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Processing strategies and secondary memory in very rapid forgetting

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When a memory test is unexpected, recall performance is quite poor at retention intervals as short as 2–4 seconds. Orienting tasks that change encoding conditions are known to affect forgetting in such "very rapid forgetting" paradigms where people are misled to believe that recall will not be required. We evaluated the hypothesis that differences in forgetting among orienting tasks are attributable to contributions of secondary memory during encoding in two experiments. In Experiment 1, short-term recall performance was inversely related to task demands during encoding, although long-term memory performance was not. Task demands were assessed by making the duration of stimulus presentation dependent on the time required to perform three different orienting tasks. In Experiment 2, we compared performance of that variable-length stimulus presentation to the fixed-length presentation used in most prior research. The results suggested that additional encoding or rehearsal time does not have an appreciable impact on short-term performance. Thus, differences in forgetting appeared to be a function of the contribution of secondary memory rather than a function of the time available to engage in primary memory rehearsal strategies.

The distractor paradigm developed by Brown (1958) and Peterson and Peterson (1959) is commonly believed to provide an accurate estimate of the time course of retention in primary memory. The Brown–Peterson paradigm, as it is called, involves presenting a small number of items (e.g., words, numbers, letters) to people who then engage in a distractor task (e.g., counting backward) in order to prevent rehearsal. As this filled retention interval is lengthened, a steep, monotonic decline in recall is observed until an asymptote is reached at approximately 18 sec. Originally, the decline in recall over time was thought to reflect forgetting from primary memory, whereas the asymptote was assumed to provide a measure of secondary memory (Waugh & Norman, 1965). However, because people know they will have to recall on each and every trial, they may attempt to form a more lasting secondary memory trace (e.g., Watkins & Watkins, 1974). Two independent lines of research have demonstrated that even the initial portions of the recall functions derived from the standard Brown–Peterson paradigm are not free of contributions from secondary memory.

In the first set of investigations, Muter (1980) argued that forgetting from primary memory could be assessed more accurately when people do not expect a recall test because they are less likely to engage in processing that contributes to secondary memory. He devised a paradigm that led people to believe that they would rarely, if ever, have to recall items after a distractor-filled retention interval. His technique was to embed a very few *critical* Brown–Peterson trials among many trials of three other kinds: (1) *counting* trials, which did have a distractor-filled retention interval but did not require recall; (2) *maintenance* trials, which had an unfilled retention interval and required recall; and (3) *irrelevant* trials, which were used to disguise the nature of the task. Muter's method led people to believe that they would have to recall items only after an *unfilled* retention interval, and not after a distractor-filled period, as required on every trial of the standard Brown–Peterson paradigm. His results showed dramatic forgetting in as little as 2–4 sec on the critical trials as compared to the standard Brown–Peterson measures. Muter concluded that low expectancy of recall reduced strategic processing contributions of secondary memory.

The Muter paradigm, however, may not completely eliminate the contamination of secondary memory from

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measurement of primary memory because expectancy might be only one of several sources of secondary memory contributions. To assess this possibility, Sebrechts, Marsh, and Seamon (1989) used Muter's paradigm introducing three standard encoding strategies (i.e., semantic, acoustic, and reading tasks) from the levels of processing literature (e.g., Craik & Lockhart, 1972). The results replicated Muter's very rapid forgetting phenomenon under low recall expectancy, but reliable performance differences existed among the three encoding conditions in both short-term and long-term recall. The differences in short-term performance suggested that additional secondary memory contributions exist in the Brown–Peterson paradigm beyond the expectation of having to recall items on every trial. Because very rapid forgetting was observed for all three encoding conditions when recall expectancy was low, the results also suggested that a specific type of encoding is not the central determinant of Muter's phenomenon.

A second set of investigations, by Healy, Fendrich, Cunningham, and Till (1987; and more recently, Cunningham, Healy, Till, Fendrich, & Dimitry, 1993), has focused on the loss of order information from primary memory. As originally proposed, Estes's perturbation model (Lee & Estes, 1981) accounts for the loss of order information on the basis of a single primary memory rehearsal parameter. But Healy et al. demonstrated that a secondary memory component must be added to the primary memory rehearsal component of that model. In their paradigm, people were presented with two strings of letters and were either precued or postcued as to which of the two strings they would be requested to recall. The precued group outperformed the postcued group, suggesting that the advantage to precuing one of the two segments of letters could lie in either (1) an encoding advantage attributable to secondary memory or (2) a rehearsal effect attributable to primary memory. In modeling their data, however, Healy et al. found that performance differences in the precue and postcue conditions were best accounted for by adding a secondary memory encoding parameter to Estes's original primary memory rehearsal parameter.

