American Psychological Association

Journal of Experimental Psychology: Learning, Memory, and Cognition

0278-7393/20/$12.00

2020, Vol. 46, No. 1, 60–76
http://dx.doi.org/10.1037/xlm0000719

Do Familiar Memory Items Decay?

Timothy J. Ricker
College of Staten Island and The Graduate Center, City University of New York

Joshua Sandry
Montclair State University

Evie Vergauwe
University of Geneva

Nelson Cowan
University of Missouri

There is a long-standing debate over whether the passage of time causes forgetting from working memory, a process called trace decay. Researchers providing evidence against the existence of trace decay generally study memory by presenting familiar verbal memory items for 1 s or more per memory item, during the study period. In contrast, researchers providing evidence for trace decay tend to use unfamiliar nonverbal memory items presented for 1 s or less per memory item, during the study period. Taken together, these investigations suggest that familiar items may not decay while unfamiliar items do decay. The availability of verbal rehearsal and the time to consolidate a memory item into working memory during presentation may also play a role in whether or not trace decay will occur. Here we explore these alternatives in a series of experiments closely modeled after studies demonstrating time-based forgetting from working memory, but using familiar verbal memory items in place of the unfamiliar memory items used to observe decay in the past. Our findings suggest that time-based forgetting is persistent across all of these factors while simultaneously challenging prominent views of trace decay.

Keywords: working memory, short-term memory, recognition, trace decay, forgetting

There is a long history of debate over the causes of forgetting in working memory. Is forgetting from working memory better explained with a contribution of decay, or with only interference and no decay? Working memory is the temporarily accessible information that is immediately available for ongoing use in cognition. One can think of it as the mental workspace for cognition. From the birth of cognitive psychology, a causal role of time leading to forgetting has been central to many theories of working memory (e.g., Atkinson & Shiffrin, 1968; Baddeley & Logie, 1999; Barrouillet & Camos, 2012; Cowan, 1995; J. Brown, 1958). This causal role for time in forgetting is called trace decay. Temporary memory-trace decay has also been questioned ever since that period (e.g., Keppel & Underwood, 1962; Lewandowsky, Oberauer, & Brown, 2009; Nairne, 2002). The main aim of the present research is to continue to resolve conflicting findings whereby some paradigms and stimulus configurations result in findings favoring decay while others do not.

The most straightforward demonstration consistent with trace-decay-based forgetting is to show that memory performance declines as the length of the retention interval between study and test increases. Longer intervals should result in more decay and thus, in more forgetting. This pattern of results was first demonstrated several decades ago when researchers showed that verbal information is lost over short periods of time when the retention interval is filled with a distracting task (e.g., J. Brown, 1958; Peterson & Peterson, 1959; see Ricker, Vergauwe, & Cowan, 2016, for a recent review). These findings are not in question, but they are not sufficient to demonstrate decay because they occur in situations in which interference from a distracting task during the retention interval may accumulate across intervals, resulting in loss over time not due to decay but rather to interference. Dissociating interference and decay theories of forgetting is often complex (Barrouillet, Plancher, Guida, & Camos, 2013).

The rationale for having participants carry out a distracting task during retention, such as counting backward, was that it would prevent participants from engaging in maintenance processes that

This article was published Online First May 20, 2019.

Timothy J. Ricker, Department of Psychology, College of Staten Island-City University of New York, and Psychology, The Graduate Center-City University of New York; Joshua Sandry, Department of Psychology, Montclair State University; Evie Vergauwe, Department of Psychology and Educational Sciences, University of Geneva; Nelson Cowan, Department of Psychological Sciences, University of Missouri.

All data reported in this work are available at the Open Science Framework https://osf.io/fnwrb/. This work was completed with support from Swiss National Science Foundation Grant PZ00P1-154911 to Evie Vergauwe and NIH Grant R01-HD21338 to Nelson Cowan. Thanks to the following research assistants for helping with data collection: Stephanie Aguayo, Alexandra Caminneci, Renee Callanan, Vanessa Garcia, Jinal Kapadia, Paula Zapata, Erin DeMaio, Rachel Reyes, Luis Torres, Alison Ward, and Alex Buchanan.

Correspondence concerning this article should be addressed to Timothy J. Ricker, Department of Psychology, College of Staten Island-City University of New York, 2800 Victory Boulevard, 4S-234, Staten Island, NY 6521. E-mail: timothy.ricker@csi.cuny.edu

could counteract decay-based forgetting. This paradigm has provoked intense debates between decay and interference-only advocates. Proponents of interference-only models of forgetting have argued that distractor tasks typically introduce interference. Longer retention intervals typically contain more distracting events resulting in more interference, mimicking the effects of decay where none exist. From this premise, it is argued that interference-based accounts may provide a better explanation of the observed decline in working memory performance with longer retention intervals (e.g., Berman, Jonides, & Lewis, 2009; Lewandowsky & Oberauer, 2015; Oberauer & Lewandowsky, 2008). Distracting tasks designed to manipulate the duration of the retention interval while holding the level of interference relatively constant often do not provide evidence for time-based forgetting (e.g., Lewandowsky, Duncan, & Brown, 2004; Oberauer & Lewandowsky, 2008; although for exceptions, see Cowan & AuBuchon, 2008; Cowan, Nugent, Elliott, & Geer, 2000; Reitman, 1974; Watkins, Watkins, Craik, & Mazuryk, 1973), instead these investigations support interference-only accounts of forgetting in working memory.

The current study addresses the mixed evidence for trace decay in the field. In particular, two sets of findings helped inform the current research: a set of findings related to articulatory rehearsal, and a set of findings related to short-term consolidation. Below, we briefly describe each of these, before describing the current study.

Trace Decay and Verbal Rehearsal in Working Memory

Several prominent theories of working memory argue that decay can only be observed if articulatory rehearsal of the memory items is prevented (Baddeley, 1986; Camos, Lagner, & Barrouillet, 2009; Cowan, 1988). At the same time, arguments against the existence of trace decay make clear that interference must be held constant across differing retention intervals if one is to rule out interference as the sole cause of forgetting over time. This creates a problem as procedures that prevent rehearsal tend to introduce interference that increases with longer retention intervals. One must prevent rehearsal to observe decay, but doing so paradoxically introduces interference and negates the ability to test for decay. A few researchers have managed to avoid this circular problem with clever experimental design, sometimes resulting in conclusions against the existence of decay and other times in conclusions favoring the existence of decay.

An elegant example of procedures minimizing interference differences across retention interval differences while also controlling for the role of articulatory rehearsal comes from Lewandowsky, Duncan, and Brown (2004). In this study participants were tasked with remembering lists of six letters, presented serially, and then recalling those letters in the same serial order. Participants were trained to recall the letters at different speeds, thereby changing the length of memory retention required. Slower recall speeds resulted in longer memory retention. Critically, participants were sometimes required to engage in articulatory suppression between the recalls of each memory item. Articulatory suppression is a procedure in which participants repeat an irrelevant word or phrase in order to prevent them from verbally rehearsing target memory items. Lewandowsky et al. (2004) observed no significant increase in forgetting with slower recall speeds even when articulatory suppression was required, thereby implying a lack of trace decay. This finding and the conclusions that follow are in sharp clear opposition to the existence of trace decay as a general mechanism of forgetting.

Oberauer and Lewandowsky (2008) also asked participants to remember a list of six letters. In their experiments, participants were trained to articulate a suppressor word, "super," either once or four times between either each memory item presentation or each recall. Articulatory suppression always produced lower accuracy compared to a baseline no-suppression condition. However, manipulation of the number of iterations of the suppressor did not affect recall accuracy. It takes considerably longer to utter "super" four times than once and given that participants engaged in suppression between each item presentation or each item recall, the manipulation of retention duration was of considerable length. If trace decay was occurring during memory retention, then a large change in accuracy across suppression conditions should have been observed. Again, these findings are in clear opposition to the expectation that time causes forgetting through trace decay.

In these studies, retention duration was manipulated by increasing the number of times the suppressor was uttered. Longer retention intervals by definition also included more utterances that could be predicted to induce interference, but no change in accuracy was observed across retention duration conditions. The explanation is that the suppresser caused little to no additional forgetting beyond the first utterance. Repeated utterances of the same stimulus are not assumed to cause additional forgetting in some interference models of forgetting (Farrell & Lewandowsky, 2002; Oberauer, Lewandowsky, Farrell, Jarrold, & Greaves, 2012).

This approach is consistent with past work examining the effect of suppression across a retention interval. Vallar and Baddeley (1982) asked participants to remember three consonants with a variable retention interval of up to 15 s before memory recall. Several secondary tasks were also required during memory retention. When asked to concurrently count backward by 3 s during the memory retention, gradual forgetting was observed across the retention interval. When asked to concurrently engage in articulatory suppression during the retention interval, performance was equivalent across all retention intervals. Just as with Oberauer and Lewandowsky (2008), articulatory suppression produced lower accuracy overall compared with a no-suppression baseline. These findings are consistent with the approach of modern interference models positing that distracting tasks with changing-state stimuli induce increasing interference-based forgetting as they continue, but constant state articulatory suppression should not induce more than a minimal initial amount of forgetting after its first iteration.

In sharp contrast to the above-described studies showing no time-based forgetting during memory retention, supporting an interference-only approach to forgetting, several studies have found time-based forgetting across longer retention intervals. In particular, visual array studies using articulatory suppression have found time-based forgetting across a retention interval (Cohen-Dallal, Fradkin, & Pertzov, 2018; Woodman, Vogel, & Luck, 2012; Zhang & Luck, 2009). In visual array studies, two or more items are presented simultaneously for a brief period of time. In a typical experiment, three to eight items are presented for roughly 750 ms. A few seconds later, participants are presented with either the entire array or a single item from the array and they must indicate whether or not an item has changed. In some variations of the task, participants must reproduce the color, orientation, or some similar feature of the memory item by moving the mouse to the proper position on a response slider/probe (e.g., Cohen-Dallal et al., 2018). In all variations of the task the retention interval between presentation and test is manipulated so that any time-based forgetting that exists can be observed. The time-based forgetting observed across differing retention intervals in these studies introduces a clear challenge to interference models that posit that there is no trace decay and that articulatory suppression does not cause forgetting.

One counterargument has been to indicate that many visual array studies use articulatory suppression that does not include a constant state utterance. Often participants must articulate two numbers or words, not simply repeat a single word (e.g., Woodman et al., 2012; Zhang & Luck, 2009). The identity of the suppressers also often changes from trial to trial, with new suppressor words presented at the beginning of each trial. Interference-only proponents opposed to the existence of decay can argue that interference comes from the suppresser in these contexts, although there are some findings showing time-based loss without suppression (C. C. Morey & Bieler, 2013; Lilienthal, Hale, & Myerson, 2014). This interference explanation of time-based forgetting with visual arrays can accommodate many findings using memory items composed of familiar features such as color or orientation when combined with articulatory suppression.

Other visual array studies present even greater difficulty for interference-only theories. Specifically, these studies use unfamiliar memory items as memoranda and observe forgetting across the retention interval in the absence of any secondary task and without articulatory suppression (Ricker & Cowan, 2010, 2014; Ricker, Spiegel, & Cowan, 2014; Sakai & Inui, 2002; Vergauwe, Camos, & Barrouillet, 2014). These visual array studies are similar to other visual array change detection tasks in all regards except that they used unfamiliar symbols as memory items. Ricker and Cowan (2010) also included the same visual array task, but instead of using unfamiliar symbols they used familiar letters as memoranda. They observed no time-based forgetting when letters were used. Surprisingly, when Ricker, Spiegel, and Cowan (2014) also performed a visual array memory task with familiar letters as the memory stimuli, they found strong evidence for trace decay across the retention interval under conditions with and without articulatory suppression. This conflict in findings between those who find evidence for decay (e.g., Ricker & Cowan, 2010; Ricker et al., 2014) and those who find evidence against decay (e.g., Lewandowsky et al., 2004; Oberauer & Lewandowsky, 2008) introduces confusion as to whether or not we should observe trace decay of familiar verbal memory items in a visual array task.

