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Frequency of Prospective Use Modulates Instructed Task-Set Interference

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Recent studies have demonstrated that keeping an instructed task set in working memory (WM) for prospective use can interfere with behavior on an intervening task that employs shared stimuli—the prospective task-set-interference effect. One open question is whether people have strategic control over prospective task-set interference based on their expectations of whether task instructions will have to be implemented or recalled. To answer this question, we conducted two experiments that varied the likelihood with which a set of prospective task instructions would have to be implemented or recalled. Based on the hypothesis that participants are able to strategically modulate the manner in which a prospective task set is encoded in WM, we predicted that, as the frequency of implementing task instructions increased, so too would the magnitude of the prospective task-set-interference effect. We found that task instructions held in WM caused significant task-set interference, even in mostly recall conditions, but—crucially—that this interference effect scaled positively with the likelihood of having to implement the prospective set. These data suggest that task instructions are obligatorily encoded as a procedural task set, but that the degree to which this set impinges on ongoing stimulus processing is subject to some strategic control, possibly via modulation of the associations between declarative and procedural WM contents.

Public Significance Statement

Tasks that we keep in mind for later use can interfere with what we are doing right now. It is important to understand the interaction between mental content and ongoing behavior, as it may allow us to help people avoid making unintended actions. In this study, we have shown that the influence of future goals in ongoing behavior can be moderated based on the likely use for those goals; when people expect to use—for example in carrying out a task—rather than just remember the information they have in mind in the future, their future goals have a greater impact on what they are doing right now.

Keywords: prospective task-set interference, working memory, strategic control

Modern life involves many situations in which different tasks vie for our limited time and attention, yet the exact manner in which we manage to keep one task in mind (i.e., in working memory; WM) while performing another one is not well understood. One certainty is that the shielding of information in WM is not always perfect, as WM contents can unintentionally impact how we pay attention (reviewed in Soto, Heinke, Humphreys, & Blanco, 2005) or select responses (reviewed in Cole, Braver, & Meiran, 2017) in an unrelated, intervening task. For example, a recent line of research has documented that keeping in mind a prospective task set, such as a newly instructed stimulus–response (S–R) mapping, can interfere with behavior in an intervening task if the two tasks contain overlapping stimulus features, i.e., the *prospective task-set-interference effect* (Braem, Liefooghe, De Houwer, Brass, & Abrahamse, 2017; Liefooghe, Wenke, & De Houwer, 2012; Meiran, Pereg, Kessler, Cole, & Braver, 2015; Theeuwes, Liefooghe, & De Houwer, 2014; Wenke, Gaschler, & Nattkemper, 2007). Specifically, responses to a stimulus in the intervening task are less accurate and slowed if they are incompatible with the response that would be required for the same stimulus under the task set held in WM than if the two were unrelated or compatible. Understanding the determinants and boundary conditions of this interference effect can elucidate some of the mechanisms and architecture of WM. The present study pursues this goal by addressing the manner in which task instructions encoded in WM can modulate prospective task-set interference.

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A Theoretical Account of Prospective Task-Set Interference

The prospective task-set-interference effect has typically been conceptualized within the framework of Oberauer's model of WM (Oberauer, 2009, 2010). According to this influential model, WM consists of two components: a declarative WM, responsible for maintaining items (such as words, objects, etc.) in memory, and a procedural WM, responsible for keeping task rules such as S–R mappings, in mind. The two are thought to operate in conjunction to carry out cognitive or motor actions associated with declarative item representations. Within each of these components, several items or rules can be kept active, but only one at a time is allowed to drive behavior, by virtue of being in the focus of *internal attention* (in declarative WM) or being placed in *the bridge* (in procedural WM). Specifically, active procedural representations in the bridge, such as the relevant response elements in an instructed task set, are thought to be linked via excitatory associative bindings (or *condition–action links*) with declarative representations (the stimuli) to allow for stimulus-specific response selection (Brass, Liefooghe, Braem, & De Houwer, 2017; Liefooghe et al., 2012; Oberauer, 2009). At the same time, procedural representations in the bridge exert an inhibitory influence on other active rules in procedural WM, such that a single condition–action link can be triggered while alternatives are suppressed (Oberauer, 2009, 2010).

The condition–action bindings between active declarative and procedural representations have been posited as a mechanistic account for the prospective task-set-interference effect, sometimes termed *instruction-based reflexivity* (Meiran et al., 2015). First, by virtue of the intention to execute the prospective task set, the instructed S–R rules would be stored as procedural WM representations, encoded into the bridge in preparation for implementation (Brass et al., 2017; Liefooghe et al., 2012; Oberauer, 2009, 2010; but see Meiran, Cole, & Braver, 2012). Note that within this paradigm, the intervening task is a highly practiced task that would rely on associations in long-term memory and would therefore not occupy the bridge in WM (Cole, Laurent, & Stocco, 2013; Waszak, Hommel, & Allport, 2003). Second, when its associated declarative element becomes the focus of attention, the execution of a procedurally represented action would be triggered (Oberauer, 2009), akin to a *prepared reflex* (Hommel, 2000). Thus, if the prospective task set is activated in procedural WM, the occurrence in an intervening task of a stimulus that forms part of that set would lead to an obligatory response activation that, if inappropriate, would then have to be inhibited to preserve accurate performance (Liefooghe & De Houwer, 2017; Liefooghe et al., 2012; Meiran et al., 2015; Oberauer, 2009). In this way, the Oberauer model can account for the findings of inaccurate or slowed responses for stimuli that are associated with incompatible responses across the current and prospective task.

Can Prospective Task-Set Interference Be Modulated Strategically?

