

ADDRESS OF CHAIRMAN OF SECTION I (Psychology)

Implications of Short-Term Memory for a
General Theory of Memory¹

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Memory has never enjoyed even a small fraction of the interdisciplinary interest that has been expressed in symposia, discoveries, and methodological innovations during the last five years. Therefore, it seems probable that the next ten years will see major, perhaps even definitive, advances in our understanding of the biochemistry, neurophysiology, and psychology of memory, especially if these disciplines communicate with one another and seek a unified theory. My thesis is, of course, that psychological studies of human short-term memory, and particularly the further exploitation of new techniques for investigating human short-term memory, will play an important role in these advances toward a general theory of memory. Even now, some critical issues are being sharpened by such observations.

¹ This paper comprises, in substance, the author's Vice-Presidential Address to Section I (Psychology) of the American Association for the Advancement of Science, 1962. The author is particularly indebted to the Center for Human Learning, University of California, Berkeley, where a research appointment during the Fall semester of 1962-1963 gave the freedom from academic routine and the stimulating discussions that led to the repetition of the Hebb experiment and also supported the preparation of this paper. Early exploratory studies on short-term memory and the experiment on the recall of different sized verbal units were supported by Project MICHIGAN under Department of the Army Contract DA-36-039-SC-78801, administered by the United States Army Signal Corps. Reproduction for any purpose of the United States Government is permitted.

The confluence of forces responsible for this sanguine prediction about future progress is reflected in this AAAS program on memory (see other articles in this issue of this JOURNAL). Advances in biochemistry and neurophysiology are permitting the formulation and testing of meaningful theories about the palpable stuff that is the correlate of the memory trace as an hypothetical construct (Deutsch, 1962; Gerard, 1963; Thomas, 1962). In this work there is heavy emphasis on the *storage* mechanism and its properties, especially the consolidation process, and it may be expected that findings here will offer important guide lines for the refinement of the psychologist's construct once we are clear as to what our human performance data say it should be.

Within psychology several developments have focused attention on memory. In the first place, among learning theorists there is a revival of interest in the appropriate assumptions to be made about the characteristics of the memory traces (engrams, associations, bonds, sH_r 's) that are the products of experiences and repetitions of experiences. Thus, Estes (1960) has questioned the validity of the widespread assumption (e.g., Hull, 1943; Spence, 1955) that habit strength grows incrementally over repetitions, and has proposed an all-or-none conception as an alternative. More recently, he has examined (Estes, 1962) in detail the varieties of the incremental and all-or-none conceptions and the evidence

related to them. Already, some defenders of the incremental concept (Jones, 1962; Keppel and Underwood, 1962; Postman, 1963) have taken issue with Estes' conclusions, and it would appear that this fundamental question about memory will loom large in theory and experiments for some time to come. At a somewhat different level, the revival of experimental and theoretical interest in the notion of perseveration or consolidation of the memory trace (Glickman, 1961), and attempts to embody it in a general theory of learning (Hebb, 1949; Walker, 1958), have also focused attention on a theory of memory as a fundamental component of a theory of learning.

A second strong stimulus to research on memory from within psychology are several findings of the last few years that have forced major revisions in the interference theory of forgetting and consequently a renaissance of interest in it (Postman, 1961). First, there was the discovery by Underwood (1957) that proactive inhibition had been grossly underestimated as a source of interference in forgetting. Then, the unlearning factor as a component of retroactive inhibition was given greater credibility by the findings of Barnes and Underwood (1959). And finally, the joint consideration of the habit structure of the individual prior to a new learning experience, the compatibility or incompatibility of the new learning with that structure, and the unlearning factor (among others) led to the formulation of the interference theory of forgetting in terms that made it applicable to all new learning (Melton, 1961; Postman, 1961; Underwood and Postman, 1960). Thus, this development focuses attention on the interactions of memory traces during learning as well as their interactions at the time of attempted retrieval or utilization in recognition, recall, or transfer.

But perhaps the most vigorous force directing attention within psychology to the need for a general theory of memory is

the spate of theorizing and research on immediate and short-term memory during the last five years. In 1958, and increasingly thereafter, the principal journals of human learning and performance have been flooded with experimental investigations of human short-term memory. This work has been characterized by strong theoretical interests, and sometimes strong statements, about the nature of memory, the characteristics of the memory trace, and the relations between short-term memory and the memory that results from multiple repetitions. The contrast with the preceding thirty years is striking. During those years most research on short-term memory was concerned with the memory span as a capacity variable, and no more. It is always dangerous to be an historian about the last five or ten years, but I venture to say that Broadbent's *Perception and Communication* (1958), with its emphasis on short-term memory as a major factor in human information-processing performance, played a key role in this development. Fortunately, many of the others who have made important methodological and substantive contributions to this analysis of short-term memory have presented their most recent findings and thoughts in these Meetings on Memory, and they thus adequately document my assessment of the vigor and importance of this recent development. Therefore I will refrain from further documentation and analysis at this point, since the impact of some of these findings on our theory of memory is my main theme.

THE DOMAIN OF A THEORY OF MEMORY

A theory of memory is becoming important for a number of different reasons, and somehow all of these reasons properly belong to a comprehensive theory of memory. Its storage mechanism is the principal concern of biochemists and neurophysiologists; the morphology of its storage—whether as a multiplexed trace system with one trace per

repetition, or a single trace system subjected to incremental changes in "strength" by repetition—is becoming a principal concern of learning theorists; its susceptibility to inhibition, interference, or confusion both at the time of new trace formation and at the time of attempted trace retrieval or utilization is the concern of forgetting and transfer theorists; and the perhaps unique properties of its manifestation in immediate and short-term retention is the principal concern of psychologists interested in human information-processing performance. One knows intuitively that all of these different approaches emphasize valid questions or issues that must be encompassed by a general theory of memory, but nowhere—with perhaps the exception of Gomulicki's (1953) historical-theoretical monograph on memory-trace theory—will one find explicit systematic consideration of these several different facets of the problem of memory.

Since my present intention is to marshal some data relevant to one of the main issues in a general theory of memory—namely, the question of whether single-repetition, short-term memory and multiple-repetition, long-term memory are a dichotomy or points on a continuum—I feel compelled to discuss briefly what I believe to be the proper domain of a theory of memory and to differentiate it from a theory of learning.

After some exclusions that need not concern us here, learning may be defined as the modification of behavior as a function of experience. Operationally, this is translated into the question of whether (and, if so, how much) there has been a change in behavior from Trial n to Trial $n + 1$. Any attribute of behavior that can be subjected to counting or measuring operations can be an index of change from Trial n to Trial $n + 1$, and therefore an index of learning. Trials n and $n + 1$ are, of course, the presentation and test trials of a so-called test of immediate memory or they may be any trial in a repeti-

tive learning situation and any immediately subsequent trial. By convention among psychologists, the change from Trial n to Trial $n + 1$ is referred to as a learning change when the variable of interest is the ordinal number of Trial n and not the temporal interval between Trial n and Trial $n + 1$, and the change from Trial n to Trial $n + 1$ is referred to as a *retention* change when the variable of interest is the interval, and the events during the interval, between Trial n and Trial $n + 1$. Learning and retention observations generally imply that the characteristics of the task, situation, or to-be-formed associations remain the same from Trial n to Trial $n + 1$. When any of these task or situation variables are deliberately manipulated as independent variables between Trial n and Trial $n + 1$, the object of investigation is *transfer* of learning, i.e., the availability and utilization of the memorial products of Trial n in a "different" situation.

