Attention, Perception, & Psychophysics (2023) 85:2210–2225
https://doi.org/10.3758/s13414-023-02757-7

The slow rate of working memory consolidation from vision is a structural limit

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Accepted: 22 June 2023 / Published online: 26 July 2023

Abstract

The speed with which information from vision is transformed into working memory (WM) representations that resist interference from ongoing perception and cognition is the subject of conflicting results. Using distinct paradigms, researchers have arrived at estimates of the consolidation time course ranging from 25 ms to 1 s – a range of more than an order of magnitude. However, comparisons of consolidation duration across very different estimation paradigms rely on the implicit assumption that WM consolidation speed is a stable, structural constraint of the WM system. The extremely large variation in WM consolidation speed estimates across measurement approaches motivated the current work's goal of determining whether consolidation speed truly is a stable structural constraint of WM encoding, or instead might be under strategic control as suggested by some accounts. By manipulating the relative task priority of WM encoding and a subsequent sensorimotor decision in a dual-task paradigm, the current experiments demonstrate that the long duration of WM consolidation does not change as a result of task-specific strategies. These results allow comparison of WM consolidation across estimation approaches, are consistent with recent multi-phase WM consolidation models, and are consistent with consolidation duration being an inflexible structural limit.

Keywords Dual-task performance · Dual task procedures (PRP) · Working memory

Introduction

Working memory (WM) – the mental workspace that allows maintenance and manipulation of information – is a crucial part of many cognitive processes, guides behavior and decision making (e.g., Diamond, 2013; Malenka et al., 2009), yet is highly capacity limited (e.g., Oberauer et al., 2016). The mechanisms and limitations of human WM have been debated for decades. One such debate centers on the temporal dynamics of WM consolidation – the process by which WM information attains a form that can survive disruption by new sensory information or cognitive demands. The duration of consolidation has not been definitively established. Visual masking studies show that consolidation is rapid: 20–50 ms per item (Gegenfurtner & Sperling, 1993; Vogel et al., 2006; Woodman & Vogel, 2005), though recent evidence that multiple items may be consolidated in parallel complicates inferences (Mance et al., 2012; Rideaux et al., 2018). Other approaches suggest much longer-lasting consolidation: psychological refractory period (PRP)/dual-task proactive interference experiments (Jolicoeur & Dell'Acqua, 1998, 1999) and attentional blink studies (e.g., Chun & Potter, 1995; Raymond et al., 1992) show that consolidation can take hundreds of milliseconds.

A potential culprit for such widely varying estimates is the choice of event used to terminate consolidation. Paradigms to estimate the duration of consolidation typically vary the time between initial sample presentation and subsequent termination; these are followed by a memory probe. In one approach, the duration of consolidation is inferred from WM report performance under the assumption that the second event terminates consolidation, so diminished memory performance indicates incomplete consolidation. Alternatively, consolidation duration may be inferred from the time period over which the WM sample interferes with processing of the second event. Each alternative has advantages and disadvantages, with a key advantage for the former approach (termination of consolidation via retroactive interference) being that it yields a more direct measure of WM

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consolidation. The latter approach (proactive interference) instead assumes that the observed proactive interference in processing the second item must stem from consolidation, but it is possible that it could also stem from additional processes (e.g., maintenance).

The most rapid estimates of consolidation come from visual masking studies, which rely on retroactive interference (the assumption that consolidation is terminated by the onset of a mask). This assumption stems from the ideas that consolidation can only continue for as long as the sensory representation (e.g., iconic memory) of an item is available, and that masking diminishes sensory memory (Bays et al., 2011; Breitmeyer & Ogmen, 2000; Bundesen, 1990; Shibuya & Bundesen, 1988; Vogel et al., 2006; Zhang & Luck, 2008, see also Fuller et al., 2005).

Intermediate estimates of consolidation come from the attentional blink (AB), an impairment in reporting the second of two proximate rapid serial visual presentation (RSVP) targets. The AB suggests slow consolidation because, if the second target (T2) goes undetected, consolidation of the first target (T1) must still be ongoing (Lagroix et al., 2012; Shih, 2008; Taatgen et al., 2009; Wyble et al., 2009). The reduction in T2 accuracy for hundreds of milliseconds suggests a much longer duration compared to masking paradigms (Bowman & Wyble, 2007; Chun & Potter, 1995; Jolicoeur & Dell'Acqua, 1998) and that consolidation must continue after a visual mask, since each stimulus is masked by following RSVP items.

Other proactive interference approaches avoid RSVP, instead presenting discrete targets at varying time offsets using the PRP approach (Pashler, 1994; Telford, 1931; Welford, 1952). The PRP effect is observed when the response time for the second of two sequential tasks is slowed with decreasing stimulus-onset asynchronies (SOAs) between the two tasks. Conversely, with longer SOAs, T2 responses return to their typical timing. The AB and PRP stem from overlapping cognitive processes (Wong, 2002) and brain mechanisms (Marti et al., 2012). Both phenomena result from the occupation of central processing (e.g., response selection) by ongoing processing of T1, delaying or diminishing processing of T2. One prominent explanation posits an immutable central bottleneck that prevents processing of T2 until processing of T1 has concluded (Arnell & Duncan, 2002; Jolicoeur, Dell'Acqua, & Crebolder, 2001; Marois & Ivanoff, 2005; Pashler, 1994; Ruthruff & Pashler, 2001). Thus, by varying the T1-T2 SOA, the time required to process T1 can be inferred by the minimum required SOA for T2 response time to recover (Marois & Ivanoff, 2005; Pashler, 1994). While PRP tasks typically entail sensorimotor decision T1s and T2s, similar results have been obtained with WM encoding T1s, suggesting that WM consolidation is a central operation that is either costly (Koch et al., 2018; Tombu & Jolicoeur, 2003) or impossible to conduct in parallel with other processes requiring the same central mechanisms (e.g., Jolicoeur & Dell'Acqua, 1998; Tombu et al., 2011).

The slowest estimates of consolidation stem from dual-task retroactive interference. Nieuwenstein and Wyble (2014) sought to resolve the discrepancy between rapid (masking) and slow (AB, PRP) consolidation estimates by factorially crossing dual-task interference and masking manipulations. They examined the interval during which WM consolidation can be disrupted by speeded two-alternative forced-choice (2AFC; number parity judgment) between sample and probe, when the sample was or was not immediately followed by a visual mask. In other words, they measured retroactive interference of a choice T2 on T1 WM consolidation as a function of T1-T2 SOA and T1 masking. They found that a speeded 2AFC during the WM delay diminished WM performance, but that this effect abated with increasing WM array-2AFC SOAs, regardless of the presence or absence of a visual mask, suggesting that WM consolidation continued up to the maximum tested SOA (1 s). To unpack this logic further, Nieuwenstein and Wyble's observation can be summarized as an interaction of the SOA and second task presence factors, with WM performance improving with increasing WM sample-T2 SOAs. This pattern of results indicates that, at short SOAs, there is not adequate time to complete consolidation before it is interrupted by the second task, reducing performance. However, with increasing SOAs, consolidation has enough time to be complete before the decision T2, leading to improved WM performance. When there is no T2, performance is high regardless of SOA (in that case, to an additional blank screen rather than a T2). This pattern indicates interference with consolidation rather than interference with maintenance because the latter should lead to a main effect of T2 presence – once information is lost from maintenance, it is gone, regardless of whether it is lost early or late.

