, Freund & Rundus (24) had subjects both judge the recency of items and recall them in a continuous short-term memory task with paired associates. They found that the accuracy of recency judgment was highly contingent upon correct responding on probe recall tests. Even when a tested item was well within the range of the typical recency effect, judgment of temporal position was quite inaccurate if the subject could not respond correctly on that paired-associate test.

Because of the correlational nature of these data it is difficult to interpret them, but an obvious possibility is that recall of a pair of items in this situation is mediated by reasonably precise and readily accessible temporal codes of both stimulus and response members of a pair, rather than, or in addition to, being determined by a "direct association" between the two.

Recognition memory.—An upsurge of interest in recognition memory, apparent in recent literature, serves as yet another symptom of changing times. The pretheoretical assumptions underlying the stimulus-response associative approach to problems of verbal learning always made those who followed those assumptions wary when they were faced with phenomena of recognition memory. Nothing is more admirable, from a mechanistic point of view, than a stimulus eliciting an associated response. But a subject who observes a test item in a recognition memory task and tells the experimenter that he "remembers" seeing that item in the earlier study list, that the test item "looks familiar," or that he is quite "confident" that the stimulus is an "old" one, even if he communicates these messages to the experimenter by pushing a lifeless button, conjures up images of introspecting homunculi sitting in the middle of the head, deeply absorbed in thought. Under the circumstances, by far the best action indicated was to steer clear of recognition memory, and a study of the literature over the past few decades proves that this is exactly what students of verbal learning did.

Now it appears that recognition memory is here to stay. To those who despair of the limited capacity shown in some tests of memory, studies in recognition memory offer the solace of impressive levels of performance. Shepard's (145) subjects looked through a series of approximately 600 stimuli—words, sentences, or pictures—and correctly recognized "old" stimuli 90, 88, or 98 per cent of the time, for the three classes of stimuli respectively.

To those who just do not have the heart to administer 120 paired-associate trials to their subjects (66), or who otherwise hesitate to bore them to tears with rote learning of nonsense syllables, recognition memory experiments also offer a way out. One can do these experiments with pictures as items to be remembered, and one can even blur them or turn them upside down. This sounds like fun! But such experiments also provide instructive data. For instance, degradation does not seem to affect recognizability of pictures too much if it occurs at both the input and test, but degradation at only one point does lead to impairment in performance (51).

Students of memory who are on the lookout for interesting puzzles find them in recognition memory. Two perennials remain as mysterious as ever: first, why does recognition performance deteriorate with the increase in the proportion of "lures," that is "new" items (30, 84, 150); and second, why are unfamiliar or less frequently occurring verbal items recognized more easily than familiar or frequently occurring items (2, 144, 145)? Although it is possible to treat the first finding as a self-evident truth not requiring any explanation, the challenge of explaining the phenomenon by relating it to other known facts about memory remains. The second finding is also successfully resisting attempts at its conceptual clarification.

A new puzzle for those who have already tried the other ones is to figure out whether "organization" of the learning material has an effect on recognition memory or not. Everyone agrees that appropriate organization facilitates recall, but its role in recognition performance is unclear. Some investigators find that organization does not affect recognition (18, 85), while others find that it does (17, 97). Mandler et al. (97) have proposed the notion of a "postrecognition retrieval check" to account for their finding of the correlation between degree of categorization of material and recognition performance. The notion sounds intriguing, but since we do not understand it, we prefer to classify Mandler et al. as part of the puzzle.

Measurement of retention by recognition procedures provides interesting insights into the format of storage of materials such as connected discourse. Experiments show that subjects have excellent memory for the meaning of sentences, and rather poor memory for the exact lexical units and their grammatical arrangement (131, 141). When words are presented to the subjects one at a time, however, subjects are no more seduced by synonym lures than they are by unrelated distractors (97). This finding suggests a rather fine discrimination of meaning.

Signal detection analysis.—Recognition memory also provides good research opportunities for those students who like exotic quantitative techniques that work in other fields. We refer here to the new fad of the signal detection analysis. The theory of signal detection, in our opinion, will rank with the invention of the memory drum and the paired-associate list-learning method in changing the course of history in our field. Whether the change will be for good or for bad remains to be seen. We think it will have some meritorious effects: it will stifle wild fantasy, put a clamp on silly speculation, and keep many people from raising new and therefore irrelevant questions about recognition memory. It is so much easier to explain experimental data by talking about differences in sensitivity, criterion, or both, than to admit the existence of naughty problems!

