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Moving forward: Exploring the role of interference on prospective memory deactivation

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Abstract:	Recent prospective memory (PM) studies have shown that an intention may be erroneously executed despite no-longer-needed (i.e., commission errors), especially under demanding ongoing activities. In the current study, we examined whether PM deactivation benefits from a retroactive interference mechanism. For this, we set up a procedure in which participants are first asked to perform a PM task which is critically declared finished afterwards. Next, they encoded a new and dissimilar PM intention to accomplish later (Experiment 1) or performed filler tasks with increased working memory difficulty levels (Experiment 2). Lastly, all participants encountered several (but irrelevant) PM cues. Together, our findings provide evidence that the efficiency of the deactivation process can be modulated by encoding novel and dissimilar PM tasks and by the type of processing after intention completion. These findings are discussed in terms of strategic or spontaneous retrieval processes and linked to a retroactive interference mechanism which helps to overwrite or deteriorate the old-PM task representation.
Suggested Reviewers:	Michael Scullin Michael_Scullin@baylor.edu Dr. Scullin is an expert in prospective commission errors research.
	Francis Anderson anderson.f@wustl.edu Dr. Anderson is an expert in prospective memory research.
Response to Reviewers:	

August 10, 2020

Wim Notebaert, PhD

Editor, Acta Psychologica

Dear Dr. Notebaert,

I am enclosing a submission to the Acta Psychologica entitled *Moving forward: Exploring the role of interference on prospective memory deactivation*. The manuscript is 36 pages long and includes two tables and three figures.

In the current study, we examined which factors might prevent the likelihood of performing a prospective memory (PM) intention despite it is no longer needed (i.e., commission errors) and, thereby, to elucidate the mechanisms underlying PM deactivation.

My coauthors and I do not have any conflicts of interests to disclose. APA ethical standards were followed in the conduct of the study, and we received approval from the University of Minho institutional review board.

I will be serving as the corresponding author for this manuscript. My coauthor has agreed to the byline order and to submission of the manuscript in this form. I have assumed responsibility for keeping my coauthor informed of our progress throughout the editorial review process, the content of the reviews, and any revisions made to the manuscript.

Sincerely.

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Manuscript ACTPSY_2020_423R1

Response to Reviewer

Dear Dr Notebaert,

Thank you for allowing us to submit a second revised draft of the manuscript "*Moving forward: Exploring the role of interference on prospective memory deactivation*" for publication in Acta Psychologica. We appreciate the time and effort you and the reviewer dedicated to providing another feedback on our manuscript. We have incorporated most of the suggestions made by the reviewer, and those changes are underlined within the manuscript. Please see below, in blue, for a point-by-point response to the reviewer' comments and concerns. All line numbers refer to the revised manuscript file.

Reviewer Comments to the Authors:

Reviewer 1

1. The revision has taken into account many points that have been raised in the previous review and thus it is substantially improved, however, there are still some lingering issues, which are summarized below.

Author response: Thank you.

2. First, there is still a mix up between the commission error (CE) findings and findings from the repeated cycles paradigm in which the focus is on RTs. It is really crucial to note that in the latter paradigm CEs are very rare. However, at several points in the MS the authors describe the (RT) results (i.e., the slowing on previously relevant PM cues) as if they were identical to CE (e.g., lines 111-114; 633ff, 646 ff etc.). If the authors think RT effects and CE are identical, they should introduce and justify this.

Author response: Thank you for pointing this out. We agree that this should be clarified, and so we have revised the text accordingly (please see the several changes underlined in the manuscript).

3. Second, in the design section, the authors note that design is a 2 x 3 mixed factorial. However, in the results section many different kinds of designs are calculated. It would be good to be consistent.

Author response: Thank you for your comment. In fact, there were several relevant analyses to be carried out and, for the sake of synthesis, we choose to remove the design section in this revised manuscript.

4. Third, in the results it was not clear whether the new analysis of formerly relevant PM trials includes only correct LDTs, incorrect or both.

Author response: We appreciate your suggestion. To clarify this issue, RTs to formerly relevant PM trials analyses were conducted on correct trials, faster than 300 ms, and were trimmed at 3 standard deviations from each participant´s mean (please see lines 272-274).

5. Fourth, I still think that the RT analysis that compares the conditions across blocks and particularly the post-hoc tests are problematic due to the slow performance of the non-PM control group in the "active phase". As there is no requirement for monitoring for the control group compared to the PM groups this is surprising and should be considered. I suggested a difference score to circumvent this problem. Again, I would like to draw the attention to the fact that the speed-up between the active and the finished phase is much bigger in the No-PM condition than in the PM conditions. In Experiment 1 this is 220 ms compared to appr. 140 ms in each PM condition. This suggests that in contrast to the authors claim that there were monitoring costs in the new PM condition compared to the no new condition, it suggests that there are no monitoring differences between the "no new PM"- and the "new" PM condition.

Author response: We appreciate your rationale. In Experiment 1, according to the difference score analysis between the active- and finished-PM phases, a one-way ANOVA showed a significant effect of PM condition in RTs, *F* (2, 122) = 7.55, *p* < .001. Post-hoc Scheffe tests showed a speed-up in the control condition (*M* = 218, *SD* = 122) than in the no-new-PM condition (*M* = 139, *SD* = 99), p = .004, 95% CI [21.90, 137.10]; as well as compared to the new-PM condition (*M* = 141, *SD* = 90), p = .005, 95% CI [20.21, 135.41]. However, we should underline some aspects. First, in line with the standard analysis of previous studies, it seemed to us more appropriate to examine the presence of preparatory monitoring via group differences in each of the PM phases (especially during the finished-PM block during which previously irrelevant PM cues reappear). Second, we must be aware that the speed-up between the active and finished-PM phases might result from a learning effect since we consistently observed that participants were slower in response to the LDT in the active compared to the finished-PM phase in all our studies (see also Matos et al., 2020). The LDT had to be performed in each and almost every trial and, importantly, *the* lexical decision is a cognitive process where a reader automatically accesses knowledge about a familiar written word (Castles & Nation, 2008). Third, we did not observe a main effect of the PM group in the analyses of variances conducted in both experiments in the active-PM phase, indicating that lexical decision responding was similar between participants in the experimental conditions and those assigned to the no-PM control condition (in both PM-phases, except for the new-PM condition in Experiment 1). In other words, although they were numerically slower compared to the other groups in the active phase, such a data pattern did not show evidence of significantly slow performance in the no-PM control group in comparison to the other experimental conditions.

6. In Experiment 2, we again observe in the active phase the slowest performance for the control group that had no PM task to monitor. Again, the speed up is about 210 ms in the No-PM condition compared to appr. 140 ms in the no WM load, 180 ms in the low load, and 200 ms in the high load condition. The authors note that "consistent with our prediction and

replicating Experiment 1, we observed similar accuracy and RTs between the conditions". Please note that first, this is not consistent with what you wrote about Experiment 1, second again the difference score seems to tell a different story. Therefore, I think it would be really necessary to run a difference score analysis (i.e., the RT difference between active and finished PM blocks) and discuss this pattern of results.

Author response: In line with your relevant suggestion, we examine once again whether the results differ when analysing the difference score between the active- and finished-PM phases across conditions. The descriptive analysis was the following: No-PM, *M* = 207, *SD* = 126; No-Load, *M* = 139, *SD* = 100); Low-WM Load, *M* = 176, *SD* = 108; and, High-WM Load, *M* = 213, *SD* = 125. However, in Experiment 2, a one-way ANOVA did not show a significant effect of PM condition in RTs, *F* (3, 103) = 2.26, *p* = .09. Post-hoc Tukey tests did not reveal a speed-up in the control condition compared to the other experimental conditions, as well as between the experimental conditions.

PM conditions	***p-value***	**Lower and Upper Limits (95% CI)**
No-PM vs. No-load	.15	[-15.42, 151.65]
No-PM vs. Low-WM	.77	[-52.61, 114.46]
No-PM vs. High-WM	.99	[-89.46, 77.61]
No-Load vs. Low-WM	.65	[-120.72, 46.34]
No-Load vs. High-WM	.10	[-157.57, 9.50]
Low-WM vs. High-WM	.66	[-120.38, 46.69]

7. Another point that stroke me when I read the discussion was the comment on line 620 that the new intention did not encourage focal processing of the PM cues. Numbers occurring in an environment that consists otherwise only of letters are very salient cues and this make the focal/nonfocal distinction obsolete here. I also think that the following paragraph with the speculations on monitoring for new-pm cues is not justified by the results. In the subsequent paragraph (lines 633 ff) the Walser et al 2014 study is referred as if they would have found fewer commission errors with increased load (which is simply not correct).

