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Time-Based Loss in Visual Short-Term Memory is from Trace Decay, not Temporal Distinctiveness

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Abstract

There is no consensus as to why forgetting occurs in short-term memory tasks. In past work, we have shown that forgetting occurs with the passage of time, but there are two classes of theories that can explain this effect. In the present work, we investigate the reason for time-based forgetting by contrasting the predictions of temporal distinctiveness and trace decay in the procedure in which we have observed such loss, involving memory for arrays of characters or letters across several seconds. The first theory, temporal distinctiveness, predicts that increasing the amount of time between trials will lead to less proactive interference, resulting in less forgetting across a retention interval. In the second theory, trace decay, temporal distinctiveness between trials is irrelevant to the loss over a retention interval. Using visual array change detection tasks in four experiments, we find small proactive interference effects on performance under some specific conditions, but no concomitant change in the effect of a retention interval. We conclude that trace decay is the more suitable class of explanations of the time-based forgetting in short-term memory that we have observed, and we suggest the need for further clarity in what the exact basis of that decay may be.

Keywords

Short-Term Memory; Working Memory; Forgetting; Decay; Temporal Distinctiveness

The last decade has seen vigorous debate about the factors that lead to forgetting from short-term memory. Claims have been made against a role of memory loss as a function of time

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³At first glance the following alternative explanation of why we did not observe any temporal distinctiveness effects that seems plausible and deserves a mention. Verbal interference during the inter-trial interval could have led to disruption of PI from the previous trial, which would otherwise cause temporal distinctiveness effects. This potential interference could have been produced by presentation of the phrase “The next trial will start shortly” on the screen during the ITI (see Figure 4). This would be a concern if we had not observed time-based forgetting in our experiments, but that was not the case. The logic of why any potential interference from that verbal instruction is not a problem is the following. Assume that the phrase during the ITI led to abolition of any temporal distinctiveness effects we would otherwise have observed. The key point is that we still observed time-based forgetting despite the abolition of any PI that temporal distinctiveness models claim causes time-based forgetting. We therefore can assume, at least as a viable working hypothesis, that trace decay leads to the forgetting we observed in the present paradigm. It is possible that if we had not had the verbal phrase present during the ITI that there would have been more forgetting but, of the hypotheses under consideration, the effect of RI that we observed here is only consistent with trace decay.

and, instead, in favor of interference as the sole cause of forgetting (Lewandowsky, Duncan, & Brown, 2004; Lewandowsky, Oberauer, & Brown, 2009; Oberauer & Lewandowsky, 2008). Many of the classic effects in the short-term memory literature can be explained based on item-based interference alone (e.g., Bjork & Whitten, 1974; Jalbert, Neath, Bireta, & Surprenant, 2011; Keppel & Underwood, 1962). However, some recent data apparently are explained best with recourse to time-based forgetting (Altmann & Schunn, 2012; Barrouillet, De Paepe, & Langerock, 2012; Portrat, Barrouillet, & Camos, 2008). In several recent studies, moreover, time-based forgetting was directly observed (McKeown & Mercer, 2012; Ricker & Cowan, 2010, 2014; Zhang & Luck, 2009). Here we seek to clarify the basis of discrepant results regarding memory loss over time. In particular, we ask whether time-based forgetting can itself be explained by one kind of proactive interference (PI), namely temporal distinctiveness effects.

It has long been known that, when rehearsal processes are prevented, one can observe dramatic loss over time (e.g., Brown, 1958; Peterson & Peterson, 1959). Researchers argued for many years about whether this loss over time was the result of decay or of a loss of temporal distinctiveness of the materials from present trial, which can be confused with previous trials (e.g., Bjork & Whitten, 1974; Brown, Neath, & Chater, 2007; Crowder, 1976; Glenberg & Swanson, 1986; Keppel & Underwood, 1962). In a surprising twist, Lewandowsky and colleagues found no forgetting over time when retroactive interference was eliminated. Lewandowsky et al. (2004) presented lists for serial recall with each item in recall separated by one or three iterations of the word *super* spoken by the participant, and found no significant difference between conditions despite more time for loss in the three-iteration condition. The followup work (e.g., Oberauer & Lewandowsky, 2008) showed little effect of time on forgetting even when attention and articulation both were engaged during this time. The implication was that all loss of memory over retention intervals in previous studies resulted from retroactive interference from the items used to prevent rehearsal or reactivation of the items during the retention interval (RI).

In contrast to the findings of Lewandowsky and colleagues, Ricker and Cowan (2010) did find forgetting across time. On each trial, they presented a spatial array of unfamiliar characters followed by a variable, unfilled delay and then a probe item to be judged present in the array or absent from it. They found that the probability of successfully maintaining an array of visual items in memory decreased as the length of the RI increased to 6 s. Although decreased accuracy with longer retention times is nothing new (see, Brown, 1958; Cowan, 1995; Peterson & Peterson, 1959), Ricker and Cowan observed time-based forgetting in the absence of any secondary task during the RI. During trials that contained no secondary task, and therefore no experimenter-induced retroactive interference, the only external factor that could account for the loss in performance was the passage of time. Generally, secondary tasks are required to prevent verbal rehearsal and other strategy usage, but in this study participants were asked to remember unfamiliar characters that could not be rehearsed (Ricker, Cowan, & Morey, 2010). Related results were obtained by McKeown and Mercer (2012) using auditory memory items composed of a complex timbre, and by Zhang and Luck (2009) using spatial arrays of colored squares or shapes differing in the degree of rotation.

A reason for the discrepancy between the results of the studies by Lewandowsky and colleagues versus Ricker and Cowan (2010) was suggested by Ricker and Cowan (2014). They found that a very important factor in determining whether memory was lost across some seconds was the amount of time available to think about each item without distraction before an additional stimulus requiring attention was presented, i.e., consolidation time. In fact, consolidation time was important no matter whether the items were presented one at a time or all at once.

Ricker and Cowan (2014) did not determine exactly what processes were carried out during each consolidation period. The present work begins to provide an answer to that further question. We did not manipulate the consolidation period but instead used a timing expected to produce loss over the RI while manipulating the time between trials, or inter-trial interval (ITI). We presented spatial arrays of 3 unfamiliar characters (Experiments 1 and 3) or arrays of 6 English letters (Experiments 2 and 4) and manipulated the schedule of events preceding each trial. According to one straightforward hypothesis, the consolidation period should be taken up primarily with processing of the stimuli just presented. Another possibility, though, is that the stimuli that were just presented need to be retrieved after the RI, and that these stimuli can become confused in memory with the stimuli presented on previous trials. Perhaps what participants do during consolidation to prevent memory loss is to separate the memory of the current trial's stimuli from those of previous trials. If so, then lengthening the preceding ITI should reduce forgetting across the RI.