Together the literature on trace decay indicates several clear findings: (a) serial recall of familiar verbal items does not produce time-based forgetting even in the presence of articulatory suppression (Lewandowsky et al., 2004; Oberauer & Lewandowsky, 2008; Vallar & Baddeley, 1982); (b) familiar visual memory items are forgotten over time in the presence of articulatory suppression (Woodman et al., 2012; Zhang & Luck, 2009); and (c) unfamiliar visual memory items are forgotten over time even without the presence of articulatory suppression (Ricker & Cowan, 2010, 2014; Ricker et al., 2014; Sakai & Inui, 2002; Vergauwe et al., 2014). While this set of findings makes it clear that some conditions are conducive to finding evidence for decay and others are not, it remains unclear why each of these situations is somewhat different with respect to the effects of time and articulatory suppression. The present study addresses this issue.

Trace Decay and Consolidation in Working Memory

Ricker and Cowan (2014) examined several different presentation procedures that may be responsible for the observation of time-based forgetting in some studies and not in others by using unfamiliar symbols as memory items. The unfamiliar characters included letters, numbers, and single-character words drawn from languages not spoken by the participants, but similar in visual complexity to English numbers and letters that were familiar to the participants. They found evidence for decay in all conditions, regardless of whether sequential or simultaneous presentation of memory items was used. Differences in mask onset time following stimulus presentation did not affect the presence of time-based forgetting. Changes in the amount of free time following each memory item presentation, referred to as the consolidation time, did result in changes in the forgetting rate, although some amount of forgetting over time was still observed across all consolidation conditions. Although Ricker and Cowan (2014) manipulated consolidation time by manipulating the amount of free time following item presentation, total consolidation time is generally measured beginning at memory item onset and continuing as long as attention remains focused on the memory item (Jolicoeur & Dell'Acqua, 1998; Ricker & Hardman, 2017; Wyble, Potter, Bowman, & Nieuwenstein, 2011).

Ricker and Cowan (2014) proposed that sensory memory traces undergo a short-term consolidation process immediately following memory item presentation (for similar proposals see also, Jolicoeur & Dell'Acqua, 1998; Nieuwenstein & Wyble, 2014). This process requires that attention is directed toward the memory representation and increases the trace's resistance to forgetting processes such as interference and trace decay[1] (Ricker, 2015; Ricker & Cowan, 2014). The consolidation process differs from encoding processes in this view. Encoding processes are responsible for establishing a working memory trace and are ended by masking (Ricker, 2015; Ricker & Sandry, 2018; Vogel, Woodman, & Luck, 2006; Woodman & Vogel, 2005). Short-term consolidation solidifies newly created working memory traces against forgetting processes, thereby allowing their continued maintenance. Short-term consolidation is not ended by masking, but rather continues as long as attention is directed toward the memory item immediately following presentation or until consolidation is complete (Ricker, 2015; Ricker, Nieuwenstein, Bayliss, & Barrouillet, 2018). The finding of Ricker and Cowan (2014) that more free time after each memory item results in less time-based forgetting implies that the existence of forgetting over time in one study, but not another, may simply be due to differences in the amount of time available for consolidation across studies. Unlike the findings related to articulatory rehearsal, the role of short-term consolidation in trace decay

[1] The reduction in time-based loss following short-term consolidation could result from a decreased rate of forgetting or a decreased probability that forgetting will occur on any given trial. Both scenarios would reduce the mean rate of time-based forgetting observed across many trials.

is largely unexplored, making its exploration a priority for understanding trace decay.

This explanation that trace decay results from insufficient consolidation time is not without its flaws. It has trouble explaining the observation of time-based forgetting by Lilienthal et al. (2014), even with presentation times longer than the proposed limits of working memory consolidation (Jolicoeur & Dell'Acqua, 1998; Ricker & Hardman, 2017; Ricker & Sandry, 2018). It also does not adequately address long running assumptions about the role of articulatory suppression in observing decay (Baddeley, Thomson, & Buchanan, 1975; Cowan et al., 1992) and does not examine whether familiar memory items decay when presented with insufficient time for full consolidation into working memory.

The Present Study

The present work addresses the issues raised in the preceding pages by investigating whether we can observe trace decay with familiar memory items. In the following experiments, we investigate whether memory for arrays of letters decay with the passage of time across retention intervals of different durations. To foreshadow, multiple large sample-size experiments demonstrate strong time-based forgetting of familiar memory items in a variety of contexts. Having established this premise, we explore a number of questions raised by contrasting findings in the literature. Specifically, we explore (a) the role of articulatory suppression in determining the rate of forgetting, (b) whether the time available for consolidation determines whether time-based forgetting is observed, and (c) whether familiar verbal and visual memory stimuli behave differently with respect to the passage of time. An overview of the experiments can be found in Table 1.

Experiment 1

In our first experiment, we explore trace decay with familiar memory items using a procedure closely modeled after two studies: Ricker and Cowan (2010), who provide evidence for trace decay of unfamiliar visual memory arrays but not for familiar verbal item arrays, and Ricker et al. (2014), who provide evidence for trace decay of both unfamiliar visual and familiar verbal memory arrays. In our current procedure, English-speaking participants were briefly presented with visual arrays of six English letters that had to be remembered for use in a change-detection test seconds later (see Figure 1). The length of retention was manipulated to allow the observation of any trace decay across the retention interval, should any exist. This procedure was conducted under three different articulatory suppression conditions in order to examine the role of verbal rehearsal in observing trace decay.

If familiar memory items do not decay we should see no time-based forgetting across the variable retention interval duration in any suppression condition. Alternatively, familiar items may decay and suppression may prevent ongoing maintenance activities such as verbal rehearsal. In this case, we should observe time-based forgetting in the articulatory suppression conditions that prevent verbal rehearsal and no time-based forgetting in the no suppression condition because verbal rehearsal can take place. Finally, if visual array presentation always leads to conditions under which trace decay will be observed, then we should see time-based forgetting in all conditions.

Method

Participants. Eighty students attending the University of Missouri participated in the experiment. Twenty participants were excluded from data analysis for one of the three following reasons. First, 17 participants demonstrated near chance level performance (53% correct or less) in one or more conditions. These participants were excluded in order to avoid floor effects. Second, two participants did not follow articulation instructions after repeated prompts. Third, data from one participant was lost due to a computer error. This left a sample size of 60 participants (31 female) for use in all analyses.[2] All were fluent speakers of English with normal or corrected-to-normal vision. This experiment was approved by the University of Missouri Institutional Review Board.

Materials. On each trial, all memory items were selected at random without replacement from the full set of 26 English letters. All letters were presented in upper-case 18 point Courier New font. The masking item consisted of the symbols ">" and "<" superimposed upon one another at the location of all memory items simultaneously. These masking symbols were designed to be roughly the same size and dimensions as the average uppercase letter. All letters, masks, and the fixation cross were presented in black while the rest of the screen was gray in color.

Design. A breakdown of the methodological differences across all experiments is presented in Table 1. Experiment 1 had three suppression conditions. In the "none" condition participants did not engage in articulatory suppression. In the "maintenance-only" condition participants only engaged in suppression during the retention interval. In the "full trial" condition participants engaged in articulatory suppression during the entire trial. There were also three retention duration conditions that varied in total length. This resulted in a 3 (suppression condition: none, maintenance-only, full trial) × 3 (retention duration: 1,500 ms, 3,000 ms, or 6,000 ms) within-subjects design. The retention durations were the same as used by Ricker and Cowan (2010). Suppression condition, but not retention duration, was manipulated between blocks, with the order of conditions fully counterbalanced across participants. There were nine experimental blocks in total. All three blocks of each suppression condition were completed before the next suppression condition was introduced. Each suppression condition was broken into three blocks in order to allow the participants to rest after prolonged articulation. Our analyses indicated that there was no effect of counterbalance order so we do not discuss counterbalancing further.

[2] To be thorough we also ran our analyses of Experiments 1–5 without excluding any participants. The results demonstrated the same pattern of effects as indicated in the results sections of each experiment. In Experiment 1, only 15 of the 20 excluded participants had usable data files, so only these 15 participants were included in the reanalysis. The addition of the excluded participants in Experiment 1 did introduce a suppression condition by counterbalance order interaction. A large amount of the excluded participants happened to be from one counterbalance order. The interaction appears to be a function of the lower overall accuracy in the suppression conditions of this counterbalance order relative to other counterbalance orders. It is also important to note that the overall difficulty of our task was not too hard for the participants to complete. Of the 471 participants across our studies, only six demonstrated overall mean accuracy at or below chance. Our exclusion criteria were fairly aggressive in an attempt to prevent any possible floor effects in only the most difficult conditions.

Table 1
Manipulations Used Across Experiments

Experiment	Presentation duration	Other factor manipulated	Stimuli	Set size
Exp. 1	750 ms	Articulatory Suppression	Letters (all)	6
Exp. 2a	3 × 250 ms	Consolidation Time	Letters (all)	6
Exp. 2b	3 × 250 ms	Consolidation Time	Letters (no vowels)	6
Exp. 3	3 × 50 ms	Consolidation Time	Letters (no vowels)	6
Exp. 4	3 × 250 ms	Consolidation Time	Letters (no vowels)	3 or 6
Exp. 5	3 × 250 ms	Consolidation Time	Colors	3 or 6

Retention duration was manipulated between trials such that each block contained 10 trials at each retention duration, for a total of 30 trials per block or 270 trials in all. Retention duration on any given trial within a block was determined by random sampling without replacement from this set of durations. Before the first experimental block of each suppression condition (i.e., Blocks 1, 4, and 7) 10 practice trials were given along with training on how to perform articulatory suppression correctly for the relevant condition. All practice trials used a 3,000-ms retention duration.

Procedure. An example of an experimental trial is depicted in Figure 1. Each trial was initiated by button press and began with the presentation of a fixation cross for 1,000 ms. Next, six English letters were presented concurrently in an array for 750 ms. Participants were instructed to remember all of the letters in this memory array. All letters were presented at random locations within a 72 mms × 54 mm invisible box located at the center of the screen, with the constraint that no letter could appear at fixation or overlap another letter in the array. After letter presentation, the screen was blank for 250 ms, after which the masking array appeared for 100 ms. Following mask offset the retention interval began and lasted for either 1,500 ms, 3,000 ms, or 6,000 ms. When the retention interval ended, a single probe item was presented on screen at the location of one of the memory items from the memory array and black circles were presented at the spatial location of all other items from the memory array. Participants were asked to indicate whether the probe item was the same as the item at that location in the memory array or different by pressing the "s" or "d" key, respectively. On half of the trials within each block the probe item was different than the item in the memory array. When the item was different it was always a new item not used in any location within the memory array.

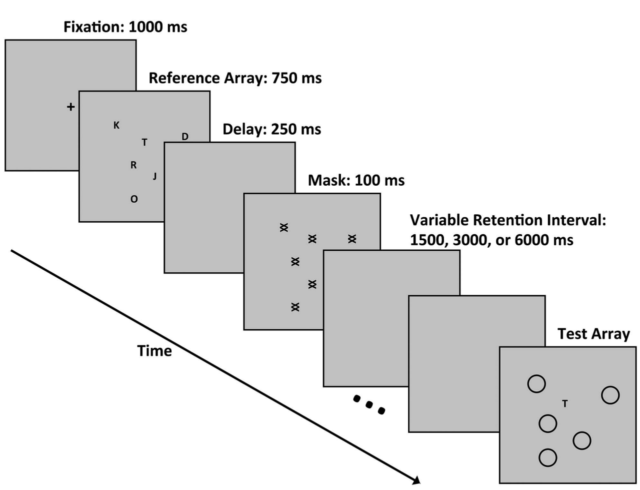

Figure 1. An example of a single experimental trial, Experiment 1.

The start and pace of articulatory suppression was indicated by onset of a repeating metronome click sequence that clicked three times per second and lasted until the probe item appeared on screen. Participants were to speak the word "the" audibly at any volume they wished every time the metronome clicked. In the maintenance-only condition the metronome sequence began at masking array offset. In the full trial condition the metronome sequence began earlier in the trial, with onset of the initial fixation cross. An experimenter monitored suppression for all participants. If a participant stopped suppression or suppressed incorrectly the experimenter waited until the trial was over then reminded the participant of the need to suppress appropriately.