Author response: We appreciate your comments**.** First, in the new-PM condition, we have interpreted LDT costs (relative to a control condition in which the ongoing task is performed alone) as evidence of monitoring for the occurrence of non-focal PM cues, which is cognitively demanding resulting in task interference (e.g., Einstein & McDaniel, 2005; Scullin et al., 2010; Walter & Meier, 2016). Here performing a LDT does not encourage participants to detect numbers in the stimulus, and thus extra processing such as target-checking and active maintenance of the new intention has been necessary. Although it has been suggested that highly salient non-focal targets are more likely to be detectable through spontaneous processes (McDaniel & Einstein, 2000), Hefer et al. (2017) showed that the focality of the PM cue plays a more crucial role in the flexibility of the monitoring process whereas the saliency of the PM cue does not. Interestingly, following your relevant comment, future studies may address this issue by presenting new-PM cues during the finished block to examine if salience serves as a type of “flag” that captures attention, eventually making strategic monitoring processes unnecessary in non-focal conditions.

Second, we also have revised the paragraph regarding Walser et al. study in line with your previous suggestion. We hope that the manuscript reads more fluidly and that all the changes have significantly improved it.

Minor

8. According to Figure 1 there are 240 LDT trials in the finished phase, according to line 256 there were 260. Please be consistent.

Author response: We appreciate your comment. We revise the text to clarify that the LDT contained 240 lexical decision trials as well as 10 trials with the former PM cues and 10 control trials presented in the salient background (as in the active-PM phase) (please see lines 227-228).

9. line 666 In contrast to the authors claim that this is one of the few studies adding a no PM condition, this is in the meantime "good practice" (see Anderson, Strube & McDaniel, 2020 for a review that includes dozens of such studies).

Author response: We appreciate your suggestion. Evidence supporting the distinction between monitoring and spontaneous retrieval has been found in a number of studies by adding a no-PM control group (as recently shown in the meta-analytic review made by Anderson et al., 2020). However, these studies are focused on exploring PM omission errors (i.e., forget to remember to perform a delayed intention at the appropriate moment in the future) rather than in PM commission errors. To the best of our knowledge, the current research is one of the few studies adding a no-PM condition to bring additional leverage on the PM retrieval process underlying PM commission failures (e.g., see Scullin & Bugg, 2013 for an exception).

Highlights

- Commission errors might occur if a prospective memory (PM) task is erroneously executed despite there is no need to do so.
- Commission error risk was reduced by the requirement to perform a new and dissimilar PM task.
- Fewer participants make commission errors after an old-PM task is declared finished when performing filler task activities with a moderate WM load than in a no-WM load condition.
- A retroactive interference mechanism seems to play a crucial role in PM deactivation.

Abstract

Recent prospective memory (PM) studies have shown that an intention may be erroneously executed despite no-longer-needed (i.e., commission errors), especially under demanding ongoing activities. In the current study, we examined whether PM deactivation benefits from a retroactive interference mechanism. For this, we set up a procedure in which participants are first asked to perform a PM task which is critically declared finished afterwards. Next, they encoded a new and dissimilar PM intention to accomplish later (Experiment 1) or performed filler tasks with increased working memory difficulty levels (Experiment 2). Lastly, all participants encountered several (but irrelevant) PM cues. Together, our findings provide evidence that the efficiency of the deactivation process can be modulated by encoding novel and dissimilar PM tasks and by the type of processing after intention completion. These findings are discussed in terms of strategic or spontaneous retrieval processes and linked to a retroactive interference mechanism which helps to overwrite or deteriorate the old-PM task representation.

Keywords: PM deactivation; commission errors; retroactive interference

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Exploring the role of interference on prospective memory deactivation

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Author Note

We have no conflict of interest to disclose. This study was supported by the Portuguese Foundation for Science and Technology (FCT) under grant number BD/123421/2016 to Patrícia Matos; and the Portuguese Ministry of Science, Technology and Higher Education through the State Budget UID/PSI/01662/2019.

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Conflict of Interest

We have no conflict of interest to disclose

1. Introduction

Anyone who has tried to remember to send an important report the next day has experienced what researchers refer to as prospective memory (PM; Einstein & McDaniel, 1990; Loftus, 1971; Rummel & McDaniel, 2019). Some intentions are frequently updated, and some of them become no longer needed. However, several studies have shown that those irrelevant prospective memories may not rapidly decay or be deactivated (i.e., actively suppressed, e.g., Anderson & Einstein, 2017; Bugg et al., 2016; Matos et al., 2020; Schaper & Grundgeiger, 2019; Walser et al., 2017). For instance, we should not send a report, after all, because we will have a face-to-face meeting by the end of the week. Yet, the environmental cue (e.g., see the computer) that may trigger retrieval of that previously PM intention (e.g., send the report) often reappear. Then, commission errors might occur if a PM task is erroneously executed despite there is no need to do so.

Prospective memory commission errors are usually investigated by asking participants to perform a PM task during an ongoing activity (e.g., press *Q* when an infrequent PM cue - the word *dancer* - is presented during a lexical decision task, LDT). Upon this active-PM phase, they are then told that the PM intention is finished and therefore no longer relevant (Bugg et al., 2016; Scullin et al., 2012). Moreover, PM tasks might be declared finished without being previously executed (termed *zero-target conditions* because participants never see PM cues while the intention is still active; Bugg et al., 2013, 2016). Critically, during the finished-PM phase that follows, unexpected (former) PM cues occur embedded in a new ongoing task (OT; Phase 2). Several studies have shown that both younger and older adults are slower in response to those (re)presented PM cues relative to control trials (i.e., called intention interference), which is inferred as a spontaneous, but erroneous, PM retrieval or even made commission errors (e.g., press *Q* in response to *dancer*; Anderson & Einstein, 2017; Bugg et al., 2016; Matos et al., 2020; Pink & Dodson, 2013; Schaper & Grundgeiger, 2019; Scullin et al. 2009; Walser et al., 2017; for a review see Möschl et al., 2020).

Different theoretical accounts have been proposed to explain the occurrence of such memory failures. First, we may hold an intention in mind and actively monitor the environment for a cue that signals the appropriate moment to fulfil the PM task (i.e., event-based PM tasks). This process requires available cognitive resources, and so it may incur costs to the performance of the other ongoing activities (e.g., Einstein et al., 2005; Smith, 2003). From this viewpoint, commission errors occur if those monitoring processes have not been discontinued upon intention completion (possibly due to a failure to deactivate or inhibit an irrelevant intention since there is no motive for participants to commit resources toward monitoring for PM cues; Scullin & Bugg, 2012).

Second, the dual-mechanisms account posits that commission errors result from a spontaneous PM retrieval and a subsequent failure to inhibit the execution of a prepotent motor response (Bugg et al., 2016; Scullin & Bugg, 2013). So far, the evidence strongly suggests that the PM cue occurrence within an OT context might trigger a more automatic retrieval without any decline in the OT, such as when the cue is salient (e.g., perceptually deviate from standard trials) or focal (i.e., the OT encourages processing of the attributes of the PM cue that was processed during initial encoding; Einstein et al., 2005; McDaniel & Einstein, 2000). The empirical support for this view stems from the finding that participants who held a PM task that becomes no longer needed have a similar OT performance compared to a control condition without any PM task to accomplish (Scullin & Bugg, 2013; see also Matos et al., 2020). That is, participants were not allocating cognitive resources to monitor for their old-PM intentions[1].

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[1] Hereafter, we use the term old-PM intention to refer to the PM task which was declared finished.

Relatedly, some studies recently showed that young adults are more vulnerable to execute a previous PM intention which becomes unnecessary under conditions of heavy cognitive load or distraction[2] (Boywitt et al., 2015, Experiment 1; Matos et al., 2020; Pink & Dodson, 2013). The idea is that working memory (WM) capacity also depends on an attentional control mechanism (executive attention) that allows us to critically inhibit contextual information irrelevant to the OT at hand (Engle et al., 1995; see also Cowan, 2005). Thus, it is arguable that if cognitive resources are divided between tasks and inhibitory mechanisms are being tapped out, it could be hard to activate the relevant information to perform the OT, eliminate the old-PM task representation, or even suppress the salient but irrelevant PM cue information (Hasher & Zacks, 1998). Simply put, the sparse resources leftover under such demanding environments might lead to a cognitive control failure and then impair the deactivation process to work sufficiently.