The degree to which, and by what mechanisms, people can strategically overcome prospective task-set interference is presently not well-understood. Previous work has shown that factors that affect encoding of a set of task instructions can modulate the prospective task-set-interference effect. Specifically, prior studies manipulated the expected utility of the prospective set by varying the likelihood of actually having to implement it. They documented that, as the likelihood of implementing the prospective task set decreases, the magnitude of prospective task-set interference also decreases (Wenke, Gaschler, Nattkemper, & Frensch, 2009). However, the origin of this modulatory effect is not entirely clear: Participants could either have encoded the prospective set into procedural WM under all conditions but strategically weakened the condition–action bindings under conditions in which the implementation of the prospective set was less likely, or, they could have simply not encoded the prospective set on many trials of the latter condition. Because participants were not probed for their memory of task instructions in trials in which the prospective set did not have to be implemented, it is not known whether that set was maintained in any form, thus leaving open the question of whether participants could strategically suppress the influence of a prospective set on an intervening task.[1]

Furthermore, prior experiments have also demonstrated that the explicit cueing of expected task demands, postencoding of task sets, can modulate the prospective task-set-interference effect. Specifically, in Experiment 4 in Meiran et al. (2015), participants were given explicit information on the duration of the implementation phase for an instructed task set, and cues instructing longer implementation phases led to less prospective task-set interference during the intervening task. A longer implementation phase presumably allowed participants more time to learn the instructed task through trial and error, and thus reduced their preparation to rapidly implement the task set (Meiran et al., 2015). However, this manipulation modulated instructed task sets postencoding. Thus, whether participants are able to implement control over the encoding (rather than maintenance) of task sets based on learned information about the expected upcoming task demands remains an open question.

That the manner in which (rather than whether) task instructions are encoded affects the prospective task-set-interference effect has been documented by Liefooghe and colleagues, however. First, Liefooghe et al. (2012) reported in a between-participants design that, when people were simply asked to recall task instructions (rather than implement them), the prospective task-set-interference effect disappeared, which suggested that task instructions are not obligatorily converted into a procedural WM representation, and absent this conversion, they do not significantly interfere with an intervening task. In contrast to those prior findings, Liefooghe and De Houwer (2017) more recently reported that simply remembering instructions for future recall can, in fact, also elicit a prospective task-set-interference effect, though seemingly to a lesser degree than in prior studies when the prospective task set had to be reliably implemented (see also Liefooghe et al., 2012). As this

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[1] Note that a recent study by Whitehead and Egner (2018) did document strategic modulation of prospective task-set interference, but that study assessed possible strategic effects subsequent to/independent of the encoding of task instructions: The instructions (for implementation) were held constant, whereas the demands (frequency of incompatible trials) of the intervening task were varied. By contrast, the present study addressed strategic modulation as a function of changes in instructions while holding the demands of the intervening task constant. The two studies thus assessed different ways in which strategic control processes can affect prospective task-set interference.

study had participants always recall (and never implement) the instructed task set, observing the prospective task-set-interference effect under those recall-only conditions suggests that procedural representations of instructed tasks may form automatically, even when participants are only told to declaratively recall the instructions. Furthermore, the observed weaker interference effect elicited under recall conditions, compared with conditions in which a prospective task set is implemented, may suggest that participants were be able to attenuate the influence of the automatically encoded procedural set in the recall condition, for example, by modulating the condition–action associations.

However, the one-armed nature of the prospective-use manipulation in Liefooghe and De Houwer (2017)—to always recall and never implement the task set—does not allow us to conclude whether participants can engage in any strategic, contextual adaptation of the way they encode the task instructions, because the task context was held constant for each participant. Thus, it remains unclear whether the formation of procedural representations can be strategically modulated in response to varying demands of the prospective use (recall vs. implementation) of a task set.

The Current Study

We aimed to investigate the ability to strategically adapt the way in which task instructions are encoded by testing in a within-subjects design whether the expected prospective use of a task set—either to be recalled declaratively or implemented procedurally—would influence the magnitude of prospective task-set interference. In line with Liefooghe and De Houwer (2017), we assumed that task instructions are automatically encoded into procedural WM, but we also hypothesized that their impact on ongoing processing can be strategically modulated. Specifically, given previous results showing a reduced magnitude for prospective task-set interference elicited under conditions in which task sets were encoded with an expectation to be recalled instead of implemented (Liefooghe et al., 2012; Meiran et al., 2015), we hypothesized that the level of prospective task-set interference should be driven by task demands, the prospective use (i.e., recall vs. implementation) of an instructed task set, and the frequency of a specific use's occurrence. Under conditions of infrequent prospective task-set implementation (vs. recall), the strength between declarative and procedural condition–action associations should be low, thus leading to relatively small prospective task-set-interference effects; by contrast, under conditions of frequent prospective task-set implementation, the strength between declarative and procedural condition–action associations should be high, thus leading to relatively large prospective task-set-interference effects.

Over the course of two experiments, using a similar experimental design to Meiran et al. (2015) and Liefooghe and De Houwer (2017), we manipulated the run-wise proportion of prospective use regarding task-set instructions: to recall or implement. Unlike previous one-armed designs or entirely between-participants comparisons (see Liefooghe & De Houwer, 2017; also Liefooghe et al., 2012), the current designs allowed us to directly investigate, within subjects, whether (a) prospective task-set interference is modulated by prospective use—either recall or implementation—when encoding a prospective task-set instruction into WM. Furthermore, employing this proportion of prospective task-set use manipulation allowed us to (b) evaluate the extent to which strategic control implemented during encoding is sensitive to the frequency of prospective use for an instructed task set, and further modulated prospective task-set interference. A difference in the prospective task-set-interference effect as a function of the prospective use and the frequency of its occurrence would indicate that task demands enable the proactive application of strategic top-down control over the strength of condition–action associations between procedural and declarative representations.