Using their cuing paradigm, Cunningham et al. (1993) also investigated whether faster forgetting rates would be observed under conditions of low versus high expectation of recall. They compared two conditions. In one, people were not expecting a recall test but were nevertheless "fooled" 25% of the time. The other featured their standard cuing paradigm, in which people were accurately cued 75% of the time. Their results replicated the dramatic forgetting that Muter (1980) and Sebrechts et al. (1989) found, but there was no evidence for a difference in the retention functions under conditions of low versus high expectancy, suggesting that forgetting rates under varying conditions of expectancy might be the same. In a second study, a previously untested 0-sec retention interval was added. The results of that study again showed dramatic forgetting, but implicated the 0-sec retention interval as "elevated" in Muter's (and Sebrechts et al.'s) procedure. Cunningham et al.'s objection to the 0-sec retention interval in the Muter paradigm was that participants' expectations might not have been reduced because people had no way to discriminate a priori between a critical trial and a maintenance trial that would require recall. However, Muter (1995) has recently challenged that claim on the basis of a number of methodological differences in the two approaches. Nevertheless, if that 0-sec critical trial duration were indeed elevated, it might contribute to Muter's (and Sebrechts et al.'s) finding of "more rapid" forgetting when people do not expect a recall test. Thus, the very rapid forgetting phenomenon may involve dramatic forgetting rather than a faster forgetting rate.

Together, both lines of investigation suggest that expecting a recall test changes encoding characteristics in order to form more lasting secondary memory traces, even at very short presentation and retention intervals. In earlier work, Sebrechts et al. (1989) suggested that their results could be explained by a model of resource allocation. They argued that expectancy determined the overall level of resource allocation. That is, when a recall test is expected for certain items and not for others, as in Cunningham et al.'s (1993) cuing task, more resources are devoted to the items that one expects to recall. In contrast, Sebrechts et al. argued that an orienting task influenced the type of encoding, which they claimed changed the distribution of the resources in ways that influenced short-term and long-term retention differently. Thus, a semantic task would result in better long-term retention than short-term retention, whereas a reading task would prove relatively more useful for short-term retention. In the case of their acoustic task, resources were allocated in a way that was relatively ineffective for both short-term and long-term retention of items.

The present experiments were designed to provide additional evidence to evaluate Sebrechts et al.'s (1989) earlier conjectures. A potential problem exists in the resource allocation account insofar as the time allocated to each orienting task may be different. In brief, those previous experiments provided no explicit measure of the processing demands of each orienting task. Each stimulus word was presented for a fixed interval of 1 sec, a duration long enough for all participants in all conditions to make a response according to their assigned task. Thus, an orienting task that was relatively less time-consuming could have left ample time for people to engage in other sorts of processing, such as rehearsing the items presented on that trial. In order to explicitly determine the resources demanded by each orienting task, we used response-terminated orienting tasks: After a judgment was made for a particular stimulus, the next one was presented immediately. Reaction times (RTs) to each orienting task can then serve as a measure of the resources demanded by that task. In addition, this procedure reduced the possibility that additional resources are allocated to other processing activities, such as rehearsal, after completion of the orienting task for a particular stimulus. Two outcomes could result from terminating processing immediately following an assigned orienting task. First, overall differences between the orienting conditions could persist. That result would suggest that, after largely eliminating primary memory rehearsal, orienting task differences in short-term performance may largely reflect contributions of secondary memory. Second, response termination could eliminate the differences in short-term performance that Sebrechts et al. found among the same three orienting tasks. That result would implicate one of two benefits to short-term performance in all of the previous studies: Either the time remaining after responding is used for primary memory rehearsal or it is used to process the stimulus further according to task instructions.

EXPERIMENT 1

Method

Participants. Sixty introductory psychology students volunteered and were awarded partial course credit for their participation. People were randomly assigned to one of three orienting task conditions (semantic, acoustic, or reading) with an equal number serving in each group. All participants were tested individually.

Materials, Design, and Apparatus. The stimuli were identical to those employed by Sebrechts et al. (1989). These materials consisted of 300 common nouns chosen from the Thorndike–Lorge (Thorndike & Lorge, 1944) word list, with half representing animate objects and half representing inanimate objects. They were further selected so that half contained a long *e* sound and half did not. A perfect orthogonal cross between the animacy and the long *e* criteria could not be achieved (with 75 words in each category) because of a limited number of words that satisfied both conditions. Therefore, the resulting distribution of words across stimulus categories was as follows: 50 animate/long *e* present, 100 inanimate/long *e* present, 100 animate/long *e* absent, 50 inanimate/long *e* absent. From these words, 10 stimulus orders were generated by random sampling without replacement. Each was randomly assigned to 2 people in each of the three task conditions. This procedure ensured that both the stimuli and their order of presentation were identical for participants across the three task conditions. The experimental software used by Sebrechts et al. (1989) was modified to collect RTs and to run on an IBM-compatible computer. As in those earlier studies, people viewed the stimuli in the center of a monochrome monitor.