However, it would seem sub-optimal to continually inhibit internal PM representations in everyday situations. Moreover, in real life, we must constantly form, maintain, retrieve, and execute several intentions rather than single intentions in isolation regardless of whether other old intentions have been completed. On this promise, we can argue that a potential mechanism of retroactive interference (by which newly encoded memories help to overwrite or degrade an existing memory trace; Barnes & Underwood, 1959) has long been held to cause forgetting may apply to PM deactivation, too. More specifically, memories appeared to decay over a retention interval because they are interfered with by additional memories that the subjects have learned (Nairne 2002; Wixted 2010). Thus, it is reasonable that a new-PM task representation might help to deactivate older

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[2] Schaper and Grundgeiger (2017, Experiment 2) and Einstein et al. (1998) did not found increased aftereffects of PM intentions as a function of cognitive load. However, these studies used an activity-based PM task and a time-based task, respectively. In such cases, target cues do not appear during the OT.

prospective memories, that is, that commission errors may be reduced while we manage to respond to the changing demands of our environment.

Therefore, an important issue is to examine whether PM deactivation may be a function of newly PM tasks replacing or interfering with the memory representation of an old-PM intention. Only a few studies empirically tested this idea (Anderson & Einstein, 2017; Walser et al., 2012, 2017). On the one hand, using the commission error paradigm described above, Anderson and Einstein (2017) asked participants to encode a new-PM intention to perform later during the finished-PM phase (i.e., when unexpected irrelevant cues associated with an old-PM intention still occur as OT stimuli). Yet, the authors did not find that such a strategy significantly reduced PM deactivation failures. On the other hand, Walser et al. (2017) observed that encoding a new intention in which no components of the old-PM task representation are needed to perform the new one helped reduce intention interference (i.e., the slowing on previously relevant PM cues). Specifically, in their procedure (termed repeated-cycles paradigm), new-PM tasks were encoded over several blocks (i.e., respond to specific words instead of symbols as in an old-PM condition) after former intentions are declared finished (Walser et al., 2012, 2017). In such cases, participants must regularly update their representations of which intention is currently relevant since they shift from one intention to the next throughout several blocks. It is also worthy to note that PM commission errors are scarce in this paradigm as well as slower responses to previously relevant PM cues seems to disappear by encoding novel memory representations in the interval between the instruction that a former PM task is finished and the later appearance of irrelevant PM cues (Walser et al., 2014).

The present study aimed to extend previous work by examining two questions: Do individuals show few intention deactivation failures if engaging novel intentions to fulfil in the future? Does performing cognitively demanding tasks after an intention becomes no longer relevant helps to override the old-PM task set and support PM deactivation? This could be how we update our PM demands, such that moving to address new and dissimilar contents deactivates the old-PM intention, reducing commission error risk.

Here, we have focused on manipulations that may decrease commission errors when there is a single active-PM phase, and the PM task is declared finished afterwards by telling participants that they should no longer respond to PM cues. A novel aspect of our research is that it explores this issue in contexts in which participants never fulfil the intention due to the absence of PM cues while it was still active (i.e., zero-target conditions). These unfilled intentions might be harder to forget due to the lack of episodic traces (of prior responding) or heightened activation (Bugg et al., 2016). Yet, it is arguable that prospective remembering might benefit from an interference mechanism that helps to deactivate such unperformed but irrelevant intentions. Moreover, in convergence with the prominent dual-mechanisms account, we also added a no-PM control condition to examine whether PM retrieval and commission errors result from an automatic rather than a controlled process. Finally, to the best of our knowledge, there is no evidence concerning which factors prevent PM commission errors under cognitively demanding environments. For instance, consider the earlier example of sending a report. We might have to do so during a day in which one must pack, and it is also the deadline for primary school enrolment. For that reason, we added a secondary OT (i.e., a counting recall task) to increase the overall demands. That is, the total amount of WM (i.e., to process and retain information temporarily) and attentional control resources deployed by the cognitive system increase to meet task demands (Conway et al., 2005).

1.1 Experiment 1

The role of retroactive interference on PM deactivation remains unclear. Besides, the procedure used in the studies on this topic does not capture many real-world situations in which PM tasks are updated and new dissimilar intentions must be carried out under loaded conditions. Thus, in Experiment 1, we manipulated whether a new and distinct PM intention must be fulfilled after the old-PM intention is declared finished using a finished-PM paradigm. To explore this possibility, we adapted the procedure proposed by Scullin and Bugg (2013). As noted, participants encoded a PM task, namely, pressing *Q* if the target cue *high* or *title*[3] in a red background appears while performing an ongoing LDT (i.e., active-PM phase). Later, participants are told that the PM intention is finished and, thus, they should no longer respond to cues. Critically, they were asked to perform a new-PM task subsequently during the same ongoing LDT. Participants make a commission error by pressing the *Q* key when cues associated with the old-PM task are presented during the finished-PM phase.

The main goal of Experiment 1 was to understand if introducing a new-PM task under a demanding OT processing reduces the level of activation associated with the old-PM representation to diminish its accessibility. Thus, reducing the number of participants who make commission errors. We expected that the *new-PM task condition* should result in fewer commission errors compared with the *no new-PM condition*, in line with an earlier work indicating few PM aftereffects when the category of both intentions differed (Walser et al., 2017). Furthermore, we explored the type of PM retrieval process that is taking place. Thus, as a third critical condition, we included a *no-PM condition* without any PM task. Examining the effect of having to perform a PM intention on the OT processing provides additional leverage for informing the theoretical views of PM retrieval stated above. According to previous work (Matos et al., 2020; Schaper & Grundgeiger, 2019; Scullin & Bugg, 2013), we reasoned that commission errors might result from a spontaneous retrieval process, and so there should be no differences in the OT performance between the no-PM and each of no new-PM and new-PM conditions. If, on the contrary, participants are devoting cognitive

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[3] From Portuguese, *alto* or *título.*

resources to monitor for PM cues, it should be expected a worse OT performance in the two experimental conditions compared to the no-PM group.

1.1.1 Method

1.1.1.1 Participants

An a priori power analysis (based on $p_{1\ (\text{No-load})} = .40$ and $p_{2\ (\text{Moderate-load})} = .74$ and sample size, $N = 70$ of our previous work, Matos et al., 2020) indicated that a sample of 3 × 42 participants was needed (two-tailed, $\alpha = .05$, power = .90; conducted for a Chi-Square test of independence using PS-Power and Sample Size Calculation, Dupont & Plummer, 1990). Thus, 137 students of the University of Minho participated in an exchange of course credits. All participants had normal or corrected to normal vision, reported no psychiatric history and were Portuguese native speakers. Fourteen (10%) participants were excluded from the analyses ($N_{\text{No new-PM}} = 8$; $N_{\textit{New}\text{-PM}} = 6$), either because they could not correctly recall the PM task or the finished-PM instruction at the end of the experiment ($n = 8$) or due to depression and anxiety symptoms ($n = 6$; see Bowman et al., 2019). The 123 participants (14 male, $M_{\text{age}} = 21.50$, $SD = 4.23$) were randomly assigned to the no-PM ($n = 39$), no new-PM ($n = 42$), and new-PM ($n = 42$) conditions. The local ethical committee for Research in Social and Human Sciences approved this study (SECSH 016/2018).

1.1.1.2 Materials

Sixty-eight words were extracted from the Minho Word Pool (Soares et al., 2017, 2019). For the LDT, 36 words ranged between five to eight letters long, word frequency higher than 75 occurrences per million, and response times between 550-750 ms. The pseudo-words (i.e., letter strings that, although they do not have any meaning, are combined according to the linguistic rules of a given language) were created by changing one or two syllables of 32 new words 5-8 length. Further, two out of four words between four to six letters (i.e., *phase/wait*; *high*/*title*[4]) served as PM targets (i.e., signalled the appropriate moment to execute the PM task) or, in counterbalance, control words (i.e., matched PM cues in frequency and length). Forty words and pseudo-words (20 each) were selected for Phase 1, and every item was presented twice. Forty-eight words and pseudo-words were chosen for Phase 2 (24 each), in which half of the words were repeated from Phase 1, and a half were new. Every item was presented five times to match the frequency of target/control words.