The rationale for this prediction can be gleaned from past literature. According to a well-known temporal distinctiveness hypothesis (e.g., Bjork & Whitten, 1974; Brown et al., 2007; Crowder, 1976; Glenberg & Swanson, 1986), the confusion of information from the present trial with information on previous trials should increase as the RI increases. Crowder offered the analogy of a row of telephone poles, with each pole representing the memoranda on a trial and distance representing time. When one is standing just beyond the last pole, it is very distinguishable from previous poles, whereas when one moves further back from the last pole, all of the poles begin to blend together. According to this analogy, the difficulty in retrieving the items from the last spatial array in our study should be that this array can become temporally indistinct from the stimuli in the preceding trial or trials. If one places more distance between the last two poles (i.e., increases the ITI), it should be possible to stand back further (i.e., to increase the RI) without causing confusion between the last two poles (or trials).

Ricker and Cowan (2010, 2014) allowed participants to initiate each trial almost immediately after the termination of the previous trial, so they were not far apart in time. In the first two experiments within the present study, we inserted a waiting period of 1, 6, or 12 s between trials. The within-trial RIs fell in a range roughly comparable with the ITIs. The question is whether longer ITIs will result in slower rates of forgetting across RIs than are found with the shorter times. One would expect so if time-based forgetting occurs because of confusion in memory between the stimuli presented in successive trials as in the temporal distinctiveness hypothesis. If this is not the finding, then the consolidation period presumably is used to process the present trial's stimuli, and future work will need to be done to determine just what processes do take place during consolidation of items in

memory. The term *decay* may then apply to the results, even though there are several different exact mechanisms that we subsume under that term, to be discussed (Cowan, Saults, & Nugent, 1997; Zhang & Luck, 2009).

It seems important to provide terminology to serve as a bridge between data and theory. In our presentation of results, we use the term *temporal distinctiveness effect* to refer to an interaction of the RI on the current trial with the immediately preceding ITI (specifically, more forgetting across RIs when the previous ITI was shorter). It is also possible to have a main effect of ITI that does not interact with RI, meaning that although an ITI effect exists, it does not modify the rate of forgetting across an RI. We refer to that as a *trial spacing effect*, for which a different theoretical interpretation must be suggested.

Experiment 1

The first experiment was done using unfamiliar characters as in the studies of Ricker and Cowan (2010, 2014). Unlike those experiments, the waiting time or ITI before each trial was randomly set at 1, 6, or 12 s, and the RI was randomly set at 1, 6, or 12 s as well (see Figure 1). These values are meant to impose a wide range of waiting periods without being so long as to challenge participants' ability to remain on task. The question was simply whether the longer ITIs would result in a reduction of time-based forgetting across RIs, as predicted by temporal distinctiveness approaches to memory.

Method

Design—Experiment 1 used a 3 (Retention-Interval Duration) $\times$ 3 (Temporal Spacing) design. Temporal Spacing was varied by changing the length of the ITI. ITI and RI were varied randomly on each trial, with the constraint that there was an equal number of each ITI and RI combination.

Participants—Seventy-two college students (46 female, 26 male, ages 18-25) at the University of Missouri participated in the experiment in exchange for partial course credit. After removing participants with at least 20% of their trials rejected for RTs outside of the acceptable range 68 participants remained. Filtering the data in this way produced results that lead to the same conclusions as if we had used all trials from all participants. No participants spoke or read any of the languages from which the memory stimuli were taken or lived in any of the countries in which they may have been exposed to the figures used in the experiment on a regular basis. All participants had vision which was normal or corrected-to-normal.

Materials—The stimuli were presented to participants on standard cathode-ray tube monitors while seated in a quiet room. Participants sat at a comfortable distance from the screen and made responses by button press on a keyboard. All stimuli were black figures presented on a grey background. These items were 231 figures from written languages that did not resemble the English alphabet, numerals used in English, or other figures likely to be familiar to students attending a university in the rural United States. This stimulus set was used to ensure that participants could not easily verbally encode the figures. We selected the figures by searching through a text database for figures that did not resemble any English

letter, number, shape, or other common symbol. We then rotated these figures 90 degrees to further reduce any familiarity. Several researchers looked through the character set and removed any items they felt resembled a known or easily labeled figure or shape prior to finalizing the set. Ricker et al. (2010) used a similar item set and demonstrated that verbal recoding of the stimuli did not contribute to memory performance. In the present study, each stimulus was presented in an area of roughly 2.3×2.3 degrees of visual angle. The entire memory array was presented within an area at the center of the screen subtending roughly 15.3×11.5 degrees of visual angle.

Procedure—In this experiment the ITI lengths were 1, 6, or 12s. The RI lengths were also 1, 6, or 12s. The RI and the ITI preceding a trial were determined quasi-randomly on each trial. The departure from random selection was that each combination of RI and ITI had to occur an equal number of times in each block. Participants completed 10 practice trials and 4 blocks of 36 experimental trials.

Each trial began with an ITI of 1, 6, or 12 s. After the ITI a fixation cross appeared on the screen for 500ms, followed by the memory array which remained on the screen for 750 ms. The memory array always consisted of 3 unfamiliar characters. Offset of the memory array was followed by a blank screen with only the fixation cross presented for 250 ms and then a post-perceptual pattern mask for 100 ms. This mask consisted of the symbols “>” and “<” superimposed on top of one another at the position of each of the objects in the memory array. We use this post-perceptual pattern mask to overwrite any lingering sensory memory traces which might otherwise be used to perform the task. In this way we can be sure that any time-based effects we observe can be attributed to short-term memory processes and not sensory memory processes (see Saults & Cowan, 2007). Next was the RI, which lasted 1, 6, or 12 s. After the RI a single item probe was presented. The participant was to push the “s” key if the item was the same as the item presented in that position in the memory array, or the “d” key if the item was different. A different item was presented on half of the trials. When the item was different it was randomly selected from the entire set of 231 items with the constraint that it could not be an item presented at a different position in the memory array. Finally, participants received feedback informing them whether they responded correctly or incorrectly. Feedback was given by placing either the word “correct” or “incorrect” on the screen for 750 ms. Correct feedback was accompanied by a high-pitched beep, while incorrect feedback was accompanied by a low-pitch beep.

Data Analysis

In this paper we use Bayes factors in the context of a standard ANOVA as our primary inferential statistic. Bayes factor is an assessment method from Bayesian statistics that has been recommended for inference in the psychological literature (Edwards, Lindman, & Savage, 1963; Rouder, Speckman, Sun, Morey, & Iverson, 2009; Wagenmakers, 2007). The Bayes factor is the probability of the data under one hypothesis relative to the same probability under an alternative hypothesis. In addition to its superiority over p-values as a tool of inference, Bayes factor also has the pragmatic advantage of being able to provide evidence in favor of the null hypothesis. This is helpful here as any evidence for trace decay and against temporal distinctiveness explanations of time-based forgetting will be based on

acceptance of a null effect of temporal distinctiveness along with a non-null effect of the retention interval per se.