Now, these operational definitions of learning, retention, and transfer are completely aseptic with respect to theory, and I think it is important to keep them so. In part, this is because it is useful to keep in mind the fact that *learning* is never observed directly; it is always an inference from an observed change in performance from Trial n to Trial $n + 1$. Furthermore—and this is the important point for theory—the observed change in performance is always a confounded reflection of three theoretically separable events: (i) the events on Trial n that result in something being stored for use on Trial $n + 1$; (ii) the storage of this product of Trial n during the interval between Trials n and $n + 1$; and (iii) the events on Trial $n + 1$ that result in retrieval and/or utilization of the stored trace of the events on Trial n . For convenience, these three theoretically separable events in an instance of learning will be called *trace formation*, *trace storage*, and *trace utilization*.

Obviously, a theory of learning must encompass these three processes. However, it

must also encompass other processes such as those unique to the several varieties of selective learning and problem solving. Some advantages will accrue, therefore, if the domain of a general theory of memory is considered to be only a portion of the domain of a theory of learning; specifically, that portion concerned with the *storage* and *retrieval* of the residues of demonstrable instances of association formation. This seems to me to fit the historical schism between learning theories and research on memory and the formal recognition of this distinction may well assist in avoiding some misconceptions about the scope of a theory of memory. Historically, our major learning theories have not felt compelled to include consideration of the question whether storage of the residue of a learning experience (Trial n) is subject to autonomous decay, autonomous consolidation through reverberation, or to even consider systematically the memory-span phenomenon. On the other hand, much of the controversy between learning theorists surrounds the question of the necessary and sufficient conditions for association (or memory trace) formation. And even though most learning theories must say something about the conditions of transfer, or utilization of traces, they do not always include explicit consideration of the interference theory of forgetting or alternative theories. As for those who have been concerned with memory theory, they have, following Ebbinghaus (1885), employed the operations of rote learning, thus avoiding in so far as possible the problems of selective learning and insuring the contiguous occurrence of stimulus and response under conditions that demonstrably result in the formation of an association. Their emphasis has been on the storage and retrieval or other utilization of that association, i.e., of the residual trace of it in the central nervous system (CNS), and on the ways in which frequency of repetition and other learning affect such storage and retrieval.

The implication of this restriction on the domain of a theory of memory is that the theory will be concerned with post-perceptual traces, i.e., memory traces, and not with pre-perceptual traces, i.e., stimulus traces. It seems to me necessary to accept the notion that stimuli may affect the sensorium for a brief time and also the directly involved CNS segments, but that they may not get "hooked up," associated, or encoded with central or peripheral response components, and may not, because of this failure of being responded to, become a part of a memory-trace system. This view is supported by the recent work of Averbach and Coriell (1961), Sperling (1960), and Jane Mackworth (1962) which shows that there is a very-short-term visual pre-perceptual trace which suffers rapid decay (complete in .3 to .5 sec.). Only that which is reacted to during the presentation of a stimulus or during this post-exposure short-term trace in potentially retrievable from memory. While it is not necessary to my argument to defend this boundary for memory theory, because if I am wrong the slack will be taken up in a more inclusive theory of learning, it is of some interest that it is accepted by Broadbent (1963) and that it is consistent with a wealth of recent research on "incidental learning" in human subjects (Postman, in press).

What, then, are the principal issues in a theory of memory? These are about either the storage or the retrieval of traces. In the case of the storage of traces we have had four issues.² The first is whether memory traces should be given the characteristic of *autonomous decay* over time, which was

² For the purposes of this discussion, I am ignoring the hypothetical property of autonomous, dynamic changes within memory traces in the directions specified by gestalt laws (Koffka, 1935). While the need for such an hypothetical property is not yet a dead issue (Duncan, 1960; Lovibond, 1958), it has had very little support since the classical treatment of the matter by Hebb and Foord (1945).

dignified by Thorndike (1913) as the Law of Disuse and which recently has been vigorously defended by Brown (1958). The antithesis is, of course, the notion that associations, once established, are permanent—a position initially formulated by McGeoch (1932) and incorporated in a radical form in Guthrie's (1935) theory of learning.

The second storage issue is again an hypothesis about an autonomous process, but one involving the *autonomous enhancement* (fixation, consolidation) of the memory trace, rather than decay. The hypothesis was first formulated in the perseveration theory of Müller and Pilzecker (1900), with emphasis on the autonomous enhancement, or strengthening, of a memory trace if it was permitted to endure without interruption. As such, the emphasis was on a property of automatic "inner repetition" if repetition and duration are given a trade-off function in determining the strength of traces. More recently, the hypothesis has been that the memory trace established by an experience *requires* consolidation through autonomous reverberation or perseveration if it is to become a stable structural memory trace in the CNS (Deutsch, 1962; Gerard, 1963; Glickman, 1961; Hebb, 1949). Presumably, the alternative view is that every experience establishes a structural memory trace without the necessity of consolidation through reverberation or perseveration, but also without denying that such reverberation or perseveration, if permitted, may strengthen the trace.

The third issue about storage is the one previously referred to as *morphological* (at the molecular level) in our brief reference to the current controversy about the all-or-none versus the incremental notions of association formation. The all-or-none notion implies that the increment in the probability of response on Trial $n + 2$ is a consequence of establishment of independent and different all-or-none trace systems on Trials n and $n + 1$; the incremental notion implies that

the same trace system is activated in some degree on Trial n and then reactivated and strengthened on Trial $n + 1$. It is, of course, possible that both notions could be true.

The fourth issue about trace storage is actually one that overlaps the issues about retrieval or utilization of traces, and is perhaps the most critical current issue. This is the question whether there are two kinds of memory storage or only one. A duplex mechanism has been postulated by Hebb (1949), Broadbent (1958), and many others, and on a variety of grounds, but all imply that one type of storage mechanism is involved in remembering or being otherwise affected by an event just recently experienced, i.e., "immediate" or short-term memory for events experienced once, and that a different type is involved in the recall or other utilization of traces established by repetitive learning experiences, i.e., long-term memory or habit. Since a clean distinction between "immediate" memory and short-term memory is not possible (Melton, 1963), we shall henceforward refer to these two manifestations of memory as short-term memory (STM) and long-term memory (LTM).

Some principal contentions regarding the differences between the two memory mechanisms are that: (a) STM involves "activity" traces, while LTM involves "structural" traces (Hebb, 1949; 1961); (b) STM involves autonomous decay, while LTM involves irreversible, non-decaying traces (Hebb, 1949); and (c) STM has a fixed capacity that is subject to overload and consequent loss of elements stored in it, for nonassociative reasons, while LTM is, in effect, infinitely expandable, with failure of retrieval attributable mainly to incompleteness of the cue to retrieval or to interference from previously or subsequently learned associations (Broadbent, 1958; 1963). On the other hand, the monistic view with respect to trace storage is one which, in general, accepts the characteristics of LTM storage as the characteristics of STM storage

as well, and thus ascribes to the traces of events that occur only once the same "structural" properties, the same irreversibility, the same susceptibility to associational factors in retrieval, as are ascribed to LTM.

The bridge to the theoretical problems of trace retrieval and utilization as major components of a theory of memory is obviously wrought by the issue of memory as a dichotomy or a continuum. Those who accept a dichotomy do so on the basis of data on retention, forgetting, or transfer that suggest two distinct sets of conditions for retrieval and utilization of traces; those who accept a continuum do so on the basis of data that suggest a single set of conditions or principles.