During the first delay interval, depressive and anxiety symptoms were evaluated with the Beck Depression Inventory (BDI; Beck et al., 1961; Portuguese version Vaz-Serra & Pio-Abreu, 1973) and the State-Trait Anxiety Disorder (STAI; Spielberger et a., 1983; Portuguese version Silva, 2003), respectively. The BDI is a 21-item self-report rating inventory that measures attitudes and symptoms of depression; and the STAI-State Scale is a 20-item self-report rating inventory measuring symptoms of state-anxiety. Finally, the Vocabulary Test (Wechsler, 2008), a verbal comprehension task in which participants must define the words presented, was performed during the second delay interval.

1.1.1.3 Procedure

The procedure had four main sections: (1) Instructions, (2) active-PM phase, (3) finished-PM phase, and (4) debriefing. First, participants in all conditions were informed about the OT, namely, a LDT in which they had to quickly and accurately make word/non-word judgments by pressing keyboard keys “5” and “6”, respectively (see Figure 1). All words and pseudo-words were presented in white, Arial, 24-point font on a black background. Participants were instructed to use their index fingers and to keep them on the keys throughout the experiment. Each lexical decision trial started with a fixation cross presented for 300 ms followed by the stimulus, which was presented until the participant responded by pressing the 5, 6, *Q* key, or after 2500 ms.

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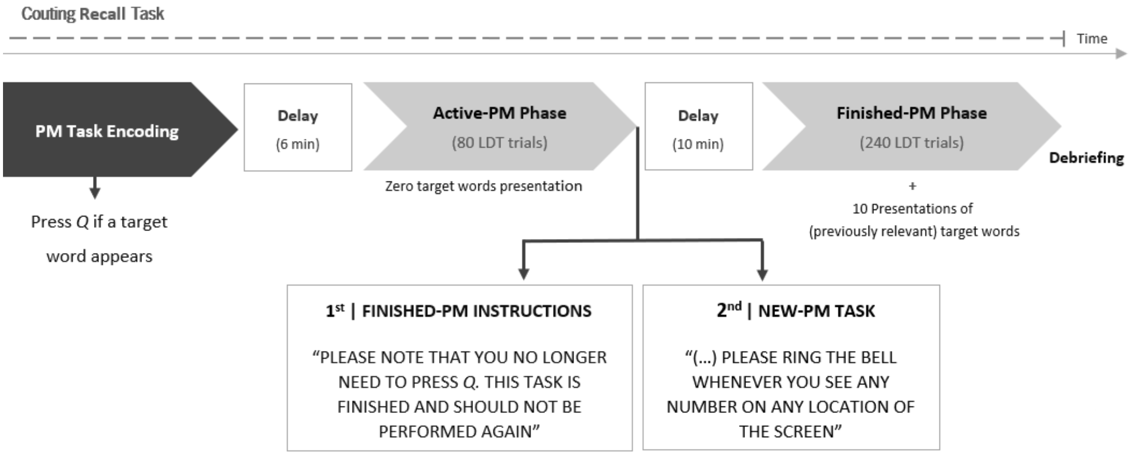
[4] From Portuguese, *fase*/*espera* and *alto*/*título*.

Figure 1

Illustration of the Commission Error Paradigm. Note. Adapted from Bugg & Scullin (2013). LDT = lexical decision task; PM = prospective memory.



After 10 practice trials, participants were asked to count the number of yellow screens to recall it at the end of the session to approximate demanding daily settings. More specifically, these trials were pseudorandomised (i.e., 1/20 lexical decision trials; 4 and 12 yellow screens during the active-PM and finished-PM phases, respectively) to ensure the same level of cognitive load throughout the experiment. Then, except in the no-PM condition, participants received the instruction for the PM task, i.e., press the *Q* key immediately if they saw one of two target words in a red (or, in the counterbalance, blue) background[5]. As in Bugg and Scullin (2013), another pair of words (i.e., the two words not used as targets) were used as controls and appeared against the background colour not used for target cues. The word pairs were counterbalanced (Pair 1: *phase* or *wait*; Pair 2: *high* or *title*). PM instruction encoding was confirmed or corrected by having participants write down

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[5] Previous work has shown that participants are more vulnerable to make a commission error with salient cues, the contextual overlap between the active- and the finished-PM phase, and with finished (zero-target) PM instructions (e.g., Schaper & Grundgeiger, 2017; Scullin et al., 2012). Thus, we followed these laboratory parameters to avoid floor effects.

the PM intention and then repeat them to the experimenter in their own words. To provide a short delay between the encoding and test phases (Einstein & McDaniel, 1990), participants completed the BDI and the STAI-State Scale for approximately 6 min.

In the active-PM phase that follows, they perform 80 lexical decision trials without PM cues or control trials, so they did not have the opportunity to perform the PM intention. Then, the PM task was declared finished by telling participants that they no longer needed to press the *Q* key. That task was declared finished and should not be performed. In the new-PM task condition, participants were immediately asked to press a bell (placed next to the keyboard) whenever they saw numbers in any screen location, either in the presented words/non-words or in the background screen. Note that, in the new-PM condition, the numbers were never presented, so there was no opportunity to perform the new-PM task. We used a new-PM task that presumed to place greater demands on attention and planning (Bugg & Ball, 2017; Meier & Zimmermann, 2015). To ensure that the new-PM task had the same encoding as the old-intention, participants were again asked to reproduce the instructions in writing and orally.

After a 10 min delay during which both groups performed a vocabulary test (Wechsler, 2008) and a 24 LDT as filler tasks, the finished-PM phase begun. They were further instructed that their sole aim was to respond as quickly as possible to a LDT containing 240 lexical decision trials, 10 trials with the former PM cues. and 10 control trials presented in a salient background (as in the active-PM phase). A commission error occurs when participants perform the PM task (i.e., pressed *Q*) despite being instructed that the PM task was finished.

Finally, participants were asked to describe all the instructions received during the experiment. If participants did not do it spontaneously, we asked them to (1) recall the target words and target key; (2) if they received the instruction that the PM task was finished and, if so, when did that happen; and (3) whether they ever press *Q* after they were instructed not to, and if so, to describe why. The entire experiment was implemented individually and lasted approximately 45 minutes.

1.1.1.4 Statistical Analyses

The JASP software package (JASP Team, 2018, Version 9.0.1) was used for standard NHST, using an alpha level of .05. A Bayesian analysis was also implemented to support these results to provide evidence supporting either the null or the alternative hypothesis (Wagenmakers et al., 2018). In short, the Bayes Factor (*BF*) allows updating the beliefs about the data with evidence collected after the analysis. As stated, for instance, if the null hypothesis is that $M1 = M2$, and the alternative hypothesis is that $M1 \neq M2$, a $BF = 3$ shows moderate evidence in favour of $H1$. Simply put, we had a prior belief that $M1 = M2$ ($H0$). However, after observing the data, we have to update that belief because it is three times more likely that $M1 \neq M2$ than $M1 = M2$. Here we will follow the recommendation of the JASP Team (2016): A *BF* of 1 shows no evidence in support of either hypothesis. Evidence accumulated in favour of $H1$ when *BF* increases and in favour of $H0$ when it decreases. A *BF* from 1 to 3 is interpreted as anecdotal evidence favouring $H1$, from 3 to 10 is moderate evidence, from 10 to 30 is strong, and more than 30 shows extreme evidence supporting $H1$. A *BF* from 0.33 to 1 indicates anecdotal evidence in support of H0, from 0.10 to 0.33 is moderate evidence, from 0.03 to 0.10 is strong evidence, and lower than 0.03 is considered extreme evidence in support of $H0$. Results concerning PM commission errors are presented first, followed by LDT performance and then by the counting recall task performance.

1.1.2 Results

1.1.2.1 PM commission errors

A PM commission error was defined as at least one *Q* press in the trial with the PM cue during the finished-PM phase. The no-PM condition was excluded from the analyses because participants did not have any PM task to accomplish. There was a higher percentage of participants making a PM commission error in the no new-PM (30/42; 71.43 %) than in the new-PM task condition (14/42; 33.33 %), $\chi^2 = 12.22$, $p < .001$, $\phi = -.38$[6] (see Figure 2). To further explore the effect of interference by a new-PM task, the *BF* was calculated and examined using the dichotomic variable of whether participants made a commission error. There was extreme evidence for H1 ($BF_{10} = 120.44$), that is, a different proportion of participants making commission errors in the no new-PM relative to the new-PM task condition (see Figure 3). Taken together, results showed that fewer participants made a commission error in the new-PM task condition, and Bayesian analyses provided support for that finding.