We follow the developments in Rouder, Morey, Speckman, and Province (2012), who address Bayes factor analyses in factorial designs, and perform computations with the “BayesFactor” package (Version 0.8.8; Morey & Rouder, 2012) for the R statistical-analysis language. Bayes factor computations are predicated on specifying a reasonable range of possible effect-size values under alternatives, and we used the default settings in the package with a standard deviation of effect size at $(2)/2$. It is important to note that the F-statistics and other ANOVA results are not changed by the additional computation of Bayes Factors.

In addition to our analysis of proportion correct we also consider our data in terms of k , the number of items stored in working memory (Cowan, 2001). This measure can be helpful for thinking about the impact of the manipulations on cognitive processes and because it takes into account guessing when determining how many items a participant has maintained in working memory. This measure is based on the notion that an individual can answer correctly if the probed item is stored in working memory at the time of the test, and otherwise must guess. The resulting formula is $k = (\text{Hit Rate} + \text{Correct Rejection Rate} - 1) \times \text{Memory Set Size}$. All analyses in terms of k ended up showing an identical pattern of results to the analysis of proportion correct so we forgo reporting statistical results for k and instead simply report condition means for the interested reader.

Results

Mean accuracy for each condition is presented in Figure 2. Visual inspection of mean performance levels reveals a decrease in accuracy as the RI increases during each ITI condition. Performance in the 1s ITI condition was worse than the 6s ITI condition for each RI duration. The pattern of performance was less clear for the 12s ITI, but similar to the 6s ITI condition. Mean performance for all conditions in terms of Cowan's k can be found in Table 1.

The 2-way within-participant Bayes factor ANOVA analysis produces a main effect of Retention-Interval Duration (means: 1s= 0.81, 6s=0.75, 12s=0.71), $F(2,134)=47.86$, $p<.001$, $\eta_p^2=0.42$. The corresponding Bayes factor is 7.25×10^{17} , meaning that the data are well beyond a trillion times more probable under a model with this main effect than one without it. A main effect of Temporal Spacing was observed (means: 12s=0.76, 6s=0.78, 1s=0.74), $F(2,134)=6.36$, $p<.01$, $\eta_p^2=0.09$. The corresponding Bayes factor is 15.04 in favor of the alternative. That is, the data are about 15 times more probable under a model with a main effect than under the null.¹ The observed interaction of these effects is tiny, $F(4,268)=1.46$, $p>.2$. (See Figure 2 for each cell mean.) The Bayes factor for this contrast is 52.87 in favor of the null.

¹We also tested whether the ITI between trials N-1 and N-2 had any effect on accuracy using a 3 (Recent ITI) $\times$ 3 (Past ITI) within-participant Bayes factor ANOVA of proportion correct. This analysis produces a small main effect of Recent ITI (means: 12s=0.77, 6s=0.80, 1s=0.74) with a Bayes factor is slightly more than 15 in favor of an effect. There was no observed effect of Past ITI, with a Bayes factor is 85 in favor of the null. The observed interaction of these effects was also miniscule, with a Bayes factor for this contrast is 82 in favor of the null. Very similar results were observed in Experiment 2.

While the Bayes Factor indicates considerable evidence for an effect of Temporal Spacing, visual inspection of the means in Figure 2 shows that this effect may not be present across the full range of ITI lengths analyzed. If there is an effect of Temporal Spacing at some time intervals and not others it would indicate problems with both a simple decay and a temporal distinctiveness explanation of the results.

In order to examine this potential issue we conducted two separate 2-way within-participant Bayes factor ANOVA analyses of ITI length, one examining the effect of Temporal Spacing across only the 1 and 6s ITI lengths and a second examining the effect of Temporal Spacing across only the 6 and 12s ITI lengths. The analysis using only the 1s and 6s ITIs produced a sizable main effect of Temporal Spacing $F(1,67)=10.85$, $p<.01$, $\eta_p^2=0.14$. The corresponding Bayes factor is 122.31 in favor of the alternative. In contrast, the analysis using only the 6s and 12s ITIs demonstrated only a tiny effect of Temporal Spacing $F(1,67)=1.89$, $p>.1$, $\eta_p^2=0.03$. The corresponding Bayes factor is 4.44 in favor of the null. These additional analyses demonstrates very strong support for a difference in accuracy when ITI-length is increased from 1 to 6s but moderate evidence against an effect of increasing the ITI-length from 6 to 12s. This result is in conflict with the predictions of both temporal distinctiveness theories and pure decay theories.

Examination of the hit (correctly noticing a change) and correct rejection (correctly noticing that there was no change) rates gives some suggestive evidence of what cognitive processes lead to the overall accuracy results. While the hit rate improved with the longer retention interval (means: 1s=.73, 6s=.84, 12s=.85), the correct rejection rate plummeted with the longer retention interval (means: 1s=.89, 6s=.67, 12s=.57). The pattern of hit and correct rejection rates across the random ITIs is much less impressive for both hit (means: 12s=.82, 6s=.82, 1s=.79) and correct rejection (means: 12s=.71, 6s=.73, 1s=.69) rates.

Discussion

One can conclude from this experiment that, although there was an effect of the ITI, it did not interact with the retention interval in a manner that would be needed by the temporal distinctiveness hypothesis. The mean accuracies (Figure 2) show practically parallel loss functions across RIs for 1- and 6-s ITI conditions. For a 12-s ITI the loss was, if anything, greater than it was at the shorter ITIs. If this long ITI were helpful to eliminate confusion due to longer RIs, it should produce an RI forgetting function that is shallower than for shorter ITIs, and the trend is in the opposite direction. Although there may be some effect of PI present, it is clearly not of the form predicted by temporal distinctiveness accounts and does not explain the loss over time observed here.

Further support for a decay interpretation of our results over a temporal distinctiveness interpretation comes from examination of the hit (correctly noticing a change) and correct rejection (correctly noticing that there was no change) rates reported in the results section. A straightforward interpretation of the pattern we observed is that participants quickly forgot the items across RIs and therefore, more often at the longer RI, failed to recognize the same memory item when it was presented again at test on no-change trials. In this circumstance, failing to recognize the old item would tend to lead to a false alarm (failure to make a correct rejection). On change trials however, performance may have remained constant

across RIs for essentially the same reason. Not recognizing an old item, participants would be likely to respond that the probe item had changed to a new item, which in this case was correct (a hit). The data suggest that forgetting across RIs is a matter of diminishing array-item memory, rather than diminishing change detection per se.