Given the choice—and we are afraid that we will have none—we would take information about hits and false alarms any day in preference to ROC (Receiver Operating Characteristic) or MOC (Memory Operating Characteristic) curves with slopes of less than unity (2, 55, 144, 176, 186). To the extent that the slopes of ROC curves depart from unity, measures of sensitivity and criterion are not independent. Lack of independence between the two measures removes the only possible reason for using signal detection measures. What is wrong with forced-choice methods?

The section on recognition memory would be incomplete without mentioning an extremely interesting finding reported by Cofer et al. (43). Cofer and his associates showed their subjects nouns in presence or absence of adjectival modifiers and tested subjects' recognition memory for the nouns (Exp. VII). Recognition was considerably higher for the "nouns alone" than for the "modified nouns" condition. To us this finding signals the importance of retrieval problems in recognition memory: available information is not automatically accessible when an "old" test item is presented.

When Memory Fails

When a person recalls or recognizes something now, but does not on a subsequent occasion, forgetting is said to have occurred. Understanding of the mechanisms and processes underlying forgetting in its infinitely many forms ranks among the major objectives of students of ecphoric processes. In the following short section that we can devote to the topic, we will concentrate on theoretical problems and issues.

Forgetting in short-term memory.—Most current models of short-term memory postulate two separate storage mechanisms, a short-term store and a long-term store (6, 63, 140, 174). The two-store notions usually hold that (*a*) all the material presented under typical conditions enters the short-term store, and (*b*) some of this material is transferred into the long-term store (6, 174). This means that some items accessible in the short-term store now cannot be recalled later. What do contemporary thinkers say about this type of forgetting?

The most popular position at the present time appears to be what George Miller called the "leaky bucket" hypothesis: as items enter the short-term store, a point is reached at which old items "leak out" as fast as new ones are put in, or existing memory traces fade away as fast as new ones are created (106). The modern versions of this hypothesis are couched in more "scientific" terms, but the basic conception is the same. The short-term store is assumed to have a limited capacity for holding information, and once that capacity is reached, further incoming items displace those currently in the short-term store according to some probabilistic schedule.

Research concerned with the properties of such a displacement mechanism has barely begun. It has been known for a few years that some 15 to 30 sec of interpolated "neutral" activity, such as counting numbers backwards, "empties" the short-term store (63, 135). Results of recent research by Glanzer et al. (64) now permit the short-term store to be "emptied" of all the material contained in it in two seconds flat. We leave it as an exercise to the reader to figure out how this could be done. If he fails he can consult the interesting paper by Glanzer et al.

If the interpolated material contains items already in the short-term store, it produces less displacement than does material completely different from that in the store (175). This finding implies, if one believes in the existence of the short-term store, a comparison of the incoming information with that contained in the short-term store prior to the placement of the incoming information into the store. Depending upon the outcome of this comparison, redundant information is not entered into the store or is entered into the same "slot" in which its copies already exist.

Most theorists agree on the nature of the mechanisms that save the material in the short-term store from being "lost" or forgotten: transfer of information from the short-term store into the long-term store is a consequence of an active process called "rehearsal" (6, 174). Some rather ingenious techniques have been used by investigators such as Peterson (128), Rabbitt (138), and Crowder (49) in support of the reality of rehearsal-like processes responsible for converting information in the short-term store into a more permanent trace. The next important step to be taken should be the working out of the characteristics and properties of the rehearsal process. To say that rehearsal provides a vehicle for the transfer of information from short-term into long-term memory defines an interesting problem, but obviously does not solve it.

Decay theory.—The attempts to revive the ancient notion of decay of memory traces and to make use of it in contemporary theoretical thinking about forgetting seem to have run out of steam. The British psychologists who advocated the decay notion—Broadbent, Brown, and Conrad—have not had much to add to their earlier writings, and American psychologists who briefly flirted with the notion have given it up, unless one identifies displacement with decay. Most experts, however, think of displacement as more akin to interference than to decay (174).

Some remnants of decay theory of forgetting, however, are found in various disguises in theoretical accounts of recognition memory. Theorists such as Bower (17), Kintsch (85), and Murdock (112) all assume that recognition tests eliminate the problem of accessibility of stored traces: the test stimulus somehow automatically—presumably because it is nominally equivalent with an earlier item—provides access to the stored trace. Given this assumption, forgetting in recognition memory, that is failure to recognize an "old" test item as "old," must be attributable to some kind of deterioration or modification of the stored trace.