We also analysed whether participants were slower in response to ongoing task trials containing PM cues compared to control trials (i.e., trials matching - in this case, the frequency and length of the - PM cues but that never serve as retrieval cues; Pink & Dodson, 2013; Scullin & Bugg, 2013; Scullin et al., 2012; Walser et al., 2012, 2014). <u>RTs analyses were conducted on correct trials, faster than 300 ms, and were trimmed at 3 standard deviations from each participant´s mean</u>. A 2 (Trial type: target and control) × 2 (PM condition: no new-PM and new-PM) mixed-factorial ANOVA was conducted on RTs. There was not a main effect of trial type, $F(1, 52) = .01$, $p = .95$, $\eta 2 = .00$, of PM condition $F(1, 52) = .03$, $p = .24$, $\eta 2 = .03$, nor an interaction between trial type and PM condition, $F(1, 52) = 1.21$, $p = .28$, $\eta 2 = .02$.

Next, we also analysed the frequency of commission errors made per participant (i.e., the total number of *Q*-presses/10 targets). An independent Student´s sample *t*-test indicated

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[6] Participants were only included if, at the end of the procedure, they recall the target words and target key, as well as the instruction that the PM task was finished (either spontaneously or if they recall the episodic event after a prompt). Importantly, participants were not significantly more likely to make a commission error if they recall the finished-PM instructions spontaneously ($n = 57$) or with a prompt ($n = 27$), $\chi^2 = 3.25$, $p = .071$, $\phi = .20$. Moreover, when excluding those participants (n = 27), we still observe significantly more commission errors under cognitive load, $\chi^2 = 7.41$, $p = .006$, $\phi = -.36$.

that the frequency of commission errors per participant was significantly higher in the no new-PM (*M* = .59, *SD* = .44) than in the new-PM task condition (*M* = .26, *SD* = .41), *t*(82) = 3.56, *p* < .001, Cohen´s *d* = .77, 95% CI [.15, .52]. Bayesian t-tests support the previous finding revealing extreme evidence in favour of H1, BF_{10} = 39.88 (i.e., a different frequency of commission errors committed by participants in the no new-PM than in the new-PM task condition).

1.1.2.2 Lexical decision task

Another interest was comparing OT performance across conditions in the active- and finished-PM phases. As a reminder, the idea was that if participants were spontaneously retrieving the PM intention, there should be no differences in the LDT between the no-PM control condition and each of the experimental conditions. For LDT accuracy and RTs analyses, the target and control trials, the trials immediately following each target cue were excluded as responding to PM targets may slow subsequent OT performance and must be considered as an additional source of costs (Meier & Rey-Mermet, 2018; Smith & Hunt, 2014). Likewise, the trials immediately following each coloured screen were excluded. Accuracy and RTs analyses were conducted on correct trials, faster than 300 ms, and were trimmed at 3 standard deviations from each participant´s mean (Ratcliff, 1993) calculated separately for each active-PM and finished-PM phases (Smith, 2010).

Figure 2

Percentage of Participants who Made at Least one PM Commission Error Across Conditions. Note. **p* < .05; ** *p* < .001.

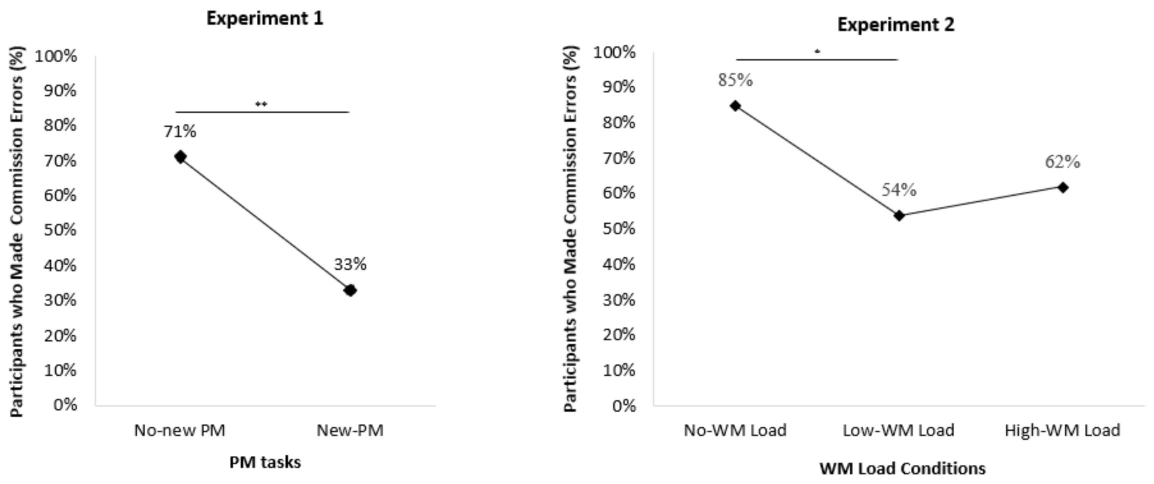
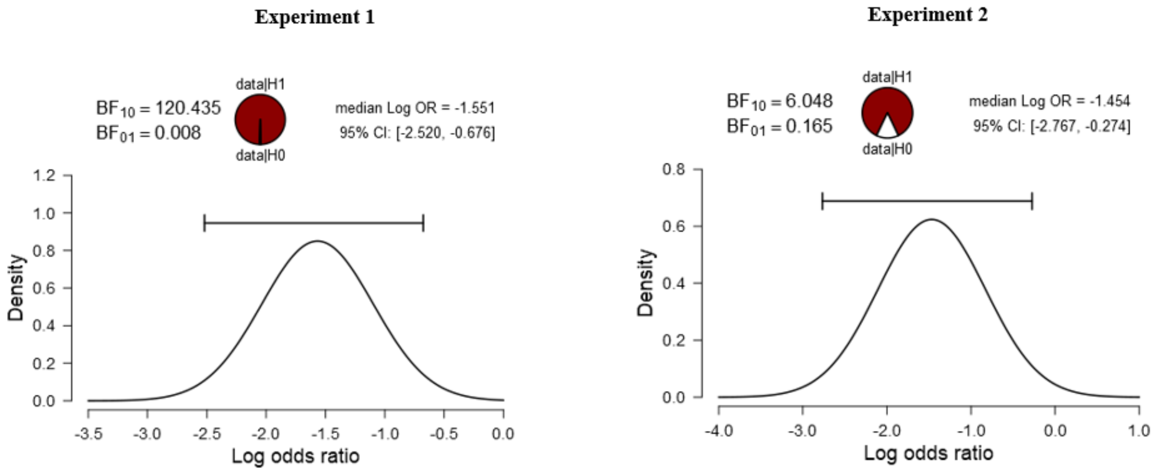


Figure 3.

Posterior Distribution for the Chi-Square Test for the Proportion of Participants who Made Commission Errors Across Conditions. Note. The default two-sided Bayes factor in Experiment 1 (left side) is visualised by the ratio between the prior and posterior ordinate at *ρ* = 0 and equals 120.44 in favour of the alternative hypothesis over the null hypothesis. The default two-sided Bayes factor in Experiment 2 (right side) is visualised by the ratio between the prior and posterior ordinate at *ρ* = 0 and equals 6.05, favouring the alternative hypothesis over the null hypothesis. Figures from JASP.



Results are summarised in Table 1. Mean accuracy and RTs were submitted to a 2 (PM-phase: active and finished) × 3 (PM condition: no-PM, no new-PM, and new-PM) separate mixed-factorial analyses of variance (ANOVA). For OT accuracy, participants were less accurate in the active-PM phase (*M* = .93, *SD* = .07) compared with the finished-PM phase (*M* = .95, *SD* = .06), *F*(1, 120) = 15.63, $p < .001$, $\eta^2 = .12$. There was no main effect of PM condition, *F*(1, 120) = 2.64, *p* = .08, $\eta^2 = .04$, but there was a significant interaction between PM-phase and PM condition, *F*(1, 120) = 8.11, $p < .001$, $\eta^2 = .12$. Pairwise comparisons showed that the interaction arises from the observation that participants in the new-PM task condition were less accurate (*M* = .91, *SD* = .04) than those in the no-PM task condition (*M* = .95, *SD* = .05) during the active-PM phase, *p* = .010, IC 95% [.01, .09]. There were no other significant effects, all *ps* ≥ .37. There were also no significant differences in their LDT accuracy in the finished-PM phase across conditions, all *ps* ≥ .08.