Analysis. Before conducting any analyses we removed all trials from the data set that had reaction times (RTs) shorter than 300 ms or longer than 3,000 ms (6% percent of all trials). This was done to remove easily identifiable responses in which the participant was not paying attention to the current trial sequence.

For our main inferential statistic we calculate Bayes factors for ANOVA and *t* tests. These Bayes factors indicate the probability of the data when assuming an effect is present relative to the probability of the data when assuming no effect is present. In other words, a Bayes factor of 10 in favor of an effect indicates that the data are 10 times more probable if there is an effect than if there is not an effect present. A Bayes factor of 14 in favor of the null would indicate that the data are 14 times more probable if there is no effect present than if there is an effect present. In the case of ANOVA we calculate Bayes factors for both main effects and their interaction. Bayes factor calculations for ANOVA follow the method described by Rouder, Morey, Speckman, and Province (2012) while Bayes factor calculations for *t* tests follow the method described by Rouder, Speckman, Sun, Morey, and Iverson (2009), both implemented through the "Bayes Factor" package (Version 0.9.12–2; R. Morey & Rouder, 2013) for the R statistical computing language.

The Bayes factor is also an updating factor for updating beliefs logically and consistently in light of data (Kass & Raftery, 1995; Rouder, Morey, Verhagen, Province, & Wagenmakers, 2016). It indicates the amount that prior beliefs should change given the data. In the context of our experiments a Bayes factor of 2 in favor of the alternative hypothesis indicates that your posterior beliefs about the odds of alternative hypothesis generating the data are twice as likely as your prior beliefs about the odds of alternative hypothesis generating the data.

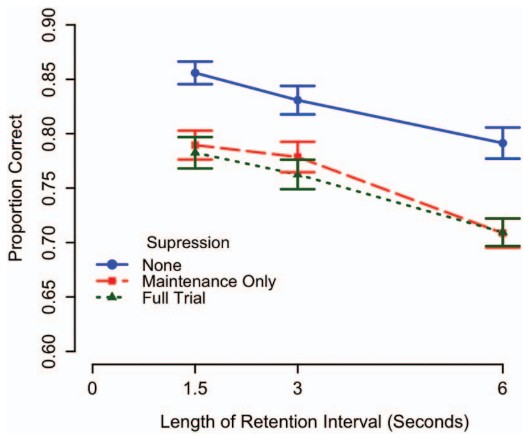

Figure 2. Mean proportion correct as a function of retention duration and suppression condition in Experiment 1. Error bars represent standard error of the mean. See the online article for the color version of this figure.

Results

Mean performance in each condition is presented in Figure 2. It is clear from the figure that forgetting occurred as a function of time. The presence of articulatory suppression impaired performance, but did not change the rate of forgetting. A 3 (retention duration) × 3 (suppression condition) repeated measures ANOVA of mean proportion correct confirmed these observations. This test produced a clear main effect of retention duration, $F(2, 118) = 44$, $\eta_p^2 = .43$, Bayes factor $= 2.7 \times 10^{11}$ in favor of an effect, with performance decreasing as the retention interval increases. There was a clear main effect of suppression dondition, $F(2, 118) = 33$, $\eta_p^2 = .36$, Bayes factor $= 1.07 \times 10^{14}$ in favor of an effect, with superior performance in the no suppression condition relative the suppression conditions. There was no interaction between the two effects, $F(4, 236) = 0.66$, $\eta_p^2 = .01$, Bayes factor $= 156$ in favor of the null.

In order to compare the rate of forgetting with familiar materials as observed in the present work to the rate of forgetting with unfamiliar materials as observed in previous studies, we computed the rate of forgetting for each participant in the present study as well as in Experiment 1 of Ricker and Cowan (2010). It is important to note that the present study was modeled after of Ricker and Cowan (2010) and, as such, replicated the procedure except in that the present work used familiar memory items and the secondary task was articulatory suppression. The six-item array size used in the current study was selected because it results in accuracies that are very similar to the accuracies observed by Ricker and Cowan (2010) with three unfamiliar characters. Thus, to compare the rates of forgetting between familiar and unfamiliar materials, we calculated the rates of forgetting for each participant in both studies. The rate of forgetting was operationalized as the difference in mean accuracy between the shortest and longest retention intervals. A larger difference score represents more forgetting. We then conducted an independent samples t test comparing the rates of forgetting in each study. There was no observable difference in the rate of forgetting between the familiar and unfamiliar memory items, $t(51) = 0.26$, Bayes factor $= 4.17$ in favor of the null.

Discussion

In Experiment 1 we varied the presence of articulatory suppression in three conditions, from no suppression to suppression throughout the entire encoding and delay periods. Time-based forgetting occurred to the same degree in all conditions, even with no suppression or secondary task to induce interference. The loss across delay periods in all conditions cannot be explained by interference, inasmuch as it occurred even during an unfilled interval with no suppression. The large main effect of suppression apparently was caused by some other mechanism (e.g., distraction) that does not interact with delay period, unlike the putative role of this suppression in preventing rehearsal.

While this experiment demonstrates time-based forgetting of familiar material across a retention interval, many other studies have not found the same result (Barrouillet, Bernardin, & Camos, 2004; Lewandowsky et al., 2004; Oberauer & Lewandowsky, 2014). One clear difference between our study and other common paradigms is in the amount of short-term consolidation time per item. In the same paradigm as we use here, but with visual, nonverbal materials, we have previously argued that the rate of trace decay is determined by the amount of time given for short-term consolidation immediately following item presentation (Ricker & Cowan, 2014). Short-term consolidation is the attention-based stabilization process that allows strengthening of the sensory memory trace into a working memory trace. The assumption is that short-term consolidation protects against forgetting that may result from increased time and/or interference (Jolicoeur & Dell'Acqua, 1998; Ricker, 2015). Ricker and Cowan (2014) varied the amount of time available for attention to dwell on the memory items by slightly increasing the length of time between item presentations and found that more time to attend to the memory set resulted in less forgetting over time. This finding implies that poorly consolidated working memory traces decay in a similar manner to sensory memory traces (Cowan, 1984; Ricker, 2015).

To operationalize this concept, we can consider poorly consolidated memory traces to be the memory traces for any items that are not given ample time for attention to dwell on their internal representation immediately following their presentation. Ricker and Hardman (2017) estimated that about 600–700 ms are required to fully consolidate simple visual memory items when each memory stimulus is presented sequentially. Studies supporting trace decay of unfamiliar memory items follow this pattern in that nearly all of these studies allow only brief periods of time for item consolidation (e.g., Ricker & Cowan, 2010, 2014; Ricker et al., 2014; Vergauwe et al., 2014). Studies examining forgetting of familiar verbal materials, however, have often used slow, sequential presentation of memoranda, resulting in longer consolidation times per item and, thus, in firmly consolidated memory traces.

The importance of consolidation in working memory is clear from past work. Jolicoeur and Dell'Acqua (1998) show similar consolidation processes in both verbal and visual materials. When consolidation times were limited to less than 1 s, Nieuwenstein and Wyble (2014) demonstrated slightly larger accuracy impairments with familiar memory items than with unfamiliar memory items. When these results are considered in combination with the finding that consolidation duration modifies time-based forgetting (Ricker & Cowan, 2014), it is not surprising that time-based loss is observed with brief presentation of familiar verbal memory items.

In fact, it is the predicted result, as the present experiment utilizes a short consolidation time that has resulted in considerable forgetting in past work. In Experiments 2 and 3 we test whether varying the time available for consolidation also leads to a change in the rate of forgetting with familiar memory items as it does with unfamiliar items. Because articulatory suppression did not influence the rate of forgetting in Experiment 1, we did not include any articulatory suppression conditions in the remaining experiments.

Experiment 2

Recent work suggests that demonstrating trace decay with familiar verbal memoranda requires careful consideration of the amount of time available for short-term consolidation of the to-be-remembered information. Indeed, Ricker and Cowan (2014) recently addressed the contradiction between the existence of time-based forgetting in some studies and not in others. This study demonstrated that the amount of time given for memory consolidation determined the rate of forgetting over time observed in change detection of unfamiliar visual character arrays. When shorter presentation times were used, less consolidation was possible and faster rates of forgetting were observed.

This brings up a crucial methodological detail that is often overlooked in studies of forgetting that may account for the fact that we have observed decay for familiar materials in Experiment 1, while several previous studies have not. Familiar verbal memoranda are often presented in sequences, with the presentation time in studies using familiar verbal memory items often much longer per-item than the presentation times for studies using arrays of visual memory items. In Experiment 2 we test whether time-based forgetting over unfilled retention intervals is greater for poorly consolidated familiar verbal memory items than for well-consolidated familiar verbal memory items, just as it is with visual memory items.

In Experiments 2a and 2b we manipulated the time available for attention to dwell on the memory array while maintaining a constant presentation time. We did this by presenting the memory array three times in a row, for 250 ms each time, with blank-screen periods of either 150 ms or 500 ms between each array presentation. This manipulation was used by Ricker and Cowan (2014) and maintains a 750 ms presentation time while also allowing attention to dwell on the memory representation for a variable period of time immediately following each array presentation. Longer blank-screens between presentations result in better short-term consolidation because of this increased dwell time (Bayliss, Bogdanovs, & Jarrold, 2015; Nieuwenstein & Wyble, 2014; Ricker & Cowan, 2014; Ricker & Hardman, 2017; Ricker & Sandry, 2018).

Experiments 2a and 2b differ in only one methodological detail. We wished to test whether the identification of familiar word chunks was part of the consolidation process. In Experiment 2a the memory items were sampled from the full set of 26 English letters to allow the presence of word chunks. This is the same stimulus set used in Experiment 1. In Experiment 2b vowels were excluded to reduce the chance of participants chunking the letters as a word. We found no differences in the pattern of results across experiments, other than a small overall increase in accuracy when vowels were present, so we present the experiments together, with Experiment treated as a between-participants factor in the ANOVA.

Method

Participants. All participants were students attending Montclair State University. Seventy-nine adults participated in the Experiment 2a and 76 adults participated in Experiment 2b. Twelve participants were excluded (four in Experiment 2a; eight in Experiment 2b) from data analysis because they demonstrated near chance level performance (53% correct or less) in one or more conditions (see Footnote 2). This left a sample size of 143 participants for use in all analyses. All participants were fluent in English with normal or corrected-to-normal vision. This experiment was approved by the Montclair State University Institutional Review Board.

Materials. In Experiment 2a the memory items were chosen from the same set of 26 English letters as in Experiment 1. In Experiment 2b the same English letters were used, excluding vowels (A, E, I, O, U, and Y). Otherwise, all materials were the same as in Experiment 1.

Design. Both experiments used an identical 2 (consolidation condition: 150 ms or 500 ms) $\times$ 3 (retention duration: 1,500 ms, 3,000 ms, or 6,000 ms) within-subjects design. These presentation and post-presentation consolidation durations roughly match those used to observe an effect of consolidation time in Ricker and Cowan (2014). There were six blocks of 36 trials each. There were an equal number of all combinations of trial types within each block. Trial order was determined randomly without replacement within each block. 10 practice trials (500 ms consolidation time and 3,000 ms retention time), with the option to repeat, preceded the experimental blocks.

Procedure. The trial sequence was the same in Experiments 2a and 2b. The procedure was the same as in Experiment 1 except for the following changes. During memory item presentation, the memory array was no longer presented for 750 ms. Instead, the memory array was presented three separate times in quick succession, with two periods during which the screen was blank dividing these presentations (see Figure 3). Each of the individual memory array presentations lasted for 250 ms (equating to 750 ms total presentation time to match the presentation time in Experiment 1). The blank screen periods lasted for either 150 ms or 500 ms, depending on the condition. Within a trial, both of the blank periods were the same duration. In Experiment 2 there was no articulatory suppression.

Analysis. The analysis was the same as in Experiment 1.