The history of our thought about the problems of retrieval and utilization of traces reveals three main issues. The first is the question of the dependence of the retrieval on the completeness of the reinstatement on Trial $n + 1$ of the stimulating situation present on Trial n . Psychologists have formulated several principles in an attempt to describe the relevant observations, but all of them may be subsumed under a principle which asserts that the probability of retrieval will be a decreasing function of the amount of stimulus change from Trial n to Trial $n + 1$. Changes in directly measured and manipulated cue stimuli, like the CS in a classical conditioning experiment, that result in decrement in response probability are generally referred to a sub-principle of stimulus generalization (Mednick and Freedman, 1960); changes in contextual stimuli that result in forgetting are usually referred to a sub-principle of altered stimulating conditions or altered set (McGeoch and Irion, 1952); and stimulus changes that occur in spite of all attempts to hold the stimulating situation constant are referred to a sub-principle of stimulus fluctuation (Estes, 1955). Since these are all principles of transfer, when they are employed to interpret failure of retrieval on Trial $n + 1$, it is clear that all principles of transfer of learning, whether they emphasize

the occurrence of retrieval in spite of change or the failure of retrieval in spite of some similarity, are fundamental principles of trace retrieval and utilization. At this moment I see no necessary difference between the dual- and single-mechanism theories of memory with respect to this factor of stimulus change in retrieval, but there may be one implicit and undetected.

The second issue relates to the interactions of traces. Here, of course, is the focus of the interference theory of forgetting which has, in recent years, led us to accept the notion that retrieval is a function of interactions between prior traces and new traces at the time of the formation of the new traces, as well as interactions resulting in active interference and blocking of retrieval. This theory was given its most explicit early expression in the attack by McGeoch (1932) on the principle of autonomous decay of traces, and has been refined and corrected in a number of ways since then (Postman, 1961). In its present form it accepts the hypothesis of irreversibility of traces and interprets all failures of retrieval or utilization as instances of stimulus change or interference. Therefore, it implicitly accepts a one-mechanism theory of memory. However, it has been recognized (Melton, 1961) that the principal evidence for the theory has come from the study of retrieval following multiple-repetition learning, and that the extension of the theory to STM is not necessarily valid. Since dual-mechanism theorists assert that retrieval in STM is subject to disruption through overloading, but not through associative interference, a prime focus of memory theory becomes the question of associative interference effects in STM.

A third important issue related to retrieval is the relationship between repetition and retrieval probability. While the fact of a strong correlation between repetition and probability of retrieval seems not to be questionable, there are two important questions about repetition that a theory of memory must encompass. The first of these is the

question of whether repetition multiplies the number of all-or-none traces or whether it produces incremental changes in the strength of a trace. This has already been listed as a problem in storage, but it is obvious that the alternative notions about storage have important implications for the ways in which repetitions may be manipulated to increase or decrease probability of retrieval. The second is the question of whether there is a fundamental discontinuity between the characteristics of traces established by a single repetition and those established by multiple repetitions (or single repetitions with opportunity for consolidation). This appears to be the contention of the dual-mechanism theorists; whereas, a continuum of the effects of repetition in the establishment of "structural," permanent traces seems to be the accepted position of the single-mechanism theorists.

In summary so far, when the domain of a theory of memory is explicitly confined to the problems of the storage and retrieval of memory traces, it becomes possible to formulate and examine some of the major theoretical issues under the simplifying assumption that the formation of the associations or memory traces has already occurred. Then it becomes clear that the conflicting notions with respect to the properties of trace storage and the conflicting notions with respect to the principal determinants of trace retrieval, or failure thereof, converge on the more fundamental issue of the unitary or dual nature of the storage mechanism. My plan is to examine these alleged differences between STM and LTM in the light of some recent studies of human short-term memory, and then return to a summary of the implications these studies seem to have for the major issues in a general theory of memory.

STM AND LTM: CONTINUUM OR DICHOTOMY?

The contrasting characteristics of STM and LTM that have led to the hypothesis that there are two kinds of memory have not, to

my knowledge, been considered systematically by any memory theorist, although Hebb (1949), Broadbent (1957; 1958; 1963), and Brown (1958) have defended the dichotomy.

The decay of traces in immediate memory, in contrast to the permanence, even irreversibility, of the memory traces established through repetitive learning, is the most universally acclaimed differentiation. For Hebb (1949) this rapid decay is a correlate of the non-structural, i.e., "activity," nature of the single perception that is given neither the "fixation" effect of repetition nor the opportunity for "fixation" through reverberation. For Broadbent (1957; 1958) and Brown (1958) this autonomous decay in time is a property of the postulated STM mechanism, and attempts have been made (e.g., Conrad and Hille, 1958) to support the notion that time per se is the critical factor in decay. Obviously, this autonomous decay can be postponed by rehearsal—recirculating through the short-term store (Broadbent, 1958)—and Brown (1958) has maintained that such rehearsal has no strengthening effect on the structural trace. However, the decay of a specific trace begins whenever rehearsal is prevented by distraction or overloading of the short-term store (Broadbent, 1957; 1958). A corollary of this last proposition is that the initiation of the decay process, by dislodging the trace from the short-term store, is not dependent on new learning and therefore not on the associative interference principles which account for most if not all of the forgetting of events that reach the long-term store through repetition, reverberation, or both (Broadbent, 1963).

These characteristics contrast sharply with those attributed to LTM by the interference theory of forgetting which has dominated our thinking since McGeoch's (1932) classical attack on the Law of Disuse and which has gained new stature as a consequence of recent refinements (Melton, 1961; Postman, 1961). This theory implies: (a) that traces, even those that result from single repetitions, are

“structural” in Hebb’s sense, and are permanent except as overlaid by either the recovery of temporarily extinguished stronger competing traces or by new traces; and (*b*) that all persistent and progressive losses in the retrievability of traces are to be attributed to such associative interference factors, and not to decay or to a combination of nonassociative disruption plus decay. And, as a consequence of these two implications, it is assumed that the effect of repetition on the strength of the single type of trace is a continuous monotonic process. On this basis a continuum is assumed to encompass single events or sequential dependencies between them when these events are well within the span of immediate memory and also complex sequences of events, such as in serial and paired-associate lists, that are far beyond the span of immediate memory and thus require multiple repetitions for mastery of the entire set of events or relations between them.

My discussion of the question: “STM or LTM; continuum or dichotomy?” will therefore examine some experimental data on STM to see (*a*) whether they are interpretable in terms of the interference factors known to operate in LTM, and (*b*) whether the durability of memory for sub-span and supra-span to-be-remembered units is a continuous function of repetitions.

The reference experiments that provide the data of interest are those recently devised by Peterson and Peterson (1959) and Hebb (1961), with major emphasis on the former. While a number of ingenious techniques for investigating STM have been invented during the last few years, I believe that the Petersons’ method is the key to integration of retention data on immediate memory, STM, and LTM. This is because, as you will see, it can be applied to to-be-remembered units in the entire range from those well below the memory span to those well above it, and the control and manipulation of duration and frequency of presentation are essentially con-

tinuous with those traditionally employed in list memorization.

In what must have been a moment of supreme skepticism of laboratory dogma, not unlike that which recently confounded the chemist’s dogma that the noble gases are non-reactive (Abelson, 1962), Peterson and Peterson (1959) determined the recallability of single trigrams, such as X-J-R, after intervals of 3, 6, 9, 12, 15, and 18 sec. The trigrams were presented auditorily in 1 sec., a 3-digit number occurred during the next second, and S counted backward by 3’s or 4’s from that number until, after the appropriate interval, he received a cue to recall the trigram. The S was given up to 14 sec. for the recall of the trigram, thus avoiding any time-pressure in the retrieval process. The principal measure of retention was the frequency of completely correct trigrams in recall.

