Active Maintenance in Working Memory Reinforces Bindings
for Future Retrieval from Long-Term Episodic Memory

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Abstract

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Many theories assume that actively maintaining information in working memory (WM) predicts its retention in episodic memory (EM), as revealed by the beneficial effects of more WM time. Here, we examined whether affording more time for intentional WM maintenance does indeed drive EM. Participants either intentionally or incidentally encoded sequences of four words presented during trials of simple span (short time) and complex and slow span (long time). Long time intervals entailed a pause of equivalent duration between the words that presented an arithmetic problem to read aloud and solve (complex span) or a blank screen (slow span). To ensure similar encoding of the words across the intentional and incidental encoding groups, participants silently decided whether each word was a living or nonliving thing via keypress (i.e., an animacy judgment; Experiment 1) or read the words aloud while pressing the spacebar (Experiment 2). A surprise delayed recall test at the end of the experiment assessed EM. Longer time in WM, particularly during slow span, improved retrieval from EM for both intentional and incidental encoding groups relative to short time, but for different reasons: modeling of the data indicated that longer intentional encoding increased binding memory (i.e., retrieval of the items' positions in the trial; Experiments 1 and 2), whereas longer elaborative but incidental encoding increased item memory (i.e., memory of items irrespective of their bindings; Experiment 1). This suggests that time spent actively keeping information in WM is special for EM because WM is a system that maintains bindings.

Keywords: working memory, long-term memory, episodic memory, cognitive modeling

The classic “textbook” view of human memory entails a “transfer” of the current contents of consciousness to a more permanent and durable store (Atkinson & Shiffrin, 1968; Waugh & Norman, 1965). Although many models of memory have been developed since, this fundamental idea has been influential: Many theoretical views assume that the processes involved in keeping information temporarily active and accessible in working memory (WM) likewise impact the likelihood of retrieving that information later on from long-term episodic memory (EM). These processes have included articulatory rehearsal (Atkinson & Shiffrin, 1968; Waugh & Norman, 1965), elaborative rehearsal (Craik & Tulving, 1975; Craik & Lockhart, 1972), refreshing (Camos & Portrat, 2015; Johnson et al., 2002), and consolidation (Cotton & Ricker, 2021), among others. Rather than detail their specific operational definitions and the differences between them, for the purposes of this work we will focus on the two common assumptions among them: (1) participants actively engage the process in the service of maintaining or acting on the information held in WM, and (2) the more frequent or longer the opportunities to engage in the process in WM, the more likely it is that the information will be transferred to EM.

These assumptions are reasonable and often justified in the literature: Regarding the first assumption of *active maintenance*, manipulations intended to vary the opportunity to engage a given process in WM often show effects on EM (e.g., Camos & Portrat, 2015; Cotton & Ricker, 2021; Jarjat et al., 2018; Johnson et al., 2002; Rose et al., 2014), and participants’ self-reported WM maintenance strategies often correlate with subsequent retrieval from EM (e.g., Loaiza & Lavilla, 2021; Rundus, 1971; Tan & Ward, 2008). However, these effects are not always consistently demonstrated (Bartsch et al., 2018; Bartsch, Loaiza, Jäncke, et al., 2019) or conclusively causative (Souza & Oberauer, 2018, 2020), and there is often controversy over

whether a given manipulation does indeed vary the process in question given that the processes are not directly observable (Oberauer et al., 2012).

Perhaps the most reliable and directly observable evidence for the importance of ongoing processing in WM for later retrieval from EM is that of elaborative rehearsal: Asking participants to make verifiable decisions that pertain to the deep, semantic characteristics of memoranda is much more beneficial for EM relative to shallow decisions (Craig & Tulving, 1975; Hyde & Jenkins, 1969, 1973; Loaiza et al., 2011; Rose et al., 2014). However, this levels-of-processing effect can be observed regardless of whether participants are aware of a final memory test (Craig & Tulving, 1975; Hyde & Jenkins, 1969), suggesting that the long-term benefit of elaborative rehearsal does not depend on actively trying to keep information in mind. This is at odds with the assumption of the importance of active maintenance in WM. Indeed, most models of WM assume that the processes crucial to its functioning are intentional and controlled, including those that impact later retrieval from EM. Thus, if actively processing information in WM is important to its long-term retention, then whatever essential process this might be, it has to be intentionally applied to keeping information active in WM. Nevertheless, the evidence for this key assumption of the unique importance of active maintenance in WM is either inconsistent and sometimes controversial (e.g., Bartsch et al., 2018; Souza & Oberauer, 2018), or, in the case of elaborative rehearsal as explained here, contradictory.

There is at least ample evidence for the second assumption of the *benefits of time*: Hartshorne and Makovski's (2019) recent meta-analysis of over 60 experiments revealed that the longer an item is held in WM, the greater the likelihood that it is later retrieved from EM. However, it is not clear exactly what participants are doing given the aforementioned persistent

drawback in this literature that WM processes are not directly observable. It may be that these benefits of time do not rely on actively keeping the information in mind, but instead reflect the mere benefits of temporal distinctiveness or spacing. Indeed, spacing effects can be observed both when participants intentionally encode the memoranda for a future test and during incidental encoding in which they are unaware and do not expect a test (Cepeda et al., 2006). Combined with the previous doubts about the importance of active maintenance in WM, these results may suggest that processing information in WM is not particularly special or important for its long-term retention in EM. This possible conclusion would dramatically shift many theoretical understandings of the importance of WM for EM, and thus it is important to specifically test.

Current Experiments

Here, we explicitly tested the often-taken-for-granted intuition that actively keeping information accessible in WM as long as one can is an essential driver of the long-term retention benefits of time to EM. To this end, we used a paradigm from prior work that has demonstrated the benefits of time in WM to EM (Loaiza & Lavilla, 2021; Souza & Oberauer, 2017). In these studies, participants viewed four successively presented words in three different types of trials: Simple span (words are presented successively without interruption), complex span (words are interleaved with a distracting arithmetic problem to read aloud and solve), and slow span (words are interleaved with a blank screen of equivalent duration to the arithmetic problem in the complex span trials). Participants recalled the words immediately at the end of each trial (to test WM) and after a delay (to test EM). The results have generally shown that words studied during complex span and slow span (which afford longer WM time) are more likely to be recalled after

a delay than words from simple span trials (shorter WM time), thus consistent with the assumption that affording greater time to process information in WM is more likely to lead to its successful retrieval from EM. We used this paradigm to test whether this benefit of time is exclusive to when participants actively try to keep the information in mind.

Figure 1

Illustration of the Tasks in Experiments 1 and 2. Panel A Shows Flow of Events in the Encoding Phase. Panel B Shows the Immediate Test that Followed Each Encoding Phase for the Intentional and Incidental Encoding Groups (30 repetitions). Finally, Panel C Presents the Surprise Delayed Test at the End of the Experiment.