Experiment 1

To test whether prospective task-set interference could be modulated by the nature and proportion of that prospective use, we employed a three-banded mixed-effects design. Within each group, participants completed both runs in which they were asked to "mostly implement" or "mostly recall" an instructed task set, allowing for an elicitation of prospective task-set interference under recall and implement conditions in a within-participants design. To see if the impact of this manipulation was subject to strategic control, we employed a between-participants manipulation of the specific proportion level—low, medium, or high—to investigate whether the prospective task-set-interference effect would be further modulated by the frequency of the prospective demands.

Method

Participants. Based on pilot data acquired on a closely related protocol, we determined that we would require at least 30 participants for each of our three groups to grant .80 power for detecting the effect of interest (an interaction effect between stimulus compatibility and prospective task use) at $p < .05$. Our power analysis was based on a 1,000-iteration data simulation using effect size, variance, and correlation among repeated-measures parameters derived from pilot data. Our participants were recruited from Amazon Mechanical Turk's (MTurk) online platform. The use of reaction time (RT) and accuracy as dependent measures in cognitive psychology experiments run on the MTurk platform has been widely validated (Crump, McDonnell, & Gureckis, 2013). However, based on our previous work (Whitehead & Egner, 2018) and the exclusion of so-called "master workers" (who are highly experienced and thus not very representative participants), we anticipated a relatively high participant attrition rate because of poor performance in this fairly difficult task. We therefore recruited between 40 and 50 workers for each group—a total of 138 workers—who provided informed consent in accordance with the policies of the Duke University Institutional Review Board (mean age = 33.61, *SD* = 9.08; 69 men, 69 women). Participants could sign up for the experiment regardless of age or country of residence. Thirty-nine subjects performed under 75% accuracy in either the recall or the implementation of instructions and were excluded, leaving 33 participants in each arm of the between-participants "degree-of-prospective-use-bias" manipulation (i.e., low, medium, or high), with a total of $N = 99$ for the entire experiment.[2]

Stimuli and procedure. The task followed the basic inducer–diagnostic task design of the NEXT paradigm (Meiran et al., 2015; see also Cole et al., 2013) and of Liefooghe and De Houwer (2017), in which an initial period of encoding an instructed task set into WM (known as the "inducer task"), is followed by a phase during which the inducer task set has to be kept in mind while participants perform another, "diagnostic task." In this way, interference from the inducer task can be assessed; see below. This phase is then followed by the inducer-task probe phase, in which memory for the instructed task set is assessed. Specifically, our basic paradigm consisted of 128 miniblocks, with each miniblock presenting an initial unique two-alternative-forced-choice (2AFC) task to be remembered for future implementation or recall. The presentation of a consecutive sequence of miniblocks is called a "run."

At the beginning of each miniblock (the inducer task-set encoding period) an instruction screen appeared for 5,000 ms with two unique word–action S–R pairings, chosen randomly without replacement for every miniblock (see Figure 1). For example, the instructions could read, "If *rabbit* press D; If *turtle* press J," with the first S–R mapping presented above the other and the key words (i.e., "rabbit" and "turtle") presented in blue ink. Instructed task sets were presented the same way, regardless of their prospective use. The two instructed S–R pairings were unique on each trial, but always mapped to the D key and J key. They were also always accompanied by a third instruction that remained constant throughout the entire experiment: to press the D key in response to a red word presented in the center of the screen (the diagnostic task).

The task-set instruction period was then followed by the diagnostic phase (the task-set maintenance period): a series of trials, lasting 0–3 trials in length, in which a single word in red text is presented in the center of the screen. Here, participants are required to press the D key to advance through the diagnostic-task trials to reach the stage at which they will need to recall or implement the task set they are holding in WM. Following the diagnostic phase was the inducer-task probe, in which participants were asked to either recall or implement the instructed task set. All participants were exposed to both task-set recall and task-set implementation miniblocks, but we varied the relative likelihood of encountering these miniblock types within runs between three groups of participants to create low, medium, and high degree-of-prospective-use-bias conditions: For the low-proportion group, runs differed only slightly in their bias for either the prospective use of implementing or recalling the instructed task set (17:15 proportion of miniblocks per run), for the medium-proportion group, that bias was stronger (25:7 proportion of miniblocks per run), and for the high-proportion group, the bias was extreme (all 32 miniblocks per run were of the same type of instruction).

When asked to simply recall the instructed task set, a set of S–R pairings appeared in the same instruction format in which they had initially been presented, and participants were asked whether (a) both S–R pairings were the same, (b) whether one S–R pairing differed, or (c) whether both S–R pairings differed. This is similar in design to Liefooghe and De Houwer (2017) and it forced participants to fully encode both S–R pairings. When asked to implement the instructions, participants were simply presented with a single word in blue text in the center of the screen. They had previously been instructed at the beginning of the task that this indicated they were to implement the instructions they were currently remembering (e.g., press D for *rabbit*). Each stimulus or set of stimuli in both the diagnostic and inducer-probe phase was presented until a response was selected. After a response was selected, an intertrial stimulus screen with a central fixation cross was presented for 500 ms before a new stimulus or set of stimuli was presented. There was a 750-ms interval between miniblocks, after the completion of the inducer phase task, and before the presentation of new instructions.