Regarding OT RTs, participants reacted more slowly in the active-PM (*M* = 873, *SD* = 164) compared to the finished-PM phase (*M* = 708, *SD* = 102), *F*(1, 120) = 309.75, $p < .001$, $\eta^2 = .72$. There was no significant main effect between PM conditions, *F*(1, 120) = 1.20, *p* = .31, $\eta^2 = .02$, but the interaction between PM-phase and PM condition was significant, *F*(1, 120) = 7.55, *p* = 001, $\eta^2 = .11$. Pairwise comparisons showed no significant differences in their RTs in the active-PM phase across conditions, all *ps* ≥ .25, while in the finished-PM phase participants in the new-PM task condition were slower (*M* = 740, *SD* = 119) compared to those in the no-PM condition (*M* = 683, *SD* = .97), *p* = .034, IC 95% [3.20, 110.81]. There were no other significant effects, all *ps* ≥ .18.

Table 1.

Experiment 1 Means (M) and Standard Deviations (SD) of Lexical Decision Task Performance (Accuracy and RTs)

	No-PM		**No new-PM**		**New-PM**	
	Accuracy *M (SD)*	RTs (ms) *M (SD)*	Accuracy *M (SD)*	RTs (ms) *M (SD)*	Accuracy *M (SD)*	RTs (ms) *M (SD)*
PM-phase						
Active-PM	.95 (.05)	901 (188)	.93 (.10)	838 (139)	.91 (.04)	881 (162)
Finished-PM	.96 (.03)	683 (97)	.93 (.09)	699 (80)	.96 (.04)	740 (119)

1.1.2.3 Counting recall task

Counting recall accuracy was computed as the proportion of correct responses (in a total of 16) per participant. Importantly, we did not found significant differences between the no new-PM (*M* = .91, *SD* =.16) and the new-PM task conditions (*M* = .92, *SD* =.12), *t*(82) = -.04, *p* = .97, *d* = .07, IC 95% [-.06, .06]. Bayesian *t*-tests revealed moderate evidence in favour of the null hypothesis, BF_{10} = .22, that is, a similar counting recall accuracy across conditions. We also examined whether there were no differences in the lexical decision trials immediately following the counting recall task. A 2 (PM-phase: active and finished) × 2 (PM condition: no new-PM and new-PM) mixed-factorial ANOVA was conducted for OT RTs[7]. There was a main effect of PM-phase, indicating that participants were slower in the active-PM (*M* = 1018, *SD* = 271) compared to the finished-PM phase (*M* = 785, *SD* = 166), *F*(1, 82) = 2.29, *p* = .13, η^2 = .03. There was not a main effect of PM condition, *F*(1, 82) = 2.29, *p* = .13, η^2 = .03., nor an interaction between PM-phase and PM condition, *F*(1, 82) = .57, *p* = .45, η^2 = .01. These results demonstrate that the effect on PM commission errors is due to the new-PM task set and not due to a differential attention allocation strategy.

1.1.3 Discussion

The main goal of Experiment 1 was to assess whether a reduction of PM commission errors is evidenced when new intentions must be accomplished. This question was addressed by means of a new and dissimilar PM task to perform during the finished-PM phase. The key finding was that fewer participants made commission errors in the new-PM task (33%) compared to those in the no new-PM task condition (71%). According to our first hypothesis, this result provided initial evidence that encoding a novel and dissimilar intention might overwrite or degrade the old-PM representation (Barnes & Underwood, 1959; Wixted, 2010). Additionally, we observed a similar counting recall task accuracy between participants in the

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[7] We elected not to trim responses to avoid the problem of having a low number of observations.

no new-PM and new-PM task conditions. Thus, it is reasonable to propose that this result strengthens the evidence that the lower number of commission errors was due to the new-PM task set and not driven by a general differential attention allocation strategy.

Moreover, based on previous work (Bugg et al., 2016; Scullin & Bugg, 2013, PM commission errors occur due to a combination of spontaneous retrieval of a previously relevant intention and a subsequent failure to exert cognitive control over performing it. For that reason, we reasoned that there should be no differences in the OT performance between the no-PM condition and each of the experimental conditions. Interestingly, we found that OT performance (both accuracy and RTs) did not differ between the no-PM and the no new-PM condition. As previously hypothesised, this finding indicates that it is likely that in the no new-PM task condition, participants spontaneously retrieved the old-PM task although it was no longer necessary (Bugg et al., 2016; Scullin & Bugg, 2013; Scullin et al., 2012). On the contrary, the new-PM group performed the OT in the finished phase slower than the no new-PM group showing potential monitoring costs or response delays (Smith. 2003; Strickland et al., 2018). It is arguable to consider that participants in the new-PM task condition may have monitored heavily for the new-PM task or strategically delayed their ongoing-task responding (Schaper & Grundgeiger, 2019; Smith, 2003).

In sum, findings from Experiment 1 bring additional evidence that, while performing demanding ongoing activities, an old-PM intention might be spontaneously retrieved and, most importantly, the memory trace of an old and irrelevant PM task might be degraded by a new and dissimilar PM intention. Consequently, it reduced the probability of making PM commission errors.

1.2 Experiment 2

In Experiment 2, we further explored the role of retroactive interference on PM deactivation, reasoning that the old-PM task memory should also be disrupted or interfered with by new information subsequently encoded in WM. The limited amount of information suggests that it may help deactivate an old memory task representation (Walser et al., 2014). For instance, Walser et al. (2014) investigated the effect of intervening activities showing that performing a high WM demanding task (i.e., read letter strings aloud in backward order) after the active-PM phase reduced intention interference compared to a control condition (i.e., in which they had to read letter strings aloud). Their finding supports the role of an overwriting-like mechanism that might facilitate PM deactivation.

Although it seems possible to reduce PM commission errors by encoding novel memory representations before the appearance of irrelevant cues during a finished-PM phase, the mechanisms underlying this effect are unclear. Thus, we thought it was valuable to investigate further the beneficial effect of retroactive interference in prospective remembering (Dewar et al., 2007; Wixted, 2004) by manipulating the filler task difficulty. For this purpose, three conditions were implemented in a between-subjects design: a *no-WM load*, a *low-WM load,* and a *high-WM load* condition. As in Experiment 1, participants performed a LDT and were then informed that they should no longer perform the PM task. However, we crucially manipulated the task demands during the following delay interval. Specifically, participants performed a verbal comprehension task in the no-WM load condition requiring semantic knowledge and retrieval of information from long-term memory.

Conversely, in the *low-WM* and *high-WM load conditions*, they were asked to perform an *n*-back task with two increasing difficulty levels (1- and 3-back, respectively). Previous work has shown that increasing *n*-back load should limit WM capacity since it required a higher ability to maintain, continuously update and process information (Braver et al., 1997; Lewis-Peacock et al., 2016). Finally, in the finished PM phase, they performed a new LDT with 10 former PM cues (except for the no-PM condition in which they did not have any PM task to accomplish). If this idea has merit, we would expect fewer commission errors due to the increased demands of the filler activities that are expected to interfere with the old-PM task representation retroactively.

Moreover, we included a condition without any PM task to examine OT performance as additional research is needed to support the that a spontaneous PM retrieval contributes to the occurrence of PM commission errors (e.g., Scullin & Bugg, 2013; Scullin et al., 2012). Considering the dual-mechanisms account, we reasoned to find no difference in the LDT performance regardless of PM condition (no-PM, no-WM load, low-WM load, high-WM load) assuming a spontaneous PM retrieval, replicating results from Experiment 1.

1.2.1 Method

The method for Experiment 2 followed the method of Experiment 1. Hence, only deviations are described below.

1.2.1.1 Participants

An a priori power analysis (based on the proportions of Experiment 1, $p_{1\ (\text{New-PM})} = .33$ and $p_{2\ (\text{No new-PM})} = .74$ and sample size, $N = 84$) a sample of 4 × 26 participants was recruited (two-tailed, $\alpha = .05$, power = .80; Dupont & Plummer, 1990). Thus, 131 students of the University of Minho participated in the current study in exchange of course credits. Twenty-seven participants (20%) participants were excluded from the analyses ($N_{\text{No-WM load}} = 4$; $N_{\text{Low-WM load}} = 9$; $N_{High\text{-}WM\ load} = 14$) because they could not correctly recall the PM task or the finished-PM instructions at the end of the experiment ($n = 22$), or due to depression and anxiety symptoms ($n = 5$). Therefore, 104 young adults (15 male, $M_{\text{age}} = 21.22$, $SD = 3.86$) were randomly assigned to no-PM ($n = 26$), no-WM load ($n = 26$), low-WM load ($n = 26$), and high-WM load ($n = 26$) conditions.