In our opinion, the advantages of the postulation of "automatic access" to the stored trace in recognition memory tasks are the same as the advantages of theft over honest toil, to borrow an analogy from Bertrand Russell as quoted by Stevens (157, p. 14). We can only ascribe the automatic access assumption to the corrupting influence or abuse of the theory of signal detection, and hope that the record will soon be set straight.

Interference theory.—At a major verbal learning conference held in 1959 (40), attended by a dozen or so most prominent investigators, Postman began his review of the state of affairs with respect to interference theory by stating that, "Interference theory occupies an unchallenged position as the major significant analysis of the process of forgetting" (132, p. 152). Now, barely 10 years later, the theory lies in a state of ferment if not chaos.

The analysis and description of events bridging Postman's optimistic assessment of the health of the theory in 1959 and the present confusion would make for fascinating reading to anyone who has a bit of the sociologist of science in him. Those who are impatient with the slow pace at which our knowledge of the phenomena of memory expands would also find food for thought from a close study of the recent history of the interference theory. The extinction-recovery version of the interference theory (81, 101, 132, 166) that indeed ruled the domain of the study of forgetting without challenge since its inception in 1940 (105) has for all practical intents and purposes been relegated to the attic full of past curiosities.

The handwriting had been on the wall for some time. The Underwood-Postman theory of extra-experimental forgetting (166) had run into great resistance from the data. Theoretical expectations about laboratory-produced retroactive and proactive interference also rather regularly came out second best. While some of the loyal adherents of the theory either tried to explain away the discrepancies between the data and theory, or to patch up the theory by adding little bits and pieces where needed, "neutral" observers had felt for some time that the demise of the theory was simply a matter of time.

The historic moment came when Postman delivered his vice-presidential address to a symposium at the meeting of the American Association for the

Advancement of Science in New York, on the last day of 1967. The substance of his comments was subsequently published in two papers of Postman co-authored by Stark & Fraser (137) and Stark (136). Those who are familiar with the interference theory and who read and study these two papers in detail will realize what an impact the two papers will have on future experimentation and theorizing.

Knowing full well that any summary of the rather unsettled state of affairs can only distort and confuse the picture even further, we offer what we understand to be the situation now. Unlearning is still a major concept in the theory but it has changed its character drastically. Rather than referring to the extinction of both specific (stimulus-term and response-term) and general (experimental context and specific response terms) associations, it is now envisaged as a kind of suppression of the whole first-list repertoire of responses in the course of second-list learning. During learning of the first list, the subject limits his response selection to those occurring in that list. When he comes to learn the second list containing different responses, new "criteria of selective arousal" must be established. These criteria require the suppression of the first-list repertoire. When the subject is asked, immediately after learning the second list, to recall the first list, the selector mechanism cannot shift back to the criteria used during first-list acquisition because of its "inertia." With passage of time, however, the set to give second-list responses dissipates, resulting in the lifting of the suppression of the first-list responses and consequent observable "spontaneous recovery."

This formulation by Postman and his associates does not represent a new theory, but rather only some initial guidelines for the development of a new theory. But even in its tentative form, it is radically different from the old theory. The emphasis on response repertoires—rather than on specific associations—the central position of the concept of the selector mechanism, the postulated relation between the scanning process and criteria of selection, and the elevation of the concept of "generalized response competition" (118) to a position of dominance have drastically changed the nature of the theory.

It is far too early to tell exactly what is going to happen next. In the present confusion it is difficult to see the forest for the trees. We suspect it will be several years before the new theory will acquire clearly identifiable properties and characteristics. In the meantime, interference phenomena will become fair game for heretics such as Slamecka (153), who has raised some interesting questions of fundamental importance about some of the laboratory rituals developed over the years, such as the use of multiple lists, in studying retroactive and proactive phenomena.

The current revolution in the domain of interference theory contains two important lessons to be heeded by all researchers and theorists. First, the fact that the theory was found wanting testifies to its truly singular position in our field. Very few collections of speculations politely referred to as theories have been specified precisely enough so that they could be proved wrong. The specificity of the extinction-recovery version of the interference theory was its major strength. By all means, let us have more theories that can be proved wrong. Therein lies our only hope for progress.