Results

Mean performance in each condition is presented in Figure 4. It is clear from the figure that forgetting occurred as a function of time. Reducing the time for consolidation impaired performance, but did not change the rate of forgetting. A 3 (retention duration) $\times$ 2 (consolidation condition) $\times$ 2 (experiment) Repeated Measures ANOVA of mean proportion correct confirms these observations. This test produced a clear main effect of retention duration, $F(2, 282) = 39.70$, $\eta_p^2 = 0.22$, Bayes factor $= 3.9 \times 10^{16}$ in favor of an effect, with performance decreasing as the retention interval increases. There was also a clear main effect of consolidation time, $F(1, 141) = 85.62$, $\eta_p^2 = 0.38$, Bayes factor $= 3.9 \times 10^{15}$ in favor of an effect, with more time for consolidation leading to better performance. There was a main effect of experiment, $F(1, 141) = 6.67$, $\eta_p^2 = 0.05$, Bayes factor $= 3.7$ in favor of an effect (means;

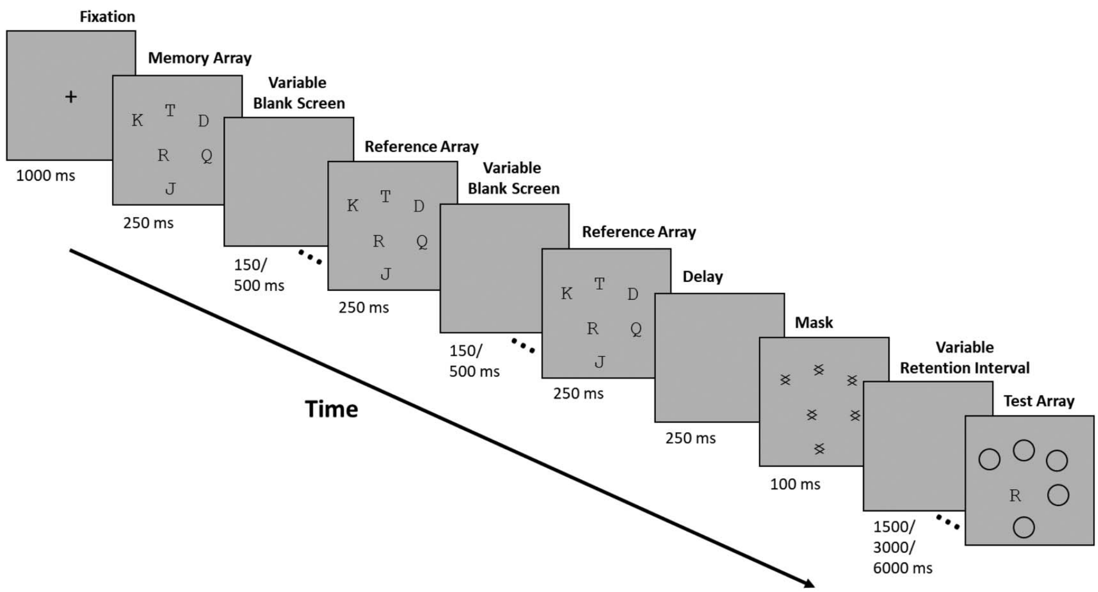

Figure 3. An example of a single experimental trial, Experiment 2.

Experiment 2a/vowels = 0.85, Experiment 2b/no vowels = 0.82), with superior performance when vowels were included in the memory set. No interactions were present, all Bayes factors >4 in favor of the null. In particular, the interaction between consolidation time and retention duration produced a Bayes factor of 10 in favor of the null.

Discussion

As in Experiment 1, we again found decay in all conditions. In contrast to Ricker and Cowan (2014) who explored memory for unfamiliar visual characters, we did not see a change in the decay rate following a change in the time for consolidation of familiar letters. One possibility for this discrepancy is that we designed the presentation sequence timing in the current experiment, which used familiar letters, using the consolidation times observed to have an effect in Ricker and Cowan's (2014) study of unfamiliar symbols. Past work has shown faster consolidation of letters than of symbols (Jolicoeur & Dell'Acqua, 1998). Perhaps the consolidation durations we allowed in Experiment's 2a and b were too long to observe the consolidation process for a set of letters. We test this hypothesis in Experiment 3 by decreasing the time available for consolidation across all conditions.

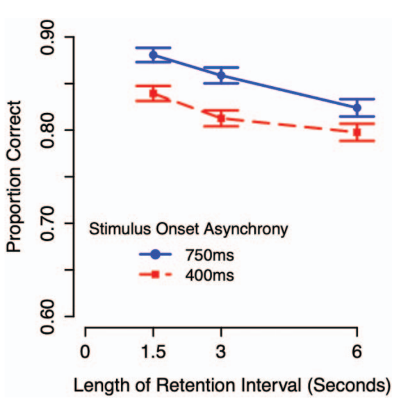

Figure 4. Mean proportion correct as a function of retention duration and consolidation condition in Experiment 2. Error bars represent standard error of the mean. See the online article for the color version of this figure.

Experiment 3

In this experiment we replicated Experiment 2, but shortened the presentation time of the memoranda from 250 ms exposures to 5 0ms exposures to reduce the time available per item for short-term consolidation. Consolidation of unfamiliar materials into working memory is quite fast, with orientation being consolidated into memory within about 600 ms (Ricker & Hardman, 2017; Ricker & Sandry, 2018). It is plausible that familiar items such as letters could complete consolidation into working memory in under 500 ms.

Method

Participants. Seventy-five students attending Montclair State University participated in the experiment. Fifteen participants were excluded from data analysis because they demonstrated near chance level performance (53% correct or less) in one or more conditions (see Footnote 2). This left a sample size of 60 participants for use in all analyses. All were fluent in English with normal or corrected-to-normal vision. This experiment was approved by the Montclair State University Institutional Review Board.

Materials. All materials were the same as in Experiment 2b (i.e., English consonants).

Design. This experiment used the same design as in Experiments 2a and 2b.

Procedure. The trial sequence was the same as in Experiment 2b, except that each of the three memory array presentations was reduced from 250 ms in Experiment 2b to 50 ms in Experiment 3, for a total onscreen time of 150 ms ($3 \times 50 = 150$).

Analysis. The analysis was the same as in Experiments 1 and 2.

Results

Mean performance in each condition is presented in Figure 5. It is clear from the figure that forgetting occurred as a function of time. Reducing the time for consolidation again impaired performance, but did not change the rate of forgetting. A 3 (retention duration) $\times$ 2 (consolidation condition) repeated measures ANOVA of mean proportion correct confirms these observations. This test produced a clear main effect of retention duration, $F(2, 118) = 8.60$, $\eta_p^2 = 0.13$, Bayes factor = 54.5 in favor of an effect, with performance decreasing as the retention interval increases. There was again a clear main effect of consolidation time, $F(1, 59) = 59.33$, $\eta_p^2 = 0.50$, Bayes factor = 1.1×10^9 in favor of an effect, with more time for consolidation leading to better performance, and there was no interaction between the two factors, $F(2, 118) = 0.30$, $\eta_p^2 = 0.01$, Bayes factor = 20.8 in favor of the null.

Discussion

In Experiment 3 we again observed time-based forgetting in both consolidation conditions. In contrast to Ricker and Cowan (2014), we again failed to observe a change in the rate of forgetting with changes in the time for consolidation. This seems to indicate that increased consolidation only alters the forgetting rate of unfamiliar materials. We return to the discrepancy between the present results and the results of Ricker and Cowan (2014) in the General Discussion.

Despite the failure of consolidation manipulations in altering the rate of forgetting over time, we still observe time-based forgetting in the present experiment. If participants are reciting the letter sets to themselves following memory stimulus presentation then we should not observe forgetting across the retention interval (Baddeley, 1986; Camos et al., 2009; Cowan, 1995; Page & Norris, 1998). Our set of six letters clearly fits within the proposed limit of the articulatory loop, about 1.8 s of spoken material (Baddeley et al., 1975; Cowan et al., 1992; Schweickert & Boruff, 1986) or a similar amount of articulatory planning (Caplan, Rochon, & Waters, 1992; Service, 1998). Although rehearsing all of the presented letters was clearly attainable, participants may have perceived a set size of six memory items as large and difficult to remember. In other studies, change detection tasks with set sizes over four items often lead to a decrease in the number of items that are maintained relative to when smaller set sizes are used. This is presumably because participants become overwhelmed and encode items from the memory set inefficiently (Cusack, Lehmann, Veldsman, & Mitchell, 2009).

Perhaps the number of items we present, coupled with the relatively brief presentation time in our current experiments contributes to the observed forgetting rate because participants feel overwhelmed and do not attempt to actively maintain the memory items. This would lead to observable trace decay across the retention interval because participants are not counteracting the loss through maintenance activities as they normally would if a smaller number of items were presented. If this is the case, then we should observe reduced or completely absent decay in conditions with smaller memory sets that are not perceived as overwhelming by participants. These small set sizes would be rehearsed, or otherwise actively maintained, by the participants. This open question is examined in Experiments 4 and 5.

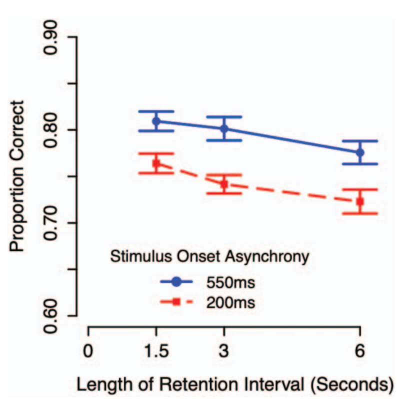

Figure 5. Mean proportion correct as a function of retention duration and consolidation condition in Experiment 3. Error bars represent standard error of the mean. See the online article for the color version of this figure.

Experiment 4

In Experiment 4 we presented either three or six memory items on each trial, using the presentation method of Experiment 2b. If large memory sets discourage participants from engaging in active maintenance, thereby allowing observations of trace decay, then the smaller set size of three items should encourage verbal rehearsal and lead to less observed decay when compared with a set size of six. We also manipulated consolidation time as we did in the previous two experiments in order to check whether the effects of consolidation differ at smaller set sizes compared to larger set sizes.

Method

Participants. Eighty-four students attending Montclair State University participated in the experiment. The threshold for participant exclusion used in the previous experiments (less than 53% accuracy in one or more conditions) proved too strict in the remaining experiments reported, with the majority of participants removed. To be clear, participants did not have difficulty in performing the general task in the remaining experiments. This strict criterion would exclude participants for poor performance in only the most difficult combination of conditions, even if they performed well in the remaining experiment as a whole. This was the case for most of the participants that were excluded using the strict criteria. A new threshold was selected that allowed us to maintain the majority of participants while still minimizing floor effects. Participants with very poor overall performance (overall mean proportion correct less than or equal to .60) were excluded from analysis to minimize floor effects (see Footnote 2). Data from four participants was lost due to a computer error and an additional 19 participants were excluded from data analysis because they demonstrated near chance performance (mean accuracy 60% or less). This left a sample size of 61 participants for use in all analyses. All were fluent in English with normal or corrected-to-normal vision. This experiment was approved by the Montclair State University Institutional Review Board.

Materials. All materials were the same as in Experiment 2b.

Design. The experiment used a 2 (set size: 3 or 6 items) × 2 (consolidation condition: 150 ms or 500 ms) × 3 (retention duration: 1,500 ms, 3,000 ms, or 6,000 ms) within-subjects design. There were six blocks of 72 trials. There were an equal number of all combinations of trial types within each block. Trial order was determined randomly without replacement within each block. Ten practice trials with an optional repetition preceded the experimental blocks.

Procedure. The procedure was the same as in Experiment 2b, except for two changes. First, the set size was either three or six memory items in the present experiment. Second, the final memory array presentation was no longer followed by a 250 ms blank period before the mask. Instead the memory array was followed by either a 150 ms or 500 ms blank screen. Addition of the set size manipulation while maintaining equal trials in each condition doubled the length of the experiment relative to Experiments 1–3.

Analysis. The analysis was the same as in Experiment 1.