Figure 1 shows the tasks of Experiments 1 and 2. In both experiments, participants viewed four successively presented words in trials of simple, complex, and slow span, but only half of the participants were instructed to maintain these words for an immediate recall test at the end of the trial (intentional encoding group), whereas the other half of the participants completed an unrelated no-recall task at the end of each trial (incidental encoding group). Thus, only half of the participants were assumed to intentionally encode and maintain the items in WM in the service of their immediate recall, whereas the other half of the participants were led to believe that the experiment was about juggling different demanding tasks and only incidentally encoded the items. In Experiment 1, participants read silently and decided whether the words were living or nonliving things (i.e., an animacy judgment) whereas participants in Experiment 2 read aloud and pressed the spacebar as each word appeared. This was to ensure that participants in either encoding group similarly processed the words (e.g., the incidental encoding group did not simply ignore the words altogether). All the participants of both encoding groups were surprised at the end of both experiments with a delayed recall test to assess EM.

This design allowed us to test the following pre-registered predictions (which can be found on the OSF: <https://osf.io/gq2t9/>): If the benefits of time are due to actively engaging in some process to keep the information accessible in WM that is also important to EM, then the previously observed delayed recall advantages of complex span and slow span (i.e., long time) over simple span (i.e., short time) should only occur when participants intentionally encode the items in WM. Conversely, there should be no complex span or slow span advantage to delayed recall when participants only incidentally encode the items given that they would not have actively engaged this key WM process during the free time in-between the words. However, if

any time is time well spent simply due to temporal distinctiveness and/or spacing, regardless of any active maintenance, then both intentional and incidental encoding groups should exhibit advantages of complex span and slow span to delayed recall.

It is possible that we may observe enhanced delayed performance in both of the encoding groups but for different reasons if we assume that EM reflects a combination of binding memory and item memory. Binding memory involves the retrieval of a studied item and its associated contextual details, such as its original serial position in the trial, whereas item memory refers to memory of the studied items irrespective of their associations or details. This is especially relevant given the theoretical view of WM as a system to establish new bindings and remove outdated and irrelevant ones (Oberauer, 2002, 2009, 2019). Thus, participants in the incidental encoding group may exhibit advantages of complex span and slow span that are driven more strongly by item memory, but not binding memory because the item-position bindings were irrelevant to their task. Conversely, the intentional encoding group may exhibit the advantages in binding memory in addition to item memory given that these item-position bindings were relevant for the WM test. Such a dissociation would suggest that time in WM benefits EM for two reasons: it may enhance item memory regardless of any active maintenance, but time used intentionally may further enhance memory for bindings that were relevant to actively encode and maintain in WM. We investigated this possibility as well by applying hierarchical Bayesian measurement models to estimate binding and item memory in the two encoding groups. Overall, these experiments allow us to directly address whether there is something truly special about active maintenance in WM for the benefit of time for EM.

Experiment 1

Method

Participants.

We recruited participants via Prolific to take part in both of the current experiments. There was a pre-screening so that prospective participants were only able to view the advertisement and take part in the experiment if they were native English speakers aged 18-35 years old, reported normal or corrected-to-normal vision, reported no history of cognitive impairment, who were using a desktop/laptop computer, and who had not taken part in any similar prior studies (Loaiza et al., 2020; Loaiza & Lavilla, 2021). The experiment took 30-40 minutes for most participants, and they were compensated with £7.50/hour. The project received ethical approval from the University of Essex Ethical Review Committee, and all participants in both experiments provided informed consent prior to starting the experiment and were debriefed at the end of the experiment.

Table 1

Sample Details and Exclusions. Final Sample of the Reported Analyses is Printed in Bold.

Sample details	Experiment 1		Experiment 2	
	Intentional	Incidental	Intentional	Incidental
Total N attempted	64	100	63	62
N quit in the middle of the experiment	10	16	12	5
N excluded for pre-registered reasons:	11	44	8	13
1. Experienced issues (e.g., technical) during the task*	1	0	2	1
2. Failed to conform to encoding group assignment*	2	42	5	13
3. Expected final memory test*	8	8	3	1
Final N after pre-registered exclusions	43	40	43	44
Further N reporting not following instructions	6	9	1	5
Subsetted N after excluding those not following instructions	37	31	42	39

* Note that these may not sum to the total N excluded given that participants could report more than one reason that resulted in exclusion.

Table 1 shows the details of the sample and the exclusion criteria. According to our pre-registration, we aimed to collect data for at least 80 valid datasets (40 per group) in each experiment, with a maximum of up to 120 valid datasets (60 per group) if necessary to establish the robustness of the findings. Participants were randomly assigned to either the intentional encoding group or the incidental encoding group.¹ This sample size was determined from prior work using the same paradigm (Loaiza et al., 2020; Loaiza & Lavilla, 2021). Data from participants were excluded from analysis and replaced if they quit in the middle of the experiment. Data were also excluded and replaced depending on participants' responses to a final survey (see Materials and Procedure section) for the following three reasons: (1) participants who reported experiencing legitimate issues (e.g., technical difficulties) that affected their ability to do the tasks sufficiently; (2) participants who did not conform to their group assignment (i.e., participants in the intentional encoding group who reported that they did not actively try to remember the words; participants in the incidental encoding group who reported that they actively tried to remember the words); and (3) participants who reported that they expected a final memory test. We conducted the analyses on the full sample following these exclusions as well as on the subsetting data with only the participants who reported complying with the task instructions, the latter of which can be found on the Open Science Framework (OSF). As shown in Table 1, the final sample size of Experiment 1 was 83 participants ($N = 43$ and 40 participants in the intentional and incidental encoding groups, respectively) after the pre-registered exclusions.

¹ Note that, in Experiment 1, many more exclusions were necessary for the incidental encoding group given our pre-registered criteria, and thus the remainder of participants collected in this experiment were assigned to the incidental encoding group to achieve the target sample size in each group.

Materials

The materials for both experiments can be found on the OSF: <https://osf.io/gq2t9/>. Both experiments were programmed with Inquisit (*Inquisit 5*, 2018; and *Inquisit 6*, 2021, for Experiments 1 and 2, respectively) and was very similar to Loaiza and Lavilla (2021, Experiment 2): A list of 10 practice and 360 critical concrete nouns served as the memoranda, half of which represented living things (letters: $M = 5.04$, $SD = 1.15$, range = 3-7; syllables: $M = 1.52$, $SD = 0.50$, range = 1-2, log HAL frequency: $M = 8.59$, $SD = 1.28$, range = 5.52-12.15; concreteness: $M = 4.64$, $SD = 0.43$, range = 3.00-5.00) and the other half represented non-living things (letters: $M = 5.08$, $SD = 1.05$, range = 3-7; syllables: $M = 1.45$, $SD = 0.50$, range = 1-2, log HAL frequency: $M = 8.65$, $SD = 0.93$, range = 5.81-10.58; concreteness: $M = 4.64$, $SD = 0.40$, range = 3.07-5.00). There were no significant differences between the characteristics of the living and non-living words, $ps > .20$. The experiment drew from these practice and critical lists randomly without replacement during the relevant task.