The results of this experiment are shown in Fig. 1. It is noteworthy that the curve has the Ebbinghausian form, even though the maximum interval is only 18 sec., and that there is an appreciable amount of forgetting after only 3 and 6 sec. Other observations reported by the Petersons permit us to estimate that the recall after zero time interval, which is the usual definition of immediate memory, would have been 90%, which is to say that in 10% of the cases the trigram was misperceived, so that the forgetting is actually not as great as it might appear to be. Even with this correction for misperception, however, the retention after 18 sec. would be only about 20%, which is rather startling when one remembers that these trigrams were well below the memory span of the college students who served as Ss.

The rapid deterioration of performance over time is not inconsistent with the decay theory, nor is it necessarily inconsistent with the notion that traces from single occurrences of single items are on a continuum with traces from multiple items learned through repetition. However, additional data with the same

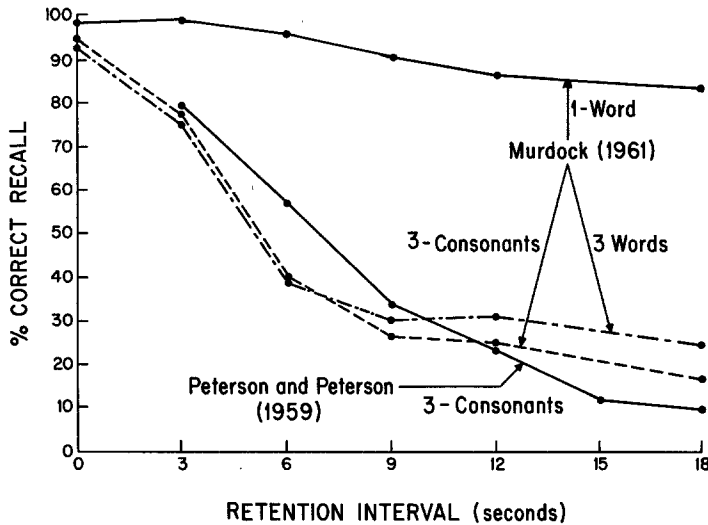


FIG. 1. Percentage frequency of completely correct recall of 3-consonant trigrams (Peterson and Peterson, 1959; Murdock, 1961), and 1-word and 3-word units (Murdock, 1961).

method were soon forthcoming. Murdock (1961) first replicated the Peterson and Peterson experiment with 3-consonant trigrams, and then repeated all details of the experiment except that, in one study he used single common words drawn from the more frequent ones in the Thorndike-Lorge word lists, and in another study he used word triads, i.e., three unrelated common words, as the to-be-remembered unit.

Murdock's results from these three experiments are shown alongside the Petersons' results in Fig. 1. His replication of the Petersons' study with trigrams gave remarkably similar results. Of considerable significance, as we will see later, is his finding that single words show less forgetting than did the trigrams, but that *some* forgetting occurs with even such simple units. Finally, the most seminal fact for theory in these experiments is his discovery that word triads act like 3-consonant trigrams in short-term retention.

Murdock's data strongly suggested that the critical determinant of the slope of the short-term retention function was the number of Millerian (1956) "chunks" in the to-be-remembered unit. Of even greater importance,

from my point of view was the implication that, other things being equal, the rate of forgetting of a unit presented once is a function of the amount of intra-unit interference, and that this intra-unit interference is a function of the number of encoded chunks within the item rather than the number of physical elements, such as letters, or information units.

The first of several projected experimental tests of this hypothesis has been completed.³ The to-be-remembered units were 1, 2, 3, 4, or 5 consonants. The unit, whatever its size, was presented visually for 1 sec., and read off aloud by *S*. Then .7 sec. later a 3-digit number was shown for 1 sec. and removed. The *S* read off the number and then counted backward aloud by 3's or 4's until a visual cue for recall, a set of 4 asterisks, was shown. The delayed retention intervals were 4, 12, and 32 sec., and a fourth condition involved recall

³ This study and a subsequent one are graduate research projects of David Wulff and Robert G. Crowder, University of Michigan, and will be reported under the title: Melton, A. W., Crowder, R. G., and Wulff, D., *Short-term memory for individual items with varying numbers of elements*.

after only .7 sec., hereafter referred to as the zero interval. The Ss were given 8 sec. for the recall of each item. In the course of the experiment each S was tested four times at each combination of unit size and interval for a total of 80 observations. Every condition was represented in each of 4 successive blocks of 20 observations, and there was partial counterbalancing of conditions within the blocks and of to-be-remembered units between the blocks. Through my error, the to-be-remembered units of each specific size were not counterbalanced across the four retention intervals. Thanks only to the power of the variable we were investigating, this did not, as you will see, materially affect the orderliness of the data.

The results for the last two blocks of trials are shown in Fig. 2. Again, the measure of recall performance is the percentage of completely correct recalls of the to-be-remembered unit, i.e., the single consonant had to be correct when only one was presented, all five consonants had to be correct and in the proper order when the 5-consonant unit was presented. The same relationships hold when

Ss are not as well-practiced in the task, i.e., in Blocks 1 and 2, although the absolute amounts of forgetting are greater. The data in Fig. 2 are to be preferred to those for the earlier stages of practice, because all five curves in this figure have their origin very near to 100% recall. That is, in all cases it is possible to assume that Ss had, in fact, learned the to-be-remembered unit during the 1-sec. presentation interval.

Aside from the self-evident generalization that the slope of the short-term forgetting curve increases as a direct function of the number of elements in the to-be-remembered unit, two features of these data are worthy of special attention. First, it should be noted that the slope of the curve for the 3-consonant units is not as steep as was reported by both Peterson and Peterson (1959) and by Murdock (1961). We do not know why there is this discrepancy, although it occurs consistently in our work with the Petersons' method.

The other point of interest is the obvious forgetting of the one-consonant unit. This curve looks very much like the one obtained by Murdock for single words. Both findings

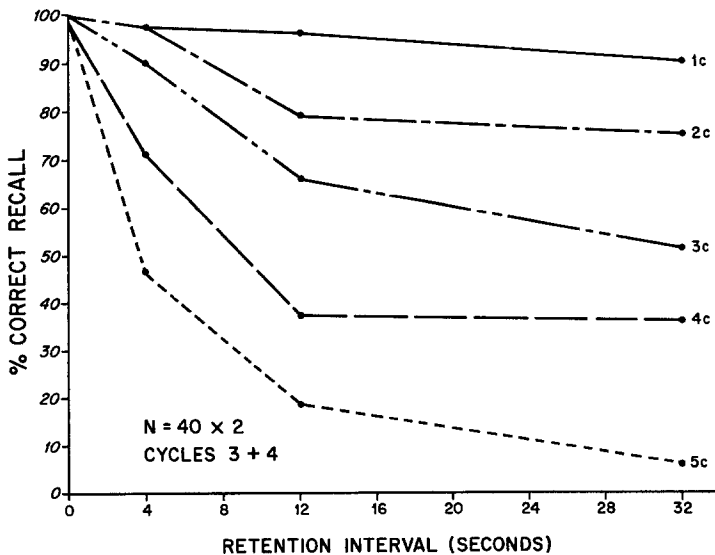


FIG. 2. Percentage frequency of completely correct recall of units of 1 to 5 consonants with well-practiced Ss (Blocks 3 and 4).

have significance for theory because they represent instances of forgetting when the intra-unit interference is at a minimum for verbal units. But before giving additional consideration to this point, a further set of data from this experiment needs to be presented and a more general statement of the observed relationships deserves formulation.