Critically, overlap in word use between the diagnostic and inducer tasks created compatible and incompatible stimuli in the diagnostic task. Compatible stimuli occur when the response to the word in the diagnostic task (i.e., always the D key) was the same as instructed for the recall or implementation conditions in the inducer task. To continue our previous example, if participants saw the word *rabbit* during the diagnostic phase (i.e., in red text), this would be a compatible trial, as the response associated with this word in the inducer task—pressing the D key—was consistent with the correct response in the diagnostic phase—pressing the D key. Incompatible stimuli occurred when the response to the word in the diagnostic task did not match the instructed task set (i.e., the instructed task set indicated a J key response). For example, if participants saw the word *turtle* during the diagnostic phase (i.e., in red text), this would be an incompatible trial, as the response associated with this word in the inducer task—pressing the J key—was inconsistent with the correct response in the diagnostic phase—pressing the D key. Word stimuli were between four and six letters in length, did not contain proper nouns, were close in frequency rating by logarithmic scale (Lund & Burgess, 1996), and were compiled using the English Lexicon Database (Balota et al., 2007).

Results and Discussion

Diagnostic task. Accuracy during the diagnostic task was at ceiling (100%) and did not constitute a dependent variable of interest. Before analysis of response times (RTs), trials faster than 100 ms or slower than 3,000 ms were removed (0.63% of data; the same boundaries used by (Meiran et al., 2015). Table 1 displays the mean RTs and standard deviations for these data. Data were then submitted to a 3 (between-participants' degree of prospective-use-bias manipulation: low proportion vs. medium proportion vs. high proportion) × 2 (prospective use: recall vs. implement) × 2 (compatibility: compatible vs. incompatible) repeated-measures analysis of variance (ANOVA; see Table 2).

Mean RT compatibility effects as a function of the prospective-use condition are displayed in Figure 2. Replicating previous work, there was a main effect of compatibility, such that compatible trials (626 ms) were, on average, substantially faster than incompatible trials (695 ms). Critically, however, the compatibility effect was qualified by a three-way interaction between Degree of Prospective-Use Bias × Prospective Use × Compatibility (see Table 2); the difference in the compatibility effect between the

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[2] Exclusion of subjects performing just above chance instead of the 75% cut-off did not change the pattern of significance in the results. Inclusion of all subjects, including those performing at or under chance led to a qualitatively similar pattern of results.

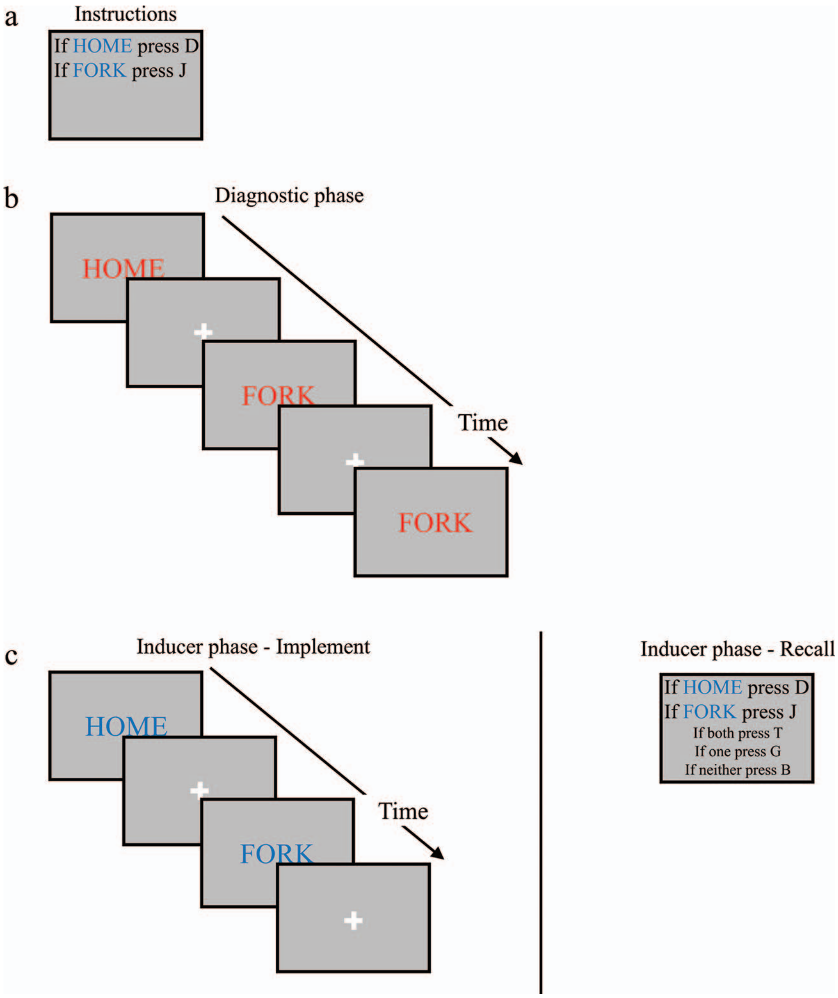

Figure 1. Experimental paradigm illustrating the components of each miniblock. A sequence of miniblocks is presented in continuous succession throughout the experiment: (a) sample miniblock instructions; (b) all participants completed a diagnostic phase (this example being three trials); here, HOME represents a congruent stimulus and FORK an incongruent one; (c) depending on the specific task configuration, participants would then see either the implement-inducer phase, in which they would implement the instructed task set, or the recall-inducer phase, in which they would answer whether both S–R mappings were the same as instructed, whether only one was the same, or whether neither was the same as instructed. See the online article for the color version of this figure.

prospective-use recall and implementation manipulations varied as a function of degree of prospective-use bias. In the high-proportion group, the prospective use of the instructed task set (recall vs. implementation) modulated the compatibility effect to a larger extent (52 ms) than in the medium-proportion group (19 ms), and the low-proportion group (8 ms). Specifically, the high-proportion group displayed the largest amount of interference in the "mostly implement" condition (92 ms, medium-proportion group: 59 ms, small-proportion group: 75 ms), and the smallest amount of interference in the "mostly recall" condition (39 ms, medium-proportion group: 78 ms, small-proportion group: 67ms).