Procedure

A study advertisement on Prolific invited participants to take part in an experiment concerning how humans juggle different tasks, with no mention of memory. Participants were advised to be prepared to do the experiment in one continuous sitting in a quiet, distraction-free environment. They were instructed to carefully read and follow instructions, and they were informed that they would be able to view their general performance at the end of the experiment in order to enhance interest and motivation in the study. After signing up, participants installed an Inquisit plugin that forced the experiment to fill the screen, thereby preventing participants from engaging in other tasks on their computers during the experiment. Participants then

received instructions for and completed three main phases: practice, the immediate task, and the distraction and delayed task. After the final phase, participants completed a final survey and had the chance to view their overall performance on the tasks.

Practice Phase. During the practice phase, participants practiced each relevant element of the immediate task on its own for one block of 10 trials until passing an 85% criterion to move onto the next part. During the words practice, participants viewed a series of words successively presented for 1.5 s (0.1 s interstimulus interval, ISI)². Participants were instructed to read the words silently and decide whether each word was a living or non-living thing (i.e., an animacy judgment) using the right- and left-hand arrow keys, respectively. During the arithmetic practice, participants viewed a series of multiplication problems (e.g., $7 \times 4 = 30?$) presented for 3.5 s (0.1 s ISI) to read aloud and solve whether the answer provided was true or false with the arrow keys. Finally, participants in the incidental encoding group completed practice on the digits task, wherein a series of double-digit numbers (e.g., 23, 48, 51) were successively presented for a self-paced, unfixed duration (0.1 s ISI). Participants were instructed to read the digits silently and decide with the arrow keys regarding whether the digits composing each stimulus were both even (e.g., 42) or not (e.g., 43).

Immediate Test Phase. During the immediate task phase, participants completed trials of simple, complex, and slow span, with the only difference between the encoding groups being how the trials ended: either with a recall attempt (intentional encoding group) or an unrelated

² Note that we had written 1.25 s in the pre-registration, but this was an uncorrected typo from an earlier version. Testing the experiment out on ourselves prior to data collection prompted us to extend the presentation duration of the words to make the animacy decision feasible for the participants, but we failed to update the pre-registration thereafter.

digits task (incidental encoding group) as illustrated in Figure 1A and 1B. There was one block of 30 trials (10 of each type of task), randomly intermixed, with pauses for breaks after every 10 trials. Participants were informed that for each trial, a fixation (*****) would appear at the center of the screen (presented for 1.5 s), followed by four words (each presented for 1.5 s, 0.1 s ISI) to read silently and judge with regard to their animacy. The instructions informed participants that sometimes the four words would appear successively (simple span), whereas during other trials, either an arithmetic problem to read aloud and solve (complex span) or a brief blank screen (slow span) would appear after each word (presented for 3.5 s, 0.1 s ISI). At the end of the trial, participants in the intentional encoding group saw all four presented words among four never-presented distractors, with all eight words randomly arranged in a 2 x 4 grid of frames on the screen. These participants were instructed to use the mouse to select the four presented words in the order that they were presented. For participants in the incidental encoding group, eight double-digit numbers were randomly arranged in a 2 x 4 grid of frames on the screen. These participants were instructed to use the mouse to select the four double-digit numbers that were both even. For both groups of participants, their selections were echoed back to them in a row of four frames at the bottom of the screen. An intertrial interval of 1 s then preceded the start of the next trial.

To ensure that participants stayed on task, they were informed that they must follow these instructions carefully or risk being sent back to the practice round. Specifically, if participants made at least 2 mistakes on the words or arithmetic problems (i.e., they failed to respond or respond incorrectly), they received an initial warning, after their recall/digit selection but before the next trial began. This warning reminded them of the instructions and informed

them that if they continued making mistakes, then they would be sent back to the practice round. If participants then made 2 mistakes on any subsequent trial after having been warned, they returned to either the words or arithmetic problems practice round, or both, depending on where they fell short. After achieving the 85% criterion, they returned to the main experimental phase, wherein the same warning system continued to apply. Prior work that has implemented this paradigm online has demonstrated that this warning system is effective for motivating compliance with the task instructions in online experiments where an experimenter cannot be present to verify compliance (Loaiza et al., 2020; Loaiza & Lavilla, 2021).

Delayed Test Phase. During the distraction and delayed recall test phase, participants completed 1 min of a symmetry judgment task (taken from Foster et al., 2015; see Loaiza & Lavilla, 2021), which served as a distraction, followed by delayed recall of the words presented in each of the previous trials of the immediate task. During the delayed test, the words of each trial from the previous immediate test were probed in a new random order, wherein the four originally presented words of a given immediate trial were randomly intermixed with four never-presented lures in a 2 x 4 grid of frames. Participants were instructed to recall the words by clicking on the remembered words in their original order, with their responses echoed back to them in a row of frames at the bottom of the screen.

After completing these critical phases, participants completed a final survey wherein they reported basic demographic information (age, gender, native language) and their compliance with the task instructions as well as for their group assignment. Specifically, participants were asked whether they read and answered the arithmetic problems out loud while pressing the keys, whether they read the words silently while making the animacy judgments, whether they actively

tried to remember the words when they were first presented, whether they expected there to be a final memory test at the end, and whether they completed the experiment in one sitting in a quiet distraction-free environment. The survey also asked participants to note any issues arising (e.g., technical) that may have affected their performance. Participants were encouraged to be as honest as possible when answering these questions, with explicit instruction that their answers would not affect their compensation. Responses to these questions were used to justify any exclusions as explained in the Participants subsection.

Data Analysis

We conducted all pre-processing and analyses in R, using the BayesFactor, brms, and rjags packages (Bürkner, 2018; Morey & Rouder, 2015; Plummer, 2016). We anonymized the participant data by stripping the Prolific IDs from the data and replacing them with random ID numbers before putting the raw data on the OSF. All the raw data and scripts for the analyses described further on can be found on the OSF: <https://osf.io/gq2t9/>.

In general, we relied on Bayesian inference for each of our analyses explained further on, wherein one's prior beliefs about some parameters of interest (e.g., the effect of task type on retrieval from EM) are updated in light of observed data. The updated beliefs are the posterior distributions of these parameters, each of which has a mean and 95% credibility interval (CI) that gives a sense of how certain one can be that the true value of any given parameter lies within that range and whether it overlaps with 0 (i.e., a null effect). Furthermore, the means by which one's prior beliefs are updated is the Bayes factor (BF), which reflects a ratio of evidence for one model over another (e.g., an alternative model assuming an effect of task type over a null model assuming no effect of task type, BF_{10}). For greater clarity and comparison between analyses, we

will express BFs in favor of the null as their inverse (e.g., a BF_{10} of 0.8 will be expressed as $1/1.25$). Like CIs, BFs can be interpreted continuously, such that we can be increasingly convinced by the evidence for or against the null hypothesis as the CI is further away from/more centered on 0 or as the BF increases. Thus, where relevant, we report means [and 95% CIs] for the parameter estimates and/or BFs for one tested model over another.