If the increased slopes of the forgetting curves shown in Fig. 2 are attributed to an increase in intra-unit interference, it is of some importance to show that the more frequent breakdown of complete recall as one increases the number of letters in the to-be-remembered unit is not merely a breakdown in the sequential dependencies between the letters, but is also reflected in the frequency of correct recall of the first letter of the unit. In Fig. 3 are shown the percentages of first-letter recalls in the last two blocks of our experiment. Although they are lacking in the monotonic beauty of the curves for whole units correct, I am willing to accept the generalization that first-letter recall suffers interference as a function of the number of other letters in the to-be-remembered unit. Thus, what Peterson (1963) has called "background

conditioning," and is measured by the recall of first letters, and what he has called "cue learning," and is represented by sequential dependencies in recall, are affected alike by the number of elements in the to-be-remembered unit. This is expected in so far as there is functional parallelism between "free" recall and serial or paired-associate recall with respect to the effect of learning and interference variables (Melton, 1963).

In Fig. 4 the results obtained so far have been generalized and extrapolated. This set of hypothetical curves will be used as the conceptual anchor for three points that are related to the question whether short-term and long-term memory are a dichotomy or points on a continuum. The first, and most obvious, point about the figure is that it reaffirms the notion that intra-unit interference is a major factor in the short-term forgetting of sub-span units, but now the parameter is the number of encoded chunks, instead of the number of physical elements or information units. This is consistent with Miller's (1956) cogent arguments for the concept of chunk as the unit of measurement of human information-processing capacities. It is also the unit most likely to

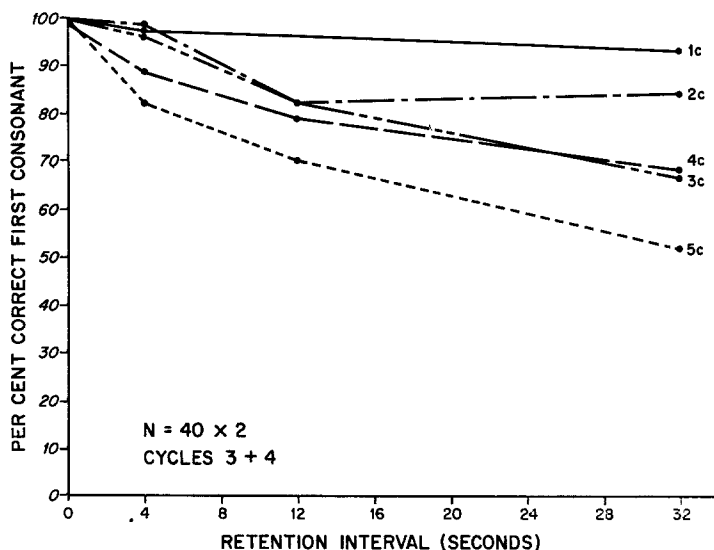


FIG. 3. Percentage frequency of correct recall of the first letter in 1- to 5-consonant units with well-practiced Ss (Blocks 3 and 4).

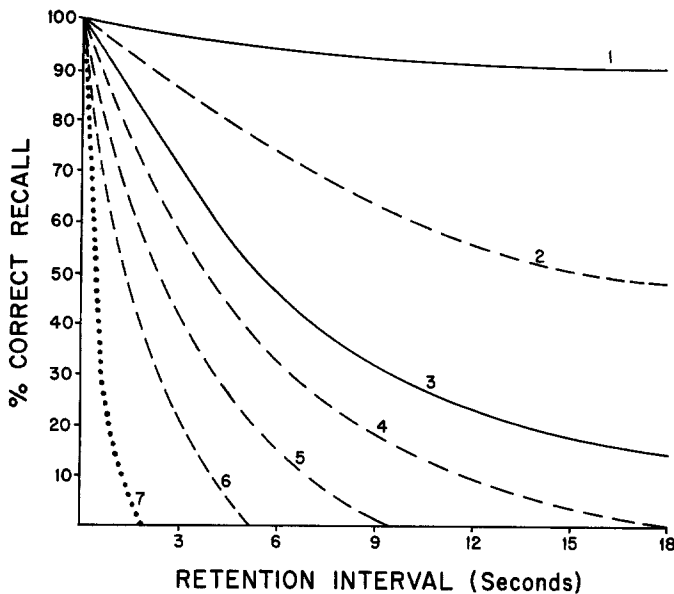


FIG. 4. The expected relationship between the number of recoded units ("chunks") in the to-be-remembered unit, the duration of the short-term retention interval, and the percentage frequency of completely correct recall, when each to-be-remembered unit is presented once, i.e., with just sufficient duration for one completely correct perceptual encoding. The solid-line curves represent some of the empirically determined functions; the dashed lines represent extrapolated functions; the dotted line represents the expected short-term memory function for a to-be-remembered unit that is at memory-span length for the individual S.

have a one-to-one relationship to the memory trace. Obviously, it is also the concept demanded by the parallelism of the findings of Murdock with 1 and 3 words and our findings with 1 to 5 consonants, even though it cannot, of course, be asserted that the number of elements beyond one in these experiments, be they words or consonants, stand in a one-to-one relationship to the number of chunks. Even though the strings of consonants in our experiment were constructed by subtracting from or combining consonant trigrams of Witmer (1935) association values less than 60%, there were surely some easy-to-learn letter sequences and some hard-to-learn letter sequences. That such differences in meaningfulness are correlated with chunkability is well known (Underwood and Schulz, 1960). Also, Peterson, Peterson, and Miller (1961) have shown, although on a limited scale, that the meaningfulness of CVC trigrams is positively

correlated with recall after 6 sec. in the Petersons' situation. But perhaps the greatest gain from the use of the chunk as the unit of measurement in formulating the otherwise empirical generalization is a suggestion this yields about how we may get a handle on that intervening variable. It suggests to me that we may be able to establish empirical benchmarks for 1, 2, 3, . . . , n chunks in terms of the slopes of short-term memory functions and then use these slopes to calibrate our verbal learning materials in terms of a chunk scale.

The evidence that the slope of the short-term forgetting curve increases dramatically as a function of the number of encoded chunks in the unit is evidence against autonomous decay being a major factor, but it does not deny that such decay may occur. It is evidence against decay as a major factor because: (a) a single consonant *was* remem-

bered with very high frequency over a 32-sec. interval filled with numerical operations that surely qualify as overloading and disrupting activities (if one grants that the Petersons' method adequately controls surreptitious rehearsal); and (b) the major portion of the variance in recall is accounted for by intra-unit interference, rather than time. It does not deny that decay may occur, since there was *some* forgetting of even the single consonant (and of the single word in Murdock's experiment) even though only one "chunk" was involved, and intra-unit interference was at a minimum.

The reason for the forgetting of the single chunk is, I believe, to be found in the other sources of interference in recall in this type of experiment. In the first place, I presume that no one will argue that counting backward aloud is the mental vacuum that interference theory needs to insure the absence of retroactive inhibition in the recall of the to-be-remembered unit, nor is it necessarily the least interfering, and at the same time rehearsal-preventing, activity that can be found for such experiments. However, we must leave this point for future research, because we have none of the systematic studies that must be done on the effects of different methods of filling these short retention intervals, and we also have no evidence, therefore, on the extent to which retroactive interference and intra-unit interference interact.