Nieuwenstein and Wyble (2014) revealed slow consolidation despite assessing consolidation directly (by WM performance) rather than indirectly (by proactive interference from WM consolidation onto T2). This helps to rule out a potential explanation for the widely varying conclusions about consolidation speed in prior studies – namely, that retroactive interference would reveal fast consolidation and proactive interference would reveal slow consolidation that might be contaminated by maintenance or other processes.

Present study

The present study investigates whether consolidation speed is under flexible control, or is instead a structural constraint. The "structural versus strategic" debate is not new (e.g., Meyer & Kieras, 1997a; Pashler, 1994; Salvucci & Taatgen, 2008; Schumacher et al., 2001; Strobach & Schubert, 2017;

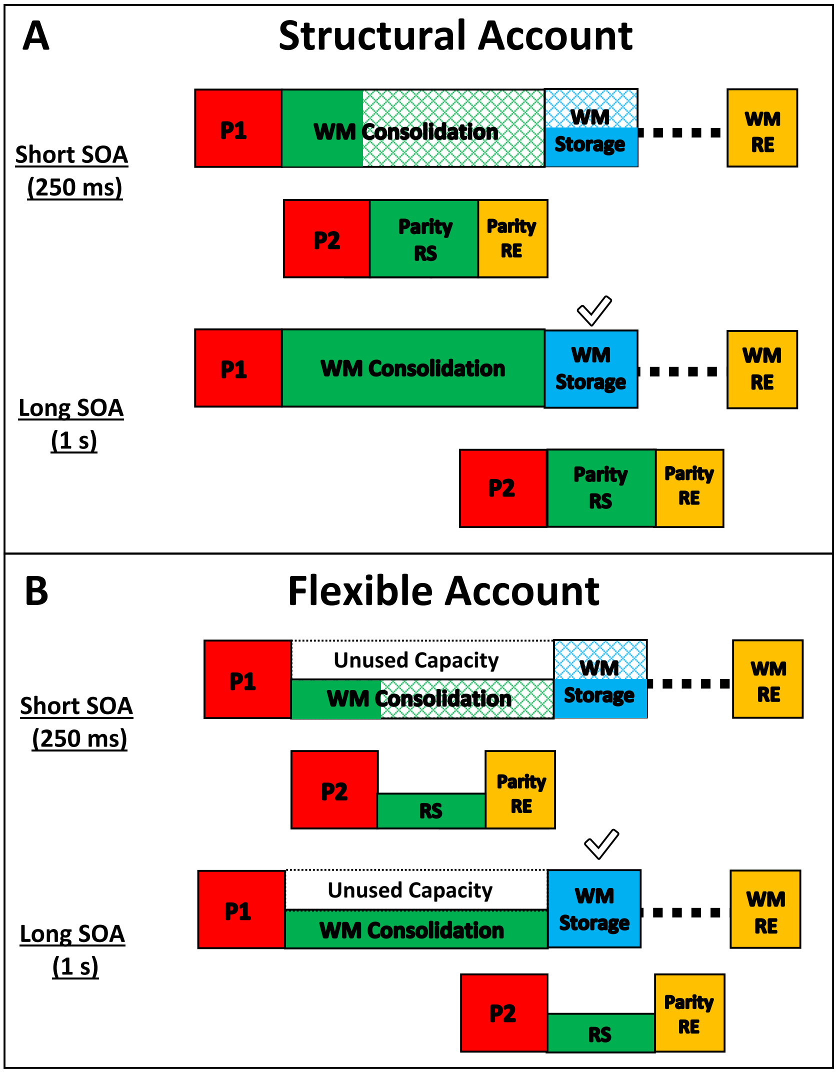

Fig. 1 A depiction of the structural (**A**) vs. flexible (**B**) accounts of consolidation in the context of the Nieuwenstein and Wyble (2014) task. The red boxes represent visual perception of either stimulus 1 (P1), or stimulus 2 (P2). The green box represents central resource-demanding processing: either working memory (WM) consolidation of P1, or T2 parity response selection (RS) for P2. The length of the green rectangle indicates the duration of WM consolidation, or duration of RS of P2, whereas the height represents available capacity. The blue box represents the amount of information that was successfully stored into WM. The yellow box represents response execution (RE) for the WM or parity tasks. The critical difference between the two accounts regards the amount of capacity allocated to the central processing stages of the two tasks. The structural account (top) suggests that consolidation is slow because it is a product of the structure of the system. The alternative flexible account (bottom) suggests that because resources are budgeted for both tasks, WM consolidation is prolonged (compared to if all resources were dedicated to WM consolidation). Note that this latter alternative is not depicted in the figure

Strobach et al., 2014; Tombu & Jolicoeur, 2003), but studies have largely used two sequential sensorimotor decision tasks. The current study recapitulates arguments from the dual-task literature,[1] but combines a WM encoding T1 with a sensorimotor decision T2 (Jolicoeur & Dell'Acqua, 1998, 1999; Nieuwenstein & Wyble, 2014; Stevanovski & Jolicoeur, 2007; Tombu et al., 2011). Nieuwenstein and Wyble's (2014) results may be interpreted as supporting the structural account – i.e., that slow WM consolidation is obligatory and not under strategic control (see Fig. 1A) – because diminished WM performance at short SOAs could indicate that long SOAs allowed full consolidation before T2, while short SOAs led to incomplete consolidation before disruption by T2. However, flexible accounts proposed for non-WM PRP

[1] Complex span studies have examined decision/WM dual-tasking, but were not formulated for inference about structural versus strategic resource allocation. See, for example, Rhodes et al. (2019), Doherty et al. (2019), and Duff and Logie (2001).

tasks (see Fig. 1B; Meyer & Kieras, 1997a; Meyer & Kieras, 1997b; Schumacher et al., 2001; Tombu & Jolicoeur, 2003) and in WM consolidation (reviewed in Ricker et al., 2018; also see Nieuwenstein et al., 2014; Woyteszek, 2020) could suggest that WM consolidation speed is instead controlled by task-specific factors or is even under volitional strategic control. If a task led participants to budget some processing capacity for an expected second task, that capacity would no longer be available to WM consolidation. Thus, a flexible account could be compatible with the range of reported consolidation speed estimates because WM consolidation may proceed especially slowly when a dual task is anticipated, making it vulnerable to interruption for an extended time. The primary goal of the present study is to adjudicate between structural and flexible accounts of WM consolidation. The basic approach taken in the present research is to manipulate the relative priority of the memory (T1) and decision (T2) components of the overall task; evidence for a change in consolidation duration due to a change in priority would be taken as evidence for flexible control over consolidation.[2]

Experiment 1

Some study designs used to evaluate consolidation (e.g., Nieuwenstein & Wyble, 2014) could have inadvertently led to implicit prioritization of T2 over T1 (WM). Specifically, the immediate, speeded T2 response required by such tasks could lead to higher priority compared to the unspeeded response to T1. Given limited resources (Kahneman, 1973; Koch et al., 2018; Navon & Gopher, 1979), increasing T2 priority could decrease available resources for T1 (WM consolidation) (cf., Schumacher et al., 2001) – leading to slower or queued performance. Thus, Experiment 1 investigated whether slow WM consolidation might be driven by such implicit prioritization. Specifically, Experiment 1 modified the task of Nieuwenstein and Wyble (2014) by making T2 unspeeded and deferring its response until after the WM probe. If slow consolidation stems from strategic resource allocation, then this manipulation would be expected to lead to relatively fast WM consolidation, and thus, little retroactive interference. Alternatively, if slow WM consolidation stems from a structural constraint, robust retroactive interference would be expected.