Rather than a temporal distinctiveness explanation, what seems more likely, given these data, is an event segmentation process such as that proposed by Zacks & Swallow (2007). In this style of PI model items are maintained in short-term memory until a new event occurs. At the time a new event is perceived, items in short-term memory are offloaded into the general long-term store. If our short 1s ITI was perceived as unexpectedly short in the context of randomly presented 6s and 12s ITIs, it may be that the 1s ITI sometimes caused the event segmentation step between trials to be missed. PI would then be increased on these trials due to the presence of information from the previous trial in short-term memory. In this model the PI effect we observed and the time-based forgetting effect have different causes: failed event segmentation and trace decay, respectively.

Our interpretation of an event segmentation approach is one example of why only our shortest ITI would lead to decreased performance. Regardless of the accuracy of this explanation, it is clear that there is no interaction between RI and ITI effects, meaning temporal distinctiveness should not be the explanation of choice for explaining forgetting across a retention interval.

Experiment 2

The results of Ricker and Cowan (2010, 2014) and those of the present Experiment 1 were all obtained with unfamiliar characters. Given that these materials are so different from those used in the rest of the short-term memory literature, the next experiment was conducted to determine whether these results could be extended to arrays of English letters. We added articulatory suppression (Baddeley, 1986) to avoid rehearsal of the verbal items in the otherwise blank RI. Based on previous work, we found that we could stay in approximately the same range of performance as Experiment 1 by replacing the 3 unfamiliar characters with 6 English letters.

Method

Design—The design was the same as Experiment 1, with the addition of articulatory suppression. Articulatory suppression, consisting of repeating the word “the” at a rate of 2 times per second, started at offset of the mask, and continued until the onset of the test screen. An experimenter sat in the testing room with the participant to monitor compliance.

Participants—Fifty-three college students (26 female, 27 male, ages 18-22) at the University of Missouri participated in the experiment in exchange for partial course credit. After removing participants with at least 20% of trials rejected for RTs outside of the acceptable range of 300-3000ms 52 participants remained. Filtering the data in this way produced results that lead to the same conclusions as if we had used all trials from all participants. All participants had vision which was normal or corrected-to-normal.

Materials—The stimulus set in this experiment included all 26 English letters, presented in upper-case 18-point Times-New Roman font. Each letter occupied a space of roughly 0.8×0.8 degrees of visual angle. All letters were presented within an area at the center of the screen subtending roughly 8×6 degrees of visual angle. The materials were otherwise the same as in Experiment 1.

Procedure—The procedure was the same as in Experiment 1 except that six additional practice trials were added so that all participants would have sufficient experience with articulatory suppression before the experimental trials began.

Results

Mean accuracy for each condition is presented in Figure 3. Visual inspection of mean performance levels reveals a decrease in accuracy as the RI increases during each ITI condition. Performance in the 1s ITI condition was worse than the 6s ITI condition for each RI duration. The pattern of performance was less clear for the 12s ITI, but again similar to the 6s ITI condition. Mean performance for all conditions in terms of Cowan's k can be found in Table 2.

A 2-way within-participant Bayes factor ANOVA analysis was performed to assess the evidence for or against main effects and interactions. This analysis produces a main effect of Retention-Interval Duration (means: 1s= 0.83, 6s=0.77, 12s=0.74), $F(2,102)= 28.56$, $p<.001$, $\eta_p^2=0.36$. The corresponding Bayes factor is slightly larger than 64-billion-to-one. Only a very small main effect of Temporal Spacing was observed (means: 12s=0.78, 6s=0.79, 1s=0.76), $F(2,102)=4.06$, $p<.05$, $\eta_p^2=0.07$. The corresponding Bayes factor is 1.37 in favor of the null. The observed interaction of these effects is tiny, $F(4,204)=0.06$, $p>.6$. (See Figure 3 for each cell mean.) The Bayes factor for this contrast is 121.16 in favor of the null.

While the Bayes Factor is not definitive about any potential effect of Temporal Spacing, visual inspection of the means in Figure 3 shows that an effect may be present across a subset of the range of ITI lengths analyzed. If there is an effect of Temporal Spacing at some time intervals and not others it would indicate the presence of PI modified by time, but not in the manner predicted by the temporal distinctiveness explanation of the time-based forgetting.

As in Experiment 1, we conducted two separate 2-way within-participant Bayes factor ANOVA analyses of ITI length, one examining the effect of Temporal Spacing across only the 1 and 6s ITI lengths and a second examining the effect of Temporal Spacing across only the 6 and 12s ITI lengths. The analysis using only the 1s and 6s ITIs produced a main effect of Temporal Spacing $F(1,51)=9.25$, $p<.01$, $\eta_p^2=0.15$. The corresponding Bayes factor is 3.68 in favor of the alternative. In contrast the analysis using only the 6s and 12s ITIs demonstrated only a tiny effect of Temporal Spacing $F(1,51)= 0.46$, $p>.5$, $\eta_p^2=0.01$. The corresponding Bayes factor is 8.83 in favor of the null. These additional analyses demonstrate support for a difference in accuracy when ITI-length is increased from 1 to 6s but evidence against an effect of increasing the ITI-length from 6 to 12s. This result is in conflict with the predictions of both temporal distinctiveness theories and pure decay theories.

Examination of the hit (correctly noticing a change) and correct rejection (correctly noticing that there was no change) rates reveals that while the hit rate did not change across retention intervals (means: 1s=.86, 6s=.85, 12s=.85), the correct rejection rate decreased with the longer retention interval (means: 1s=.80, 6s=.68, 12s=.63). Temporal spacing did not appear to have an effect on hit rates (means: 12s=.86, 6s=.85, 1s=.85) or correct rejection rates (means: 12s=.71, 6s=.73, 1s=.67). These results are comparable to Experiment 1 and, as in that experiment, suggest that what decreases across RIs is array-item memory, not change detection per se.

Discussion

The results of Experiment 2 were quite similar to those in Experiment 1, which used unfamiliar characters instead of the English letters of the present experiment. There was no evidence of a change in the rate of forgetting across retention intervals as the ITI increased from 1 s to 6 s, and if anything the rate of forgetting was a bit faster with an ITI of 12 s, just the opposite of what would be expected if the added time allowed the participant to avoid confusion with the previous trial. Instead it appears that an independent combination of decay and limited PI are at work. This is to our knowledge the first experiment to show directly a substantial forgetting of English letters across unfilled RIs.

The very high similarity in the pattern of results between Experiments 1 and 2 is uncharacteristic of short-term memory research and deserves further comment. It is often assumed that verbal and visual items are represented and maintained through fundamentally different processes. If different mechanisms are used to maintain and represent these two types of representations, one would expect very different patterns of results in Experiments 1 and 2. It seems that the verbal-visual representation difference is not important for the durability of the short-term memory trace, at least with visual presentation and articulatory suppression.