We assessed long-term memory by having participants complete a final recognition memory test. For each of the 10 stimulus orders, a 220-item paper-and-pencil test was constructed. Half of these items were new distractor words chosen to equate both word frequency and the proportion of original items fulfilling the animacy and long *e* criteria. The other half were old items that had been presented during the short-term retention testing. The old items were chosen to include (1) all words (i.e., 15) presented on the critical trials, (2) one third of the words (i.e., 20) shown on the counting trials, and (3) one third of the items (i.e., 75) presented on the maintenance trials. In order to counterbalance items across the orienting conditions, 10 recognition tests were prepared that were unique to the 10 stimulus orders.

For the conditions of interest, the design was a 3 × 3 mixed factorial. The first (between-subjects) factor manipulated the orienting task to be used in the Muter short-term memory paradigm. The semantic task required people to decide whether each word represented something animate. The acoustic task required people to decide if the word contained a long *e* sound. In the reading condition, people merely read each word aloud. The second factor was manipulated within-subjects and included three retention intervals of 0, 2, and 4 sec for the critical trials (as originally used by Sebrechts et al., 1989). Two additional factors that were included for counterbalancing (type of response and critical trial order) are described next.

Procedure. The general procedure followed Sebrechts et al. (1989) exactly except for several minor aspects concerning the collection of response latencies. Each person was tested on a total of 100 short-term memory trials: 75 maintenance, 20 counting, and 5 critical trials. The initial portion of a trial was identical across tasks and conditions. A preparatory tone was followed by a 0.5-sec delay and the sequential presentation of three words. A word remained on the screen until a response was made according to a person's assigned condition. In the semantic and acoustic groups, half of the participants responded out loud "yes" or "no" while simultaneously pressing one of two labeled keys on the keyboard that corresponded to their decision (this verbal response was included to replicate our earlier procedure, in which RTs were not collected). The other half of the participants only responded with their keypresses, not giving an additional verbal response. This manipulation was included to assess whether participants in Sebrechts et al.'s earlier study were unable to phonetically encode the items because of articulatory suppression from the *yes/no* responses spoken aloud. In the reading condition, people read the presented word aloud while simultaneously pressing a single key to indicate that they had finished reading. Each keyboard response terminated presentation of the current word and initiated the presentation of the next word or task. RTs for all keypress responses were software collected. At the offset of the third word in a triad, the remainder of the trial sequence varied according to one of three trial types. The order of these trials was randomized under the constraint that 4 counting trials occurred in each of the 5 blocks of 20 trials.

On maintenance trials, participants received an unfilled retention interval (i.e., the computer screen was blank) for 2 sec, followed by a 4-sec recall interval marked by a "WORDS?" prompt. People had been previously instructed to recall the three words for that trial, in their presented order, whenever they saw that prompt. In addition, people were instructed to guess when they were not sure, and to use the filler word "blank" to preserve the order of items they had not forgotten. On counting trials, people saw a computer-generated, random, three-digit number. A filled retention interval of 6 sec (a period that equated the total length of the trial to that of maintenance trials) was generated by decrementing this number by 3s at the rate of one decrement per second (e.g., 574, 571, 568, 565, etc.). Participants had previously learned (and had practiced) that, whenever they saw a number, they were to begin counting backward out loud by 3s as quickly as possible, trying to report their result before it appeared on the monitor. No recall was required on these counting trials.

On critical trials, participants received a typical Brown–Peterson sequence in which a filled retention interval (backward arithmetic, as on counting trials) was followed by a request for recall of the words (as on maintenance trials). There were only five critical trials at three retention intervals in the entire 100-trial sequence. Two trials required retention for 2 sec, two required it for 4 sec, and a fifth critical trial had a 0-sec delay. Following Sebrechts et al. (1989), this 0-sec trial (because it had no delay) was always located as the last trial in the experimental sequence; this procedure was utilized in order to minimize subjects' expectancy for the other four critical trials.[1] The 2- and 4-sec critical trials were fixed at trial locations 20, 40, 60, and 80 within the experimental sequence, subject to the constraint that a critical trial of one duration was never followed by another trial of that duration. A between-subjects factor counterbalanced whether a 2- or 4-sec trial was encountered first. As such, half the participants received a 2, 4, 2, 4 trial sequence, and the other half received a 4, 2, 4, 2 sequence across the critical locations.

Upon conclusion of the final trial, the recognition memory test was administered. People were provided with a two-page test in which new and old items were randomly mixed among four columns printed on each page. People were instructed to circle those words they recognized as having appeared somewhere in the 100-trial sequence.

Instructions and practice. Prior to the experimental trials, written and verbal instructions were given to each person. A practice trial was given consisting of the initial portion of a trial that was identical across all trial types (i.e., the preparatory tone and the presentation of three words). Participants responded to each word according to their assigned task condition. Each participant was then given five practice trials on backward counting by threes. Finally, the "WORDS?" prompt was displayed and people were queried about critical aspects of the procedure.