The second lesson to be learned from the story of interference theory is a bit more embarrassing. Why did it take so long for the theory to be found out? At the same conference in 1959 at which Postman declared the interference theory to occupy "an unchallenged position," Melton (102) explicitly and in no uncertain terms pointed to two kinds of critical evidence necessary for the acceptance of the unlearning of specific associations as the major mechanism of interference. First, Melton wanted to have evidence about the correlation between the unavailability of specific first-list pairs following second-list learning and frequency of nonreinforced intrusions of the same specific responses during the second-list learning. Secondly, he wanted to have evidence that in a situation in which an A-B list is followed by learning of A-C and C-D pairs in a mixed list, items given the A-C treatment in the interpolated list would be significantly less available in the retention test than the items not represented in the interpolated list.

Why was the evidence Melton wanted in 1959 not produced in 1960 or 1961? Ceraso (38, 39) did report experiments of this kind in 1964 and 1968, with results largely at variance with those expected on the basis of the notion of unlearning of specific A-B associations, but for reasons beyond our ken, nobody apparently paid much attention to them.

Melton made some other rather interesting observations at the 1959 conference. When Postman discussed the notion of generalized response competition that now has assumed central importance in the "new look" of the interference theory, Melton said that there seemed to be a "strong commonality" between the finding of unlearning of the first list in absence of any specific formal or meaningful similarity to the second list on the one hand, and "the problems of mental set and shift" on the other hand. Melton may have seen more than 10 years into the future when he added that "an independently useful contribution to the science of memory phenomena will come from systematic pursuit of the concept of generalized response interference with this commonality in mind" (102, p. 186).

Trace-dependent and cue-dependent forgetting.—The currently popular conceptualizations of the forgetting process, be they interference, displacement, or decay, completely ignore and have nothing to say about a very common type of forgetting: the information sought for is available in the memory store, but is inaccessible at a given time because of inadequate retrieval cues. If a subject cannot recall a particular item in the absence of specific retrieval cues, but can do so if the experimenter gives him a part of the missing information (8, 48, 56), the apparent forgetting under the noncued recall conditions cannot be attributed to unlearning, displacement, or decay of the association or trace, nor to competition, but must be understood in terms of the presence or absence of appropriate retrieval cues.

It would be useful, therefore, to draw a distinction between trace-dependent and cue-dependent forgetting, and to evaluate extant theories of forgetting in the light of the distinction. Trace-dependent forgetting is a function of processes that have been described in the past by terms such as unlearning, displacement, and decay. Cue-dependent forgetting is attributable to the changes in the quantity and quality of retrieval cues between two retention tests separated in time. This distinction is an old one (100, 103), and in our opinion it is high time that investigators and theorists start paying attention to its implications.

Forgetting in most situations probably represents some combination of changes in the nature of stored information with a retention interval and the lack of appropriate retrieval cues that otherwise would provide access to the information. An important experimental task is to work out the relative contribution of these two sources of forgetting in any given task. This whole approach might be useful also for testing the most fundamental assumption underlying interference theory, namely that the retroactive and proactive designs used in the laboratory merely accelerate the rate of forgetting that would "normally" occur outside the laboratory (81, 132). We may find that retroaction designs produce a different kind of forgetting altogether, different in the sense that the contribution from the two sources—changed memory traces and changed retrieval cues—differs from that characterizing forgetting outside the laboratory.

Theories of Memory

Among the important events that took place during the period under review was the emergence of attempts at the construction of "comprehensive" theories of memory. The precursor of these theories was Broadbent's (26) sketch of a memory system composed of a short-term store, a limited capacity channel, and a "store of conditional probabilities of past events." It has taken 10 years since this modest beginning for the first relatively full-fledged conceptual account of human memory to appear (6).

Despite the frequent claims to the contrary, researchers in verbal learning never shunned theory completely. There was a brief period in the 1950s when analysis of variance tables in the Results sections were accompanied by explanations in the Discussion sections as to the reasons for using one or another error term, as there are papers today in which the Discussion sections are empty, but by and large researchers in verbal learning have been as much concerned with the deeper meaning of their findings as have been psychologists in other fields. The earlier attempts to make more general sense out of experimental findings produced theories concerned with specific phenomena: serial position curve, reminiscence, paired-associate learning, transfer, retroactive inhibition, intralist similarity, distributed practice, and the like. A general theory of verbal learning, however, was never attempted.