Table 1
Means and Standard Deviations for Response Times (ms) in the Diagnostic Phases of Miniblocks From Experiment 1

Proportion level	Mostly implement		Mostly recall	
	Compatible	Incompatible	Compatible	Incompatible
Low	635 (285)	711 (361)	659 (321)	726 (381)
Medium	641 (286)	700 (350)	626 (286)	704 (357)
High	587 (276)	679 (336)	611 (292)	650 (305)

Post hoc, Bonferroni-corrected pairwise comparisons indicated that there was a prospective task-set-interference effect in both the mostly implement prospective-use condition ($|t| = 6.929$; $p < .001$) and the mostly recall prospective-use condition ($|t| = 8.731$; $p < .001$), replicating demonstrations that task sets encoded under the presumption of only recalling, not implementing, the task set still elicit a prospective task-set-interference effect (Liefooghe & De Houwer, 2017). Further post hoc Bonferroni-corrected comparisons indicated that the Prospective Use × Compatibility interaction was significantly different between the medium and high

Table 2

Results from the 3 (Between-Participants Proportion of Prospective-Use Manipulation: Low, Medium, High) × 2 (Prospective Use: Recall vs. Implement) × 2 (Compatibility) Repeated-Measures Analysis of Variance on Response Times (ms) for the Diagnostic Phase Trials of Miniblocks from Experiment 1

Effect	*F*	*df*	*p*	η_p^2
Between participants				
Between	.92	2,96	.402	.019
Prospective use	1.47	2,96	.235	.030
Compatibility	.16	2,96	.852	.003
Prospective Use × Compatibility	3.47	2,96	.035	.067
Within participants				
Prospective use	.33	1,96	.568	.003
Compatibility	87.42	1,96	<.001	.477
Prospective Use × Compatibility	2.72	1,96	.102	.028

degree of prospective-use bias groups (|t| = 2.616; *p* = .031). There were no significant differences in the Prospective Use × Compatibility interaction between low and high degree of prospective-use bias between participant groups (|t| = 1.58; *p* = .352), or between low and medium degree of prospective-use bias between participant groups (|t| = 1.04; *p* = .908). There were no other significant main effects or interactions. In sum, these results document that the frequency with which participants are asked to either implement or recall an instructed prospective task set modulates the degree to which that task set interferes with an intervening task. That is, a high certainty about the likely prospective use exacerbates the differential effects of keeping a task set in mind for future recall versus implementation.

Inducer task. The results of the inducer task were not of primary interest; nevertheless, RTs and accuracy were analyzed. Before analysis, trials faster than 100 ms or slower than 3,000 ms were removed (14.7% of data; the same boundaries used by Meiran et al., (2015). In addition, for the RT analysis, error trials were removed (3.47% of remaining trials). Table 3 displays the mean RTs and accuracy measures, along with their respective standard deviations. Data were then submitted to two 3 (between-participants degree of prospective-use bias manipulation: low proportion vs. medium proportion vs. high proportion) × 2 (prospective use: recall vs. implement) repeated-measures ANOVAs, one with the dependent variable of RT and the other with accuracy as the dependent measure (see Table 4).

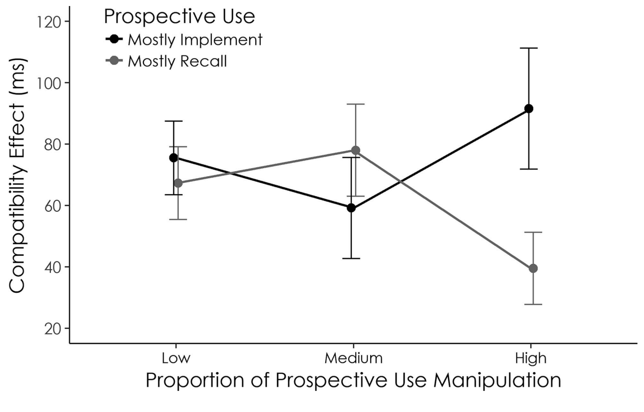

Figure 2. The compatibility effect (*MS* ± *SE*) for trials in the diagnostic phase, plotted as a function of the between-participants proportion of and prospective use.

Table 3

Means and Standard Deviations for (a) Response Times (ms) and (b) Percent Error (%) in the Inducer Phases of Miniblocks from Experiment 1

Proportion level	Implement	Recall
(a)		
Low	743 (6.07)	2,133 (14.30)
Medium	769 (6.31)	2,159 (15.04)
High	647 (5.43)	2,034 (14.46)
(b)		
Low	5.21 (.34)	2.84 (.47)
Medium	2.91 (.26)	1.09 (.31)
High	3.64 (.29)	1.75 (.37)

There was a significant main effect of the between-participants degree of prospective-use bias manipulation on accuracy, and a trending main effect in RTs, such that the low-proportion group had lower accuracy (95%) and slower RT (1,073 ms) than the medium- (97% accuracy; 1,060 ms RT) or high- (97% accuracy; 969 ms RT) proportion groups, which indicates that when participants had a high expectation for implementing the instructed task set, that implementation was performed faster and more accurately. There was also a main effect of prospective use for both accuracy and RT, such that implemented task sets had faster RTs (720 ms) but lower accuracy (96%) than recalled task sets (2,107 ms RT; 98% accuracy).