Results

Mean performance in each condition is presented in Figure 6.[3] In line with the previous experiments, forgetting occurred as a function of time. Reducing the time for consolidation impaired performance slightly, but did not change the rate of forgetting. The set size manipulation had a large effect, as expected, but did not appear to alter the rate of forgetting. A 3 (retention duration) × 2 (consolidation condition) × 2 (set size) repeated measures ANOVA of mean proportion correct confirms these observations. This test produced a main effect of retention duration, $F(2, 120) = 9.37$, $\eta_p^2 = 0.14$, Bayes factor = 39.1 in favor of an effect, with performance decreasing as the retention interval increases. There was indeterminate evidence for the presence of a main effect of consolidation time, $F(1, 60) = 6.95$, $\eta_p^2 = 0.10$, Bayes factor = 1.2 in favor of an effect, providing only weak support for performance improvements with more time for consolidation. There was a clear main effect of Set Size, $F(1, 60) = 354$, $\eta_p^2 = 0.86$, Bayes factor = 1.07×10^{133} in favor of an effect, with a larger Set Size leading to poorer performance. No interactions were present, all Bayes factors >10 in favor of the null.

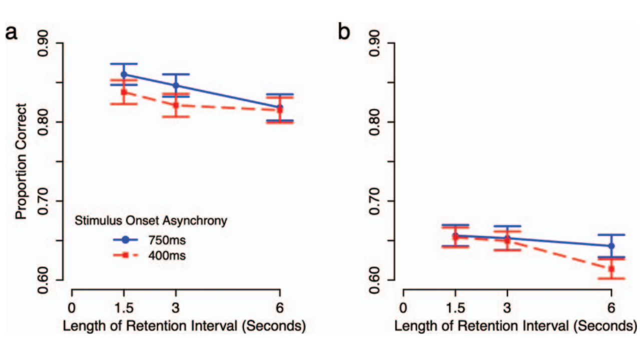

Figure 6. Mean proportion correct as a function of retention duration, consolidation condition, and set size in Experiment 4. Panel a shows mean performance at Set Size 3, Panel b shows mean performance at Set Size 6. Error bars represent standard error of the mean. See the online article for the color version of this figure.

Discussion

Experiment 4 again reveals the same rate of time-based forgetting across all conditions. While changes in the set size clearly have a large effect on overall performance, the rate of forgetting across the retention interval did not change in response to changes in the set size. At this point it seems clear that time-based loss of familiar verbal materials is consistently observed in change detection. Most past change detection work has differed in that it tends to use familiar visual items as the memoranda. In the next experiment we test whether the presence of time-based forgetting is similarly observed with familiar visual memory items.

Experiment 5

Many current models of working memory propose no distinction in how memory items from differing domains are treated if there is no difference in the ability to apply maintenance strategies such as rehearsal or chunking (Barrouillet & Camos, 2012; Cowan, 1988; Oberauer et al., 2012; Oberauer & Lin, 2017). In this experiment we replicated Experiment 4, but used colored squares as memory items to test whether the same patterns are observed in the loss of familiar visual items as seen with familiar verbal items in Experiment 4. The results of Experiments 4 and 5 can be compared to test whether forgetting rates are equivalent for familiar visual and verbal memory items.

Method

Participants. Seventy-seven students attending Montclair State University participated in the experiment. Fourteen participants were excluded from data analysis because they demonstrated near chance performance (overall mean proportion correct less than or equal to .60; see Footnote 2). This left a sample size of 63 participants for use in all analyses. All were fluent in English with normal or corrected-to-normal vision. This experiment was approved by the Montclair State University Institutional Review Board.

Materials. Memory items were colored squares 24 × 24 pixels in size, arranged randomly on the screen. Square color was randomly selected without replacement from a larger set of colors (RGB values: 255, 255, 255; 255, 0, 0; 0, 255, 0; 0, 0, 255; 255, 255, 0; 0, 255, 255; 255, 0, 255; 128, 0, 0; 128, 128, 0; 0, 128, 0; 128, 0,128; 0, 128, 128; 0, 0, 128). Masks were the same size and appeared at the same location as the colored squares. Each mask was comprised of a pattern of 16 (4 × 4 grid) randomly selected colors drawn from the same color set. No mask patterns were the same on any given trial.

[3] Mean accuracy for the Set Size 6 condition was reduced in Experiments 4 and 5 relative to the previous experiments. This is likely due to two causes. First, the number of total trials was doubled in Experiments 4 and 5 relative to previous experiments, resulting in additional fatigue. Second, in Experiments 1–3, set size was always fixed at six, whereas in Experiments 4 and 5 there was also an easier set size condition. The presence of relatively easy Set Size 3 trials likely resulted in a relative feeling of difficulty when completing Set Size 6 trials. Because of this perceived difficulty the effort exerted during Set Size 6 trials was likely reduced.

Design. The design was the same as in Experiment 4.

Procedure. The procedure was the same as in Experiment 4, except that colors were presented in place of letters.

Analysis. The analysis was the same as in Experiment 4.

Results

Mean performance in each condition is presented in Figure 7. The data look very similar to Experiment 4, but perhaps with a greater rate of forgetting. Forgetting again occurred as a function of time. As in Experiment 4, reducing the time for consolidation reduced performance slightly, but did not change the rate of forgetting. The set size manipulation had a large effect that did not induce a change in the rate of forgetting. A 3 (retention duration) × 2 (consolidation condition) × 2 (set size) repeated measures ANOVA of mean proportion correct confirms these observations. This test produced a clear main effect of retention duration, $F(2, 124) = 39.32$, $\eta_p^2 = 0.39$, Bayes factor $= 4.8 \times 10^{11}$ in favor of an effect, with performance decreasing as the retention interval increases. There was weak evidence for the presence of a main effect of Consolidation Time, $F(1, 62) = 9.86$, $\eta_p^2 = 0.14$, Bayes factor $= 2.3$ in favor of an effect, providing some support for performance improving with more time for consolidation. There was also a clear main effect of set size, $F(1, 62) = 524.2$, $\eta_p^2 = 0.89$, Bayes factor $= 3.1 \times 10^{143}$ in favor of an effect, with the larger set size resulting in poorer performance. No interactions were present, all Bayes factors >10 in favor of the null.

To test whether overall performance or the rate of forgetting differed between Experiments 4 (letters) and 5 (colors) we conducted a 2 (experiment) × 3 (retention duration) × 2 (consolidation condition) × 2 (set size) Mixed Effects ANOVA of mean proportion correct on the combined data from both experiments. There was no main effect of experiment, $F(1, 122) = 0.06$, Bayes factor $= 7.4$ in favor of the null. In addition, there was no evidence for an interaction between experiment and any other factor, all interactions involving the experiment factor produced Bayes Factors >20 in favor of the null.

Discussion

In Experiment 5 we again observed the same rate of forgetting over time across all conditions. Beyond this, there was no observable difference in performance between Experiment 4, using familiar verbal letters as memory items, and Experiment 5, using familiar colored squares as memory items. This makes it unlikely that stimulus domain is a factor in determining if one will observe decay or in determining the rate of any observed decay. The finding of a common temporal forgetting rate across differing domains is consistent with a single working memory system acting on both types of memory items (Barrouillet & Camos, 2012; Cowan, 1995; Oberauer et al., 2012; Oberauer & Lin, 2017).

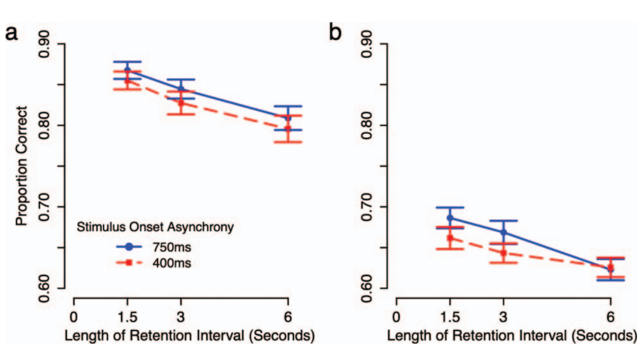

Figure 7. Mean proportion correct as a function of retention duration, consolidation condition, and set size in Experiment 5 (colors). Panel a shows mean performance at Set Size 3, Panel b shows mean performance at Set Size 6. Error bars represent standard error of the mean. See the online article for the color version of this figure.

Cross-Experiment Comparison

In each of our five experiments we observe a constant rate of forgetting across retention durations, regardless of all other manipulations. This raises the question of whether the rate of forgetting is also constant across all experiments. If so, it indicates that the forgetting rate represents something fundamental and predictable about human memory. In order to test this, we calculated each participant's mean accuracy for each retention duration across all experiments. All of our experiments used the same retention interval durations, allowing us to then directly enter this data into a mixed-factors ANOVA with retention duration as a within-participant factor and Experiment as a between participant factor. This analysis produced a clear main effect of retention duration, Bayes factor $= 1.42 \times 10^{42}$ in favor of an effect, with longer retention intervals leading to lower levels of performance. There was a clear effect of experiment, Bayes factor $= 1.64 \times 10^{16}$ in favor of an effect, with overall performance varying across experiments. More importantly the interaction between retention duration and experiment produced a Bayes Factor with considerable evidence for a null effect, interaction Bayes factor $= 6.18$ in favor of the null. This invariance across a large number of participants and experimental conditions provides considerable evidence for a fundamental cognitive process reflected by the forgetting rate. It should be noted that a Bayes factor of 6 in favor of the null is difficult to find, as the peak of the alternative hypothesis is at an effect size of 0 in the default Bayes factor analysis used here.

General Discussion

The present study clearly demonstrates time-based forgetting of familiar memory items, supporting the possibility that there is trace decay. Memory performance declined as the length of the unfilled retention interval between study and test increased in all tested conditions. We used considerable samples sizes of between 60–143 participants across each of the five experiments to demonstrate the reliability of time-based forgetting. These large sample sizes and the consistent replication of the time-based forgetting effects are important as past studies finding evidence both in favor of and against decay often used small samples sizes. In our experience change detection tasks often produce unreliable results with sample sizes of 20 or smaller, regardless of the *p* value attained in an ANOVA. Having established that time-based forgetting can be observed with familiar verbal materials, we explored three specific questions about trace decay. We asked (a) whether articulatory suppression plays any roles in determining the rate of decay, (b) whether the presence of trace decay is a result of insufficient time for consolidation, and (c) whether familiar verbal and visual memory items show different patterns of trace decay.

To answer these questions, we manipulated a number of factors that should affect the presence of observable trace decay under various working memory models (Baddeley, 1986; Cowan, 1995; Page & Norris, 1998). These factors include the presence of articulatory suppression, the time allowed for consolidation after each memory item, the total memory set size, and the stimulus modality. None of these factors had an appreciable effect on the rate of information loss. The answer to all three of our secondary questions was a clear no, these factors do not alter decay rates. The rate of forgetting seems to be a constant that occurs regardless of the strategies available within the memory paradigm or the overall performance level of the stimuli. Having established that trace decay does not vary across a variety of contexts we now explore the implications of our findings in depth.

Decay, Articulatory Suppression, and Verbal Rehearsal

In adding articulatory suppression to our change-detection task we assumed that we would be preventing verbal rehearsal of all or some subset of the memory items in Experiment 1 (Baddeley, 1986; Camos et al., 2009; Cowan et al., 1992). Adding articulatory suppression during retention lowered overall memory performance but did not alter the rate of forgetting (see Figure 2). This indicates that the availability of verbal rehearsal as a maintenance mechanism does not always prevent forgetting, consistent with past research. Specifically, several previous studies investigating serial recall of familiar verbal materials demonstrated an overall reduction in accuracy with the addition of articulatory suppression, but no increase in decay across a retention interval (Lewandowsky et al., 2004; Oberauer & Lewandowsky, 2008; Vallar & Baddeley, 1982).

That articulatory suppression did not prevent or alter the rate of decay is at odds with traditional phonological loop based models of working memory (Baddeley, 1986; Baddeley & Logie, 1999; Page & Norris, 1998). In these models, memory items enter modality-specific stores and must be continuously recycled through that store by an active rehearsal process in order to prevent trace decay. In the case of the verbal store, articulatory suppression is assumed to occupy the rehearsal mechanism, thereby preventing the phonological loop from recycling the memory items. This class of model predicts that when the rehearsal component of the phonological loop is free, such as in our no suppression conditions, no forgetting is expected for items being rehearsed. On the other hand, when articulation of the memory set is prevented, as in our suppression conditions in Experiment 1, there should be considerable trace decay. We saw no indication of the predicted interaction of the retention duration and the suppression condition in Experiment 1.