On the other source of interference which may explain the forgetting of the single chunk—namely, proactive interference (PI)—we do have some evidence. Peterson (1963) has maintained, on the basis of analysis of blocks of trials in the original Peterson and Peterson (1959) study, that there is no evidence for the build-up of proactive inhibition in that experiment, only practice effects. However, this evidence is unconvincing (Melton, 1963) when practice effects are strong, and if it is assumed that proactive inhibition from previous items in the series of tests may build

up rapidly but asymptote after only a few such previous items. Such an assumption about a rapidly achieved high steady-state of PI is given some credence by the rapid development of a steady-state in frequency of false-positives in studies of short-term recognition memory (Shepard and Teghtsoonian, 1961).

A second, and powerful, argument for large amounts of PI throughout the Peterson type of experiment is the frequency of overt intrusions from previous units in the series during the attempt to recall an individual unit. Murdock (1961) found such intrusions in his studies of short-term retention of words, and there was the strong recency effect among these intrusions that is to be expected if the steady-state notion is valid. The analysis of such intrusions in studies involving letters rather than words is limited by the identifiability of the source of the intrusions, but all who run experiments with letters become convinced that such intrusions are very common and usually come from the immediately preceding units.⁴

More systematic evidence for strong PI effects in STM in the Petersons' situation is given by Keppel and Underwood (1962). A representative finding is shown in Fig. 5. A three-consonant item which is the first item in the series is recalled almost perfectly after as long as 18 sec., and PI builds up rapidly over items, especially for the longer retention interval. These data support the notion that there is substantial PI in the Peterson and Peterson experiment on short-term memory for single verbal units. As such, they, as well as the other evidence cited, indicate that the small amount of forgetting of single consonants or

⁴ Apparent intrusions from preceding to-be-remembered units were very common in the 1- to 5-consonant experiment reported here, but the experimental design did not counterbalance first-order sequence effects over conditions and nothing meaningful can be said about such intrusions except that they occur with substantial frequency.

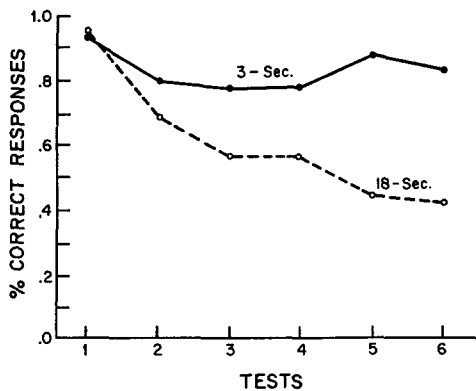


FIG. 5. Percentage frequency of completely correct recall of 3-consonant trigrams after 3 and 18 sec., as a function of the ordinal position of the test in a series of tests. The decline in recall reflects the build-up of proactive inhibition (Keppel and Underwood, 1962).

single words over short intervals of time may be partly, if not entirely, attributable to the PI resulting from sequential testing of recall of such items. Keppel and Underwood's results do not, however, support the view that the PI reaches a steady state in as few as five items, but this does not necessarily deny the steady-state notion. Also, a careful study of these data and the data on intra-unit interference suggests some strong interactions between PI, intra-unit interference (II), and the retention interval, all of which would support the interference interpretation, but discussion of these interactions would be tedious and unrewarding until properly designed experiments have been performed.

My conclusion from all this is that there is sufficient direct or inferential evidence for PI, RI, and II in the short-term retention of single sub-span verbal units, and that the PI and potential RI may account for the observed forgetting of one-chunk units, that is, when II is minimal. So much for interference.

The other line of investigation that needs to be considered before the question of continuum versus dichotomy can be properly assessed has to do with the effect of repetition on the short-term memory for sub-span and

just supra-span strings of elements or chunks.

The concept of the memory span is rather important in this discussion because it is the boundary between the number of elements, or chunks, that can be correctly reproduced immediately after a single repetition and the number of elements, or chunks, that require two or more repetitions for immediate correct reproduction. Interestingly enough, the short-term forgetting curve for a unit of memory-span length turns out to be the limiting member of the hypothetical family of curves that has been used to generalize the relationship between the slope of the forgetting curve and the number of chunks in the to-be-remembered unit. The extrapolated forgetting curve for a unit of memory-span length is shown as the dotted-line curve of Fig. 4.

The origin of this limiting curve on the ordinate will, of course, depend on the statistical definition of the span of immediate memory, but in order to be consistent I have placed it in Fig. 4 at or near 100% recall after zero interval. It is also assumed that the presentation time for this and all other smaller numbers of chunks is just sufficient for one perceptual encoding of each element, i.e., for one repetition. For a unit of span length it is not surprising that a precipitous decline of completely correct recall to zero is expected when only very short, but filled, delays are introduced before recall begins. No experiment in the literature fits exactly these operational requirements, but the prediction is a matter of common experience in looking up telephone numbers, and we also have Conrad's (1958) evidence that Ss show a radical reduction in correct dialing of 8-digit numbers when required merely to dial "zero" before dialing the number.

At this point we are brought face to face with the question of the effects of repetition of sub-span and supra-span units on their recall. Such data are important for at least two reasons. In the first place, the argument for a continuum of STM and LTM requires

that there be only orderly quantitative differences in the effects of repetition on sub-span and supra-span units. In the second place, if repetition has an effect on the frequency of correct recall of sub-span units, such as consonant trigrams, this must certainly have some significance for the conceptualization of the strength of a memory trace—whether it is all-or-none or cumulative.

The effect of time for rehearsal of a set of items before a filled retention interval was first studied by Brown (1958). His negative results led him to the conclusion that recirculation of information through the temporary memory store merely delays the onset of decay, but does not strengthen the trace. However, the original Peterson and Peterson (1959) report on the retention of consonant trigrams included an experiment which showed a significant effect of instructed rehearsal on short-term retention.

Fortunately, we now have available a report by Hellyer (1962) in which consonant trigrams were given one, two, four, or eight 1-sec. visual presentations before retention intervals of 3, 9, 18, and 27 sec. His data are shown in Fig. 6 and require little comment.

Obviously, a consonant trigram is remembered better with repetition even though it is completely and correctly perceived and encoded after only one repetition, as judged by the immediate recall of it. The slopes of the retention curves in our hypothetical family of curves based on the number of chunks in the to-be-remembered unit are, therefore, a joint function of chunks and repetitions. Or perhaps a better theoretical statement of this would be to say that repetition reduces the number of chunks in the to-be-remembered unit. This is why one word and one consonant have the same rate of forgetting.

As for the effect of repetition on just supra-span units, we have no data directly comparable to those of Hellyer for sub-span units, but we have data from a much more severe test of the repetition effect. I refer to the method and data of Hebb's (1961) study in which he disproved to his own satisfaction his own assumption about "activity" traces. In this experiment he presented a fixed set of 24 series of 9-digit numbers. Each of the digits from 1 to 9 was used only once within each to-be-remembered unit. The series was read aloud to *S* at the rate of about 1 digit/sec.,

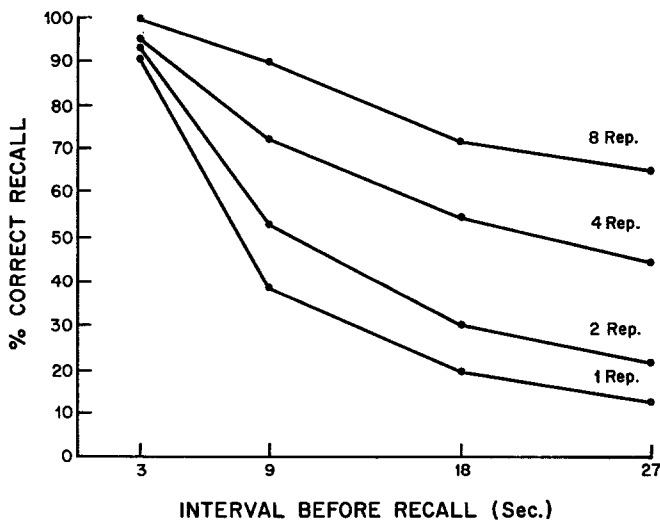


FIG. 6. Percentage frequency of completely correct recall of 3-consonant trigrams as a function of the frequency of 1-sec. presentations of the trigram before beginning the retention interval (Hellyer, 1962).

and *S* was instructed to repeat the digits immediately in exactly the same order. The unusual feature of the experiment was that exactly the same series of digits occurred on every third trial, i.e., the 3rd, 6th, 9th . . . 24th, the others varying in a random fashion.