Experiment 2

The significant three-way interaction between degree of prospective-use bias, prospective use, and compatibility demonstrated that prospective use (i.e., to implement or recall an instructed task set) in combination with the proportion of employing

Table 4

Results From Both 3 (Between-Participants Proportion of Prospective Use Manipulation) × 2 (Prospective Use: Recall vs. Implement) Repeated-Measures Analyses of Variance on (a) Response Times (ms) and (b) Accuracy (%) for the Inducer-Phase Trials of Miniblocks from Experiment 1

Analysis/Effect	*F*	*df*	*p*	η_p^2
(a)				
Between participants				
Between	2.64	2,96	.076	.052
Prospective use	.32	2,96	.724	.007
Within participants				
Prospective use	2,467.07	1,96	<.001	.963
(b)				
Between participants				
Between	5.25	2,96	.007	.099
Prospective use	.10	2,96	.906	.002
Within participants				
Prospective use	17.99	1,96	<.001	.158

said use, resulted in strategic modulation of the prospective task-set-interference effect. We designed the proportion manipulation as a between-participants factor to create conditions in which task demands within participants only changed as a function of the prospective use and not the likelihood of that use, allowing for optimal conditions for strategic control implementation. However, the results of the post hoc tests of the Prospective Use × Compatibility interaction between different proportion groups (i.e., between participants) resulted in some ambiguity, particularly with respect to nonsignificant differences between the high- and low-proportion groups, potentially due to the increase in variance accompanying between-participants designs. Given the above results, we next sought to replicate and strengthen these findings in an entirely within-participants design. Specifically we wanted to determine whether strategic control processes could modulate the compatibility effect dynamically in response to both changing demands in the prospective use and the frequency of that use across the course of a single experiment. Therefore, in Experiment 2 we followed a procedure similar to Experiment 1, manipulating the prospective use and the frequency of encountering that use, but now varying both of these factors entirely within participants to determine if control was flexibly implemented as a function of these task demands to modulate the prospective task-set compatibility effect.

Method

Participants. Based on simulations using data from Experiment 1, we determined that we would require at least 40 participants to grant .80 power for detecting the effect of interest (the three-way degree of prospective-use bias, prospective use, and compatibility interaction) at $p < .05$. Based on Experiment 1, we also anticipated a relatively high participant attrition rate because of poor performance in the recall task-set use condition. We therefore recruited 57 workers in total, from MTurk, who provided informed consent in accordance with the policies of the Duke University Institutional Review Board (mean age = 31.67, *SD* = 9.45; 30 men, 27 women). Participants could sign up for the experiment regardless of age or country of residence. Seventeen subjects performed under 75% accuracy in either the recall or the implementation of instructions and were excluded, leaving 40 participants.[3]

Stimuli and procedure. The task was the same as in Experiment 1, except for the following key differences. To increase power, this paradigm consisted of 160 miniblocks, still with each miniblock presenting an initial unique two-alternative forced-choice (2AFC) task to be remembered for future implementation or recall. All participants were exposed to both task-set recall and task-set implementation miniblocks. However, we varied within participants the relative likelihood of encountering these miniblock types within runs to create low and high degree of prospective-use bias conditions, which differed from Experiment 1. For the low-proportion condition, runs differed slightly in their bias for either implementing or recalling the instructed task set (24:16 proportion of miniblocks per run), whereas for the high-proportion condition, the bias was more extreme (36:4 proportion of miniblocks per run). The presentation of these runs was pseudorandom, with the following constraints: The run-wise order of the prospective use of that run followed an ABAB pattern, while the run-wise order of the proportion manipulation of applying that prospective use followed an A-B-B-A pattern. This was to prevent situations in which two runs of the same likely prospective use occurred back to back, and then to prevent the possibility of both high- or low-proportion manipulations occurring at the beginning and end of the experiment.

Results and Discussion

Diagnostic task. Accuracy during the diagnostic task was at ceiling (100%) and did not constitute a dependent variable of interest. Before analysis of RTs, trials faster than 100 ms or slower than 3,000 ms were removed (1.17% of data). Table 5 displays the mean RTs and standard deviations for these data. Data were then submitted to a 2 (degree of prospective-use bias manipulation: low proportion vs. high proportion) × 2 (prospective use: recall vs. implement) × 2 (compatibility: compatible vs. incompatible) repeated-measures ANOVA (see Table 6).

Mean RT compatibility effects as a function of the prospective-use condition are displayed in in Figure 3. Notably, there was a main effect of compatibility; compatible trials (617 ms) were, on average, substantially faster than incompatible trials (684 ms). Critically, replicating the results from Experiment 1 and supporting our hypothesis, the compatibility effect was further modified by a three-way interaction between Degree of Prospective-Use Bias × Prospective Use × Compatibility (see Table 6); in the high-proportion condition, the prospective use of the instructed task set (recall vs. implementation) modulated the compatibility effect to a larger extent (32 ms) than in the low-proportion condition (24 ms). Specifically, the high-proportion manipulation led to the largest amount of interference in the mostly implement condition (82 ms; small-proportion group, 49 ms), and the smallest amount of interference in the mostly recall condition (56 ms; small-proportion group, 80 ms).