Students of memory, on the other hand, have shown no inhibitions against grandiose speculation under the guise of general theories of memory. A variety of more or less general and more or less ambitious theories have already made their appearance (6, 146, 155, 174), and at the time of this writing several additional ones are in press. A book edited by Norman and entitled *Models of Memory* (121) contains chapters by Norman & Rumelhart, Shiffrin, Bernbach, Morton, Kintsch, and Wickelgren, each of which strikes out in the direction of a comprehensive theory of memory.

To this list we must also add theoretical papers by Bower (16), Crowder & Morton (50), Mandler (92), Neisser (117), and Norman (120). All these papers—in Neisser's case the final chapter of his book—deal with theoretical issues of memory, although they do not aspire to the status of a complete system like the papers in the previous category.

Most of the "comprehensive" conceptual schemes have been proposed by psychologists who would not mind being called mathematical modelers. Most distinguish between a general theoretical framework and specific models that relate the general framework to experimental facts. Many, but not all, represent what we will refer to as "boxes-in-the-head" type of theory of memory: the memory system is thought of as consisting of several different storage "compartments," with mnemonic information being transferred from one compartment to another.

The number of similar but different theories seems to be multiplying at an accelerated rate, and they all vie for the attention of potential adherents. In our more pessimistic moments we have visions of hundreds of different theories of memory, say around 1975, being subjected to factor analysis and discriminated from one another in terms of weights on different major factors.

Because of limitations of space, we can say only a few things about what we believe to be the most ambitious and most highly developed theory, namely that proposed by Atkinson & Shiffrin.

Atkinson and Shiffrin theory.—The theory of Atkinson & Shiffrin (6, but see also 4, 5, 23, 25, 146) in many ways represents the distilled wisdom of many contemporary thinkers who have been imprinted on the electronic computer. It is the most enterprising, comprehensive, and detailed account of human memory seen from the information processing point of view.

The structure of memory is held to consist of three major components: the sensory register, the short-term store, and the long-term store. Incoming sensory information enters the sensory register, is "lost" from it or transferred into short-term store, from which it is also either "lost" or transferred (copied) into the long-term store. The description of such a structure consists of the specification of the characteristics and capabilities of different components of the system as well as the specification of the nature of the mechanisms underlying the loss of information from various components and their transfer from one part of the system to another. It is complemented by a description of "control processes," flexible aspects of the system controlled by the subject.

In most verbal learning and memory experiments, the sensory buffer can be ignored, since all incoming information in typical experiments readily reaches the short-term store. An item such as a number-letter pair that enters the short-term store can always be recalled if it is tested immediately after its presentation, but it decays in a few seconds unless it is placed into the "rehearsal buffer" which constitutes the essential part of the short-term store. An item in the buffer is transferred from the buffer into the long-term store with a probability that is proportional to the length of its stay in the buffer, and it is displaced from the buffer by other incoming items also on a probabilistic basis.

The long-term store serves primarily two functions: it is used to identify stimuli in the sensory buffer so that meaningful representations of these stimuli can be entered into the short-term store, and it serves as a storage "space" for information copied into it from the short-term store.

An item can always be recalled from the short-term store as long as it stays in the rehearsal buffer. The success of retrieval of items from the long-term store depends upon the efficacy of the "search process" which is made along some dimension or on the basis of some available cues. Atkinson & Shiffrin have little to say about retrieval—although it is said to be the "crux" of the theory—other than to offer names for some putative components of retrieval: search, recovery, and response generation.

The fate of the information copied into the long-term store from the short-term store depends upon the mechanisms responsible for transfer. Some information can enter the long-term store as a consequence of rehearsal of that information in the buffer. In that case the information transferred into the long-term store "would be in a relatively weak state and easily subject to interference" (6, p. 150). Alternatively, rehearsal operations may be replaced with various "coding operations which will increase the strength of the stored information" (6, p. 115). A coding process is thought of as a "select alteration and/or addition to the information in the short-term store as the result of a search of the long-term store" (6, p. 115).

Atkinson & Shiffrin's theoretical framework for human memory is, as they themselves put it, "extremely general." As such it can be easily criticized on many counts, as can any other theory that has ever existed in psychology. We would like to express our dissatisfaction with only three matters:

1. Some fits between "observed and theoretical probabilities," e.g., Figures 14 and 20 in (6), in our humble opinion, at best oversimplify and at worst distort the facts. These figures are useful only for some kinds of didactic purposes.