Results and Discussion

Short-term retention was scored using the same two measures employed by Muter (1980) and Sebrechts et al. (1989). Under a *strict* criterion, responses were considered correct only if all three words were reported in their correct order (both item and order information preserved). Under a *lenient* criterion, words were counted correct regardless of their recalled order. The proportion recalled by both scoring criteria for each of the three encoding conditions is shown in Figure 1. In that figure, each point is bounded by a 95% within-subjects confidence interval (see Loftus & Masson, 1994, for details). Thus, two means differ in that figure when they lie outside of the bounded interval. Traditional analysis of variance (ANOVA) results for the critical tests are summarized in the Appendix.

Short-term memory performance. Two counterbalancing variables are considered first: One specified the critical trial order (i.e., whether a 2- or 4-sec trial was encountered first), and the other specified whether or not people in the semantic and acoustic groups responded "yes" and "no" aloud. As expected, neither factor was significant, nor did either interact with any other factor in a meaningful way. The fact that the responses of participants providing both verbal and keypress responses did not differ from the responses of those who gave only keypresses suggests that phonetic encoding is not affected by any sort of articulatory suppression. Neither counterbalancing factor will be considered further.

As can be seen in Figure 1, short-term retention was quite good in all three conditions at the 0-sec interval. Performance dropped reliably, however, with as little as a 2- or 4-sec delay. This result replicates both Muter's (1980) and Sebrechts et al.'s (1989) findings of rather dramatic forgetting in the absence of expecting a recall test. As in Sebrechts et al.'s earlier studies, short-term retention was generally best in the reading group, followed by the semantic group, and worst in the acoustic group. However, that overall difference in the three conditions largely reflected differences at the 2-sec interval, but not at the longer duration of 4 sec. As is evident from both panels of Figure 1, performance at 4 sec was equivalent among the three orienting groups by either scoring and at floor performance by using the strict criterion. In view of Cunningham et al.'s (1993) concern over an elevated 0-sec retention interval in the Muter paradigm, an obvious interaction exists between the 2- and 4-sec intervals in both panels of Figure 1. Given the reliable differences at 2 sec and the equivalent performance at 4 sec, this interaction suggests that different rates of forgetting are indeed occurring among the orienting conditions within the Muter paradigm, at least over the 2- to 4-sec interval tested.[2]

By having people's responses terminate presentation of the stimuli, and recording their response latency, we obtained a measure of processing demands required by the three orienting tasks, in addition to minimizing primary memory rehearsal. Mean response latency was longest on the critical trials for the acoustic group (937.38 msec), shorter for the semantic group (859.38 msec), and shortest for the reading group [737.15 msec; $F(2,54) = 5.61$, $MS_e = 36{,}520.2$, $p < .01$]. This pattern of latencies is just the reverse of the pattern found for short-term retention.

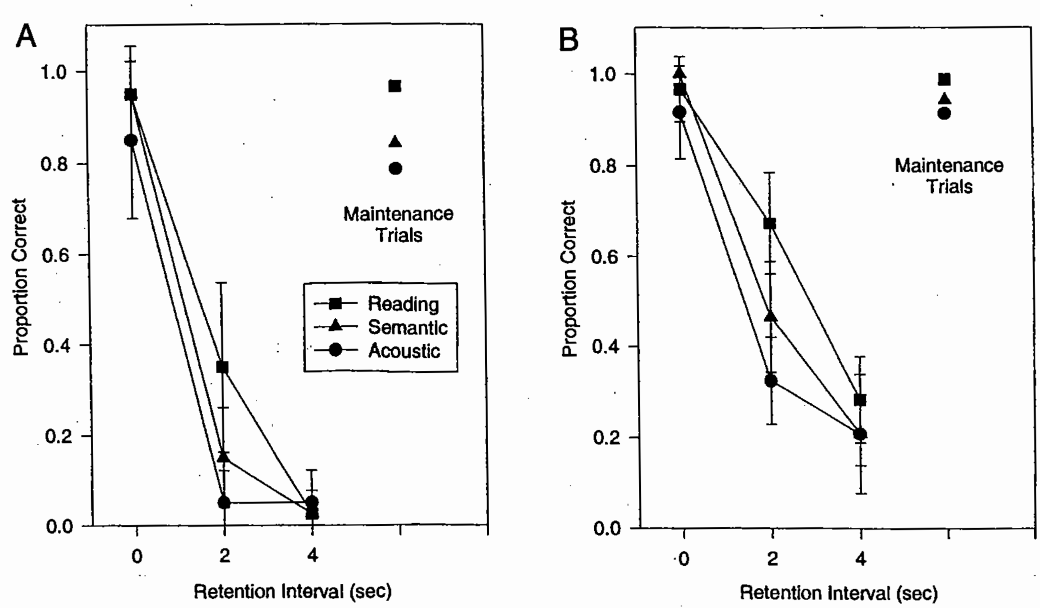

Figure 1. Mean proportion recalled on critical and maintenance trials for each condition in Experiment 1. Bars represent 95% confidence intervals. Panel A depicts a strict scoring and panel B depicts lenient scoring.