Moreover, post hoc Bonferroni-corrected pairwise comparisons indicated a significant prospective task-set-interference effect in the mostly implement prospective-use condition for both the high- and low-proportion-manipulation conditions ($|t| = 6.57$, $p < .001$; $|t| = 4.50$, $p < .001$, respectively). This was true for the mostly recall prospective-use condition as well, in which Bonferroni-corrected pairwise comparisons indicated a significant prospective task-set-interference effect for both the high- and low-proportion manipulations ($|t| = 4.14$, $p < .001$; $|t| = 5.99$, $p < .001$, respectively). This not only replicated Experiment 1, but also demonstrated that task sets encoded in situations in which they are highly likely to be only recalled still elicit a prospective task-set-interference effect (Liefooghe & De Houwer, 2017). Together, these results clarified our findings from Experiment 1 in a within-participants design and demonstrated that the degree to which an instructed prospective task set interferes with current behavior is sensitive to the changing frequencies between high- and low-proportion manipulations of the prospective demands, that is, to implement or recall a task set.

Inducer task. The results of the inducer task were not of primary interest; nonetheless, RTs and accuracy were analyzed.

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[3] The pattern of significance in the results did not change with the exclusion of subjects performing just above chance instead of the 75% cut-off, or including all subjects.

Table 5
Means and Standard Deviations for Response Times (ms) in the Diagnostic Phases of Miniblocks from Experiment 2

Proportion level	Mostly implement		Mostly recall	
	Compatible	Incompatible	Compatible	Incompatible
Low	638 (296)	702 (360)	635 (294)	715 (365)
High	636 (309)	728 (392)	627 (314)	662 (319)

Before analysis, trials faster than 100 ms or slower than 3,000 ms were removed (13.82% of data). Additionally, for the RT analysis, error trials were removed (4.24% of remaining trials). Table 7 displays the mean RTs in ms and error-rate percentages, along with standard deviations. Data were then submitted to two 2 (degree of prospective-use bias manipulation: low proportion vs. high proportion) × 2 (prospective use: recall vs. implement) repeated-measures ANOVAs, one with RT as the dependent variable and the other with accuracy as the dependent variable (see Table 8). There was a significant main effect of prospective use (i.e., implement or recall), on RTs ($p < .001$), as implementing the prospective task set was faster (722 ms) than recalling the task set (2,097 ms). There were no other significant main or interaction effects. Although the main effect of prospective use replicated Experiment 1, the lack of any differences in RTs or accuracy as a function of the proportion manipulation contrasts with the differences found in Experiment 1. The lack of differences here could be a result of the within-participants design having reduced the variance.

General Discussion

We designed this study to test whether prospective task-set interference could be strategically modulated based on the expected prospective use of a task set held in WM, which prior studies have not been able to determine unambiguously. In two experiments, using both a mixed-participants design and a within-participants design, we found that the prospective use of a set of task instructions, and expected frequency of that use, interacted to modulate prospective task-set interference. These results replicated prior work in showing a prospective task-set-interference effect, both when the prospective use was to recall (Liefooghe & De Houwer, 2017) and implement the task set (Liefooghe, De Houwer, & Wenke, 2013; Liefooghe et al., 2012; Meiran et al., 2015). Critically, however, the present data also document that, as the frequency of a particular prospective use increased, the difference between the prospective task-set-compatibility effects grew between instructed task sets that were likely to be recalled and those likely to be implemented. This was borne out in the significant three-way interaction between degree of prospective-use bias, the actual prospective use of an instructed task set, and the compatibility effect (see Tables 2 and 6). Specifically, the greatest likelihood of implementation was associated with the greatest level of prospective task-set interference, and the greatest likelihood of recall was associated with the smallest level of prospective task-set interference (see Figures 2 and 3).

Table 6
Results from the 3 (Proportion of Prospective-Use Manipulation) × 2 (Prospective Use: Recall vs. Implement) × 2 (Compatibility) Repeated-Measures Analysis of Variance on Response Times (ms) for the Diagnostic Phase Trials of Miniblocks from Experiment 2

Effect	*F*	*df*	*p*	η_p^2
Proportion	1.08	1,39	.306	.027
Prospective use	.41	1,39	.527	.010
Compatibility	54.40	1,39	<.001	.582
Prospective Use × Proportion	3.45	1,39	.071	.081
Prospective Use × Compatibility	.30	1,39	.585	.008
Proportion × Compatibility	.02	1,39	.887	.001
Prospective Use × Proportion × Compatibility	5.84	1,39	.021	.130

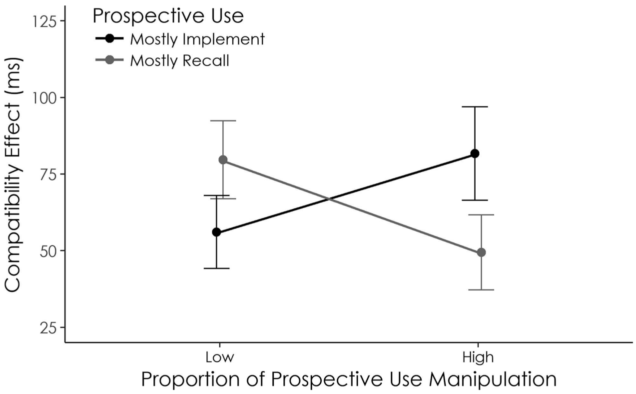

Figure 3. The compatibility effect ($MS \pm SE$) for trials in the diagnostic phase, plotted as a function of proportion of prospective-use manipulation and prospective use.