There were several different possible measures to assess. First, we detail the performance checks that are not specific to our hypotheses but are nonetheless important: The animacy judgment ensured that participants indeed encoded the words similarly between groups, and both groups should be similarly distracted by the arithmetic problems during complex span. To ensure that the participants completed these tasks sufficiently and similarly between the two encoding groups, we checked that accuracy during the animacy judgments was similarly high between groups with a 2 (group: intentional, incidental) x 3 (task type: simple, complex, slow) mixed Bayesian analysis of variance (BANOVA), with the default settings. Accuracy on the arithmetic problems during complex span between the two encoding groups was compared with a one-sided Bayesian *t*-test. Evidence supporting an absence of any effects or interactions would suggest that the tasks were performed similarly between the two encoding groups and across the different task types. We also verified that participants in the incidental encoding group showed similar accuracy on the double digits task regardless of the type of trial (simple, complex, slow) with a one-way BANOVA. Conversely, a one-way BANOVA and follow-up one-sided Bayesian *t*-tests should show that the participants in the intentional encoding group showed the typical immediate recall advantage of simple and slow span over complex span, both in terms of

free scoring (i.e., recall counted as correct regardless of original serial position) and serial scoring (i.e., recall counted as correct only if serial position is correct).

The most important analyses for our predictions pertain to the delayed test. Given that the nature of WM encoding (intentional versus incidental) is completely and unavoidably confounded with immediate recall in this experiment, we did not make any comparisons between the two groups of participants for the delayed test. Instead, we only focus on the contrast of complex and slow span over simple span trials within each of the two groups, both in terms of observed delayed performance and estimated parameters of binding and item memory.

For observed delayed performance (free and serial scoring), we fit separate Bayesian logistic mixed effects models for each group and type of scoring to predict the likelihood of recalling a given item (1 or 0) as a function of the fixed effect of task type (simple, complex, slow) and including random effects and slopes of participant. We applied weakly informative Cauchy priors (with location 0 and scale 5) on the regression coefficient, intercept, and variance for both models. The posterior parameter estimates for the models were derived via Markov Chain Monte Carlo (MCMC) sampling. We checked for convergence of the four chains by visually inspecting the chains as well as verifying that the $\hat{R}$ statistic (i.e., the ratio of between-chain variance to within-chain variance) was close to 1 for all parameters of the fitted models. We also conducted posterior predictive checks to ensure appropriate model fit to the data. Most importantly, we drew inferences by inspecting the posterior distributions of the parameter estimates and their CIs for each task type condition within each encoding group.

To estimate parameters of binding and item memory more precisely, we fit both a multinomial processing tree (MPT) model that assumes discrete states of binding and item

memory as well as Oberauer and Lewandowsky's (2019) memory measurement model (MMM) that assumes that these parameters are continuous (Oberauer, 2019). Figure 2 shows a schematic of how each model works. For both models, we followed the same structure and priors as Oberauer (2019) with set size = 4 and response set = 8 (i.e., 4 studied items and 4 never-presented lures). Given this, when participants recall from these options for each list position, they can recall either the true target item of that position (Correct), one of the three other presented options (Other), or one of the four never-presented distractors (New).

Figure 2

Visual Depictions of the Multinomial Processing Tree (MPT) Model (Panel A) and the Memory Measurement Model (MMM; Panel B).

The MPT model (see Figure 2A) assumes that recall of the correct target can occur due to accurate binding memory (P_b) of the word in its position. In the absence of binding memory ($1 - P_b$), the correct target or one of the other presented items can be guessed with equal probability

on the basis of item memory (i.e., memory of the item without its binding, P_i). Finally, in the absence of binding memory and item memory ($1-P_i$), participants guess with equal probability between all the options (see also Bartsch, Loaiza, & Oberauer, 2019; Loaiza & Srokova, 2020).

The MMM model (see Figure 2B) assumes that all eight of the response options have a baseline activation B , and the four presented items have additional activation A , representing item memory. The correct target has an additional activation, C , due to the activation of its context that represents binding memory.

The posterior distributions of these parameters were estimated with Bayesian hierarchical implementations of the respective models. We checked the convergence, model fit, and drew inferences as previously described for observed performance. Given that the convergence diagnostics of the MPT model were insufficient according to recommended guidelines (i.e., $\hat{R}s < 1.06$; Gelman et al., 2013), we have only reported the results of the MMM model. The MPT model results can be found on the OSF.

Results and Discussion

Auxiliary task performance checks

As explained previously, we first assessed performance on the auxiliary tasks (i.e., accuracy of animacy encoding task, accuracy of arithmetic problems during complex span, digit accuracy in the incidental encoding group, and immediate recall performance in the intentional encoding group; see Table 2), which do not pertain to the hypotheses but still allow us to draw firmer conclusions about them. For the encoding task accuracy, a 2 (encoding group: intentional, incidental) $\times$ 3 (task type: simple, complex, slow) mixed BANOVA showed strong evidence for only an effect of task type ($BF_{10} = 3.99e16$) that was substantially preferred ($BF = 3.22$) to the next

best model that included a further effect of encoding group ($BF_{10} = 1.24e16$). Thus, encoding accuracy was worse in complex span overall compared to simple and slow span, but this did not vary by encoding group. Next, a one-sided Bayesian t-test showed strong evidence against an effect of encoding group on accuracy during the arithmetic problems of complex span ($BF_{10} = 1/13.58$). Thus, both encoding groups were similarly engaged with the encoding task as well as the distraction of complex span, regardless of their different encoding instructions.

Table 2

Means (and Standard Deviations) of Proportion Accuracy on the Auxiliary Tasks.

Measure	Task type	Experiment 1		Experiment 2	
		Intentional	Incidental	Intentional	Incidental
Encoding task accuracy	simple	0.91 (0.07)	0.91 (0.07)	0.98 (0.02)	0.99 (0.02)
	complex	0.81 (0.10)	0.84 (0.11)	0.94 (0.07)	0.96 (0.06)
	slow	0.91 (0.06)	0.91 (0.06)	0.98 (0.03)	0.98 (0.05)
Arithmetic problems accuracy	complex	0.90 (0.07)	0.89 (0.08)	0.91 (0.08)	0.91 (0.08)
Digit task accuracy	simple	-	0.98 (0.03)	-	0.98 (0.04)
	complex	-	0.98 (0.03)	-	0.97 (0.04)
	slow	-	0.98 (0.02)	-	0.97 (0.04)
Immediate recall – free scoring	simple	0.99 (0.02)	-	0.99 (0.02)	-
	complex	0.95 (0.05)	-	0.94 (0.05)	-
	slow	0.98 (0.04)	-	0.99 (0.02)	-
Immediate recall – serial scoring	simple	0.90 (0.10)	-	0.92 (0.07)	-
	complex	0.71 (0.19)	-	0.65 (0.18)	-
	slow	0.91 (0.11)	-	0.92 (0.09)	-