Experiment 3

In the experiments conducted so far, we have randomized the ITIs from trial to trial. One limitation of that method is that the ITI length is inconsistent and uncontrolled from trial to trial. Suppose that the deficit in performance when using the 1s ITI stems from the ITI being both much shorter than the other two ITI possibilities and occurring unexpectedly due to our random ITI design. It could be that this combination of brevity of the 1-s ITI and its random and rare occurrence (on 33% of trials) was problematic. This combination could have resulted in participants being occasionally unprepared for the next trial and, consequently, in the information from the previous trial remaining in their short-term storage rather than being offloaded to long-term memory, consistent with the earlier explanation of an event segmentation approach to PI.

Under our version of the event segmentation approach, if a 1-s ITI was expected rather than unexpected we should predict that the deficit in performance for the 1-s ITI relative to the longer ITIs should disappear. We examined this possibility in a rather time-consuming procedure by providing a series of filler trials with short ITIs before each critical trial for which the preceding ITI varied.

Figure 4 depicts a critical trial and Figure 5 depicts the trial order, including the filler trials preceding each critical trial. We build up the potential PI by presenting four filler trials in the change-detection task with a short, 1-s inter-trial interval (ITI) preceding each of the target trials. Each filler trial had a brief 1.5-s RI. This provides a constant amount of PI before the ITI of the fifth target trial. Temporal Spacing was manipulated by varying the temporal proximity of the target trial to the preceding filler trials. Preceding each target trial was either a 1-s or 12-s ITI, determined quasi-randomly. The 1-s ITI preceding the critical trial represented a crowded temporal spacing, whereas the 12-s ITI represented a distant temporal spacing. Only the data from target trials was used in the analyses. Data from the filler trials was discarded. Our version of the event segmentation explanation of PI, as observed in Experiments 1 and 2, predicts no effect of Temporal Spacing in the present experiment. In contrast, a temporal distinctiveness model should predict an effect of Temporal Spacing.

Method

Design—The experiment was designed in such a way that the basic unit was a block of 5 trials. The first four trials were used to produce PI but were not formally analyzed. This was done to ensure a constant amount of PI preceding the ITI before the fifth, target, trial. The four filler trials were each preceded by 1s ITIs and contained 1.5s RIs. The fifth trial in each block of 5 trials was preceded by a 1s or 12s ITI and had either a 1.5s or 6s RI. The length of the ITI and RI on each target trial was determined randomly, with the constraint that 2 of every 8 trials had to include each of the four combinations of ITI and RI. See Figure 5 for a graphical depiction of the trial order in this experimental design.

Participants—Sixty-three college students (18 female, 45 male, ages 18-24) at the University of Missouri participated in the experiment in exchange for partial course credit. After removing participants with at least 20% of trials rejected for RTs outside of the acceptable range of 300-3000ms 58 participants remained. Filtering the data in this way produced results that lead to the same conclusions as if we had used all trials from all participants. No participants spoke or read any of the languages from which the memory stimuli were taken or lived in any of the countries in which they may have been exposed to the figures used in the experiment on a regular basis. All participants had vision which was normal or corrected-to-normal.

Materials—The materials were the same as in Experiment 1, including the same unfamiliar characters and three-character arrays.

Procedure—The Design section explains the pattern of trials. A graphical depiction of a single trial is presented in Figure 4. The length of the ITI preceding each trial and the length of the RI within each trial were in large part determined by the trial order described in the design section and graphically illustrated in Figure 5. Each trial began with an ITI which could be either 1 or 12 s. The RI lasted either 1.5 or 6 s. Otherwise, the procedure for a single trial was the same as in Experiment 1.

Results

Mean accuracy for each condition is presented in Figure 6. Visual inspection of mean performance levels reveals a decrease in accuracy for the longer RI relative to the shorter RI, but no apparent performance difference due to Temporal Spacing.

A 2-way within-participant Bayes factor ANOVA analysis was performed to assess the evidence for or against main effects and interactions. This analysis produces a considerable main effect of Retention-Interval Duration, (means: 1.5 s=0.77, 6 s=0.68) $F(1,57)=38.51$, $p<.001$, $\eta_p^2=.40$. The corresponding Bayes factor is slightly greater than 13-million-to-1. The observed main effect of Temporal Spacing is tiny, (means: distant=0.73, crowded=0.72) $F(1,57)=0.44$, $p>.5$, $\eta_p^2<.01$. The corresponding Bayes factor is 7.73 in favor of the null. The observed interaction of these effects is also small, $F(1,57)=0.80$, $p>.3$. (means: distant; 1.5s=0.772, 6s=0.695; crowded; 1.5s=0.774, 6s=0.675) The Bayes factor for this contrast is 4.90 in favor of the null. Mean performance across all conditions in terms of Cowan's k is the following; distant-spacing, 1.5s RI=1.63 and 6s RI=1.15, crowded spacing, 1.5s RI=1.65 and 6s RI=1.04.

While the hit rate improved slightly with the longer retention interval (means: 1.5s=.82, 6s=.87), the correct rejection rate dropped precipitously with the longer retention interval (means: 1.5s=.72, 6s=.50). Temporal spacing changes did not result in any meaningful difference in hit or correct rejection rates. As in our previous experiments, a straightforward interpretation of this pattern is that forgetting across RIs is a matter of diminishing array-item memory, rather than diminishing change detection per se.

Discussion

The results of Experiment 3 clearly show an effect of RI, but no effect at all of ITI. When the expectation was that the ITI would be very brief, as it was in the present experiment because all of the ITIs preceding filler trials were brief, any effect of the immediately preceding ITI was eliminated. Clearly, the results do not support the hypothesis that a long ITI will prevent confusion between the prior trials and the present trial and diminishes the loss of information across RIs. There was no observable difference between ITIs in the level of accuracy or the amount of forgetting across RIs, i.e., time-based forgetting. In concert with Experiments 1 and 2 these results provide a strong case for a trace decay approach to time-based forgetting and an event segmentation explanation of PI.

Experiment 4

Experiment 4 was the same as Experiment 3 in all ways except that participants were asked to remember 6 English letters instead of 3 unfamiliar characters. Replication of the results from Experiment 3, namely evidence for an effect of RI and evidence for no effect of temporal spacing, would strengthen the argument in favor of a model of forgetting from short-term memory that is not based on temporal distinctiveness.

Recall that Experiments 1 and 2 form a set using a random-ITI method that includes one experiment with unfamiliar characters and one experiment with English letters. In most ways, Experiments 3 and 4 form a comparable set, but using a blocked-filler-trial method.