Method

Participants

Data from 16 undergraduate students (all female; 18–41 years old, M_{age} = 21.4 years, SD = 5.54) were collected. All participants were recruited using the University of Houston SONA system. Participants were at least 18 years of age, had normal or corrected-to-normal vision, and reported no history of neurological problems or major perceptual deficits. All experiments reported in this study were approved by the University of Houston Institutional Review Board. Participants provided written informed consent at the beginning of their visit, and were compensated via course credit.

Materials

Experiment 1 and all subsequent in-lab experiments were designed and run in MATLAB using the Psychophysics Toolbox extensions (Brainard, 1997; Kleiner et al., 2007). Stimuli were displayed on a CRT monitor set to 1,600 × 1,200 pixels, with a 70-Hz refresh rate, driven by a Linux-based Dell computer.

Experiments 1-3 required participants to remember an array of four simultaneously presented letters randomly selected without replacement from the English alphabet, excluding M, W, and all vowels (thus avoiding the appearance of words in the sample arrays; Nieuwenstein & Wyble, 2014). Pound symbols ("#") were used to mask the letters on masked trials. All stimuli were presented in the center of the screen on a gray background. A 20-pt Arial font was used for letters and digits. A 24-pt boldfaced Arial font was used for the masks. Participants indicated a WM change detection response on every trial upon viewing a probe. On no-change trials (50%), the probe and sample arrays were identical. On change trials, a single letter was replaced with another letter not otherwise contained in the sample or probe for that trial. The position of the changed letter was distributed uniformly among the positions.

Design and procedure

The present study used a variation of the dual-task consolidation interruption paradigm used by Nieuwenstein and Wyble (2014). A within-subject design was used for Experiment 1, including manipulations of SOA (250, 500, or 1,000 ms), mask presence, and presence of a second task (single task vs. dual task), leading to a 3 × 2 × 2 factorial design (Fig. 2). There were 20 trials per condition, resulting in a total of 240 randomly intermixed trials.

[2] It is possible that prioritization could affect masked and unmasked conditions differentially. Thus, to be sensitive to this possibility (and to replicate the findings of Nieuwenstein and Wyble, 2014, as completely as possible), we include both masked and unmasked conditions in all experiments. We did not predict any such effect (i.e., any modulation of the key interactions for this study – SOA × presence of a dual task, or SOA × presence of a dual task × priority) and, to anticipate the results, no such modulation was found in any experiment. Thus, we do not focus on the masking manipulation in the remainder of this paper.

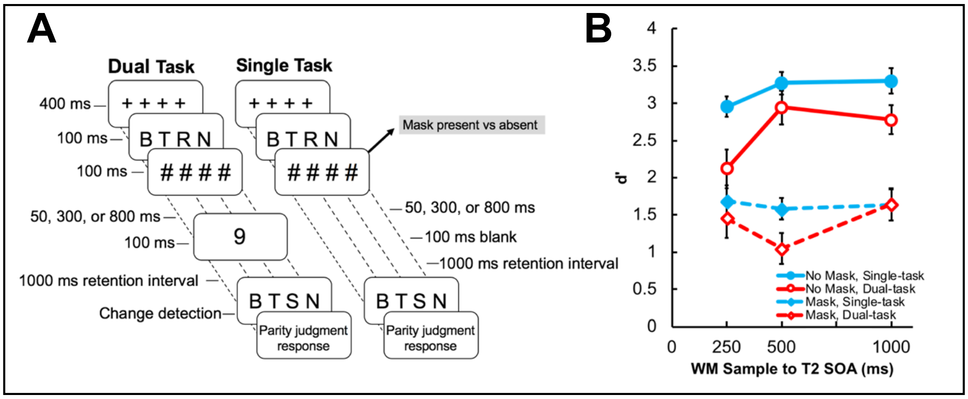

Fig. 2 (**A**) Dual- and single-task trials for Experiment 1. (**B**) Working memory (WM) performance. Error bars show the standard error of the mean

Before each trial (Fig. 2), four placeholder crosses were displayed at the center of the screen. Participants then initiated each trial by pressing the spacebar. Once the spacebar was pressed the placeholder crosses remained on the screen for 400 ms and then were replaced by the WM sample array. The sample array was removed after 100 ms and replaced with an array of four "#" symbols (masked trials; 50%) or a blank screen (unmasked trials) for 100 ms. A 50-, 300-, or 800-ms blank screen was then displayed. For single-task trials (50%), this was followed by an additional 1,100-ms blank screen before the onset of the WM probe. For dual-task trials, a random digit (1–9) was displayed at the center of the screen for 100 ms and followed by a 1,000-ms blank screen. Participants were to identify whether the digit was odd or even, but could not respond until after the WM change detection (T1) response. On all trials, the WM probe was presented after the retention interval, and remained displayed at the center of the screen until the change detection response (left hand, "Z" or "X," for "same" or "different," respectively). For dual-task trials, participants then entered their parity response (right hand, comma or period, for "odd" or "even," respectively).

Prior to the start of the experiment, participants were given written instructions for the task, which were further explained verbally. Participants completed eight practice trials (1,000-ms SOA, two each for all combinations of masking and single vs. dual task) under the supervision of the experimenter, who vacated the room following practice.

General analysis pipeline

Recent research has highlighted that using accuracy as a measure of memory performance in change detection tasks conflates memory strength with response strategy/decision bias (Williams et al., 2022). For the current study, decision bias (which may be understood as a tendency in the absence of any information to prefer to report a change over a no-change, or vice versa) is, essentially, noise. Bias could either make it more difficult to observe differences in memory strength between conditions that are actually different, or make differences appear between conditions that really have similar memory strength. To account for this problem, we computed d' to separate decision criterion from memory performance. An infinite d' could be observed for a given condition if a participant had a hit and/or false alarm rate of 0% or 100%. Thus, to account for this, a convention was adopted to artificially increase or decrease the rates. For hit and false alarm rates that were at 0 we adjusted the rate to equal .5/(number of trials in condition +1). For hit and false alarm rates that were at 100% we used 1 - .5/(number of trials in condition + 1). This approach is a variant of the log-linear approach to correction, which has been shown to be less biased than the more common approach of adjusting only the numerators (Hautus, 1995; Stanislaw & Todorov, 1999). In addition to computing d', we conducted identical analyses for accuracy (i.e., change detection proportion correct) to supplement d' results; full accuracy results for all experiments are presented in the online supplementary material (OSM).