Next, a one-way BANOVA on accuracy to select the correct double-digit numbers at the end of the trials for the incidental encoding group showed strong evidence against an effect of task type ($BF_{10} = 1/7.09$). Finally, we assessed immediate recall performance in the intentional encoding group in terms of both free and serial scoring with respective one-way BANOVAs. Both analyses showed strong evidence for an effect of task type (free: $BF_{10} = 48,733$; serial: $BF_{10} =$

6.34e13), such that recall from complex span was considerably lower than both simple span (free: $BF_{10} = 2398$; serial: $BF_{10} = 3.71e7$) and slow span (free: $BF_{10} = 1411$; serial: $BF_{10} = 9.39e8$). However, there was little evidence of a difference in recall between simple and slow span (free: $BF_{10} = 1/2.10$; serial: $BF_{10} = 1/11.12$). Thus, the typical disadvantage to immediate recall from complex span relative to simple and slow span was observed in the intentional encoding group, whereas task type had no effect on the incidental encoding group's irrelevant task, as expected. It is important to establish these results in order to ensure that the next most important results pertaining to the central hypotheses could not be due to differences between the encoding groups in these auxiliary tasks.

Observed delayed recall performance

The next analyses concern observed delayed performance, or the likelihood of correctly selecting a presented item in terms of both free and serial scoring (see Figure 3A). We first considered the differences between the span tasks in the intentional encoding group. Following prior work (Loaiza & Lavilla, 2021; Souza & Oberauer, 2018) and consistent with our hypotheses, we observed a credible advantage of slow span over simple span (posterior of their difference in free scoring: 0.22 [0.00, 0.44]; serial scoring: 0.27 [0.04, 0.50]). However, inconsistent with some prior work (Loaiza & Lavilla, 2021) and our hypotheses, there was no advantage of complex span over simple span (free: 0.11 [-0.09, 0.31]; serial: 0.18 [-0.06, 0.40]). Finally, there was no difference between slow span and complex span (free: 0.11 [-0.13, 0.34]; serial: 0.09 [-0.12, 0.30]). Thus, longer time in WM benefitted EM only in the slow span condition and not credibly for the complex span task condition for the intentional encoding group.

Figure 3

Mean Posterior Estimates of Observed Delayed Recall Performance (Panel A) and Parameter Estimates from the Memory Measurement Model (MMM; Panel B) in Experiment 1.

Note. Error bars reflect 95% credibility intervals and individual points reflect posterior predicted responses based on the models.

To further assess whether active maintenance in WM is what is most crucial to this long-term benefit of uninterrupted free time following the words, we considered these task type differences in the delayed performance of the incidental encoding group. We again observed an advantage of slow span over simple span, but only in free scoring (0.19 [0.00, 0.38]) and not serial scoring (0.01 [-0.24, 0.23]). There were no other credible differences between tasks in the incidental encoding group. This is consistent with our pre-registered suggestion that a time benefit may be observed in the incidental encoding group but only in free scoring, and not serial scoring. This indicates that longer time intervals without active maintenance in WM may still increase the overall accessibility of the individual items (i.e., item memory) without impacting

any associated contextual details (i.e., binding memory). However, free and serial scoring are not process-pure measures of item and binding memory, respectively, and thus the next reported analyses allow us to estimate these parameters more precisely.

Parameter estimates of item and binding memory

The item memory (A) and binding memory (C) parameters estimated with the MMM are shown in Figure 3B. For the intentional encoding group, there was a credible advantage of slow span over simple span (0.12 [0.06, 0.19]) as well as complex span (0.08 [0.01, 0.15]) for binding memory, but no credible advantage of complex span over simple span (0.05 [-0.01, 0.10]) nor any differences between the task types in item memory. Conversely, for the incidental encoding group, there was a credible advantage of slow span over simple span (0.05 [0.00, 0.11]) in item memory as well as a credible disadvantage of complex span versus simple span in binding memory (-0.04 [0.07, 0.00]), and all other comparisons were not credible.

In sum, rather than either an exclusive benefit for the intentional encoding group or a general benefit for both encoding groups, the benefit of time in WM accrued on different memory parameters depending on the encoding group: Actively maintaining information during slow span increased binding memory for the intentional encoding group, whereas there was a mere benefit of spacing or temporal distinctiveness of slow span to item memory for the incidental encoding group. This dissociation is congruent with our pre-registered suggestion that time in WM may benefit EM for two reasons: it may enhance item memory even without any active maintenance, but time used intentionally may specifically enhance memory for bindings that were relevant to actively encode and maintain in WM. Furthermore, the fact that no such advantage was observed for complex span, and even a disadvantage to incidentally encoded

bindings relative to simple span, suggests that uninterrupted free time following each word is most important to strengthening these different benefits of time on EM.

Experiment 2

The results of Experiment 1 indicated that the benefits of actively maintaining information in WM over uninterrupted time specifically reinforce the long-term retention of bindings. However, the relatively smaller effects and the fact that the time advantage was specific to slow span is at odds with prior work using this paradigm which Experiment 1 closely reproduced (Loaiza & Lavilla, 2021; Souza & Oberauer, 2017). For comparison, Loaiza and Lavilla (2021) showed credible delayed advantages in both free and serial scoring for both complex span over simple span (free: 0.31 [0.20, 0.42]; serial: 0.22 [0.09, 0.36]) and slow span over simple span (free: 0.48 [0.36, 0.61]; serial: 0.57 [0.41, 0.73]; see Figure 4A).

One potential procedure difference that may explain this discrepancy is that participants were required to make a silent animacy judgment during word presentation in Experiment 1 whereas prior work asked participants to simply read the words aloud. The animacy judgment of Experiment 1 was only intended to ensure that the words were encoded in both the intentional and incidental encoding groups, but this may have inadvertently engendered an elaboration effect across the three types of trials (namely, simple, complex, and slow span) that was strong enough to outweigh the benefits of longer maintenance time in WM. Indeed, delayed performance was much greater overall when numerically comparing the current intentional encoding group to that of Loaiza and Lavilla, particularly for simple span (performance in Loaiza and Lavilla, free scoring: simple = 0.73 [0.70, 0.75], complex = 0.78 [0.76, 0.81], slow = 0.81 [0.78, 0.84]; serial scoring: simple = 0.27 [0.24, 0.30], complex = 0.34 [0.30, 0.39], slow = 0.39 [0.34,

0.45]). The results of Experiment 1 appear more similar to when Loaiza and Lavilla's participants reported using elaborative strategies (free: simple = 0.80 [0.75, 0.84], complex = 0.85 [0.81, 0.88], slow = 0.88 [0.84, 0.91]; serial: simple = 0.38 [0.31, 0.45], complex = 0.42 [0.35, 0.49], slow = 0.54 [0.45, 0.63]; see Figure 4B).