That is, the orienting task that demanded the most time (i.e., the acoustic task) produced the worst short-term retention, and the least demanding task (i.e., the reading task) produced the best short-term performance, with the semantic task falling in between. If stimulus offset terminates the orienting processing and prevents additional primary rehearsal (because of the onset of the next stimulus or the distractor task), the short-term performance differences of these groups can be assumed to largely reflect secondary memory contributions, with the exception of the reading group, which might have some echoic traces available after reading the words aloud.

As expected and shown in Figure 1, performance on maintenance trials did differ for the orienting groups depending on whether they were scored for both item and order information or item information alone [strict, $F(2,54) = 13.61, MS_e = .01, p < .001$; lenient, $F(2,54) = 12.36, MS_e = .002, p < .001$]. This pattern mirrors the short-term performance on critical trials discussed previously, in which the reading group outperformed the semantic group, and the semantic group outperformed the acoustic group.

Long-term memory performance. Long-term memory was assessed by means of a standard recognition test administered after the short-term memory tasks. In our prior work, we used a surprise free recall task; however, final recall was generally very low under that procedure, approaching floor performance, and using a recognition task appeared to eliminate that problem. In order to correct for guessing, recall × trial type is presented in terms of d' in Table 1. Although analyses focused on d', the proportion of hits × trial type is also included in that table. As is evident from Table 1, recognition of items at the 0-sec interval was substantially better than for other trials and may not accurately reflect long-term memory because of that trial's fixed position at the end of the trial sequence. For that reason, we limited our analysis to the 2- and 4-sec trials, as well as to overall performance.

Overall recognition of items was best under semantic processing and worst under acoustic processing or when items were merely read aloud [$F(2,54) = 7.80, MS_e = .13, p < .001$]. The same is true of both maintenance [$F(2,54) = 6.83, MS_e = .16, p < .01$] and counting [$F(2,54) = 8.54, MS_e = .22, p < .01$] trials. Performance did not, however, decline from the 2- to the 4-sec retention interval, showing generally equivalent long-term performance on these four trials [$F(2,54) = 2.23, MS_e = .79, p > .10$]. These results were expected and are consistent with both the depth-of-processing literature and with Sebrechts et al.'s (1989) previous work. The pattern of this long-term retention, however, differs from that of short-term remembering. The fact that the reading group was best in the short term, whereas the semantic group was best in the long term, supports our earlier results of different processing consequences for the same orienting task in short-term and long-term retention (see also Mazuryk, 1974).

In summary, the stimulus termination manipulation did not change the pattern of short-term or long-term recall from that reported previously (Sebrechts et al., 1989). This result suggests that previously reported differences associated with orienting task reflect secondary memory contributions that were not dependent on available processing time or stimulus rehearsal. In fact, in the present study, short-term recall was inversely related to the time required to complete the orienting task.

EXPERIMENT 2

Experiment 1 largely replicated Sebrechts et al. (1989), using response termination instead of a fixed duration for the orienting tasks. The outcome of Experiment 1 supports the notion that effects of orienting condition are not dependent on rehearsal. In the case of the "shallow" acoustic task, however, the response-terminated group took almost as long as the fixed 1-sec group tested by Sebrechts et al. As a consequence, there was little opportunity to find any evidence of a rehearsal effect for the acoustic condition. In Experiment 2, we addressed this issue by comparing response-terminated and fixed-duration conditions for a much simpler, shallow orienting task that required substantially less time than 1 sec to complete. This new orienting task required people to determine whether or not items were printed in uppercase. Pilot work suggested that this task required less time than any of the orienting tasks used in Experiment 1. Thus, we hypothesized that this task would maximize the additional time available for primary memory rehearsal in the condition where stimulus presentation is fixed at 1 sec. If rehearsal time itself is critical, then the fixed condition should show an advantage in short-term retention. In contrast, if the orienting task is the primary determinant of short-term retention, the prediction would be that little difference between the two conditions should be observed.

Table 1
Recognition Memory Performance as d' and Proportion Correct (PC) for Each Orienting Condition and Trial Type in Experiment 1

	Trial Type											
	Critical Trials						Maintenance Trials		Counting Trials		Overall	
	0-sec		2-sec		4-sec							
Condition	d'	PC	d'	PC	d'	PC	d'	PC	d'	PC	d'	PC
Reading	2.02	.67	1.41	.53	1.11	.44	1.24	.49	.92	.39	1.21	.47
Semantic	2.81	.85	1.43	.55	1.47	.57	1.67	.64	1.43	.55	1.61	.62
Acoustic	2.84	.83	.90	.40	1.17	.45	1.27	.47	.91	.35	1.24	.45

Method

Participants. Forty introductory psychology students volunteered in return for partial fulfillment of a course requirement. Participants were drawn from the same pool as those in Experiment 1, but none had participated in that study. People were randomly assigned to one of two groups on the basis of the order in which they arrived at the laboratory. They were assigned to either a variable or fixed group, detailed below. All participants were tested individually.