One additional difference between these sets of experiments has to do with articulatory suppression. It was used in Experiment 2 but, in Experiment 4, it was omitted to determine whether allowing rehearsal would alter the pattern of results. Given that the English letters are presented in a simultaneous spatial array, it is quite possible that rehearsal is not involved.

Method

Participants—Sixty college students (34 female, 26 male, ages 18-25) at the University of Missouri participated in the experiment in exchange for partial course credit. After removing participants with at least 20% of their trials rejected for RTs outside of the acceptable range of 300-3000ms, 58 participants remained. Filtering the data in this way produced results that lead to the same conclusions as if we had used all trials from all participants. All participants had vision which was normal or corrected-to-normal.

Materials—All materials were the same as in Experiment 3, except for the memory stimuli. The stimulus set in this experiment was all 26 English letters as in Experiment 2.

Design—The experimental design was the same as in Experiment 3.

Procedure—The procedure was the same as in Experiment 3. The only difference was that 6 English letters were presented as the memory array rather than the 3 unfamiliar characters used in Experiment 3. The response probe was chosen in the same manner as in Experiment 3, but was always an English Letter in this experiment.

Results

Mean accuracy for each condition is presented in Figure 7. Visual inspection of mean performance levels reveals a decrease in accuracy for the longer RI relative to the shorter RI, but no apparent difference in performance due to Temporal Spacing.

A 2-way within-participant Bayes factor ANOVA analysis was performed to assess the evidence for or against main effects and interactions. This analysis produces a main effect of Retention-Interval Duration (means: 1.5 s = 0.83, 6 s = 0.79), $F(1,57) = 8.81$, $p < .005$, $\eta_p^2 = 0.13$. The corresponding Bayes factor is 6.40. There was virtually no observed main effect of Temporal Spacing (means: distant = 0.81, crowded = 0.81), $F(1,57) = 0.22$, $p > .6$, $\eta_p^2 < .01$. The corresponding Bayes factor is 8.80 in favor of the null. The observed interaction of these effects is tiny, $F(1,57) = 0.01$, $p > .9$. (means: distant; 1.5 s = 0.825, 6 s = 0.793; crowded; 1.5 s = 0.831, 6 s = 0.796) The Bayes factor for this contrast is 6.68 in favor of the null. Mean performance across all conditions in terms of Cowan's k is the following; distant-spacing, 1.5 s RI = 3.90 and 6 s RI = 3.50, crowded spacing, 1.5 s RI = 3.97 and 6 s RI = 3.54.

While the hit rate did not change across retention intervals (means: 1.5 s = .89, 6 s = .90), the correct rejection rate decreased with the longer retention interval (means: 1.5 s = .77, 6 s = .69). Temporal spacing changes did not result in any meaningful difference in hit or correct rejection rates. These results are comparable to Experiments 1, 2, and 3 and suggest that what decreases across RIs is array-item memory, not change detection per se.

Discussion

The results of Experiment 4 replicate the findings of Experiment 3 and extend them to include verbal stimuli when presented in a visual array. Even though verbal rehearsal was not prevented, evidence for time-based forgetting was still found. Interestingly, the number of items initially remembered appears to be about 4 letters, just as in Experiment 2, even though Experiment 2 included articulatory suppression and Experiment 4 did not. This is about the same number of items as can be remembered when the stimuli are simple visual items (Cowan, 2001, 2005), indicating that verbal rehearsal may not be important for the initial formation or encoding of arrays of letters.

The rate of forgetting across retention intervals appears to be smaller than that observed with the unfamiliar characters in Experiment 3. This difference in forgetting rate did not occur in the comparison of Experiments 1 and 2 so the shallower forgetting rate here may be related to the use of English letters without articulatory suppression. The role of articulatory suppression could be either to prevent encoding of visual materials into a phonological form or prevent rehearsal during maintenance (Baddeley, Lewis & Vallar, 1984), or to slow the rate of consolidation (Jolicoeur & Dell'Acqua, 1998; Ricker & Cowan, 2014).

Just as in Experiment 3, Temporal Spacing was found to have no effect on memory performance when a brief ITI was expected on most trials.

General Discussion

The experiments detailed in this study are the first to demonstrate a time-based forgetting effect across a retention interval which cannot be argued to be due to some sort of experimentally induced interference, whether proactive or retroactive. In four experiments, we examined time-based forgetting across a variable RI using both visual and verbal materials, while manipulating temporal spacing by varying the ITI across a wide range (1-12 s). The purpose of this study was to understand why, for spatial arrays, memory declines across an RI of up to 12 s (Ricker & Cowan, 2010, 2014). One theory is that there can be confusion from previous trials that increases across RIs. By using a longer ITI to make the most recent trial temporally distinct from the previous trial or trials, that confusion should be diminished and should reduce the amount of loss of information across RIs. This, however, did not occur. In Experiments 1 and 2, there was slightly higher performance when the ITI was increased from 1 s to 6 s, but this performance change did not reduce the amount of forgetting across the RI. When a procedure was used with very brief ITIs on the majority of the trials (Experiments 3 and 4), no main effect of the ITI was found, either. Thus, whether very brief temporal delays between trials impaired performance relative to longer delays in some cases (Experiments 1 and 2) or not (Experiments 3 and 4), the ITI length did not alter the forgetting across RIs. The result was obtained both with unfamiliar characters (Experiments 1 and 3) and with English letters both with articulatory suppression (Experiment 2) and without it (Experiment 4). We will first discuss the RI effects and then turn attention to the ITI effects.

RI Effects

Given that time-based forgetting in the present procedure cannot be attributed to the effect of temporal indistinctiveness of previous trials, it is reasonable to assume it is based on processing of items from the current trial. No matter the nature of this processing, the loss of the information across the unfilled RI is broadly consistent with the notion that the information decays across time.² This finding is helpful to theories that depend upon a notion of decay, including theories in which the speed of mnemonic processing is important because decay is a process which can supposedly be counteracted by such a mnemonic process, including rehearsal (Baddeley, 1986) or refreshing (Barrouillet, Portrat, & Camos, 2011). In contrast, the time-based forgetting mechanism often proposed on the basis of temporal distinctiveness is typically considered to be independent of any mnemonic maintenance processing (Brown et al., 2007; Keppel & Underwood, 1962).

Our evidence that increasing the temporal distinctiveness of memory items does not ameliorate the loss of information over the RI fits neatly with thinking on how PI operates in a memory system incorporating both short-term and long-term components (Craig & Birtwistle, 1971; Halford, Maybery & Bain, 1988; Loess & Waugh, 1967; Wickens, Moody, & Dow, 1981). We follow the event-segmentation approach (Zacks & Swallow, 2007) to considering the interplay between short-term and long-term storage and propose here that information in short-term storage is offloaded into long-term storage when the present event context is perceived to have ended. Whether or not the specifics of our speculation on the basis of the current result is born out in future work, what is most important is that, for the first time, we have identified a form of memory loss over a retention interval that has been shown not to be based on either PI between trials, i.e., confusions with the stimuli presented on previous trials, or retroactive interference from material presented during the RI period. In this sense, it can be said that we have for the first time directly demonstrated decay.