Rather than preventing rehearsal it could be that articulatory suppression disrupts the phonological traces used to code for the motor component of memory representations (Caplan et al., 1992; Service, 1998). While familiar verbal materials do clearly show forgetting over time, the phonological codes themselves may not decay, instead suffering only interference-based forgetting (Lewandowsky et al., 2004; Lewandowsky & Oberauer, 2008). In this case, articulatory suppression should simply cause a decrease in overall accuracy by abolishing the contribution of phonological codes to memory performance. The decay observed here would be acting upon some other portion of the memory representation, perhaps semantic, visual, spatial, or some combination of these representations.

This explanation intertwines interference explanations of forgetting (Nairne, 1990; Oberauer et al., 2012) with decay and rehearsal models of working memory (Baddeley, 1986; Barrouillet & Camos, 2015; Cowan, 1995). Interference is the sole cause of forgetting with phonological materials, but decay still occurs within nonphonological representations. In other ways, this explanation is difficult to reconcile with most past models. Many interference models explicitly exclude the existence of decay in any working memory representations (Nairne, 1990; Oberauer et al., 2012). The assumptions about the role of rehearsal in many decay models are themselves incompatible with the limited effect of articulatory rehearsal (see Baddeley, 1986; Page & Norris, 1998). In the present work, the phonological representation that is abolished by articulatory suppression in Experiment 1 only diminishes overall accuracy slightly, by about one letter remembered, from 3.9 to 3.1 letters when calculated using Cowan's k (Cowan, 2001). This is inconsistent with the consensus that rehearsal can maintain about 1.8s worth of phonological information (Baddeley et al., 1975; Cowan et al., 1992; Schweickert & Boruff, 1986). Rather than relying strongly on the phonological code, working memory performance seems to be strongly driven by maintenance of nonphonological material. This is more consistent with models emphasizing a strong role for attention-based maintenance of representations across modalities (Barrouillet et al., 2004; Cowan, 1995, 2005; Oberauer et al., 2012).

The present findings seem to imply that verbal rehearsal is not used to maintain the continued activation of phonological memory traces. Instead, there may be a relationship between verbal rehearsal and memory retention because verbal rehearsal is the outcome of maintaining phonological traces. Activity in areas representing speech information could lead to or prime speech behavior, accounting for the appearance of a causal relationship between rehearsal and memory. If the phonological memory traces do not decay, then there is no need for a memory mechanism dedicated to preventing their decay. This suggestion is not new, although it is uncommon. Several previous researchers have proposed that verbal rehearsal does not improve working memory performance (G. D. A. Brown & Hulme, 1995; Nairne, 2002). In particular Souza and Oberauer (2018) recently observed and manipulated the rehearsal schedules of participants maintaining lists of words. While those that rehearsed more often tended to have better memory performance, increasing their rehearsal rates through task-condition instructions did not improve performance. Our present results converge with these findings, suggesting that the availability of rehearsal does not change the rate of forgetting, at least for our familiar items presented in spatial arrays.

Alternative Conceptions of Decay and Forgetting

In the previous section, we proposed that the time-based forgetting observed here results from decay in the nonphonological portions of the memory trace. An alternative interpretation is that all memory traces are treated the same, regardless of the nature of the representation, that is, phonological, semantic, visual, and so forth. This is the approach taken by many models of memory (Barrouillet et al., 2004; Cowan, 1995; Oberauer & Lin, 2017), particularly interference models positing no trace decay (Nairne, 1990; Oberauer et al., 2012). In this approach to our findings, the locus of time-based forgetting is not the memory representation itself, as is the case with trace decay of neural activation, but instead follows from an increasing probability of task disengagement as time progresses. Memory performance decreases over time because the individual engaging in active maintenance has some probability of losing focus on the current task and thinking about unrelated thoughts such as their weekend plans, the comfort level of their shoes, or some other irrelevant thought. Within the working memory literature, unintentionally losing focus on the task and engaging in task unrelated thinking has been referred to as mind wandering (Feng, D'Mello, & Graesser, 2013; Kane et al., 2007; Smallwood & Schooler, 2015). If a there is a constant probability of losing focus on the current task during each unit of time, then this should lead to an increase in mind wandering with an increased retention interval.

There could be two effects as a result of mind-wandering. First, if an item decays from memory (either gradually or all at once) when it is not being refreshed via attention, then mind-wandering would remove that possibility of refreshing and allow decay to proceed. Second, during mind-wandering, internal attentional resources within working memory might be redirected away from the to-be-remembered stimulus and toward something unrelated, ultimately leading to a form of endogenous interference that disrupts the memory trace. Behaviorally we would then observe lower mean performance and more guessing with longer retention times, what we have referred to in the past as a minimal definition of decay (Cowan, Saults, & Nugent, 1997; Ricker et al., 2016). This approach does not posit that time is the cause of forgetting, but rather that the passage of time results in a predictable rate of forgetting. If the rate of mind wandering is equivalent across experiments and manipulations within the present study, then this approach provides a good fit to the present data.

Previous research suggests that a relationship between mind wandering and the rate of forgetting over time is plausible. Past work has suggested that displacement of the contents of working memory by distracting or irrelevant material is a major contributor to forgetting (Craik & Levy, 1976; Unsworth & Engle, 2007; Waugh & Norman, 1965). Greater rates of mind wandering during tasks such as reading and studying are also related to lower working memory performance across individuals (Kane et al., 2007; McVay & Kane, 2012; Unsworth & McMillan, 2013). Moment-to-moment reports of mind wandering are related to disruption of ongoing cognitive performance (Feng et al., 2013; McVay & Kane, 2009) including performance on working memory tasks (Adam & Vogel, 2017; Krimsky, Forster, Llabre, & Jha, 2017; Mrazek et al., 2012; Robison & Unsworth, 2018; Unsworth & Robison, 2016), consistent with the idea that mind wandering leads to displacement of the contents of working memory. Future studies should explicitly examine the relationship between mind wandering and rates of time-based forgetting across individuals to judge whether this explanation is viable.

If mind wandering is producing the observed time-based loss of memory performance here and elsewhere (Lilienthal et al., 2014; Ricker & Cowan, 2010, 2014; Sakai & Inui, 2002; Vergauwe et al., 2014), it implies that trace decay or memory decay may be a misleading moniker. Decay in the mind-wandering context reflects forgetting due to task disengagement or endogenous interference, not forgetting caused by loss of activation within a specific memory trace as is typically envisioned by trace decay advocates (Baddeley et al., 1975; Barrouillet & Camos, 2015; Cowan, 1995; Page & Norris, 1998; Ricker, 2015). Cowan et al. (1997, p. 396) offered three different definitions of decay, the first of which refers to any predictable rate of forgetting across time, and the second of which did refer to interference from internal thoughts that might be partly controllable and partly beyond control. Increasing displacement of traces from working memory with the passage of time is consistent with these descriptions of decay in that memory performance decreases across a retention interval at a measurable rate. It is, however, inconsistent with what is generally meant when the construct of memory decay is invoked. The decay in this approach is not occurring at the level of specific memory items, is not caused by the passage of time, and does not occur gradually across a trial. The observed performance decay is on the likelihood of the individual continuing to stay engaged in the memory task as the retention interval increases and on the resulting level of observed performance when aggregated across many trials.

Dissociating interference and decay theories of forgetting is often complex (e.g., see, Barrouillet et al., 2013). It is difficult to introduce a distracting task that will prevent rehearsal and attention-based refreshing, remove the contribution of attention so it does not diminish across retention intervals through mind-wandering, and not introduce exogenous interference. The few studies that have come closest to meeting these requirements (Reitman, 1974; Watkins et al., 1973) tend to support the existence of decay that is not caused by mind-wandering. Just as one can argue for the absence of decay in some situations despite the inclusion of some potential distraction (Oberauer & Lewandowsky, 2008), here we argue for the presence of decay in other situations even without the inclusion of a rehearsal- or refreshing-prevention task.

No Effect of Consolidation Time on Forgetting Rates

Recent work by Ricker and Cowan (2014) demonstrated that allowing more time for consolidation after the presentation of a memory item or set leads to lower rates of time-based forgetting. This study served as the template and motivation for Experiments 2a and 2b. In the present set of experiments, we failed to replicate the findings of Ricker and Cowan (2014), instead finding no effect of the time available for consolidation on the rate of forgetting over time. We replicated the present lack of an effect of consolidation time on forgetting rates in Experiments 2–5, across a range of conditions, confirming the validity of the present result.

The only major difference between Experiments 2a and 2b and the experiments of Ricker and Cowan (2014) is that the present experiments used familiar letters as the memoranda while Ricker and Cowan (2014) used unfamiliar symbols. Within a model of time-based forgetting as decay of nonphonological traces, it is unclear why consolidation should be effective at preventing memory decay with unfamiliar characters but not with familiar letters. One possibility is that the difficulty of processing unfamiliar characters during consolidation is more effortful and results in the allocation of more attention to the task, indirectly improving later performance. We can also consider consolidation effects on time-based forgetting from the mind-wandering perspective. It may be that altering the time for consolidation after each letter in the present task does not change how engaging the task is for participants, but increasing the time for consolidation after each unfamiliar character by Ricker and Cowan (2014) did change how engaging the task was for participants.

If time-based forgetting is a reflection of mind wandering, then only factors that encourage or discourage sustained task engagement should alter the rate of loss. It is easy to imagine that in Ricker and Cowan's (2014) experiments with increased free time after each unfamiliar character participants found the unfamiliar items increasingly more engaging. For example, thinking about a novel item for a longer time may emphasize its novelty, resulting in better capture of attention and a lower probability of mind wandering a few seconds later. This difference in attention capture by the memory stimulus could account for the across study difference in consolidation effects between the present work and Ricker and Cowan (2014). More attentional capture by the unfamiliar characters as compared with the letters task means more task engagement and a lower observed forgetting rate in high consolidation conditions of Ricker and Cowan (2014).

Familiar Memory Items Are Forgotten Over Time

Experiments 4 and 5 of the present work only differed in the nature of the memory items. Experiment 4 used familiar verbal letters while Experiment 5 used familiar colored squares. These memory items clearly differ in modality. Letters are generally assumed to be verbal in nature while colors are considered to be visual in nature. Despite this difference, the results of the two experiments were the same. When we entered both sets of data into a single ANOVA with experiment as a factor, there was no effect of experiment and no interaction of experiment with any other factor. This demonstrates that the locus of the time-based forgetting effect does not stem from the modality of the memory stimuli. Memory items presented in an array are lost over time regardless of the memory item modality. This resolves a conflict between findings from the serial recall literature, in which familiar verbal memory items do not show evidence of time-based loss, and findings from the visual working memory literature using array presentation of unfamiliar visual characters, in which memory items consistently show time-based loss. It is not the verbal/visual or familiar/unfamiliar distinction that drives the existence of forgetting over time, but rather something about the distinction between serial recall and visual array change detection.

There are several potential explanations for this distinction between serial recall and visual array findings. From a trace decay perspective, serial recall performance may rely primarily on phonological representations that do not show trace decay while visual array performance may encourage reliance on visual representations that do show trace decay, at the expense of phonological representations. If this is the case then we should not observe trace decay in serial recall paradigms as phonological traces do not suffer decay. Another possibility is that the visual representations of verbal materials are used only because spatial location is available as a cue. If, for example, the probe always appeared centrally, spatial location cues would no longer be useful and participants may encode the letters verbally.

If we instead follow the mind wandering explanation of time-based forgetting, it could be that visual array experiments are simply less engaging than serial recall experiments. This would result in more task-unrelated thoughts during the retention intervals of visual array trials as compared to serial recall trials. Research by Rouder et al. (2008) indicates that participants in visual array studies fail to sustain attention to the task on a significant number of trials. In their work participants did not attend to the visual array task on 12% of the total trials across the experiment, despite there being no secondary task (see also, R. D. Morey, 2011). If a considerable portion of these attentional lapses happen during memory retention, as opposed to encoding or recall, it could easily account for our observed rates of forgetting in the present work.