His results are shown in Fig. 7. Hebb considered the rising curve for the repeated 9-digit numbers, when contrasted with the flat curve for the nonrepeated numbers, to be sufficient basis for concluding that some form of structural trace results from a single repetition of an associative sequence of events. Further, he properly considers this to be a demonstration of the cumulative structural effects of repetition under extremely adverse conditions involving large amounts of RI.

Hebb's method in this experiment may well be another important invention in the analysis of human memory. But I was not completely satisfied with his experiment and the reliability of his findings, for reasons that need not be detailed here. As a consequence of these uncertainties, I have repeated and extended Hebb's experiment by giving each of 32

women *Ss* two practice numbers and then 80 tests for immediate recall of 9-digit numbers. Within these 80 tests there were 4 instances in which a specific 9-digit number occurred 4 times with 2 other numbers intervening between successive trials, 4 in which a specific number occurred 4 times with 3 intervening numbers, 4 for 4 trials with 5 intervening numbers and 4 for 4 trials with 8 intervening numbers. In addition, there were 16 9-digit numbers that occurred only once. I will not try to describe the interlocking pattern of events that was used to achieve this design, but the design chosen was used in both a forward and backward order for different *Ss*, and the specific repeated numbers were used equally often under the different spacings of repetitions. Furthermore, within the entire set of 32 different 9-digit numbers used in this experiment, inter-series similarities were minimized by insuring that no more than two digits ever occurred twice in the same order. The numbers were presented visually for 3.7 sec. and *S* recorded her response by writing on a 3×5 in. card which contained 9 blocks.

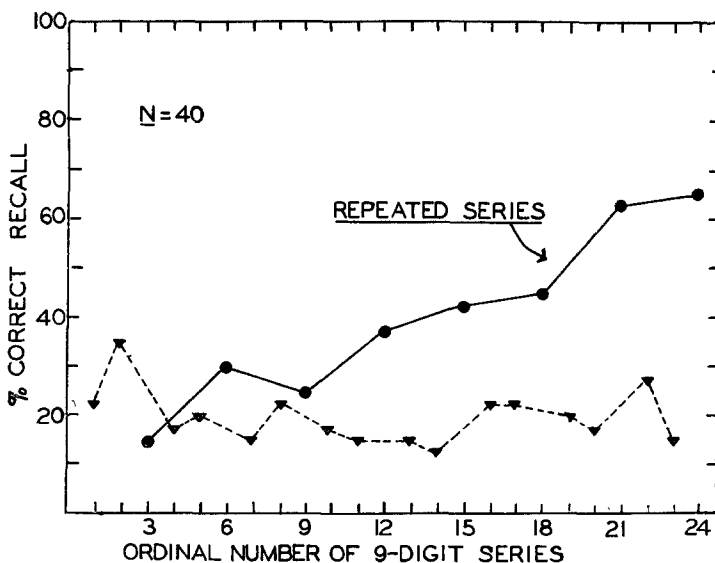


FIG. 7. Percentage frequency of completely correct recall of 9-digit numbers when tested immediately. The "repeated series" was a specific 9-digit sequence that occurred in the 3rd, 6th, 9th . . . 24th position in the series of tests. Other points represent nonrepeated 9-digit numbers (Hebb, 1961).

Recall began .7 sec. after the stimulus slide disappeared, and 8.8 sec. were allowed for recall.

Unfortunately, my Ss behaved in a somewhat more typical fashion than did Hebb's, in that they showed substantial nonspecific practice effects. This complicates the determination of the effects of specific repetition, because later trials on a particular 9-digit number must always be later in practice than earlier trials, and also because this confounding of specific and nonspecific practice effects is more serious the greater the interval between repetitions of a specific number. This confounding has been eliminated, at least to my satisfaction, by determining the function that seemed to be the most appropriate fit to the practice curve based on first occurrences of specific numbers. This function was then used to correct obtained scores on the 2nd, 3rd, and 4th repetitions of a specific number in a manner and amount appropriate to the expected nonspecific practice effect.

A preferred measure of the effect of repetition in this situation is the mean number of

digits correctly recalled in their proper positions. In Fig. 8 is shown the mean number of digits correctly recalled, as a function of ordinal position of the first occurrence of a 9-digit number within the experimental session. This merely confirms my statement about practice effects; exhibits the equation used for corrections for general practice effects; and permits observation of the large variability of mean performance in this type of experiment.

The principal data from the experiment are shown in Fig. 9. The effect of repetition of a specific 9-digit number is plotted, the parameter being the number of other different 9-digit numbers that intervened between successive repetitions of the specific number. In these curves the points for first-repetition performance are obtained points, and those for performance on the 2nd, 3rd, and 4th repetitions have been corrected for nonspecific practice effects. In Fig. 10 these last data are expressed as gains in performance over performance on the first occurrence of a number. Comparable data for gains in the frequency

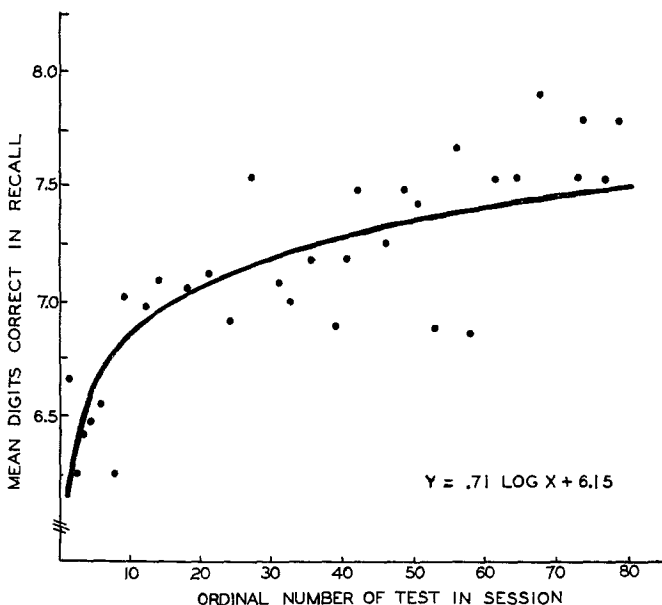


FIG. 8. The nonspecific practice effect in the recall of new and different 9-digit numbers in the course of the experiment.

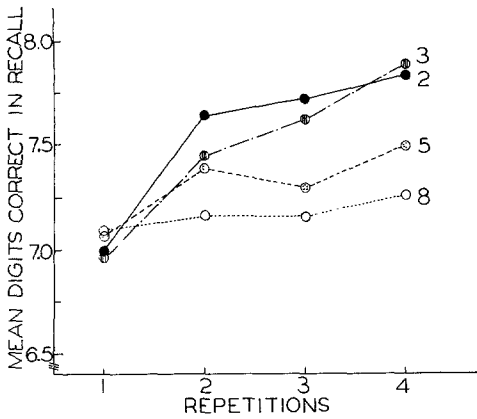


FIG. 9. Mean number of digits correctly recalled, as a function of the number of repetitions of the specific 9-digit number and of the number of other 9-digit numbers that intervened between repetitions. The data points for the first repetition are obtained values; the data points for the second, third, and fourth repetitions reflect corrections for nonspecific practice effects.