When these RI effects are considered together with the present findings and those of others, we believe our results support a model of memory, such as the embedded process model (Cowan 1988, 1995) or a modified version of the TBRS model (Barrouillet, Bernardin, & Camos, 2004; Barrouillet, Bernardin, Portrat, Vergauwe, & Camos, 2007) that relies on short-term memory, which is composed of activated long-term memory traces. These traces decay with the passage of time at a rate determined by the amount of short-term consolidation allowed (Ricker & Cowan, 2014), until eventually they are forgotten from short-term memory. While the activation of items decays with time, findings from recent research give evidence that the representations themselves do not fade in resolution as decay progresses. Rather, representations in memory persist at their initial level of clarity until activation has decayed below a threshold level of minimum activation and they are forgotten

²An alternative explanation of why we observed decreased accuracy with a longer retention interval is that elaborative encoding during the retention interval may lead to more avenues through which PI could occur. The basic idea is that a richer encoding results in more information that can be confused with past items. Given our results, the PI would have to be of a form that is not diminished by longer ITIs, hence is non-temporal in nature. For example, naming or categorization of the unfamiliar characters during the retention interval could lead to confusion with similar items from previous trials. Although we cannot discount the idea, it seems implausible as an account of our findings, for at least two related reasons. First, elaborative encoding is generally found to improve rather than impair performance on memory tasks (Craig, 2002; Craig & Tulving, 1975). Second, generating a more robust memory code should generally provide more information that can be used to distinguish the item from other similar items, reducing the likelihood of confusion (Craig & Lockhart, 1972).

in their entirety (Zhang & Luck, 2009; see also the pattern of event-related potentials across delays obtained by Winkler, Schröger, & Cowan, 2001). Once forgotten from short-term storage, traces resume their state as a dormant long-term memory and must be retrieved from long-term storage and reactivated prior to use. Although our unfamiliar characters are likely to have lacked a long-term memory representation prior to presentation (due to the high likelihood that they are either completely novel, or nearly so, for the participant), it is easy to imagine that presentation of the items probabilistically activates a perceptual representation (or set of features) that become a simple long-term trace of the item. Thus, the fact that we use stimuli that are unknown to the participant prior to the experiment does not preclude a model of performance based upon activated long-term traces.

It is important to note that others also have failed to find support for temporal distinctiveness as the basis of forgetting across an RI when the experimental approach differed substantially from the standard methodology of serial recall of repeated series of verbal lists. There are, however, limitations in these previous studies. First, they of course did not have available a statistical method capable of supporting the null hypothesis such as Bayes factor. Second, they each had other more specific possible limitations. Baddeley & Scott (1971) had participants perform only a single trial, thus preventing PI, and still observed time-based forgetting. However, Neath and Brown (2012) argued that they could have introduced some PI into the procedure by presenting items in sequence. Cowan, Saults, and Nugent (1997) used a pitch recognition task and found evidence for decay when controlling for the contribution of the temporal distinctiveness of Trial $n-1$. However, the effect of distinctiveness of trials before Trial $n-1$ might be important to consider in the case of auditory tone stimuli (see Cowan, Saults, & Nugent, 2001). McKeown & Mercer (2012) used difficult-to-label auditory timbre recognition and were not able to find temporal distinctiveness trends in their data, but their research is on a type of sensory memory that might be distinguished from the more categorical memory in play with our stimuli, inasmuch as we included a post-perceptual mask after each array.

Meaning of decay—We must acknowledge that decay is a class of models with different choices as to the exact meaning of decay, and several different possibilities as to the mechanism (Cowan et al., 1997). In one version of decay, the co-existence of several items in working memory at once would cause mutual degradation of the items' representations (cf. Davelaar, Goshen-Gottstein, Ashkenazi, Haarman, & Usher, 2005). It is difficult, however, to reconcile such a process with the finding that the precision of representations is not lost across the RI; representations drop out in a sudden-death process (Zhang & Luck, 2009). Here we demonstrate that time-based loss is not due to proactive or retroactive interference and conclude that a concept such as decay appears to be necessary, with decay defined broadly as any kind of loss over time that is not caused by either of these types of interference.

ITI Effects

To be clear, our analysis shows that temporal distinctiveness does not play a role in forgetting across an RI during visual array recognition, but it does not show that there is no effect of PI. Here we demonstrate that PI cannot account for the time-based forgetting

observed for our spatial-array stimuli. There are abundant findings demonstrating that PI exists and we would agree it is likely that there was PI introduced into our procedure. Indeed, a limited PI effect was observed in Experiments 1 and 2. The goal of the present study was not to challenge the premise that PI exists, but rather to test whether it played a role in time-based forgetting. We have proposed that PI effects could result from temporal context effects, but regardless of the accuracy of this explanation, the data we present here are clear: Temporal distinctiveness is not playing a role in time-based forgetting during visual array change detection. Our data are instead consistent with trace decay explanations of time-based forgetting. If the effect of RI were due to increasing retrieval confusion because of the loss of temporal distinctiveness of those two arrays, then the RI effect should have been modified by the ITI. Given that there were no such interactions of ITI and RI in any experiment, these effects appear to be independent. We suggest that the likely source of the ITI effect was event segmentation failure due to an unexpected brief ITI.

Although there could be several explanations for our limited ITI effect in Experiments 1 and 2, we believe an event segmentation approach is the most likely. This approach would predict that, between trials, information from the previous trial is offloaded from short-term memory to long-term memory, eliminating PI. Indeed, some work by Ecker, Lewandowsky, & Oberauer (2013) supports that a removal process does exist for information in short-term memory. This happens when a new trial event is recognized during the break between trials.

In the case of the 1-s ITI of our Experiments 1 and 2, this ITI was much shorter than the expected value of the ITI overall (6.333s) and occurred randomly and relatively infrequently (on 1 in 3 trials). This random, infrequent, and very brief ITI may have sometimes been insufficient in length for an unprepared participant to complete the removal of information from short-term memory, ultimately leading to greater levels of PI on some of these 1-s ITI trials. Consistent with this approach, in Experiments 3 and 4 the 1- and 12-s ITIs gave rise to equal levels of performance. This makes sense in an event segmentation approach because the 1-s ITI was expected. The brief but expected ITI would allow participants to be prepared for the necessity of quick removal of information during the ITI before every trial.