1.2.1.2 Materials

The materials were the same as in Experiment 1, except for the *n*-back task, which was programmed in E-Prime (software package, version 3.0, Schneider et al., 2002). The *n*-back task was a WM test in which participants were asked to compare the current stimulus to the one presented *n* steps earlier in a continuous sequence (Kirchner, 1958). The items to be updated were the following 15 letters: A, B, C, D, H, I, K, L, M, O, P, R, S, T. Stimuli were presented one by one in the centre of the screen (font: Arial bold, size: 30). Participants had to press the spacebar when the currently presented letter (i.e., target) matched the letter presented one step before (low-WM load) or three steps back (high-WM load). The first three trials of each block were always non-targets. Each stimulus appeared on the screen for 500 ms, separated by a 1500 ms intertrial interval (regardless of whether the participant pressed a key or not), during which participants must press the target response key.

After a first practice phase consisting of 32 trials, an additional practice block was administered if participants did not have any doubt. Next, there were three test blocks of 60 letters each (totalling 180 trials) separated by two breaks of 1 min each to prevent fatigue. In each block, 25% of all the stimuli presented were hit items (i.e., 8 in the practice phase and 15 per block in the test phase). The number of hits and false alarms was recorded.

1.2.1.3 Design

The design was a 2 × 4 mixed-factorial, with PM-phase (active and finished) as the within-subject variable and PM condition (no-PM, no-WM load, low-WM load, and high-WM load) as the between-subjects variable. The dependent variables were the same as Experiment 1 except for the additional *n*-back task accuracy using d-prime (*d´*).

1.2.1.4 Procedure

The procedure was identical to Experiment 1 with the following exceptions. In Experiment 2, all participants also performed filler tasks in the second delay interval for approximately 10 min. Participants in the no-WM load condition were asked to provide a definition to the presented words of a vocabulary test. In the low-WM load condition, they were asked to judge whether a letter is a repetition from the previous step (e.g., L P P), while in the high-WM load condition they were told to judge whether a letter was repeated three steps back in the list (e.g., S D E S).

1.2.2 Results

1.2.2.1 PM commission errors

Prospective memory commission errors were significantly higher in the no-WM load (22/26; 85 %) compared to the low-WM load condition (14/26; 54 %), $\chi^2 = 5.78$, $p = .016$, $\phi = -.33$[8] (Figure 2). Bayesian contingency analysis supports the previous results revealing strong evidence favouring the alternative hypothesis, $BF_{10} = 6.05$ (Figure 3). Moreover, commission errors were marginally higher in the no-WM load in comparison to the high-WM load condition (16/26; 62 %), $\chi^2 = 3.52$, $p = .061$, $\phi = -.26$. In turn, Bayesian analyses were conducted showing anecdotal evidence favouring $H1$ ($BF_{10} = 2.02$), suggesting that the number of participants making commission errors differs between the no-WM load and the high-WM load condition. Lastly, the low-WM and high-load conditions did not differ, $\chi^2 = .32$, $p = .58$, $\phi = .08$, as also indicated by the he $BF_{10} = 0.39$ showing moderate evidence in favour $H0$.

Interestingly, a one-way ANOVA showed a significant effect of PM condition in RTs to target trials, F(2, 97) = 5.55, p < .005, η2 = .10. Post-hoc Tukey tests showed that that participants responded significantly more slowly to PM (irrelevant) cues in the no-load (M = 1142, SD = 350) than in the low-WM load condition (M = 957, SD = 230), p = .024, Cohen´s d = .65, 95% CI [20.26, 350.54]; as well as compared to the high-load condition (M = 926, SD = 231), p = .005, Cohen´s d = .76, 95% CI [55.15, 376.70]. Response times did not differ

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[8] In this experiment, participants were significantly more likely to make a commission error if they recall the finished-PM instruction with a prompt (n = 25) than those who did that spontaneously (n = 5), $\chi^2 = 19.10$, $p < .001$, $\phi = .60$.

between the low-WM load condition (M = 957, SD = 230) and the high-load condition (M = 926, SD = 231), p = .87, Cohen´s d = .13, 95% CI [-115.43, 176.49]. This finding seems to strengthen the idea this manipulation seems to overwrite, deteriorate or even restrain the old-PM trace.

We further analysed the frequency of commission errors made per participant (i.e., the total number of *Q* presses/10 targets). A one-way ANOVA showed a marginal statistical difference in the frequency of commission errors between the no-WM load (*M* = .72, *SD* = .47), low-WM load (*M* = .50, *SD* = .47), and high-WM load conditions (*M* = .47, *SD* = .49), *F*(1, 77) = 2.41, *p* = .09, η_p^2 = .47.

1.2.2.2 Lexical decision task

Trimming procedures for accuracy and RTs analyses were identical to those of Experiment 1. Mean accuracy and RTs were submitted to a 2 (PM-Phase: active and finished) × 4 (PM condition: no-PM, no-WM load, low-WM load, and high-WM load) separate mixed-factorial ANOVAs. As illustrated in Table 2, the main effect of PM-phase for OT accuracy was not significant, *F*(1, 100) = 1.07, *p* = .30, η^2 = .01. The main effect of PM condition was also not significant, *F*(1, 100) = 1.32, *p* = .27, η^2 = .04, neither the interaction between PM-phase and PM condition *F*(1, 100) = .20, *p* = .90, η^2 = .01. For OT RTs, there was a main effect of PM-phase with participants being slower in the active-PM phase (*M* = 843, *SD* = 155) compared to the finished-PM phase (*M* = 659, *SD* = 77), *F*(1, 100) = 264.05, *p* < .001, η^2 = .73. There was not a main effect of PM condition, *F*(1, 100) = .94, *p* = .42, η^2 = .03, and the interaction between PM-phase and PM condition was only marginally significant, *F*(1, 100) = 2.26, *p* = .08, η^2 = .06.

Table 2.

Experiment 2 Means (M) and Standard Deviations (SD) of Lexical Decision Task Performance (Accuracy and RTs)

	No-PM		**No-WM load**		**Low-WM load**		**High-WM load**	
	Accuracy *M (SD)*	RTs (ms) *M (SD)*	Accuracy *M (SD)*	RTs (ms) *M (SD)*	Accuracy *M (SD)*	RTs (ms) *M (SD)*	Accuracy *M (SD)*	RTs (ms) *M (SD)*
PM-phase								
Active-PM	.96 (.03)	881 (172)	.95 (.09)	823 (145)	.96 (.04)	830 (144)	.95 (.06)	836 (159)
Finished-PM	.97 (.03)	674 (73)	.95 (.08)	684 (86)	.95 (.03)	654 (75)	.94 (.04)	623 (60)

1.2.2.3 Counting recall task

A one-way ANOVA showed that counting recall accuracy did not differ across conditions, F(1, 78) = 3.02, p = .06, η2 = .07 (no-WM load: M = .82, SD =.06; low-WM load: M = .93, SD =.09; high-WM load: M = .94, SD =.08. The BF10 = 1.01 value from the Bayesian ANOVA showed no evidence in support of either hypothesis.

1.2.2.4 n-back task

We next analysed the sensitivity of the participants to discriminate items as previously presented (or not) *n* steps back using the signal-detection parameter d-prime (*d´*), which was estimated as $d' = Z_{\text{Hits}} - Z_{\text{FalseAlarms.}}$ MacMillan and Creelman (2005) used the method to avoid that *d´* might be undetermined when the hit or the false-alarm rate was equal to 0 or 1. Specifically, scores equal to 0 were replaced by (false-alarms + 0.5) / (maximum number of false alarms +1) and scores equal to 1 were replaced by (hits + 0.5) / (maximum number of hits +1). An independent Student´s sample *t*-test revealed a higher *d´* in the low-WM load (*M* = 4.41, *SD* = .74) compared to the high-WM load condition (*M* = 2.30, *SD* = .95), *t*(50) = 8.85, *p* < .001, Cohen´s *d = 2*.48, 95% CI [1.63, 2.59]. A Bayesian t-test indicated moderate evidence for the H1 that *n*-back task performance differed between the low-WM and the high-WM load, $BF_{10} = 6.75$. This result gives us confidence that filler task manipulation was effective at inducing different levels of WM demands.