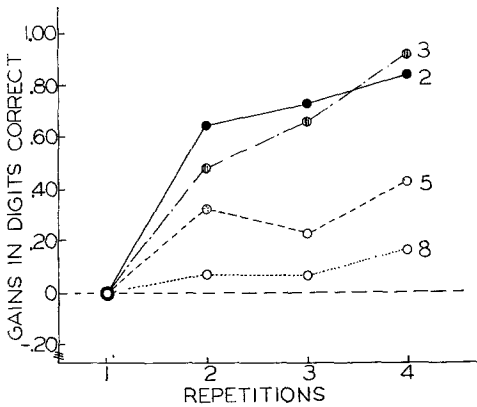


FIG. 10. Mean gains in number of digits correctly recalled, as a function of the number of repetitions of a specific 9-digit number and of the number of other 9-digit numbers that intervened between repetitions. All gain scores have been corrected for nonspecific practice effects.

with which entire 9-digit numbers were correctly recalled show the same relationships.

These data not only confirm the Hebb data, they also add material substance to an argument for a continuum of immediate, short-term, and long-term memory. Just as a continuum theory would have predicted Hebb's

results with two intervening numbers between repetitions of a specific number, it also would predict that the repetition effect would be a decreasing function of the number of intervening numbers because between-repetition retroactive inhibition is being increased. Even so, I am not sure that any theory would have predicted that one would need to place as many as 8 other 9-digit numbers in between repetitions of a specific 9-digit number before the repetition effect would be washed out. Surely, the structural memory trace established by a single occurrence of an event must be extraordinarily persistent.

With respect to our hypothetical family of retention curves based on the number of chunks in the to-be-remembered unit, we can now with some confidence say that events which contain chunks beyond the normal memory span can be brought to the criterion of perfect immediate recall by reducing the number of chunks through repetition. If this empirical model involving chunks and repetitions to predict short-term forgetting is valid, it should be possible to show that a supra-span 9-chunk unit that is reduced to 7 chunks through repetition, would have the short-term forgetting curve of a 7-chunk unit, and one reduced through repetition to a 3-chunk unit should have a 3-chunk short-term forgetting curve. Even though this prediction is probably much too simple-minded, it now requires no stretch of my imagination to conceive of the "immediate" or short-term memory for single units and the memory for memorized supra-span units, like 12 serial nonsense syllables or 8 paired associates, as belonging on a continuum.

IMPLICATIONS

We may now turn to the implications these data on short-term memory seem to me to have for a theory of memory. I will attempt no finely spun theory, because such is neither my talent nor my interest. Also, I can be brief because, aged Functionalist that I am,

I would be the first to admit—even insist—that my inferences are stated with confidence only for the storage and retrieval of verbal material demonstrably encoded by adult human Ss.

The duplexity theory of memory storage must, it seems to me, yield to the evidence favoring a continuum of STM and LTM or else come up with an adequate accounting for the evidence presented here. My preference is for a theoretical strategy that accepts STM and LTM as mediated by a single type of storage mechanism. In such a continuum, frequency of repetition appears to be the important independent variable, “chunking” seems to be the important intervening variable, and the slope of the retention curve is the important dependent variable. I am persuaded of this by the orderly way in which repetition operates on both sub-span units and supra-span units to increase the probability of retrieval in recall, and also by the parallelism between STM and LTM that is revealed as we look at STM with the conceptual tools of the interference theory of forgetting which was developed from data on LTM.

The evidence that implies a continuum of STM and LTM also relates, of course, to some of the other issues about the characteristics of memory storage. While it is perhaps too early to say that the autonomous decay of traces has no part in forgetting, whether short-term or long-term, I see no basis for assuming that such decay has the extreme rapidity sometimes ascribed to it or for assuming that it accounts for a very significant portion of the forgetting that we all suffer continually and in large amounts. On the contrary, the data from both STM and LTM tempt one to the radical hypothesis that every perception, however fleeting and embedded in a stream of perceptions, leaves its permanent “structural” trace in the CNS.

Insofar as I can understand the implications of the consolidation hypothesis about memory storage, I must concur with Hebb’s

(1961) conclusion that his experiment demonstrates the fixation of a structural trace by a single repetition of an event and without the benefit of autonomous consolidation processes. In fact, I think that our repetition and extension of his experiment establishes that conclusion even more firmly, because it shows that the retrievability of the trace of the first experience of a specific 9-digit number is a decreasing function of the amount of reuse of the elements in the interval between repetitions. Therefore, as far as our present data go, it seems proper to conclude that a consolidation process extending over more than a few seconds is not a necessary condition for the fixation of a structural trace. This does not, of course, deny that consolidation may be a necessary condition in other types of learning or other types of organism, nor does it deny that types of experience (e.g., Kleinsmith and Kaplan, 1963; Walker, 1963) other than the mundane remembering of nonsense strings of letters or words may benefit from such autonomous consolidation processes if they are permitted to occur.

The issue as to whether memory traces are established in an incremental or all-or-none fashion can be refined, but not resolved, on the basis of our observations on short-term memory. In all of the experiments with the Petersons’ method, the initial operation was to insure that *S* encoded, i.e., learned, the to-be-remembered unit in a single 1-sec. presentation of it before the retention interval was introduced. This is “one-trial” learning in a more exact sense than has been true of various attempts to demonstrate the all-or-none principle in associative learning (Postman, 1963). Yet forgetting was rapid and strongly a function of the amount of potential intra-unit interference in the to-be-remembered unit. Also, this unit that was perfectly remembered after one repetition was better remembered after multiple massed repetitions. The proper question in the case of verbal associative learning seems, therefore, to be

the characteristics of the trace storage that reflect the effects of repetitions on performance, rather than the question whether such associative connections reach full effective strength in one trial. The question of whether repetitions multiply the number of traces leading to a particular response or produce incremental changes in specific traces seems to me to be subject to direct experimental attack. Perhaps again because of my Functionalist background, I am inclined to believe that future research will show that both the multiplexing of traces and the incremental strengthening of traces results from repetition. Which mode of storage carries the greater burden in facilitating retrieval will depend on the variability of stimulation from repetition to repetition and the appropriateness of the sampling of this prior stimulation at the time of attempted retrieval.

Finally, with respect to the retrieval process, the theory of which is dominated by transfer theory for LTM, it seems that the placing of STM and LTM on a continuum—and the reasons for doing so—forces the interference theory of forgetting to include the prediction of forgetting in STM within its domain. At least, the testing of the theory in that context will extend its importance as a general theory of forgetting, if it survives the tests, and will quickly reveal the discontinuity of STM and LTM, if such is in fact the case.

Whatever may be the outcome of these theoretical and experimental issues in the next few years, of one thing we can be certain at this time. The revival of interest in short-term memory and the new techniques that have been devised for the analysis of short-term memory will enrich and extend our understanding of human memory far beyond what could have been accomplished by the most assiduous exploitation of the techniques of rote memorization of lists of verbal units. In fact, our evidence on STM for near-span and supra-span verbal units suggests that the systematic exploration of the retention of varying sizes of units over short and long

time intervals will give new meaning to research employing lists.

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