Figure 4

Mean Posterior Estimates of Observed Delayed Recall Performance as a Function of Task Type (Panel A), Task Type and Reported Strategy (Panel B), and Parameter Estimates from the Memory Measurement Model (MMM; Panel B) from Loaiza and Lavilla (2021).

Note. Error bars reflect 95% credibility intervals and individual points reflect posterior predicted responses based on the models.

Thus, while the point of the experiment was not to investigate elaboration, our results indicated that an enforced elaboration during encoding greatly minimized the long-term benefit of time in WM. This may suggest that elaboration is a crucial process underlying this benefit, but the answer to our principal research question is still unclear regarding whether whatever process

engaged (perhaps elaboration or something else) must be in service to keeping information active in mind in order to observe the long-term benefits of time in WM.

In order to address this lingering issue, we conducted a second experiment that was very similar to our first experiment, except that participants were instructed to read the presented words aloud and press the spacebar when they finished reading (so that we could monitor online performance). This was more similar to prior work while also ensuring that participants encoded the words regardless of their encoding group, as the program returned participants to the practice round for failing to follow these instructions (i.e., to press the spacebar). Moreover, participants' voice responses were recorded and transcribed offline after data collection was completed to verify that both encoding groups similarly followed these instructions.

Our predictions were the same as in Experiment 1: If actively engaging in some maintenance process underlies the long-term benefit of time in WM, then the delayed advantages of complex span and slow span over simple span should be unique to when participants intentionally encode the items in WM, whereas no such advantages should be observed under incidental encoding conditions. However, if active maintenance in WM is not necessary for these long-term benefits, then both the intentional and incidental encoding groups should exhibit delayed advantages of complex span and slow span over simple span. Furthermore, as previously explained, it may be possible that these advantages may differently occur for the two encoding groups if active maintenance in WM is particularly important for binding memory than item memory. Thus, the delayed advantages of complex span and slow span may occur only in item memory for the incidental encoding group, whereas the intentional encoding group may show these advantages in both binding and item memory.

Method

Participants

As explained previously, participant recruitment procedures and exclusion criteria for Experiment 2 were the same as for Experiment 1. No one from Experiment 1 was allowed to participate in Experiment 2. As shown in Table 1, the final sample size of Experiment 2 was 87 participants ($N = 43$ and 44 participants in the intentional and incidental encoding groups, respectively) after the pre-registered exclusions.

Materials, Procedure, and Data Analysis

The experiment materials, procedure, and data analysis methods were identical to Experiment 1, with the main exception being that participants were instructed to read the presented words out loud only one time and press the spacebar when they finished but before the word disappeared from the screen. Participants practiced this task for 10 trials before the critical phase of the experiment, and they were instructed to read at a normal rate and to not press the spacebar too quickly. The experiment only registered responses 0.2 s after stimulus onset, and participants were warned that responding too quickly or failing to respond would count as an error. As in Experiment 1, the words were presented each for 1.5 s (0.1 s ISI), and participants were warned that they would repeat the practice if they made too many errors in order to motivate them to stay on task. Participants' voice responses during the presentation of the words and the arithmetic problems were recorded.

One further minor change was to include additional instruction screens to facilitate administration of the experiment: A screen at the start of the experiment instructed participants on how to arrange their hands to avoid switch costs between the spacebar and the arrow keys

during the words and arithmetic problem tasks, respectively. Additional instruction screens began each of the main phases of the experiment (i.e., practice, critical task, and distraction/delayed recall) in order to clearly demarcate the elements of the experiment to the participants for later ease of reference during the final survey.

Results and Discussion

Auxiliary task performance checks

As in Experiment 1, we first report performance on the auxiliary tasks (i.e., accuracy of spacebar encoding task, accuracy of arithmetic problems during complex span, digit accuracy in the incidental encoding group, and immediate recall performance in the intentional encoding group; see Table 2). For encoding task accuracy, a 2 (encoding group: intentional, incidental) x 3 (task type: simple, complex, slow) mixed BANOVA showed strong evidence for only an effect of task type ($BF_{10} = 963,735$) that was substantially preferred ($BF = 3.68$) to the next best model that included a further effect of encoding group ($BF_{10} = 261,939$). Thus, encoding accuracy was worse in complex span overall compared to simple and slow span, but this did not vary by encoding group. A one-sided Bayesian t-test also showed strong evidence against an effect of encoding group on accuracy during the arithmetic problems of complex span ($BF_{10} = 1/8.07$). Thus, both encoding groups were similarly engaged with the encoding task as well as the distraction of complex span, regardless of their different encoding instructions.

Next, a one-way BANOVA on accuracy to select the correct double-digit numbers at the end of the trials for the incidental encoding group showed strong evidence against an effect of task type ($BF_{10} = 1/4.06$). Finally, we assessed immediate recall performance in the intentional encoding group in terms of both free and serial scoring with respective one-way BANOVAs. Both

analyses showed strong evidence for an effect of task type (free: $BF_{10} = 7.26e10$; serial: $BF_{10} = 8.28e22$), such that recall from complex span was considerably lower than both simple span (free: $BF_{10} = 482,453$; serial: $BF_{10} = 6.17e11$) and slow span (free: $BF_{10} = 212,269$; serial: $BF_{10} = 4.02e11$). However, there was no difference in recall between simple and slow span (free: $BF_{10} = 1/3.57$; serial: $BF_{10} = 1/7.69$). Thus, the typical disadvantage to immediate recall from complex span relative to simple and slow span was observed in the intentional encoding group, whereas task type had no effect on the incidental encoding group's irrelevant task, as expected. As in Experiment 1, these results indicate that the next most important results pertaining to the central hypotheses could not be due to differences between the encoding groups in these auxiliary tasks.

Observed delayed recall performance

We next considered observed delayed performance as a function of task type for each encoding group and method of scoring (i.e., serial and free scoring; see Figure 5A). For the intentional encoding group, there were no credible differences between the tasks in free scoring, but like Experiment 1, there was a credible advantage of slow span over simple span (0.38 [0.16, 0.59]) in serial scoring. There was also a credible advantage of slow span over complex span (0.35 [0.14, 0.54]), and no difference between complex span and simple span (0.03 [-0.16, 0.23]) in serial scoring. Thus, the long-time benefit was once again specific to slow span and not complex span in serial scoring, but different to Experiment 1, this was also reflected in an advantage of slow span over complex span.

To determine whether these effects were specific to active maintenance in WM, we assessed these task differences in the incidental encoding group. Unlike Experiment 1 for which we observed a benefit of slow over simple span in free scoring, there was only a credible

advantage of slow span over complex span in free scoring (0.17 [0.03, 0.32]), and no other effects were credible. Thus, we failed to observe any long-term benefits of time without active maintenance in the incidental encoding group, but as emphasized previously, free and serial scoring cannot be considered as process-pure measures of item and binding memory. The next analyses allow us to consider these parameters more precisely.