1.2.4 Discussion

The main purpose of Experiment 2 was to examine to what extent the demands imposed by the activities performed right after the finished-PM instruction might reduce intention deactivation failures. Following previous studies (Walser et al., 2014), our results indicated that successfully deactivating an intention seems to depend on the cognitive demands incurred before the finished-PM phase begins. This interpretation is supported by the evidence of a lower commission error risk in the low-WM load condition (54%) compared to the no-WM load (85%). Moreover, we found a marginal trend and Bayesian analyses support that fewer participants make commission errors in the no-WM load than the high-WM load (62%).

Hence, this result seemed to indicate that the vulnerability to PM commission errors is reduced by the interference caused by a subsequent mentally effortful task requiring WM abilities. Recent studies bring additional support for this claim (Craig et al., 2014; Dewar et al., 2007; Wixted, 2004, 2010). As previously noted, yet is generally assumed that similarity between original and new memories may be particularly damaging, there is evidence that an interfering activity that is not similar to the previously learned material (i.e., *mental effort per se*, as originally defined by Müller and Pilzecker, 1900) can produce forgetting, too.

Importantly, our results also reveal a clear effect of the filler task´ difficulty since the discrimination index *d´* in the *n*-back task was higher on the low-WM load (i.e., 1-back) than on the high-WM load (i.e., 3-back). This result supports the assumption that the filler task was more demanding in the 3-back compared to the 1-back condition. Finally, as in Experiment 1, counting recall performance was similar across conditions supporting the idea that PM commission error risk is due to the experimental manipulation and not due to a differential attention allocation strategy.

Another interesting finding stemmed from the OT performance. Consistent with our prediction, we observed a similar accuracy and RTs between the no-PM and the three other experimental conditions with a PM task (i.e., the no-WM, low-MW, and high-WM load conditions). Therefore, Experiment 2 provided more substantial evidence that participants automatically retrieve the (irrelevant) intention upon encountering the associated PM cue, excluding confounding factors in the occurrence of commission errors such as monitoring for PM cues.

1.3 General discussion

The present study explored a prominent topic in PM research: Does forgetting irrelevant intentions occur because new information replaces or interfere with the memory representation of an old-PM intention? In two experiments, we have shown that a retroactive interference mechanism seems to play a crucial role in PM deactivation. Recent research has pointed in this direction (Anderson & Einstein, 2017; Walser et al., 2017). However, an advantage of our experimental task (vs. Walser et al., 2012, 2017) is that we have taken a different approach: We have analysed the occurrence of PM commission errors and by using a finished-PM paradigm (i.e., not by repeating PM and OT blocks since commission errors can occur due to a source monitoring failure - because participants must continuously update the relevance of the PM cue and response throughout several blocks). Importantly, we also added a no-PM group. As previously theorised, we sought to understand if PM commission errors occur due to a failure to inhibit a spontaneously retrieved PM task or, instead, because subjects continue to monitor PM cues strategically.

First, replicating previous work (Boywitt et al., 2015; Matos et al., 2020; Pink & Dodson, 2013; Shaper & Grundgeiger, 2017), we found that young adults are prone to erroneously execute an unperformed intention when they no longer must do so if the OT is cognitively demanding. In contrast with Anderson et al. (2017), a novel finding was that, in such cases, commission errors were reduced by the requirement to perform a new and distinct PM task after the old one is declared finished (Experiment 1). In the new-PM task condition, we observed that participants were slower in response to the LDT during the finished-PM phase than in the no-PM condition. We reasoned that monitoring for novel PM cues, or people´s decision to slow down their responding, may have incurred costs to the OT performance (Einstein et al., 2005; Heathcote et al., 2015; Smith, 2003; Strickland et al., 2018). Of note, the new intention did not encourage focal processing of the PM cues (i.e., participants had to press a bell whenever they saw numbers in the context of a LDT), so it required checking the environment for the appropriate moment to perform it; see also Walser et al., 2017).

One could easily argue that participants monitored heavily for the new-PM task reducing commission error risk for an old-PM task. Still, in previous works, monitoring for novel PM cues during finished phases seemed to exacerbate intention interference (Walser et al., 2017). Alternatively, this slowing may reflect the idea of Schaper and Grundgeiger (2019) that participants might have had more time to prepare a response in the sense that they correctly evaluated the PM cue and tagged it for suppression (i.e., with the knowledge that the intention should not be executed). In both cases, we did not observe slower responses to ongoing task trials containing PM cues than control trials, that is, a residual activation of the irrelevant PM task.

Second, consistent with prior work analysing intention interference (Walser et al., 2014), in Experiment 2, we also observed that fewer participants make commission errors if they perform a task with a moderate and high-WM load immediately than in a no-WM load condition. Taken together, our results can be theoretically interpreted based on a retroactive interference (Barnes & Underwood, 1959; Wixted, 2010). Applied to the present data, encoding dissimilar new intentions or WM contents seems to overwrite, deteriorate or even restrain the old-PM trace (Engle et al., 1995; Hasher & Zacks, 1998). Hence, the old-PM intention becomes less accessible and, consequently, more easily inhibited upon encountering the associated (but irrelevant) cue during the finished-PM phase.

Importantly, we found fewer commission errors using a new-PM intention with a different PM-category (i.e. numbers in any screen location instead of a specific word) and PM-response (i.e., press a bell rather than the *Q* press on the keyboard). One possible interpretation of this inconsistent result seems to be intention´s similarity. For instance, Walser et al. (2017) showed that intention interference was reduced when the category of both intentions differed (e.g., symbols vs. words) compared to when PM cues belonged to the same category (e.g., symbols vs. symbols). From this perspective, pursuing another intention of a similar/dissimilar type after completion may affect intention deactivation such as other aspects (e.g., the existence of a strong link between retrieval and intended action, salient PM cues encountered during the same OT context or impaired cognitive control; Bugg et al., 2013, 2016; Matos et al., 2020; Schaper & Grundgeiger, 2019; Scullin & Bugg, 2013).

Moreover, the empirical evidence that memory loss is not merely caused by interference of highly similar material but also by nonspecific retroactive interference supports this reasoning (Dewar et al., 2007; Müller & Pilzecker, 1900; Wixted, 2010). The idea is that the greater and more variable the new learning is, the greater the interfering effect will be since it may elicit the most hippocampal activity and, consequently, the greatest rate of new memory formation (Wixted, 2004, 2010). A further noteworthy finding is that the reduced pattern of commission errors in the new-PM task condition could also have benefited from a cumulative mechanism of release from proactive interference (Wickens et al., 1963). This kind of interference by which older memories impair the retrieval of new memories builds up over time until people are given information that differs from the old knowledge. At that point, memory improves. Our study should highlight a reduced overlap between intentions (i.e., no components of the old-PM representation were needed for performing the new intention).

Regarding OT performance, the current research is one of the few studies adding a no-PM condition to bring additional leverage on the PM retrieval process underlying PM commission errors. The rationale here is that the ability to remember to perform delayed intentions might occur due to top-down effortful self-reminders or a bottom-up reactivation in response to external cues. The later form of retrieval has the advantage of supporting PM without effortful processes. Yet, since PM is cue-dependent, processing a strong retrieval cue might spontaneously retrieve an old and irrelevant PM intention to consciousness, which may lead, in some situations, to PM commission errors (Bugg et al., 2016; Matos et al., 2020; Scullin & Bugg, 2013; Scullin et al., 2012). So, the present finding that there were no differences in the OT performance between the no-PM and each of the experimental conditions on both experiments (except for those in the new-PM task condition in Experiment 1) supports the dual-mechanisms theory´s prediction of a spontaneous PM retrieval (Bugg et al., 2016; Scullin & Bugg, 2013).

In conclusion, an irrelevant intention might be spontaneously retrieved despite no longer-needed when greater demands are placed on the cognitive system. Interestingly, our results add significant evidence to the claim that, in such cases, encoding new dissimilar memories (i.e., new intentions or new WM contents) seems to provide an overwriting-like mechanism that facilitates PM deactivation. A remaining outstanding theoretical issues concern which specific interfering dissimilar information (e.g., verbal or visual information) are potentially at play, test the impact of PM task difficulty on the extent of overwriting, as well as the impact of WM individual differences on PM deactivation.

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