Contrast With Previous Findings

The present findings differ notably from several previous findings. One set of findings are our previous investigations of time-based forgetting in verbal working memory. Ricker and Cowan (2010, Experiment 2) investigated whether arrays of letters were forgotten over time using nearly the same task as we use in the present work. Ricker and Cowan (2010) found that there was no loss of memory over time for letter arrays when there was no secondary task and no articulatory suppression, in agreement with the predictions of phonological loop models. This is in stark contrast to the present series of experiments in which we observe robust forgetting across the same retention interval duration, set size, and presentation time as used by Ricker and Cowan (2010). Ricker et al. (2014) also found time-based loss of letter arrays in a change detection task, in agreement with the present findings, but in contrast to (Ricker & Cowan, 2010).

In the present work and in that of Ricker and Cowan (2010), the trials that contain no secondary task have no obvious methodological differences that could explain the discrepancy. There were methodological differences between the two experiments in the way the overall experimental session was experienced by the participant. Ricker and Cowan (2010) manipulated retention duration between trials within each experimental block while manipulating the secondary task (none, number reading, simple arithmetic) and memory stimulus (letters or unfamiliar characters) between blocks. Similarly, Experiment 1 of the present work manipulated retention duration between trials within each experimental block and suppression condition between blocks. Although the basic design is similar, it is possible that the between-block changes in secondary task and stimulus identity by Ricker and Cowan (2010) promoted greater task engagement during the letter trials than in the present experiments. If this is the case then we would predict that Ricker and Cowan (2010) should show lower rates of forgetting than the present work. It should be noted however that many experiments use similar task designs to the present work, with a consistent task across 40 min or more, making the design far from exceptional in this respect.

The present findings also contrast with the wide body of work in serial recall of verbal memory items showing no forgetting as a function of retention time (Lewandowsky et al., 2004; Oberauer & Lewandowsky, 2008, 2014). There are several differences between these serial recall studies and the present work that could account for the difference. One difference is that serial recall studies rely upon recall rather than correct probe recognition. Uittenhove, Chaabi, Camos, and Barrouillet (2019) recently suggested fundamental differences in how items are maintained when recall or recognition are required at test. Uittenhove et al. (2019) support this claim with data showing considerable difference in memory performance for the same task when recall is required rather than recognition. Recall by its very nature should promote greater task engagement than recognition because active production of a response is required at test with recall whereas verification of an item produced for the participant is all that is required with recognition. Studies requiring memory recall should produce lower forgetting rates than studies utilizing memory recognition tests following a task-engagement theory of time-based forgetting, so long as all other factors are equated. Balota and Neely (1980) produced data supporting this prediction, showing that participants performed better on both recognition and recall tests of memory when expecting recall compared with when they expected a recognition test.

There are a number of other differences between typical serial recall procedures and the current change detection task that could potentially encourage greater task engagement during serial recall. Typical serial recall tasks include longer presentation times, participant controlled presentation timings, manipulation of the delay period between memory item presentations or between memory item recalls (change detection typically uses a retention interval between item presentation and test), and fewer trials overall. All of these factors may lead to less mind wandering and result in lower rates of time-based forgetting.

We do not assert that the serial recall research is wrong in its finding no time-based forgetting in those procedures or that change detection is the only method that should be used when studying patterns of forgetting over time. We demonstrate that time-based forgetting of familiar memory items can be reliably observed within working memory paradigms. Temporal factors may not account for the majority of observed forgetting within any given study, but that does not imply that loss of familiar memory items with the passage of time is a myth.

Concluding Remarks

The present work shows that time-based forgetting does occur, even with familiar verbal memory items. This forgetting may not be the traditional trace decay operating upon individual memory activations, but that does not mean memory performance does not decrease as more time is required between presentation and test. Neither does our argument in favor of the existence of decay for nonphonological traces necessarily imply that interference does not cause forgetting. One possibility we explicitly endorse is that phonological representations may be lost exclusively through interference. Our goal is not to advocate for a temporal-only theory of forgetting, but rather that room must be made for temporal factors within current theories. Parsimony is not served by denying the effect of time on forgetting.

References

Adam, K. C. S., & Vogel, E. K. (2017). Confident failures: Lapses of working memory reveal a metacognitive blind spot. *Attention, Perception & Psychophysics, 79,* 1506–1523. http://dx.doi.org/10.3758/s13414-017-1331-8

Atkinson, R. C., & Shiffrin, R. M. (1968). Human memory: A proposed system and its control processes. *Psychology of Learning and Motivation, 2,* 89–195. http://dx.doi.org/10.1016/S0079-7421(08)60422-3

Baddeley, A. D. (1986). *Working memory*. Oxford, UK: Oxford University Press.

Baddeley, A. D., & Logie, R. H. (1999). Working memory: The multiple-component model. In A. Miyake & P. Shah (Eds.), *Models of working memory* (pp. 28–61). Cambridge, UK: Cambridge University Press. http://dx.doi.org/10.1017/CBO9781139174909.005

Baddeley, A. D., Thomson, N., & Buchanan, M. (1975). Word length and the structure of short-term memory. *Journal of Verbal Learning and Verbal Behavior, 14,* 575–589. http://dx.doi.org/10.1016/S0022-5371(75)80045-4

Balota, D. A., & Neely, J. H. (1980). Test-expectancy and word-frequency effects in recall and recognition. *Journal of Experimental Psychology: Human Learning and Memory, 6,* 576–587. http://dx.doi.org/10.1037/0278-7393.6.5.576

Barrouillet, P., Bernardin, S., & Camos, V. (2004). Time constraints and resource sharing in adults' working memory spans. *Journal of Experimental Psychology: General, 133,* 83–100. http://dx.doi.org/10.1037/0096-3445.133.1.83

Barrouillet, P., & Camos, V. (2012). As time goes by: Temporal constraints in working memory. *Current Directions in Psychological Science, 21,* 413–419. http://dx.doi.org/10.1177/0963721412459513

Barrouillet, P., & Camos, V. (2015). *Working memory: Loss and reconstruction*. New York, NY: Psychology Press.

Barrouillet, P., Plancher, G., Guida, A., & Camos, V. (2013). Forgetting at short term: When do event-based interference and temporal factors have an effect? *Acta Psychologica, 142,* 155–167. http://dx.doi.org/10.1016/j.actpsy.2012.12.003

Bayliss, D. M., Bogdanovs, J., & Jarrold, C. (2015). Consolidating working memory: Distinguishing the effects of consolidation, rehearsal and attentional refreshing in a working memory span task. *Journal of Memory and Language, 81,* 34–50. http://dx.doi.org/10.1016/j.jml.2014.12.004

Berman, M. G., Jonides, J., & Lewis, R. L. (2009). In search of decay in verbal short-term memory. *Journal of Experimental Psychology: Learning, Memory, and Cognition, 35,* 317–333. http://dx.doi.org/10.1037/a0014873

Brown, G. D. A., & Hulme, C. (1995). Modeling item length effects in memory span: No rehearsal needed? *Journal of Memory and Language, 34,* 594–621. http://dx.doi.org/10.1006/jmla.1995.1027

Brown, J. (1958). Some tests of the decay theory of immediate memory. *The Quarterly Journal of Experimental Psychology, 10,* 12–21. http://dx.doi.org/10.1080/17470215808416249

Camos, V., Lagner, P., & Barrouillet, P. (2009). Two maintenance mechanisms of verbal information in working memory. *Journal of Memory and Language, 61,* 457–469. http://dx.doi.org/10.1016/j.jml.2009.06.002

Caplan, D., Rochon, E., & Waters, G. S. (1992). Articulatory and phonological determinants of word length effects in span tasks. *The Quarterly Journal of Experimental Psychology Section A, 45,* 177–192. http://dx.doi.org/10.1080/14640749208401323

Cohen-Dallal, H., Fradkin, I., & Pertzov, Y. (2018). Are stronger memories forgotten more slowly? No evidence that memory strength influences the rate of forgetting. *PLoS ONE, 13,* e0200292. http://dx.doi.org/10.1371/journal.pone.0200292

Cowan, N. (1984). On short and long auditory stores. *Psychological Bulletin, 96,* 341–370. http://dx.doi.org/10.1037/0033-2909.96.2.341

Cowan, N. (1988). Evolving conceptions of memory storage, selective attention, and their mutual constraints within the human information-processing system. *Psychological Bulletin, 104,* 163–191. http://dx.doi.org/10.1037/0033-2909.104.2.163

Cowan, N. (1995). *Attention and memory: An integrated framework* (Vol. 26). New York, NY: Oxford University Press.

Cowan, N. (2001). The magical number 4 in short-term memory: A reconsideration of mental storage capacity. *Behavioral and Brain Sciences, 24,* 87–114. http://dx.doi.org/10.1017/S0140525X01003922

Cowan, N. (2005). *Working memory capacity*. Hove, UK: Psychology Press.

Cowan, N., & AuBuchon, A. M. (2008). Short-term memory loss over time without retroactive stimulus interference. *Psychonomic Bulletin & Review, 15,* 230–235. http://dx.doi.org/10.3758/PBR.15.1.230

Cowan, N., Day, L., Saults, J. S., Keller, T. A., Johnson, T., & Flores, L. (1992). The role of verbal output time in the effects of word length on immediate memory. *Journal of Memory and Language, 31,* 1–17. http://dx.doi.org/10.1016/0749-596X(92)90002-F

Cowan, N., Nugent, L. D., Elliott, E. M., & Geer, T. (2000). Is there a temporal basis of the word length effect? A response to Service (1998). *The Quarterly Journal of Experimental Psychology Section A, 53,* 647–660. http://dx.doi.org/10.1080/713755905

Cowan, N., Saults, J. S., & Nugent, L. D. (1997). The role of absolute and relative amounts of time in forgetting within immediate memory: The case of tone-pitch comparisons. *Psychonomic Bulletin & Review, 4,* 393–397. http://dx.doi.org/10.3758/BF03210799

Craik, F. I., & Levy, B. (1976). The concept of primary memory. In W. K. Estes (Ed.), *Handbook of learning and cognitive processes* (Vol. 4, pp. 133–175). New York, NY: Psychology Press.

Cusack, R., Lehmann, M., Veldsman, M., & Mitchell, D. J. (2009). Encoding strategy and not visual working memory capacity correlates with intelligence. *Psychonomic Bulletin & Review, 16,* 641–647. http://dx.doi.org/10.3758/PBR.16.4.641

Farrell, S., & Lewandowsky, S. (2002). An endogenous distributed model of ordering in serial recall. *Psychonomic Bulletin & Review, 9,* 59–79. http://dx.doi.org/10.3758/BF03196257

Feng, S., D'Mello, S., & Graesser, A. C. (2013). Mind wandering while reading easy and difficult texts. *Psychonomic Bulletin & Review, 20,* 586–592. http://dx.doi.org/10.3758/s13423-012-0367-y

Jolicoeur, P., & Dell'Acqua, R. (1998). The demonstration of short-term consolidation. *Cognitive Psychology, 36,* 138–202. http://dx.doi.org/10.1006/cogp.1998.0684

Kane, M. J., Brown, L. H., McVay, J. C., Silvia, P. J., Myin-Germeys, I., & Kwapil, T. R. (2007). For whom the mind wanders, and when: An experience-sampling study of working memory and executive control in daily life. *Psychological Science, 18,* 614–621. http://dx.doi.org/10.1111/j.1467-9280.2007.01948.x

Kass, R. E., & Raftery, A. E. (1995). Bayes factors. *Journal of the American Statistical Association, 90,* 773–795. http://dx.doi.org/10.1080/01621459.1995.10476572

Keppel, G., & Underwood, B. J. (1962). Proactive inhibition in short-term retention of single items. *Journal of Verbal Learning and Verbal Behavior, 1,* 153–161. http://dx.doi.org/10.1016/S0022-5371(62)80023-1

Krimsky, M., Forster, D. E., Llabre, M. M., & Jha, A. P. (2017). The influence of time on task on mind wandering and visual working memory. *Cognition, 169,* 84–90. http://dx.doi.org/10.1016/j.cognition.2017.08.006

Lewandowsky, S., Duncan, M., & Brown, G. D. (2004). Time does not cause forgetting in short-term serial recall. *Psychonomic Bulletin & Review, 11,* 771–790. http://dx.doi.org/10.3758/BF03196705

Lewandowsky, S., & Oberauer, K. (2008). The word-length effect provides no evidence for decay in short-term memory. *Psychonomic Bulletin & Review, 15,* 875–888. http://dx.doi.org/10.3758/PBR.15.5.875