Materials, Design, Apparatus, and Procedure. The materials and apparatus were those used in Experiment 1. The same word list and 10 different stimulus orders were randomly assigned to 2 people in each of the two conditions. The stimuli differed from those of Experiment 1 in only one way: Half of the words were printed in uppercase and half were printed in lowercase. In addition, there was no correlation between letter case used here and the dimensions of animacy or long *e* sound used previously. For the conditions of interest, the basic design was a 2 × 3 mixed factorial with the first factor (between-subjects) representing the duration of stimulus presentation. The individual responses of the variable group terminated stimulus presentation, just as in Experiment 1. The responses and latency of the fixed group were recorded, but the words appeared for one full second, as in Sebrechts et al. (1989). The second factor manipulated retention interval on critical trials, and, as in Experiment 1, these durations were set at 0, 2, and 4 sec. A third, counterbalancing, factor specified whether a 2- or 4-sec critical trial was encountered first. With the exception of the orienting task that people performed, the procedure was identical in all other respects to that of Experiment 1. Rather than making judgments of animacy or acoustic qualities of the stimuli, all participants made yes/no judgments about whether the words were printed in uppercase.

Results and Discussion

As in Experiment 1, recall was scored by both strict (item and order information) and lenient (item information only) criteria. The proportion recalled by both scoring criteria for each of the two conditions is shown in Figure 2, which features the same conventions as those adopted in Figure 1.

Short-term memory performance. As is evident in both panels of Figure 2, recall did not reliably differ between the variable and fixed presentation groups; nor did the two groups perform differently over the three critical retention intervals. As expected, however, there was a marked decline in performance as retention interval increased. For the 2- and 4-sec intervals, there was no decline by the strict criterion because of floor performance; but the decline was reliable over the 2- to 4-sec intervals when it was scored leniently. Performance on maintenance trials was not affected by the manipulation of extra processing time. Similarly, response latency on the critical trials did not differ between the fixed (566.86 msec) and variable (586.71 msec) groups [$F(1,36) < 1$, MS_e = 12,363.9]. The fact that the recall performance of the groups did not differ suggests that participants in the fixed group did not use the additional time (approximately 415 msec/word) to process the stimuli in a manner that would enhance short-term retention, such as rehearsing previously presented items.

Long-term memory performance. The results of the recognition memory test are given in Table 2 in terms of d' (and the proportion of hits) for each trial type, as well as in terms of overall performance. Not surprisingly, recognition performance was rather poor with this letter-case manipulation. As can be seen in Table 2, the fixed group did perform slightly better overall than the variable group, but the advantage was small and not reliable [$F(1,36) = 3.19, MS_e = .13, p > .08$]. The same small but unreliable advantage was found on maintenance trials for the fixed group [$F(1,36) = 3.16, MS_e = .15, p > .08$]. There was also no evidence that the two groups differed on the 2- and 4-sec critical trials [$F(1,36) = 1.89, MS_e = .57, p > .10$]. Interestingly, the fixed group did show an

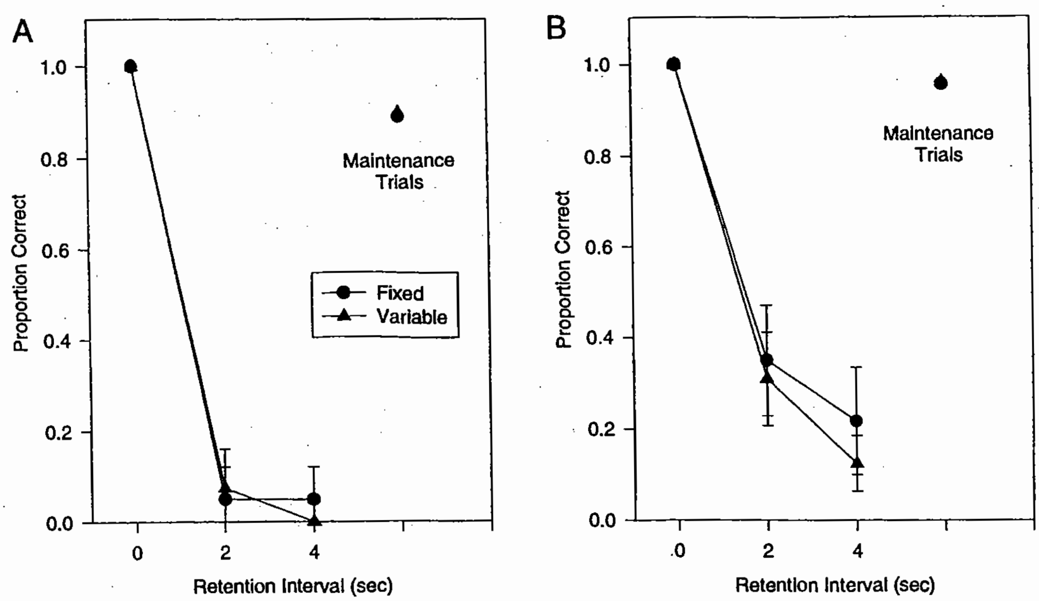

Figure 2. Mean proportion recalled on critical and maintenance trials for each condition in Experiment 2. Bars represent 95% confidence intervals. Panel A depicts a strict scoring and panel B depicts lenient scoring.

advantage on the counting trials compared with the variable group [$F(1,36) = 5.07$, $MS_e = .20$, $p = .03$].