Figure 5

Mean Posterior Estimates of Observed Delayed Recall Performance (Panel A) and Parameter Estimates from the Memory Measurement Model (MMM; Panel B) in Experiment 2.

Note. Error bars reflect 95% credibility intervals and individual points reflect posterior predicted responses based on the models.

Parameter estimates of item and binding memory. The item memory (A) and binding memory parameters (C) estimated with the MMM are shown in Figure 5B. As in Experiment 1, the intentional encoding group showed a credible advantage of slow span over simple span (0.07

[0.04, 0.10]) as well as complex span (0.08 [0.05, 0.11]) in binding memory, with no credible advantage of complex span over simple span (-0.02 [-0.04, 0.01]). However, different to Experiment 1, the intentional encoding group also showed credible advantages of complex span over simple span (0.05 [0.01, 0.10]) and slow span (0.06 [0.01, 0.10]) in item memory, with no credible advantage of slow span over simple span (-0.00 [-0.05, 0.03]). Thus, in this experiment, the long-term benefits of time depended on whether that maintenance was interrupted with a distracting secondary task as in complex span: Actively maintaining information for an uninterrupted period of free time during slow span once again reinforced the long-term retention of bindings, but in this experiment, we further observed that active maintenance under distraction during complex span increased the long-term accessibility of the individual items. For the incidental encoding group, there was a small but credible advantage of slow span over complex span in item memory (0.02 [0.00, 0.05]), and all other differences between the task types in item and binding memory were not credible.

These results complement and extend those of Experiment 1: once again, the benefit of actively maintaining information in WM during slow span improved the long-term retention of bindings for the intentional encoding group. However, unlike Experiment 1, there were no advantages of slow span for item memory in the incidental encoding group, suggesting that perhaps the additional elaborative animacy decision of Experiment 1 enhanced item accessibility during incidental encoding. Indeed, comparing Figures 3B and 5B show overall greater item memory in Experiment 1 compared to Experiment 2 wherein participants were not required to make elaborative decisions. Further different to Experiment 1 was the finding that item memory was credibly greater for complex span compared to simple and slow span in the intentional

encoding group. Thus, the elaborative decisions of Experiment 1 may have enhanced item memory of simple and slow span to obfuscate this difference that was evident in Experiment 2, although this will need further investigation. Overall, these results suggest that time in WM may differently benefit underlying parameter estimates of item and binding memory depending on whether participants actively maintained the information and whether that active maintenance was interrupted: Uninterrupted active maintenance during slow span clearly benefitted binding memory in both experiments, whereas incidental but elaborative encoding during slow span (Experiment 1) and interrupted active maintenance during complex span (Experiment 2) yielded increases to item memory relative to short maintenance in simple span.

General Discussion

The current experiments tested whether there is something truly special about active maintenance in WM for the benefit of time on long-term EM. Both experiments achieved this by encouraging either the intentional or incidental encoding of memoranda that were either presented successively following a simple span procedure (i.e., short time interval) or each followed by a period of interrupted or uninterrupted time following a complex span and slow span procedure, respectively (i.e., longer time interval). If the benefits of time on EM depend on processes carried out during WM maintenance, then we should observe that longer time intervals produce better EM than short time only in the intentional encoding condition in which the memoranda were actively encoded and maintained in WM for an immediate memory test. However, if the benefits of time occur due to mere effects of spacing or temporal distinctiveness, then both encoding groups should observe similar long-term advantages of longer time intervals.

We also tested whether this time benefit entails the build-up of different types of representations by modeling our data to estimate parameters related to item and binding memory.

Table 3

Summary of the Main Findings Across Experiments 1 and 2 in Terms of the Observed Free and Serial Scores (First Two Rows) as Well as the Item and Binding Memory Parameters Estimated from the Memory Measurement Model (MMM) Model (Last Two Rows).

Measure	Pairwise comparison	Experiment 1		Experiment 2	
		Intentional	Incidental	Intentional	Incidental
Free scoring	Complex vs. Simple	0.11 [-0.09, 0.31]	0.01 [-0.17, 0.19]	0.17 [-0.02, 0.37]	-0.13 [-0.27, 0.01]
	Slow vs. Simple	0.22 [0.00, 0.44]	0.19 [0.00, 0.38]	0.13 [-0.04, 0.30]	0.04 [-0.10, 0.19]
	Slow vs. Complex	0.11 [-0.13, 0.34]	0.19 [-0.01, 0.38]	-0.04 [-0.24, 0.17]	0.17 [0.03, 0.32]
Serial scoring	Complex vs. Simple	0.18 [-0.06, 0.40]	-0.15 [-0.38, 0.09]	0.03 [-0.16, 0.23]	-0.19 [-0.38, 0.01]
	Slow vs. Simple	0.27 [0.04, 0.50]	0.01 [-0.24, 0.23]	0.38 [0.16, 0.59]	-0.07 [-0.26, 0.13]
	Slow vs. Complex	0.09 [-0.12, 0.30]	0.16 [-0.10, 0.41]	0.35 [0.14, 0.54]	0.12 [-0.09, 0.33]
Item memory (A)	Complex vs. Simple	0.04 [-0.05, 0.12]	0.01 [-0.04, 0.05]	0.05 [0.01, 0.10]	-0.02 [-0.04, 0.01]
	Slow vs. Simple	0.04 [-0.04, 0.13]	0.05 [0.00, 0.11]	0.00 [-0.05, 0.03]	0.01 [-0.02, 0.03]
	Slow vs. Complex	0.01 [-0.08, 0.10]	0.05 [-0.01, 0.10]	-0.06 [-0.10, -0.01]	0.02 [0.00, 0.05]
Binding memory (C)	Complex vs. Simple	0.05 [-0.01, 0.10]	-0.04 [0.00, -0.07]	-0.02 [-0.04, 0.01]	0.00 [-0.02, 0.02]
	Slow vs. Simple	0.12 [0.06, 0.19]	-0.03 [-0.07, 0.01]	0.07 [0.04, 0.10]	-0.01 [-0.03, 0.01]
	Slow vs. Complex	0.08 [0.01, 0.15]	0.01 [-0.03, 0.05]	0.08 [0.05, 0.11]	-0.01 [-0.03, 0.01]

Note. Values in brackets present the 95% credible interval of posterior. Effects in bold are considered credible (i.e., the credible interval does not include 0).

As is summarized in Table 3, our results showed that long uninterrupted time during slow span trials improved EM for both intentional and incidental encoding groups, but for different reasons: Intentional encoding increased binding memory, regardless of whether participants were required to make elaborative encoding decisions on the memoranda (Experiment 1) or not (Experiment 2), whereas incidental but elaborative encoding increased item memory (Experiment 1). Furthermore, the only advantage of long interrupted time in complex span was to item memory when no elaborative decisions were required (Experiment 2), and only for the

intentional group. As we will discuss further on, these results suggest two main conclusions: (1) Time spent actively keeping information in WM is special for EM because WM is a system that maintains bindings (Oberauer, 2019); and (2) Besides opportunities for maintenance, time in WM may be used to proactively replenish depleted encoding resources (Popov & Reder, 2020; Reder et al., 2007).