Lewandowsky, S., & Oberauer, K. (2015). Rehearsal in serial recall: An unworkable solution to the nonexistent problem of decay. *Psychological Review, 122,* 674–699. http://dx.doi.org/10.1037/a0039684

Lewandowsky, S., Oberauer, K., & Brown, G. D. (2009). No temporal decay in verbal short-term memory. *Trends in Cognitive Sciences, 13,* 120–126. http://dx.doi.org/10.1016/j.tics.2008.12.003

Lilienthal, L., Hale, S., & Myerson, J. (2014). The effects of environmental support and secondary tasks on visuospatial working memory. *Memory & Cognition, 42,* 1118–1129. http://dx.doi.org/10.3758/s13421-014-0421-2

McVay, J. C., & Kane, M. J. (2009). Conducting the train of thought: Working memory capacity, goal neglect, and mind wandering in an executive-control task. *Journal of Experimental Psychology: Learning, Memory, and Cognition, 35,* 196–204. http://dx.doi.org/10.1037/a0014104

McVay, J. C., & Kane, M. J. (2012). Why does working memory capacity predict variation in reading comprehension? On the influence of mind wandering and executive attention. *Journal of Experimental Psychology: General, 141,* 302–320. http://dx.doi.org/10.1037/a0025250

Morey, C. C., & Bieler, M. (2013). Visual short-term memory always requires general attention. *Psychonomic Bulletin & Review, 20,* 163–170. http://dx.doi.org/10.3758/s13423-012-0313-z

Morey, R. D. (2011). A Bayesian hierarchical model for the measurement of working memory capacity. *Journal of Mathematical Psychology, 55,* 8–24. http://dx.doi.org/10.1016/j.jmp.2010.08.008

Morey, R., & Rouder, J. (2013). Bayes factor: Computation of Bayes factors for simple designs (Version 0.9.12–2) [Computer program]. Retrieved from https://richarddmorey.github.io/BayesFactor/

Mrazek, M. D., Smallwood, J., Franklin, M. S., Chin, J. M., Baird, B., & Schooler, J. W. (2012). The role of mind-wandering in measurements of general aptitude. *Journal of Experimental Psychology: General, 141,* 788–798. http://dx.doi.org/10.1037/a0027968

Nairne, J. S. (1990). A feature model of immediate memory. *Memory & Cognition, 18,* 251–269. http://dx.doi.org/10.3758/BF03213879

Nairne, J. S. (2002). Remembering over the short-term: The case against the standard model. *Annual Review of Psychology, 53,* 53–81. http://dx.doi.org/10.1146/annurev.psych.53.100901.135131

Nieuwenstein, M., & Wyble, B. (2014). Beyond a mask and against the bottleneck: Retroactive dual-task interference during working memory consolidation of a masked visual target. *Journal of Experimental Psychology: General, 143,* 1409–1427. http://dx.doi.org/10.1037/a0035257

Oberauer, K., & Lewandowsky, S. (2008). Forgetting in immediate serial recall: Decay, temporal distinctiveness, or interference? *Psychological Review, 115,* 544–576. http://dx.doi.org/10.1037/0033-295X.115.3.544

Oberauer, K., & Lewandowsky, S. (2014). Further evidence against decay in working memory. *Journal of Memory and Language, 73,* 15–30. http://dx.doi.org/10.1016/j.jml.2014.02.003

Oberauer, K., Lewandowsky, S., Farrell, S., Jarrold, C., & Greaves, M. (2012). Modeling working memory: An interference model of complex span. *Psychonomic Bulletin & Review, 19,* 779–819. http://dx.doi.org/10.3758/s13423-012-0272-4

Oberauer, K., & Lin, H.-Y. (2017). An interference model of visual working memory. *Psychological Review, 124,* 21–59. http://dx.doi.org/10.1037/rev0000044

Page, M. P., & Norris, D. (1998). The primacy model: A new model of immediate serial recall. *Psychological Review, 105,* 761–781. http://dx.doi.org/10.1037/0033-295X.105.4.761-781

Peterson, L. R., & Peterson, M. J. (1959). Short-term retention of individual verbal items. *Journal of Experimental Psychology, 58,* 193–198. http://dx.doi.org/10.1037/h0049234

Reitman, J. S. (1974). Without surreptitious rehearsal, information in short-term memory decay. *Journal of Verbal Learning and Verbal Behavior, 13,* 365–377. http://dx.doi.org/10.1016/S0022-5371(74)80015-0

Ricker, T. J. (2015). The role of short-term consolidation in memory persistence. *AIMS Neuroscience, 2,* 259–279. http://dx.doi.org/10.3934/Neuroscience.2015.4.259

Ricker, T. J., & Cowan, N. (2010). Loss of visual working memory within seconds: The combined use of refreshable and non-refreshable features. *Journal of Experimental Psychology: Learning, Memory, and Cognition, 36,* 1355–1368. http://dx.doi.org/10.1037/a0020356

Ricker, T. J., & Cowan, N. (2014). Differences between presentation methods in working memory procedures: A matter of working memory consolidation. *Journal of Experimental Psychology: Learning, Memory, and Cognition, 40,* 417–428. http://dx.doi.org/10.1037/a0034301

Ricker, T. J., & Hardman, K. O. (2017). The nature of short-term consolidation in visual working memory. *Journal of Experimental Psychology: General, 146,* 1551–1573. http://dx.doi.org/10.1037/xge0000346

Ricker, T. J., Nieuwenstein, M. R., Bayliss, D. M., & Barrouillet, P. (2018). Working memory consolidation: Insights from studies on attention and working memory. *Annals of the New York Academy of Sciences, 1424,* 8–18. http://dx.doi.org/10.1111/nyas.13633

Ricker, T. J., & Sandry, J. (2018). The relationship between masking and short-term consolidation during recall from visual working memory. *Annals of the New York Academy of Sciences, 1424,* 91–101. http://dx.doi.org/10.1111/nyas.13641

Ricker, T. J., Spiegel, L. R., & Cowan, N. (2014). Time-based loss in visual short-term memory is from trace decay, not temporal distinctiveness. *Journal of Experimental Psychology: Learning, Memory, and Cognition, 40,* 1510–1523. http://dx.doi.org/10.1037/xlm0000018

Ricker, T. J., Vergauwe, E., & Cowan, N. (2016). Decay theory of immediate memory: From Brown (1958) to today (2014). *Quarterly Journal of Experimental Psychology: Human Experimental Psychology, 69,* 1969–1995. http://dx.doi.org/10.1080/17470218.2014.914546

Robison, M. K., & Unsworth, N. (2018). Pupillometry tracks fluctuations in working memory performance. *Attention, Perception & Psychophysics, 81,* 407–419. http://dx.doi.org/10.3758/s13414-018-1618-4

Rouder, J. N., Morey, R. D., Cowan, N., Zwilling, C. E., Morey, C. C., & Pratte, M. S. (2008). An assessment of fixed-capacity models of visual working memory. *Proceedings of the National Academy of Sciences of the United States of America, 105,* 5975–5979. http://dx.doi.org/10.1073/pnas.0711295105

Rouder, J. N., Morey, R. D., Speckman, P. L., & Province, J. M. (2012). Default Bayes factors for ANOVA designs. *Journal of Mathematical Psychology, 56,* 356–374. http://dx.doi.org/10.1016/j.jmp.2012.08.001

Rouder, J. N., Morey, R. D., Verhagen, J., Province, J. M., & Wagenmakers, E.-J. (2016). Is there a free lunch in inference? *Topics in Cognitive Science, 8,* 520–547. http://dx.doi.org/10.1111/tops.12214

Rouder, J. N., Speckman, P. L., Sun, D., Morey, R. D., & Iverson, G. (2009). Bayesian t tests for accepting and rejecting the null hypothesis. *Psychonomic Bulletin & Review, 16,* 225–237. http://dx.doi.org/10.3758/PBR.16.2.225

Sakai, K., & Inui, T. (2002). A feature-segmentation model of short-term visual memory. *Perception, 31,* 579–589. http://dx.doi.org/10.1068/p3320

Schweickert, R., & Boruff, B. (1986). Short-term memory capacity: Magic number or magic spell? *Journal of Experimental Psychology: Learning, Memory, and Cognition, 12,* 419–425. http://dx.doi.org/10.1037/0278-7393.12.3.419

Service, E. (1998). The effect of word length on immediate serial recall depends on phonological complexity, not articulatory duration. *The Quarterly Journal of Experimental Psychology Section A, 51,* 283–304. http://dx.doi.org/10.1080/713755759

Smallwood, J., & Schooler, J. W. (2015). The science of mind wandering: Empirically navigating the stream of consciousness. *Annual Review of Psychology, 66,* 487–518. http://dx.doi.org/10.1146/annurev-psych-010814-015331

Souza, A. S., & Oberauer, K. (2018). Does articulatory rehearsal help immediate serial recall? *Cognitive Psychology, 107,* 1–21. http://dx.doi.org/10.1016/j.cogpsych.2018.09.002

Uittenhove, K., Chaabi, L., Camos, V., & Barrouillet, P. (2019). Is working memory storage intrinsically domain-specific? *Journal of Experimental Psychology: General*. Advance online publication. http://dx.doi.org/10.1037/xge0000566

Unsworth, N., & Engle, R. W. (2007). The nature of individual differences in working memory capacity: Active maintenance in primary memory and controlled search from secondary memory. *Psychological Review, 114,* 104–132. http://dx.doi.org/10.1037/0033-295X.114.1.104

Unsworth, N., & McMillan, B. D. (2013). Mind wandering and reading comprehension: Examining the roles of working memory capacity, interest, motivation, and topic experience. *Journal of Experimental Psychology: Learning, Memory, and Cognition, 39,* 832–842. http://dx.doi.org/10.1037/a0029669

Unsworth, N., & Robison, M. K. (2016). The influence of lapses of attention on working memory capacity. *Memory & Cognition, 44,* 188–196. http://dx.doi.org/10.3758/s13421-015-0560-0

Vallar, G., & Baddeley, A. D. (1982). Short-term forgetting and the articulatory loop. *The Quarterly Journal of Experimental Psychology Section A, 34,* 53–60. http://dx.doi.org/10.1080/14640748208400857

Vergauwe, E., Camos, V., & Barrouillet, P. (2014). The impact of storage on processing: How is information maintained in working memory? *Journal of Experimental Psychology: Learning, Memory, and Cognition, 40,* 1072–1095. http://dx.doi.org/10.1037/a0035779

Vogel, E. K., Woodman, G. F., & Luck, S. J. (2006). The time course of consolidation in visual working memory. *Journal of Experimental Psychology: Human Perception and Performance, 32,* 1436–1451. http://dx.doi.org/10.1037/0096-1523.32.6.1436

Watkins, M. J., Watkins, O. C., Craik, F. I., & Mazuryk, G. (1973). Effect of nonverbal distraction on short-term storage. *Journal of Experimental Psychology, 101,* 296–300. http://dx.doi.org/10.1037/h0035252

Waugh, N. C., & Norman, D. A. (1965). Primary memory. *Psychological Review, 72,* 89–104. http://dx.doi.org/10.1037/h0021797

Woodman, G. F., & Vogel, E. K. (2005). Fractionating working memory: Consolidation and maintenance are independent processes. *Psychological Science, 16,* 106–113. http://dx.doi.org/10.1111/j.0956-7976.2005.00790.x

Woodman, G. F., Vogel, E. K., & Luck, S. J. (2012). Flexibility in visual working memory: Accurate change detection in the face of irrelevant variations in position. *Visual Cognition, 20,* 1–28. http://dx.doi.org/10.1080/13506285.2011.630694

Wyble, B., Potter, M. C., Bowman, H., & Nieuwenstein, M. (2011). Attentional episodes in visual perception. *Journal of Experimental Psychology: General, 140,* 488–505. http://dx.doi.org/10.1037/a0023612

Zhang, W., & Luck, S. J. (2009). Sudden death and gradual decay in visual working memory. *Psychological Science, 20,* 423–428. http://dx.doi.org/10.1111/j.1467-9280.2009.02322.x

Received October 18, 2018
Revision received March 22, 2019
Accepted March 22, 2019 ■