Bayesian repeated-measures ANOVAs were conducted to evaluate WM (T1) performance using JASP (JASP Team, 2018). Bayes Factors (BFs) were used to quantify the ratio of evidence for or against the inclusion of each factor in the model (Rouder et al., 2017; Vandekerckhove et al., 2018; Wagenmakers et al., 2018a, b). A BF < 1 represents evidence against inclusion of a factor or interaction, whereas a BF > 1 represents evidence in favor of inclusion of a factor or interaction. For example, a BF of 0.1 is interpreted as a ratio of 10:1 against the inclusion, whereas a BF of 10 is interpreted as a 10:1 ratio for the inclusion of the factor (see van Doorn et al., 2019). BFs above approximately 3 or below approximately 1/3 are generally considered sufficiently distinct from 1 to be clearly interpretable, whereas BFs below approximately 3 but above approximately 1/3 represent more ambiguous levels of evidence for or against an effect, respectively. Retroactive interference with WM consolidation is detected by evidence in favor of including the interaction of SOA with the presence of a second task.

Sample size justification

Sixteen participants were collected for each group within each experiment (1–3) to match the number of participants used by Nieuwenstein and Wyble (2014). The concept of statistical power is not meaningful for the Bayesian analyses (Rouder, 2014; Wagenmakers, et al., 2018a), but it is reasonable to assess the adequacy of the sample based on whether most or all key analyses yield interpretable BFs (BF < 1/3 or BF > 3). The key analyses to which we apply this standard are the interactions of SOA with T2 presence (all experiments) and of SOA, T2 presence, and instructed priority (Experiments 2–4) for the memory performance measure. All key analyses in all experiments achieved this threshold, with the exception of some analyses in Experiment 2 which did not detract from the ability to draw conclusions from that experiment. We report additional analyses regardless of whether they achieved an interpretable BF for the sake of completeness. For each experiment, d's (see Figs 2, 3, 4 and 5) and accuracies (see OSM Table S1) are reported for each condition, and BFs and effect sizes for the full set of ANOVA main effects and interactions (see Tables 2, 3, 4, and 6). The use of BFs throughout the study is beneficial because this approach does not give advantage to rejection of the null, and is instead able to determine if there is strong evidence for an effect, against an effect, or if there is not enough evidence to confidently reach a decision based on the data (Rouder et al., 2017; Vandekerckhove et al., 2018; Wagenmakers et al., 2018a).

Results

Response times are not reported because neither task was speeded.

Memory task (T1) Change detection results are presented in Fig. 2. A Bayesian repeated-measures ANOVA (Table 2) revealed moderate evidence against an interaction of SOA with T2 presence, $BF_{Incl} = 0.172$, $\eta_p^2 = 0.07$. In addition to the d' results analyzed here, accuracy results for this and all subsequent experiments are reported in the OSM.

Parity judgment task (T2) Overall T2 accuracy was 90% (see Table 1 for T2 performance across experiments and conditions). Most importantly, a Bayesian repeated-measures ANOVA on T2 accuracy revealed moderate evidence against the inclusion of an SOA effect ($BF_{Incl} = 0.221$, η_p^2s = 0.074). Finally, there was anecdotal evidence for the inclusion of all other effects ($BF_{Incl} > 1$, η_p^2s > .177).

Table 1 T2 mean accuracy (& standard deviation) across experiments and conditions

	250 ms	500 ms	1000 ms
Experiment 1 (Deferred T2)			
Unmasked	0.916 (0.068)	0.919 (0.077)	0.897 (0.096)
Masked	0.853 (0.134)	0.878 (0.098)	0.922 (0.066)
Experiment 2 (Deferred T2 + Instructed priority)			
Unmasked (WM Prioritized)	0.888 (0.090)	0.884 (0.123)	0.941 (0.058)
Masked (WM Prioritized)	0.831 (0.115)	0.822 (0.126)	0.944 (0.051)
Unmasked (T2 Prioritized)	0.909 (0.117)	0.922 (0.118)	0.925 (0.105)
Masked (T2 Prioritized)	0.925 (0.111)	0.919 (0.101)	0.934 (0.111)
Experiment 3 (Instructed priority)			
Unmasked (WM Prioritized)	0.972 (0.048)	0.969 (0.068)	0.969 (0.054)
Masked (WM Prioritized)	0.931 (0.073)	0.953 (0.059)	0.966 (0.054)
Unmasked (T2 Prioritized)	0.984 (0.030)	0.975 (0.032)	0.991 (0.020)
Masked (T2 Prioritized)	0.978 (0.052)	0.978 (0.055)	0.969 (0.036)
Experiment 4 (Instructed priority)			
Unmasked (WM Prioritized)	0.957 (0.066)	0.959 (0.048)	0.968 (0.049)
Masked (WM Prioritized)	0.959 (0.055)	0.970 (0.035)	0.968 (0.046)
Unmasked (T2 Prioritized)	0.935 (0.077)	0.946 (0.072)	0.957 (0.073)
Masked (T2 Prioritized)	0.950 (0.059)	0.950 (0.072)	0.958 (0.049)

The mean values are reported for each condition with standard deviation reported in parenthesis

Discussion

Experiment 1 evaluated whether decreasing implicit T2 priority by making T2 unspeeded would reveal flexibility in the speed of consolidation. At first blush, this manipulation appeared to have abolished the interaction between SOA and the presence of a second task, consistent with the hypothesis. However, the interpretation of Experiment 1 hinges on the assumption that priority stems from response order or the requirement to make a speeded response. Thus,

Table 2 Experiment 1 Bayesian repeated measures ANOVA with WM change detection performance

Effect	d'		Log bias		Accuracy	
	η_ρ^2	BF_{Incl}	η_ρ^2	BF_{Incl}	η_ρ^2	BF_{Incl}
SOA	.160	.772	.090	.219	.102	.309
Presence of a second task	.582	923.212	.010	.164	.455	58.698
Mask	.925	3.941×10^{28}	.251	5.856	.887	1.095×10^{25}
SOA × Presence of a second task	.068	.172	.121	.429	.146	.332
SOA × Mask	.302	22.572	8.362×10^{-4}	.099	.224	2.213
Presence of a second task × Mask	.155	.724	.415	3.517	.074	.368
SOA × Presence of a second task × Mask	.126	.687	.021	.221	.159	.893

subsequent experiments examined stronger manipulations of task priority.

Experiment 2

Experiment 2 was similar to Experiment 1, but explicitly manipulated task priority between participants via instructions, trial-by-trial feedback on performance, and the use of added delays for errors, while avoiding assumptions about implicit prioritization of T2 by report order/speeding. Past research has used non-monetary reward incentives to successfully manipulate task preparation (Erkal et al., 2018; Greenhouse & Wessel, 2013; Ye et al., 2017), similar to the present approach.

The hypothesis was that Experiment 2's better-controlled manipulation of task priority would reduce retroactive interference of a parity judgment on WM consolidation when WM was prioritized more than when parity was prioritized.