Although extra processing at the time of encoding might have resulted in either better short-term or long-term retention, participants in the fixed condition did not generally show a reliable or robust advantage in either. Thus, that additional time was not or could not be used in an efficient manner to greatly benefit recall.

GENERAL DISCUSSION

The experiments reported here support the notion that, in the absence of expectancy, short-term recall reaches asymptote much sooner than when recall is expected. Sebrechts et al. (1989) suggested that even in the absence of expectancy, there were differences in recall due to secondary memory contributions, as operationalized by orienting task. One possible basis for those differences is that the orienting tasks differed in terms of the encoding time they took to complete. Experiment 1 confirmed that the orienting tasks did require different encoding times. In that experiment, short-term recall was inversely related to the amount of time required to perform the orienting task. From a different perspective, short-term recall in our previous studies might have been directly related to the amount of additional time remaining after a stimulus was encoded.

In Experiment 1, we attempted to eliminate the additional processing time after encoding a stimulus word by using a response termination manipulation. Under those conditions, the order of orienting task effects on short-term recall was generally preserved: Reading was best, semantic followed, and the acoustic task resulted in the worst performance. For long-term retention, the order also remained unchanged under conditions of response termination. The semantic condition was best, followed by reading and the acoustic task. Thus, encoding time was not a good predictor of long-term memory in that task.

Because the response termination condition for the shallow task was shown to take almost as long as a fixed 1-sec duration, a less demanding "shallow" task might still benefit from rehearsal. To test that possibility, in Experiment 2 we used a shallow graphemic task that was unlikely to support more elaborative encoding. If rehearsal were taking place, the fixed condition should have shown better performance than the response-terminated condition, but performance was equivalent for the two groups. Thus, the simple addition of more processing time was not adequate to produce significantly better short-term recall. A simple rehearsal model does not appear adequate to explain the effects of an orienting task on short-term retention. Moreover, the present results suggest that there is no benefit to providing opportunities for rehearsal except as it relates to maintenance processing on the maintenance trials (cf. Muter, 1995). Although premature, a tempting conclusion to draw from these data, especially from those of Experiment 2, is that primary memory rehearsal does not take place or is unimportant to the assessment of short-term retention in the Brown–Peterson and Muter paradigms that use adequate distractor techniques. This conclusion is also consistent with Healy et al.'s (1987) finding that differences between precued and postcued groups were best explained by holding the contribution of primary memory rehearsal constant and varying the contribution of secondary memory.

The data reported here confirm "very rapid forgetting" in the sense that the availability of item and order information is lost substantially more rapidly than predicted by the Brown–Peterson task. Sebrechts et al.'s (1989) earlier studies and those conducted by Muter (1980) have shown that the rate of forgetting was greater under conditions of low versus high expectancy. In contrast, Cunningham et al. (1993) found no difference in the rate of forgetting under conditions of low versus high expectancy. They suggested that the crucial difference between the two paradigms was in the 0-sec interval. Performance at this interval of the Muter paradigm might be artificially elevated because people's expectancies could be quite high if they were anticipating a maintenance trial that required recall of the word triad. Muter (1995) noted, however, that there were a number of important differences between his (and our) procedure and that used by Cunningham et al. In particular, he noted that both data and theory demonstrate that when people form secondary memory traces, those traces facilitate recall only after a filled retention interval and actually have a negative impact on immediate recall (e.g., Mazuryk, 1974; Mazuryk & Lockhart, 1974; but see Healy & Cunningham, 1995). The results reported here do not contradict that view. In addition, when the 0-sec trial was ignored, evidence was still found for different rates of forgetting that depended on the orienting task that people were assigned in Experiment 1 (Figure 1).

The phenomena reported from Muter's, from Healy and her colleagues', and from our laboratories are substantially similar. All studies show very poor retention at short intervals under conditions in which there is no ex-

Table 2
Recognition Memory Performance in Terms of *d′* and Proportion Correct (PC) for Each Orienting Condition and Trial Type in Experiment 2

	Trial Type											
	Critical Trials						Maintenance Trials		Counting Trials		Overall	
	0-sec		2-sec		4-sec							
Condition	*d′*	PC	*d′*	PC	*d′*	PC	*d′*	PC	*d′*	PC	*d′*	PC
Variable	1.72	.57	.65	.27	.83	.30	1.11	.38	.64	.24	1.04	.35
Fixed	1.95	.58	.90	.31	1.05	.36	1.33	.41	.96	.28	1.24	.38