Relation to Past Work Supporting Temporal Distinctiveness

It is possible that in other circumstances, the influence of temporal distinctiveness would be much larger than it is in our study. Previous authors have found effects of temporal distinctiveness when manipulating the amount of PI, the time between trials, or the time between items within a trial (e.g., Brown et al., 2007; Crowder, 1976; Geiger & Lewandowsky, 2008; Keppel & Underwood, 1962; Loess & Waugh, 1967; Turvey, Brick, & Osborne, 1970; Unsworth, Heitz, & Parks, 2008). These studies, however, differ greatly in their experimental paradigm from the present work. Whereas we used arrays of visually presented items paired with a recognition test, previous studies have tended to use a presentation and recall format that is more conducive to sequential verbal encoding of the memory items. For example, Keppel and Underwood (1962), Turvey et al. (1970) and Unsworth et al. (2008) all used letter trigrams as their memory items. Brown et al. (2007)'s temporal distinctiveness model was developed specifically to address verbal serial recall tasks and Geiger and Lewandowsky (2008) used a running span procedure with letter

memoranda. Loess and Waugh (1967) used lists of words, presented simultaneously but in a list-style format, unmasked, with a much longer presentation duration. In those situations, though, there does not tend to be forgetting over RIs in the absence of distracting material (Oberauer & Lewandowsky, 2008).

To our knowledge, the present work is the first study to use memory for briefly presented visual arrays of items as a method to investigate temporal distinctiveness explanations of time-based forgetting. Given the differences between the methodology of the present work and the methodology of past studies used to support temporal distinctiveness approaches to forgetting, we can think of at least two reasons why we do not observe temporal distinctness effects in our procedure.

Perhaps the simplest explanation in light of this literature is that when we use a recognition probe at test, there is no need for temporal information to be used as a recall cue. When participants are given a very specific recognition probe rather than a non-specific recall cue they may simply search for a match in short-term memory. If a match is found they respond that the item is the same, otherwise they respond that the item has changed. Indeed, the pattern of hits versus correct rejections supports this sort of process. While hit rates stay relatively constant or increase as the RI increases, correct rejection rates plummet with longer RIs.

Another explanation of our failure to support temporal distinctiveness effects despite past findings in support of the approach could be that when temporal information is inherent in the memory trace it is used as a retrieval cue. Temporal distinctiveness effects would then sometimes be observed during long-term memory retrieval. Previous research has largely focused on sequential presentation of verbal materials, which have an inherent temporal structure. Inclusion of this temporal information in the memory traces may have led to a focus on temporal factors during memory retrieval. In line with this thinking, when temporal information is not maintained as part of the memory trace it would be unlikely to be an important aspect of the retrieval cue and temporal distinctiveness effects would be small or absent. Distinguishing between these alternative explanations is an important goal for future research.

Another recent study also investigated the effect of ITI changes on memories for arrays of visual items using a change detection probe task similar to ours. In this study Shipstead and Engle (2013) report larger and more consistent effects of ITI than those we find, leading them to conclude in favor of temporal distinctiveness approaches. However, there are important differences between Shipstead and Engle's method and our own, and we suggest that the ITI effects they observed are not due to a temporal distinctiveness process, but rather to event segmentation. In their study Shipstead and Engle presented participants with an array of colored squares on a silver background which was followed by an RI during which the memory array was taken off the screen. After the RI a full array probe was presented in which the entire array was returned to the screen and a single item was circled. This item was sometimes the same as in the original memory array and sometimes different. Participants had to respond by indicating whether the encircled item was the same or

different. After the probe array was removed a blank screen ITI occurred, identical to the RI and then the next trial began.

The method of Shipstead and Engle (2013) was simply to alternate presentation of the nearly identical memory arrays and the probe arrays with blank screens between. Our event segmentation approach to PI would predict that the lack of a clear demarcation of the new trial context should result in PI when a brief ITI is used because there is no clear external cue prompting offloading of the contents of short-term memory. Further, there was no mask between the probe array of the previous trial and the memory array of the current trial despite the high similarity between the previous trial probe array and the current trial memory array. The lack of a mask should lead to interference at the level of sensory memory during encoding (Rouder et al., 2008; Saults & Cowan, 2007; Sligte, Scholte, & Lamme, 2008). Longer ITIs would lead to less interference due to trace decay of the sensory memory from the previous trial, creating a sensory ITI effect. In light of our data and these considerations, we believe that an event segmentation approach to PI or sensory memory interference effects provide a clearer explanation of the findings of Shipstead and Engle than does a temporal distinctiveness approach.

Concluding Remarks

That trace decay plays a role in time-based forgetting from short-term memory is a critical finding in light of recent debates over whether time-based forgetting exists (Lewandowsky et al., 2004, 2009; Oberauer & Lewandowsky, 2008) and, if so, whether time *per se* is a factor in forgetting (Brown et al., 2007; Crowder, 1976). Models without any trace decay mechanism imply very different processes leading to forgetting as compared to those that do. We also have shown how certain types of PI can work together with decay to determine the probability of correct recognition. Regardless of the eventual assessment of our specific event segmentation approach to PI, future models of short-term memory performance now have firmer ground on which to decide how to incorporate mechanisms leading to time-based memory loss.

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Figure 1.

An example of a single trial from Experiment 1. The procedure in Experiment 2 was identical with the exception that 6 English letters were presented in place of the 3 unfamiliar characters.

Figure 2.

Mean accuracy for all conditions in Experiment 1. Error bars represent descriptive standard error of the mean calculated for each mean individually.

Figure 3.

Mean accuracy for all conditions in Experiment 2. Error bars represent descriptive standard error of the mean calculated for each mean individually.

Figure 4.

An example of a single critical trial from Experiment 3. The procedure in Experiment 4 was identical with the exception that 6 English letters were presented in place of the 3 unfamiliar characters.

Figure 5.

An illustration of trial order in Experiments 3 and 4. ITI stands for inter-trial interval. Each arrow represents a single trial. Only critical trials of interest, those preceded by a variable ITI, were analyzed.

Figure 6.

Mean accuracy for all conditions in Experiment 3. Error bars represent descriptive standard error of the mean calculated for each mean individually.

Figure 7.

Mean accuracy for all conditions in Experiment 4. Error bars represent descriptive standard error of the mean calculated for each mean individually.

Table 1

Mean Number of Items (k) Stored in Short-Term Memory for Each Condition in Experiment 1

Inter-Trial-Interval Duration	Retention-Interval Duration		
	1s	6s	12s
1s	1.69	1.42	1.56
6s	1.93	1.62	1.42
12s	1.99	1.50	1.22

Table 2

Mean Number of Items (k) Stored in Short-Term Memory for Each Condition in Experiment 2

Inter-Trial-Interval Duration	Retention-Interval Duration		
	1s	6s	12s
1s	3.70	3.05	2.67
6s	4.04	3.44	3.02
12s	4.06	3.11	3.05