Method

Participants

Sixteen undergraduate students (Experiment 2a: N = 16, four males; 18–26 years old, M_{age} = 20.6 years, SD = 1.82; Experiment 2b: N = 16, five males; 18–25 years old, M_{age} = 21.1, SD = 2.78) were recruited from the University of Houston for each priority condition (Experiment 2a: WM prioritized; Experiment 2b: parity prioritized), for a total of 32 participants. Written informed consent was obtained from all participants at the beginning of their visit. Participants were compensated via

Table 3 Experiment 2 Bayesian repeated measures ANOVA with WM change detection performance

Effect	d'		Log bias		Accuracy	
	η_ρ^2	BF_{Incl}	η_ρ^2	BF_{Incl}	η_ρ^2	BF_{Incl}
SOA	.020	.049	.005	.033	.009	.037
Presence of a second task	.097	1.486	.003	.118	.061	.533
Mask	.859	2.987×10^{41}	.333	3.397×10^{4}	.805	2.56×10^{39}
Instructed priority	.123	1.386	.084	.522	.136	1.676
SOA × Instructed priority	.005	.065	.017	.080	.001	.052
Presence of a second task × Instructed priority	.003	.172	.028	.234	.003	.174
Mask × Instructed priority	.018	.225	.018	.232	.007	.182
SOA × Presence of a second task	.071	.352	.011	.067	.064	.172
SOA × Mask	.062	.261	.041	.147	.075	.430
Presence of a second task × Mask	.040	.209	5.089×10^{-4}	.156	.007	.164
SOA × Presence of a second task × Instructed priority	.009	.088	.013	.134	.012	.151
SOA × Mask × Instructed priority	.026	.180	.029	.187	.003	.077
Presence of a second task × Mask × Instructed priority	.008	.281	.040	.354	.002	.231
SOA × Presence of a second task × Mask	.040	.302	.012	.131	.061	.489
SOA × Presence of a second task × Mask × Instructed priority	.024	.294	.252	211.484	.031	.275

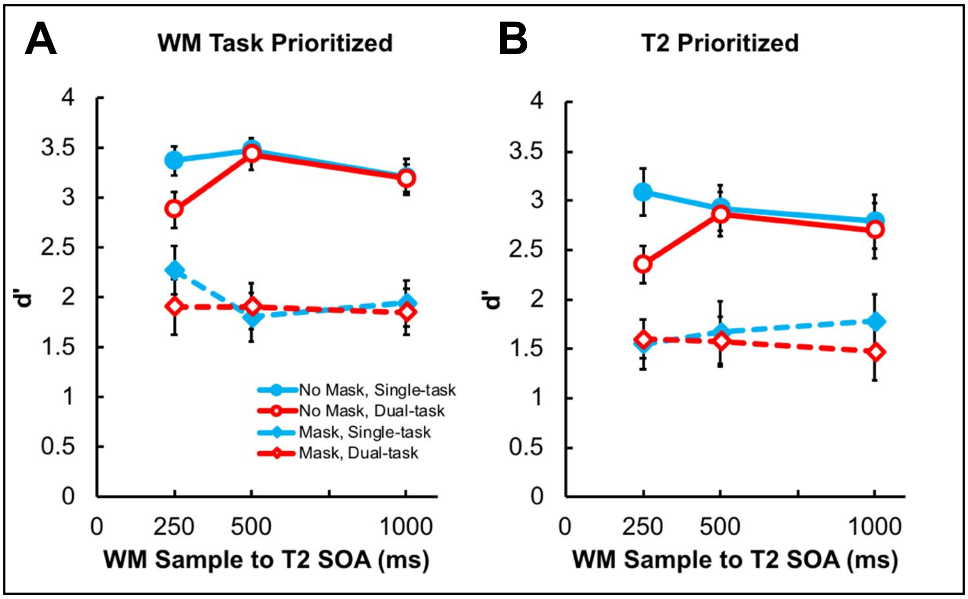

Fig. 3 (**A**) Performance on working memory (WM) task when WM was given higher priority than the parity judgement task. (**B**) Performance on WM task when parity judgement was given higher priority than WM task. Error bars show the standard error of the mean

course credit. Sufficiency of the sample size was assessed by examination of BFs for key effects as described in Experiment 1.

Materials, design, and procedure

In Experiment 2a, the WM task was given high priority, and the parity judgment task was given low priority. In Experiment 2b, the priority of each task was flipped. The designs of Experiments 2a and 2b were similar to that of Experiment 1; the only difference was to provide stronger priority manipulations via explicit instructions, performance-dependent feedback, and timing of the inter-trial interval. For each participant, one task was designated as high priority (either WM or parity; 50% of participants in each condition). For their designated high-priority task, participants earned or lost 500 (arbitrary) points for correct or error responses, respectively; trial-level feedback was provided by turning the screen green (correct responses) or red (incorrect responses) at the end of the trial. As noted above, similar non-monetary incentives can be used to manipulate task preparation (Erkal, Gangadharan, & Koh, 2018; Greenhouse & Wessel, 2013; Ye et al., 2017). In addition, high-priority task errors resulted in a 5-s delay before the next trial, providing additional motivation to prioritize the designated task. For the low-priority task, participants only gained or lost 10 points per trial, and performance did not affect the intertrial interval or screen color. Participants were given a running total of their points after each trial and a grand total at the end of the task.

Results and discussion

Full results of Experiment 2 are presented in Fig. 3 and Tables 1 and 3. When WM was prioritized (Experiment 2a), there was ambiguous evidence against an SOA × T2-presence interaction ($BF_{Incl} = 0.545$, $\eta_p^2 = 0.12$). This result initially seemed to suggest that retroactive interference with WM consolidation stems from flexible allocation of resources among dual tasks or anticipated dual tasks. However, surprisingly, we observed strong evidence against an SOA × T2 presence × Experiment (2a vs. 2b: instructed priority) interaction, $BF_{Incl} = 0.088$, $\eta_p^2 = 0.01$. This indicates a failure to find clear evidence of retroactive interference, regardless of instructed task priority. Moreover, there was minimal support for the idea that Experiment 2 successfully manipulated task priority (priority $BF_{Incl} = 1.386$, $\eta_p^2 = 0.123$). Thus, Experiment 2 must be interpreted with caution.

One possibility is that effects of the priority manipulation were swamped by implicit prioritization of the WM task (due to the unspeeded nature of T2, similar to Experiment 1). Alternatively, perhaps only a speeded response can disrupt consolidation because an unspeeded T2 could simply be encoded as an additional WM item and the actual parity decision – not just the report – could take place after reporting the initial WM sample. Because of these caveats,

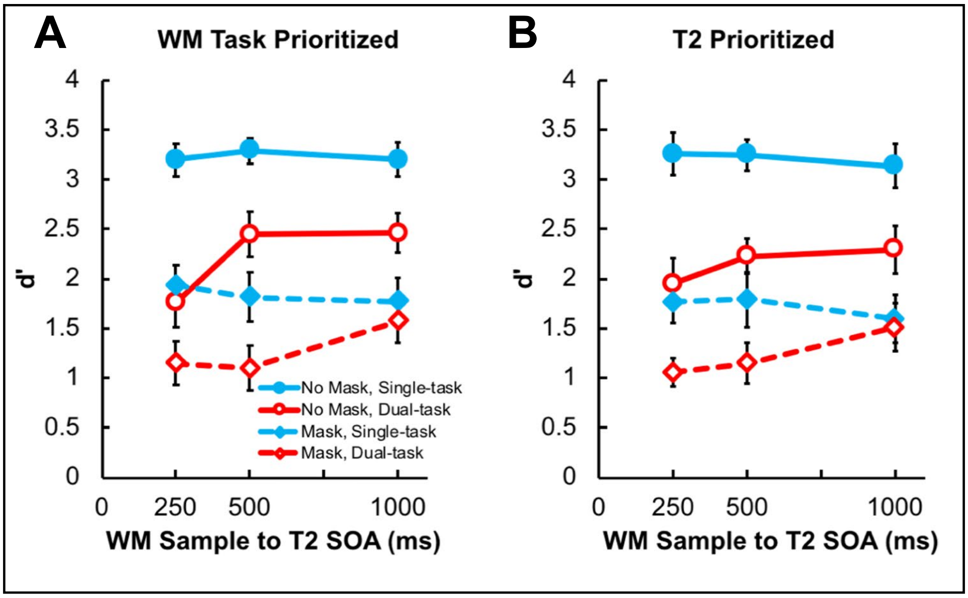

Fig. 4 (**A**) Working memory (WM) task performance when WM was given high priority. (**B**) WM task performance when parity judgement was given high priority. Error bars show the standard error of the mean

Experiment 3 re-tested the same hypothesis as Experiment 2 while still explicitly manipulating priority, but required speeded T2 responses in all conditions.