pectation of a recall test, regardless of performance at the 0-sec interval. The "rate" issue is perhaps best addressed by a direct comparison of the Muter and Brown–Peterson paradigms, which we previously reported showed different rates of forgetting (Sebrechts et al., 1989, p. 698). These similarities aside, the nature of the underlying model of short-term retention remains somewhat unclear. Cunningham et al. (1993) argued that only the secondary memory parameter was influenced by expectancy, not the primary memory parameter of their modified Estes model. In that sense, the forgetting rate from primary memory was constant, but the influence of secondary memory was changed by expectancy. Comparison of the two paradigms is difficult. However, one major difference is that Healy and her colleagues have primarily emphasized order information with their cuing paradigm. In that paradigm, the two letter strings are always composed of the same four letters on each trial (e.g., *BFHK* or *LMQR*). If temporal order tags are required for specifying order information on each trial, those tags are probably exclusively dependent upon primary memory. If so, any persistence of order information beyond the current trial may generate a form of interference that is very much unlike the proactive interference people experience in a Muter paradigm. For example, proactive interference in a Brown–Peterson paradigm is known to asymptote after the first several trials (Keppel & Underwood, 1962). Problems in comparing the two paradigms are further exacerbated by the fact that when performance is scored in the Muter task for both item and order information (i.e., a strict criterion), the results often approach floor performance, as they did in this report.

The present results do support the findings of Muter, Healy, Cunningham, and their colleagues that secondary memory components contribute to short-term recall, and that lacking expectancy for a recall test reduces those contributions. In addition, the data reported here indicate that expectancy is not the only contaminant of secondary memory to measurement of short-term retention. Changes in orienting task influence recall under conditions of both expectancy and nonexpectancy. One of the goals of the present experiments was to determine whether or not the effects of orienting task might reflect other primary memory rehearsal strategies. By eliminating the additional time between the completion of the orienting task and the onset of the next stimulus, a rehearsal account of orienting task effects was largely ruled out.

At the same time, the finding of a difference in the rate of forgetting between 2 and 4 sec that depended on orienting task suggests that a simple additive model of primary and secondary memory components may not be an adequate explanation of these data. As in Sebrechts et al. (1989), the patterns of recall × orienting task differed for short-term and long-term retention. Thus, orienting task, unlike expectancy, does not appear to simply change the overall level of resource allocation, but may instead influence the relative utility of resource allocation for short-term and long-term recall.

More generally, the dichotomy of primary versus secondary contributions may not even be an efficient way of thinking about short-term memory, as evidenced by the relative success of simulations of primary memory as a process (e.g., Barnard, 1985; Schneider, 1993; Schneider & Detweiler, 1987). If resource allocation alters that process in some way, there is no single measure of "pure" primary memory, but instead a convergence of evidence clarifying that process depending on the method for influencing resources. We suspect that the two lines of research demonstrating secondary memory influences in a paradigm that was once thought to measure only primary memory are less far apart theoretically than they are methodologically. Methodology aside, the major issue facing both paradigms is determining whether a dichotomous model composed of primary and secondary memory components is sufficient to explain both sets of results. The results described in this report suggest that it may not, but our experiments were not designed to directly answer that question. Currently, we are examining new techniques for isolating and manipulating both components in an effort to reconcile the results of these two lines of research.

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NOTES

1. In our earlier work, we experimented with a different location for the 0-sec critical trial. We quickly discovered, however, that the deviation from the other trials was so great that participants reported "being on guard" for the remainder of the experiment. We cannot fathom why "tricking" people at the 2- and 4-sec trials does not have the same subjective impact.

2. In our previous work, we acknowledged that statistical interactions may not be the optimal way to evaluate forgetting rates (see, e.g., Loftus, 1985). Alternative methods require a priori adoption of a particular model of forgetting. However, even if we had adopted these alternative measures, rather than using the "customary" procedure, the same conclusion would have resulted: Given an orienting task that produces greater initial recall, it takes longer for memory performance to fall from a given level to a lower level.

APPENDIX
Analysis of Variance Results for Experiments 1 and 2

		Strict			Lenient		
	df	*F*	*p*	MS_e	*F*	*p*	MS_e
		Experiment 1					
0-, 2-, 4-sec Critical Trials							
Task	2,54	3.03	.05	0.08	5.89	.01	0.06
Duration	2,108	267.30	.00	0.05	246.66	.00	0.03
Interaction	2,108	3.02	.02	0.05	4.77	.00	0.03
2-, 4-sec Critical Trials Only							
Task	2,54	3.33	.04	0.06	6.60	.00	0.04
Duration	1,54	18.00	.00	0.04	54.41	.00	0.03
Interaction	2,54	7.17	.00	0.04	5.25	.01	0.04
		Experiment 2					
0-, 2-, 4-sec Critical Trials							
Task	1,36	0.16	.69	0.01	1.71	.19	0.03
Duration	2,72	864.44	.00	0.01	262.50	.00	0.03
Interaction	2,72	1.03	.36	0.01	0.71	.49	0.03
2-, 4-sec Critical Trials Only							
Task	1,36	0.16	.69	0.02	1.71	.19	0.05
Duration	1,36	1.29	.26	0.02	12.01	.000	0.04
Interaction	1,36	1.29	.26	0.02	0.30	.59	0.04

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