It is also important to note that using cognitive modeling to precisely measure underlying parameters of binding and item memory allowed us to draw these firm conclusions rather than relying only on observed retrieval performance which does not clearly distinguish between different memory processes. For example, in Experiment 1 there was an advantage of slow span over simple span in both free and serial scoring, but the results of the MMM showed that this advantage was specific to binding memory. This particular result drives home the point that observed performance (e.g., free scoring) cannot be considered a process pure measure of underlying cognitive processes (e.g., item memory) given that multiple processes can contribute to performance. Overall, these findings provide novel theoretical insight into why active maintenance in WM is important to the benefits of time for EM.

Uninterrupted Active Maintenance Reinforces Bindings in Working Memory

Both experiments showed that intentional encoding of words presented during slow span specifically increased binding memory relative to short maintenance in simple span, and, against our pre-registered predictions, also compared to long interrupted maintenance in complex span. These results indicate that the long-term benefits of time in WM may be specific to active maintenance during *uninterrupted* free time following studied memoranda. In other words, longer time intervals in WM may specifically help to establish and retain bindings over the long-

term when active maintenance can be sustained without distraction, as in slow span, but is less effective to do so when active maintenance is distracted during that the same period of time, as in complex span. These results cohere with the theoretical notion that WM is a core construct in cognition because its role is to establish, update, and dissolve temporary bindings (Oberauer, 2002, 2009, 2019), some of which can be retained in EM as well. However, the fact that this was specific to slow span conflicts with previous assertions that active maintenance despite distraction during complex span enhances binding memory (e.g., Loaiza et al., 2015), and further suggests that active maintenance in WM may yield different long-term consequences depending on the conditions of the maintenance. We now discuss each of these two major implications in turn.

First, our results speak to a longstanding research question of how active maintenance in WM impacts later retrieval from EM. Whether participants were required to make silent elaborative decisions (Experiment 1) or simply read words aloud (Experiment 2), our results showed that intentional encoding during slow span enhanced later binding memory relative to simple span and complex span. This highlights that the “transfer” or “retention” of information from WM into EM does not only pertain to the individual pieces of information themselves (i.e., item memory), but also to their associated, contextual bindings (i.e., binding memory). Furthermore, the fact that intentional encoding was specific to later binding memory indicates that active maintenance in WM is unique to the benefits of time for EM because of its role to create these bindings. Thus, the current results are consistent with the binding hypothesis of WM, such that WM is important for other cognitive constructs like EM due to its capacity-limited role to maintain bindings specifically (Oberauer, 2002, 2009, 2019). For example, recent prior

work using the same cognitive modeling approaches used here have shown that varying the number of items to maintain in WM (i.e., set size) impacted binding memory, but not item memory (Bartsch, Loaiza, & Oberauer, 2019; Oberauer, 2019). Thus, dissociations of binding and item memory are evident for both set size as well as active maintenance in WM, as we have shown here, pointing to unique importance of WM for binding memory. Other recent work has highlighted the importance of intentional encoding for binding memory in terms of list positions of studied words (Oberauer & Greve, 2021) and item-context associations (Popov & Dames, 2021). Our results therefore fit into an emerging picture that active maintenance over brief periods of uninterrupted time in WM specifically enhances long-term retention of bindings, thereby providing an account of the relationship and transfer between WM and LTM.

The second main implication of our results is that enhanced binding memory particularly occurs due to active and uninterrupted maintenance in WM given that the benefit was specific to slow span and not complex span in either experiment. Thus, in contrast to other work asserting that active maintenance under distraction may increase later binding memory (Loaiza et al., 2015; Loaiza & McCabe, 2012, 2013), we observed that only item memory of complex span increased compared to slow span and simple span in Experiment 2. As noted previously, our current experiments closely followed the paradigm of prior work, particularly Experiment 2 of Loaiza and Lavilla (2021), who investigated the use of instructed and spontaneously reported strategies. When fitting the MMM to these data, there was indeed a similar advantage of slow span over simple span and complex span in binding memory, but there was also an advantage of complex span over simple span in binding memory as well (see Figure 4C). Thus, it appears that active maintenance in the face of distraction during complex span does not as reliably benefit

binding memory as uninterrupted free time in slow span. This may indicate that the distracting processing task disrupts the WM resources required for binding that are available during the uninterrupted free time of slow span. Hence, the relatively greater amount of time compared to simple span may increase the overall availability of the individual items, but the distracting processing task may impair the creation and retention of bindings in WM, consistent with interference-based models of WM and complex span more specifically (Oberauer et al., 2012, 2016).

In summary, our results suggest that the long-term retention of information in WM may depend on the nature of active maintenance, such that uninterrupted free time may enhance later binding memory whereas distraction may enhance later item memory.

Encoding Resources in Working Memory

Our results are also consistent with the idea that encoding information to WM depletes a pool of limited resources, and that this resource replenishes over time such that providing more time in-between the memoranda allows for a recovery from resource depletion leading to better memory over the short- and long-term (Mizrak & Oberauer, 2021; Popov & Reder, 2020; Reder et al., 2007). Our findings further qualify this proposal by showing that: (a) this depletion and recovery occurs mostly in intentional conditions, since benefits of time were not consistently found in the incidental conditions; (b) resource replenishment affects bindings; (c) the replenishment of binding resources may only occur during periods of free time, but not when attention is distracted with other activities as it was the case for the complex span trials that required the processing of an arithmetic task; and (d) during interrupted free time, item memory may still recover.

Furthermore, our finding that deep, semantic encoding can enhance item memory even under incidental encoding suggests that the seminal research in memory that played down the role of intentionality for long-term retention (e.g., Craik & Tulving, 1975; Hyde & Jenkins, 1969, 1973) may be reconciled with more recent work suggesting that intentionality matters (Popov & Dames, 2021). Specifically, deeper processing may increase the long-term availability of items, regardless of intentionality, whereas intentional encoding, particularly with uninterrupted free time following, may enhance the long-term availability of bindings. We anticipate that these will be fruitful avenues of future research.

Conclusions

How do we retain the information that we currently have active and accessible in mind long after it has left our immediate awareness? Our results suggest that it depends on the nature of what we are doing with that information: actively maintaining the information in WM (i.e., intentional encoding) may increase the associated, contextual details of the studied information (i.e., binding memory) when there is uninterrupted free time available (i.e., slow span), whereas encountering distraction (i.e., complex span) may increase the later availability of the individual pieces of information (i.e., item memory) compared to not having had any time available (i.e., simple span). However, some individual pieces of information may be still remembered in the long-term even if we were not trying to remember it (i.e., incidental encoding), particularly when deep, semantic processes were engaged at the time. Hence, our results help to clarify the relationship between WM and EM.

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