Experiment 3

The results of Experiment 2 failed to clarify those of Experiment 1 because of the surprising abolishment of the retroactive interference effect in both groups of participants – those who gave the WM task high priority and those who gave it low priority. Experiment 3 was thus designed to retest explicit prioritization, but with an immediate parity response. If consolidation speed is under flexible control, we should observe a SOA × T2 presence × priority interaction such that retroactive interference would be abolished in Experiment 3a (WM prioritized), but present in Experiment 3b (T2 prioritized).

Method

Participants

Thirty-two University of Houston undergraduate students participated in Experiment 3 (Experiment 3a: N = 16, four males; 19–27 years old, M_{age} = 21.6 years, SD = 2.75; Experiment 3b: N = 16, two males; 18–33 years old, M_{age} = 21.7 years, SD = 4.44). Adequacy of the sample was assessed as in Experiments 1 and 2. Participants provided informed consent before the start of the task, and were compensated via course credit.

Design and procedure

Experiments 3a and 3b were identical to Experiments 2a and 2b with one exception: instead of a deferred T2 response, participants provided an immediate, speeded T2 response for dual-task trials. On dual-task trials, the WM probe was presented immediately after the T2 response (Nieuwenstein & Wyble, 2014). On single-task trials, the WM probe was presented after the same retention interval as in Experiments 1 and 2 (100-ms blank screen in lieu of T2, plus a further 1,000 ms). The temporal order of responses matched Nieuwenstein and Wyble (2014), but also included the added manipulation of feedback that was used in Experiment 2.

Results and discussion

Full results are presented in Fig. 4 and Tables 1, 4, and 5. There was strong evidence for a SOA × T2-presence interaction (BF_{Incl} = 27.994, η_p^2 = 0.20), but evidence against a SOA × T2 presence × priority interaction, BF_{Incl} = .114, η_p^2 = 0.005. Thus, regardless of task priority, similar retroactive interference was observed, eliminating the account that retroactive interference with WM consolidation at long SOAs results from flexible prioritization of T2 (the 2AFC parity task) over WM encoding.

The absence of a SOA × T2 presence × priority interaction supports the structural account of slow consolidation:

Table 4 Experiment 3 Bayesian repeated measures ANOVA with WM change detection performance

Effect	d'		Log bias		Accuracy	
	η_p^2	BF_{Incl}	η_p^2	BF_{Incl}	η_p^2	BF_{Incl}
SOA	.104	.374	.116	1.005	.091	.247
Presence of a second task	.778	2.196×10^{23}	.303	720.855	.750	4.53×10^{22}
Mask	.886	1.071×10^{48}	.437	7.531×10^{4}	.874	1.13×10^{43}
Instructed priority	.003	.338	.177	1.553	.004	.332
SOA × Instructed priority	.010	.068	.034	.132	.000	.058
Presence of a second task × Instructed priority	6.428×10^{-4}	.154	.081	.915	.013	.204
Mask × Instructed priority	.002	.161	.009	.183	.001	.161
SOA × Presence of a second task	.197	27.994	.048	.230	.226	146.286
SOA × Mask	.060	.208	.033	.115	.131	1.072
Presence of a second task × Mask	.359	187.116	.019	.200	.097	.727
SOA × Presence of a second task × Instructed priority	.005	.114	.010	.124	.004	.102
SOA × Mask × Instructed priority	.032	.187	.032	.187	.011	.120
Presence of a second task × Mask × Instructed priority	.010	.219	1.113×10^{-4}	.229	.025	.296
SOA × Presence of a second task × Mask	.030	.217	.055	.337	.048	.342
SOA × Presence of a second task × Mask × Instructed priority	.005	.109	.177	3.213	.003	.231

retroactive interference was not affected by explicit task prioritization. If motivational factors do not affect the pace of WM consolidation, it further suggests that Nieuwenstein and Wyble's (2014) experimental paradigm cannot be thought of as prolonging an otherwise-rapid consolidation process (c.f., Gegenfurtner & Sperling, 1993; Vogel et al., 2006; Woodman & Vogel, 2005). Instead, consolidation is always slow, and, as argued by Nieuwenstein and Wyble (2014), prior measurement approaches using masking were insensitive to (some) unsuccessful consolidation. However, interpretation of Experiment 3 is hampered by the absence of an effect of priority on WM performance or T2 SOA (Table 5); it is possible that the results reflect failure to manipulate priority despite the instructions and incentives to prioritize one or the other task. Experiment 4 sought to clarify this issue.

Experiment 4

One reason a clear priority effect was not observed in Experiment 3 could be the high performance; ceiling effects in some conditions may have undermined sensitivity to the priority manipulation. Thus, Experiment 4 increased the set size to 6 items, increasing WM demands and forcing participants to either allocate all resources to WM, or to budget some for T2. In addition, the sample size for Experiment 4 was doubled to increase sensitivity; we again note that this increased sample size does not bias a Bayesian hypothesis test towards revealing an interaction as it could a frequentist hypothesis test. We predicted that we would observe effects of the priority manipulation in Experiment 4. Such effects would enable clear interpretation of the presence or absence of a SOA × T2 presence ×

Table 5 Experiment 3 Bayesian repeated measures ANOVA with T2 RT and accuracy data

Effect	RT				Accuracy			
	F(df, df)	p	η_p^2	BF_{Incl}	F(df, df)	p	η_p^2	BF_{Incl}
SOA	13.290 (2,60)	<.001	.307	11051.606	.363 (2,60)	.697	.012	.087
Mask	1.881 (1,30)	.180	.059	.554	5.282 (1,30)	.029	.150	1.576
Instructed priority	.098 (1,30)	.757	.003	.575	2.153 (1,30)	.153	.067	.697
SOA × Instructed priority	.693 (2,60)	.481	.023	.162	.526 (2,60)	.594	.017	.171
Mask × Instructed priority	1.755 (1,30)	.195	.055	.695	.685 (1,30)	.415	.022	.281
SOA × Mask	.281 (2,60)	.697	.009	.110	1.120 (2,60)	.333	.036	.151
SOA × Mask × Instructed priority	.213 (2,60)	.749	.007	.134	3.360 (2,60)	.041	.101	.695