2. We do not understand how data such as those described for Experiment IV in the Spences' book (6, p. 153 ff) and in the paper by Brelsford & Atkinson (23) can possibly be reconciled with the theory. These experiments show that overt rehearsal of items prolongs their residence in the buffer, without having any effect on the transfer of relevant information into the long-term store. These findings, we feel, rather than supporting the

theory, represent strong evidence not only against Atkinson & Shiffrin's theory, but any other "boxes-in-the-head" theory that postulates transfer of information into the long-term store through the short-term store.

3. We are worried about the bent arrows in Atkinson & Shiffrin's Figures 1 and 2 in (6). These bent arrows labeled "lost" illustrate the fact that, according to the theory, information decays from the sensory register, short-term store, and perhaps even the long-term store. We are worried because we would like to know what happens to an item "knocked out" of the short-term store. Where does "lost" information go? The arrows point to the left-hand bottom corner of the printed page, but that does not help us very much. We also feel that the whole notion of items being "lost" violates the first law of thermodynamics. We rather like the idea—expressed by Herbart over 100 years ago—that information in any store remains there, in one form or another, and sometimes simply cannot be used for the desired purposes. We can only hope that the mystery of the downward pointing arrows in the system will be cleared up eventually. Our own recommended remedy is simply to erase the "lost" arrow pointing from the short-term store into the void—this has been done in (146)—and presume that all information in short-term store is transferred into the long-term store—but this has not yet been done. Changing the name of the short-term store to something like active memory, operational memory (130), or even consciousness might also constitute a modest improvement.

The theory seems to be undergoing rapid changes. One of the problems that the readers may have in the future is to keep up with all of them. For instance, in one place (6) loss of information from the long-term store is postulated and one parameter of the model is provided to describe it, but in another place (146) the notion of decay from long-term store is abandoned, and failure of retrieval is attributed entirely to the failure of the search mechanism, that is, to the inaccessibility of the stored information.

Despite these and some other minor blemishes, Atkinson & Shiffrin's theory can be set as a paragon to all researchers. Its virtues easily outweigh its deficiences. It genuinely and explicitly attempts to come to grips with, or at least say something about, most of the important aspects of human memory. It generates some interesting implications for experimental results from the continuous paired-associate tasks, it has varied success with other paradigms, and it is comprehensive—at least in comparison with other existing theories. It may turn out to be somewhat more intractable *vis-à-vis* phenomena shaped by the orienting attitudes of traditional work in verbal learning—transfer, mediation, and interference effects in list-learning experiments—but, one might argue, the extremely complex tasks generating these phenomena should not be investigated anyhow before we have developed a modicum of understanding of simpler tasks.

Conclusion

We mentioned at the outset that nothing very much has changed over the past hundred years in the understanding of how people learn and remember things. Anyone disputing this assertion is either not familiar with the history of our field, confuses new labels for phenomena with understanding of these phenomena, or is fixated on minor details while we are talking about the general picture. We have hundreds and thousands of little facts, we can make quantitative instead of qualitative statements, we can talk about all kinds of fine details in experimental data and characteristics of underlying processes—but the broad picture we have of human memory in 1970 does not differ from that in 1870. We know of no compelling reason why a chapter like this, discussing the kind of research we have discussed, could not have been written a hundred years ago; but it was not.

What is the solution to the problem of lack of genuine progress in understanding memory? It is not for us to say, because we do not know. But one possibility does suggest itself: why not start looking for ways of experimentally studying, and incorporating into theories and models of memory, one of the truly unique characteristics of human memory: its knowledge of its own knowledge. No extant conceptualization, be it based on stimulus-response associations or an information processing paradigm, makes provisions for the fact that the human memory system cannot only produce a learned response to an appropriate stimulus or retrieve a stored image, but it can also rather accurately estimate the likelihood of its success in doing it (29, 70).

Hart (70) has reported some experimental data in support of the reality of what he calls the memory-monitoring process and has thus pointed the way to the study of the most important and the least understood aspect of human memory. We cannot help but feel that if there is ever going to be a genuine breakthrough in the psychological study of memory, one that would save the students of ecphoric processes from the fate we talked about at the beginning of this chapter, it will, among other things, relate the knowledge stored in an individual's memory to his knowledge of that knowledge.

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Reprinted from
ANNUAL REVIEW OF PSYCHOLOGY
Vol. 21, 1970