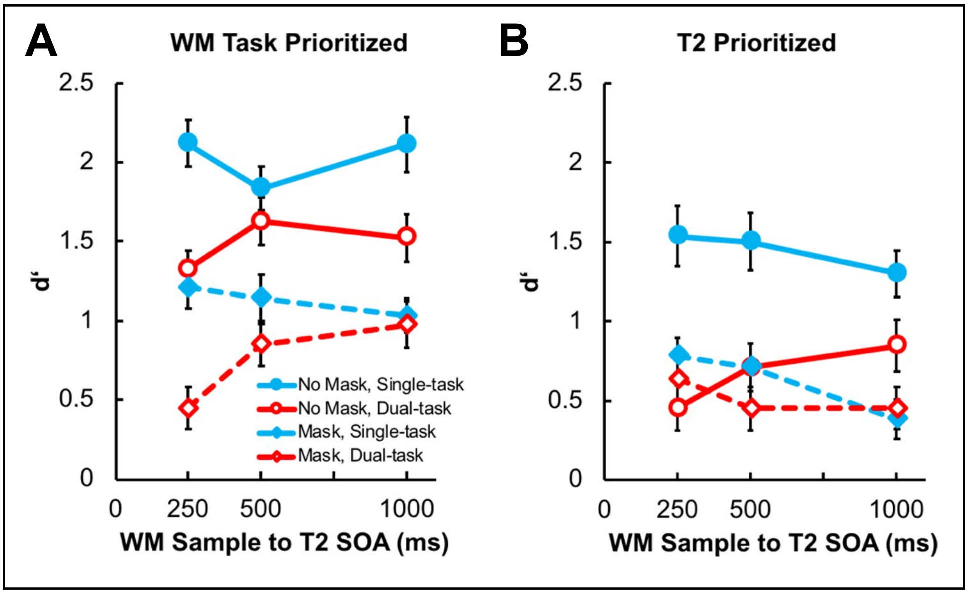

Fig. 5 (**A**) Memory strength for working memory (WM) task when WM was given higher priority than the parity judgement task. (**B**) Memory strength for WM task when parity judgement was given high priority. Error bars show the standard error of the mean

priority interaction as support for a flexible or structural account of WM consolidation, respectively.

Methods

Participants

Data were collected from two separate groups each consisting of 32 undergraduate students (Experiment 4a: seven males; 18–34 years old, M_{age} = 22 years, SD = 3.8; Experiment 4b: six males; 18–41 years old, M_{age} = 21.3 years, SD = 4.3). Participants were compensated course credit for their participation.

Materials, design, and procedure

Due to the COVID-19 pandemic, Experiment 4 was implemented online. The PsychoPy experiment builder (Peirce et al., 2019) was used to create JavaScript and HTML code to be hosted on Pavlovia.org. The University of Houston's Sona System was used to recruit participants. Similarly to past online experiments in our laboratory (Santacroce et al., 2021, 2023), participants used a link on the Sona System that directed them to Qualtrics.com. Participants then provided consent, reported demographic information, and read the task instructions. After the Qualtrics survey was completed, participants were directed to the online experiment hosted on Pavlovia.

To limit the impact of the reduced experimental control associated with online experiments, participants were instructed to isolate themselves from distractors, and to perform the task sitting up straight at a table/desk. Participants were requested to use either the Google Chrome or Microsoft Edge internet browser on a desktop computer or laptop (no tablets or phones). Finally, participants were instructed to minimize other potential distractors by powering off devices such as the TV, cell phone, and music player.

Experiment 4 closely replicated the task used in Experiment 3 with the simple modification of an increased memory set size. The WM sample was increased from four to six visually presented letters. All other aspects of the task were identical to Experiment 3. Experiment 4a matched Experiment 3a, where the WM task was given higher priority than the parity judgment task. Experiment 4b matched Experiment 3b, where higher priority was given to the parity judgment task than the WM task.

To replicate the size of stimuli used in the in-lab experiments, and to keep consistency across the various types of participants' monitors, a credit card screen scale (Morys-Carter, 2020) calibration approach was used prior to the start of the task. Here, participants were instructed to hold a credit card to the screen, and to adjust the size of the onscreen credit card image to precisely match that of their own credit card. The output obtained from the calibration was then used to scale the online task stimuli to match that of the in-lab stimuli dimensions.

Results and discussion

Full results are presented in Fig. 5 and Tables 1, 6, and 7. We observed strong effects of priority (BF_{Incl} = 526.78, η_p^2 = 0.24) and retroactive interference (SOA × T2-presence

Table 6 Experiment 4 Bayesian repeated measures ANOVA with WM change detection performance

Effect	d'		Log bias		Accuracy	
	η_p^2	BF_{Incl}	η_p^2	BF_{Incl}	η_p^2	BF_{Incl}
SOA	.004	.019	.016	.029	.078	.681
Presence of a second task	.591	7.378×10^{14}	.005	.094	.592	7.869×10^{15}
Mask	.675	3.944×10^{30}	.597	3.558×10^{19}	.659	1.814×10^{23}
Instructed priority	.244	526.780	.003	.136	.280	2346.90
SOA × Instructed priority	.036	.116	.004	.029	.056	.407
Presence of a second task × Instructed priority	4.937×10^{-5}	.103	.023	.336	1.851×10^{-4}	.111
Mask × Instructed priority	.115	18.056	.043	.612	.100	3.455
SOA × Presence of a second task	.093	23.112	.003	.034	.096	30.666
SOA × Mask	.013	.042	.016	.064	.012	.052
Presence of a second task × Mask	.285	405.771	.021	.254	.106	4.193
SOA × Presence of a second task × Instructed priority	.029	.321	.008	.074	.043	.895
SOA × Mask × Instructed priority	.033	.307	.078	4.549	.007	.078
Presence of a second task × Mask × Instructed priority	.129	4.313	.018	.295	.102	4.940
SOA × Presence of a second task × Mask	.015	.126	.016	.126	.030	.348
SOA × Presence of a second task × Mask × Instructed priority	.029	.457	.007	.159	.012	.174

interaction, $BF_{Incl} = 23.11$, $\eta_p^2 = 0.09$) with moderate evidence against a SOA × T2 presence × priority interaction, $BF = 0.321$, $\eta_p^2 = .029$. Thus, WM prioritization did not speed consolidation even when there was a clear main effect of priority,[3] supporting the structural account.

General discussion

By manipulating the relative task priority of a WM T1 and a sensorimotor decision T2 in a dual-task paradigm, the current experiments demonstrate that the long duration of WM consolidation observed by retroactive interference cannot be circumvented by prioritization of WM over T2. This suggests that the speed of consolidation is an obligatory structural limit. Equivalent retroactive interference effects lasting up to the maximum tested SOA – 1 s – were observed regardless of prioritization, even when prioritization was clearly effective at manipulating overall performance (Experiment 4). This finding refuted the account that prioritization of T2 over WM resulted in a reduction of available resources for WM, and thus the prolongation of WM consolidation, in the dual-task consolidation interruption paradigm. It was important to rule out this potential explanation of WM consolidation because the influential results of Nieuwenstein and Wyble (2014) were consistent with either a structural or flexible account of WM consolidation duration, with the latter possibility accommodating discrepant results compared to masking studies without the need for a less-parsimonious two-stage account of consolidation such as those suggested by Nieuwenstein and Wyble (2014) or by Ye et al. (2017, 2020). A two-stage consolidation model such as that of Nieuwenstein and Wyble (2014) can reconcile discrepant estimates of consolidation duration. For instance, visual masking studies assume they are measuring the consolidation process as a whole, but in reality, may only be measuring an initial stage of consolidation, whereas other measurement approaches may be manipulating a second stage of consolidation. In other words, the difference between consolidation speed estimates, as measured by masking and dual-task paradigms, is not because consolidation speed changes, but instead because the techniques are measuring different stages of consolidation, rather than an assumed unitary process. Nieuwenstein and Wyble (2014) describe this first stage as the selection-for-consolidation process. This stage is vulnerable to visual masks. However, once an item is selected, stage 2 of consolidation begins and is only vulnerable to centrally demanding tasks, not masking. Ye et al. (2017) proposed a similar two-phase WM

[3] The only difference between Experiments 3 and 4, other than WM set size, is that Experiment 4 was performed online rather than in person. Thus, it could be that the online nature of Experiment 4, rather than the increased set size, drove the differences between Experiments 3 and 4. To rule out this alternative explanation, we replicated Experiment 3 online with an identical sample size. In doing so, we again observed evidence in favor of a SOA × T2-presence interaction (d': $BF_{Incl} = 2.340$, $\eta_p^2 = 0.150$; Accuracy: $BF_{Incl} = 9.994$, $\eta_p^2 = 0.236$) and evidence against the inclusion of a SOA × T2 presence × priority interaction (d': $BF_{Incl} = 0.109$, $\eta_p^2 = 0.012$; Accuracy: $BF_{Incl} = .122$, $\eta_p^2 = 0.015$). Furthermore, there was not an effect of priority (d': $BF_{Incl} = 0.576$, $\eta_p^2 = 0.06$; Accuracy: $BF_{Incl} = .555$, $\eta_p^2 = 0.050$). Thus, the effects observed in Experiment 4 can be attributed to the increase in WM set size.

Table 7 Experiment 4 Bayesian repeated measures ANOVA with T2 RT and accuracy data

	RT				Accuracy			
Effect	F(df, df)	p	η_ρ^2	BF_{Incl}	F(df, df)	p	η_ρ^2	BF_{Incl}
SOA	7.543 (2,124)	<.001	.108	205.41	2.998 (2,124)	.054	.046	.260
Mask	13.336 (1,62)	<.001	.177	18.234	1.100 (1,62)	.298	.017	.218
Instructed priority	.141 (1,62)	.708	.002	.458	1.722 (1,62)	.194	.027	.511
SOA × Instructed priority	.327 (2,124)	.722	.005	.075	.167 (2,124)	.846	.003	.060
Mask × Instructed priority	.491 (1,62)	.486	.008	.196	.080 (1,62)	.778	.001	.151
SOA × Mask	.945(2,124)	.391	.015	.106	.285 (2,124)	.752	.005	.069
SOA × Mask × Instructed priority	.401 (2,124)	.671	.006	.108	.360 (2,124)	.699	.006	.129

resource allocation model consisting of an involuntary and voluntary phase. The first (involuntary) phase is described as a stimulus-driven phase that creates a low-resolution representation. The second (voluntary) phase then reallocates resources to create a high-precision representation of the stimulus. With the present results in hand, it seems that two-stage WM consolidation models must be invoked to explain why various measurement approaches come to different conclusions regarding the speed of consolidation. Consistent with the two-stage account, we never observed evidence in any of the present experiments for interaction of the masking manipulation with the key SOA × presence of a second task or SOA × presence of a second task × priority interactions.

A challenge for the present research regards the difficulty of manipulating the prioritization of memory versus other tasks: while much research demonstrates prioritization within memory (Astle et al., 2012; Garavan, 1998; Griffin & Nobre, 2003; Myers et al., 2017; Klyszejko et al., 2014; Myers et al., 2018; Pertzov et al., 2013; Tamber-Rosenau et al., 2011), it is more difficult to come by evidence that memory can be prioritized relative to non-mnemonic processing (Bays et al., 2022; Lin et al., 2021). We thus think that a useful second product of the present study is to document what conditions lead to prioritization of WM in a dual-task paradigm. The main shortcoming of Experiments 1–3 concerns the ineffectiveness of the priority manipulation. The lack of an effect makes it difficult to confidently dismiss the idea that WM consolidation speed can be flexibly manipulated on the basis of Experiments 1–3. Nonetheless, the results shed light on what task manipulations are sufficient to influence priority in WM dual-task paradigms. Because the main difference between Experiments 3 and 4 is the WM sample set size, it seems that the present explicit manipulation of priority requires high WM demands to lead to detectable prioritization. A potential explanation is that the near ceiling WM performance with lower set sizes may undermine the sensitivity of the priority manipulation. That is, participants cannot get better by prioritizing something that is already near perfect. Speculatively, with lower WM demands (Experiments 1–3), participants can perform the tasks without the need to budget resources between WM and a T2 that only occurs on a fraction of trials. With higher WM demands (Experiment 4), participants may not have enough cognitive resources to remember all the WM items and perform T2, so they must rely on either allocating all available resources to WM encoding, or budget some for T2.

Moreover, in Experiments 1 and 2, priority may have been biased towards T1 due either to the requirement to respond to T1 first or the option to simply remember the T2 sample and then actually perform the T2 decision at a later time (after T1 report). Either or both of these factors may have rendered the priority manipulations in Experiments 1 and 2 ineffective. Thus, the lack of retroactive interference on WM consolidation in Experiments 1 and 2 provides valuable information about what aspects of T2 are necessary to interrupt WM consolidation in dual-task paradigms. It seems that an immediate speeded sensorimotor response to T2 is required during the WM retention interval in order to interrupt consolidation. Future work should continue to explore the boundary conditions of retroactive interference on WM consolidation.

One limitation of the present study regards the use of letters as WM samples. Nieuwenstein and Wyble (2014) also observed retroactive interference on WM consolidation using complex unfamiliar visuospatial stimuli (Kanji characters) and Carlos and Tamber-Rosenau (2020) found similar results using color patches. Future research should assess structural versus flexible consolidation using visuospatial stimuli. Another limitation of the current study involves the relative weakness of between-subjects comparisons. This is somewhat circumvented by the Bayesian analysis, whose outcome is less dependent on sample size than frequentist tests. Moreover, Experiment 1 does not rely on between-subjects comparisons, and Experiment 4 uses a larger sample size.

In conclusion, the present research supports the view that slow consolidation is a structural feature of WM, not a result of flexible resource allocation. The duration of WM consolidation may be longer than originally anticipated, challenging research that relies on the assumptions that WM consolidation is a rapid unitary process that can be terminated via masking. Instead, past discrepant WM consolidation estimates likely stem from measurement of distinct stages of consolidation.

Supplementary information The online version contains supplementary material available at https://doi.org/10.3758/s13414-023-02757-7.

Acknowledgements This material is partially based upon work supported by the United States National Science Foundation under grant number 2127822.

The authors thank Harun Yoruk for helpful discussion related to the study; and Giang Le and Ryan Liu-Pham for technical assistance.

Open practices statement All data have been made publicly available on the Open Science Framework and can be accessed via the Open Science Framework at the following link: https://osf.io/fpj45/. None of the experiments were preregistered.

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