Whereas prior studies have indicated top-down control *might* be able to strategically adapt the manner of task-set encoding in line with different contexts (Liefooghe et al., 2013; Meiran et al., 2015; Wenke et al., 2009), the current study uniquely demonstrated that ability through a mixed-effects design that always probed memory for the instructed set and varied the prospective use of that set both within and between subjects. Using this approach, we observed strategic, proactive implementation of top-down control to modulate the prospective task-set-interference effect as a function of the frequency of upcoming task demands, or the expected utility of procedural coding of information in WM. This corroborates a nominal trend in prior work of a reduced magnitude for the prospective task-set-interference effect when a task set is to be

Table 7
Means and Standard Deviations for (a) Response Times (ms) and (b) Percent Error (%) in the Inducer Phases of Miniblocks from Experiment 2

Proportion level	Implement	Recall
(a)		
Low	732 (7.26)	2,145 (15.31)
High	711 (6.87)	2,051 (15.65)
(b)		
Low	4.54 (.37)	2.96 (.55)
High	4.68 (.38)	3.02 (.54)

Table 8
Results from Both 2 (Proportion of Prospective Use Manipulation) × 2 (Prospective Use: Recall vs. Implement) Repeated-Measures Analysis of Variance on (a) Response Times (ms) and (b) Accuracy (%) for the Inducer-Phase Trials of Miniblocks from Experiment 2

Effect	F	df	p	η_p^2
(a)				
Proportion	1.09	1,39	.304	.027
Prospective use	1191	1,39	<.001	.968
Proportion × Prospective Use	.00	1,39	.983	<.001
(b)				
Proportion	.95	1,39	.337	.024
Prospective use	.71	1,39	.405	.018
Proportion × Prospective Use	.87	1,39	.358	.022

recalled (Liefooghe & De Houwer, 2017) versus implemented (Liefooghe et al., 2013, 2012; Meiran et al., 2015), though the distinction had not previously been demonstrated directly. Further, our observation extends our knowledge on the extent to which participants can implement control based on learned task statistics. Notably, building on work from Meiran et al. (2015), the modulation of the prospective task-set-interference effect indicates that participants can implement control over task sets during encoding without explicitly cued instructions regarding the upcoming task demands.

In addition, previous findings suggest that the encoding of task-set instructions as procedural representations could be driven by automatic processes resulting from the semantic nature of instructions (i.e., being instructed to press a left/right button; Bundt, Bardi, Abrahamse, Brass, & Notebaert, 2015; but see Tibboel, Liefooghe, & De Houwer, 2016; Wenke et al., 2007, 2009). Although we did not use explicit directional instructions, our response-button mappings did potentially have an associated directional component (i.e., the D key and J key could practically be construed as "left" and "right," respectively). However, although condition–action rules could be formed automatically to some extent, because of this semantic association, our proportion-based modulation of the prospective task-set-interference effect offers clear evidence that control can be implemented over procedural representations.

The implementation of top-down control, and the resulting modulation of prospective task-set interference, can be understood within the framework of procedural WM (Oberauer, 2009, 2010). Within that model, there are two possible means by which top-down processes can affect how procedural representations drive behavior: First, strategic goals can guide the creation and modulate the strength of declarative–procedural associations, and second, they can modulate the strength of the mutual inhibition between procedural representations (Oberauer, 2009, 2010). The design of the current study was such that the task demands during the intervening, diagnostic task were held constant while the prospective task demands of the inducer task (i.e., either to recall or implement the instructed task set) and the expected frequency of each occurrence were manipulated. This leads us to infer that the modulation of the prospective task-set-interference effect in the intervening, diagnostic task is most likely attributable to the first mechanism: that control can modulate the strength of procedural–declarative weights in line with the expected prospective use of a task set at the time of task-set encoding. Thus, the resulting behavioral interference elicited from the prospective task set is reduced as the likelihood of simply recalling that task set increases.

Previously, Whitehead and Egner (2018) manipulated the proportion of compatible versus incompatible stimuli occurring during the intervening task while keeping the prospective task demands constant. A high proportion of incompatible stimuli during the intervening task, as compared with a high proportion of compatible ones, diminished or even reversed the prospective task-set-interference effect, which suggests that a high likelihood of task-set interference leads to a strategic adjustment in how procedural WM representations are maintained. It is important to note that the prospective task demands for the instructed task set in that study did not differ between conditions at encoding. Thus, the authors interpreted these results as commensurate with control being implemented by reactively altering the inhibitory weights between competing procedural (i.e., response) representations (Whitehead & Egner, 2018).

We propose two alternative explanations for the present results. First, it could be that, rather than the strength of the procedural–declarative associations being differentially modulated on the basis of the expected utility and prospective use of an instructed task set, the fidelity of the procedural representation was modulated instead. In this view, the strength of the associations between procedural and declarative representations would remain constant, but when implementing a task set becomes less likely than simply recalling it, the procedural representation in WM would be maintained less faithfully than when implementing the instructed task set was likely. Currently, however, at least within the model of procedural WM proposed by Oberauer (2009), this interpretation is not easily accommodated, as there is no goal-directed mechanism affecting the relative fidelity of procedural representations. A possible way of adjudicating between these possibilities is via functional neuroimaging.

Second, our results could be viewed within the framework of complex-span tasks. Under this view, task sets are stored in declarative WM, with prospective task-set interference stemming from cross-talk between the storage task—remembering the S–R task set—and the processing task—pressing the D key for a word in red ink (Liefooghe & De Houwer, 2017; Oberauer, Demmrich, Mayr, & Kliegl, 2001; van Dijck & Fias, 2011). As the task set stored in WM is refreshed, greater task demands during storage could elicit a greater refresh rate and thus a larger interference effect in the processing task (Barrouillet, Bernardin, & Camos, 2004; Liefooghe & De Houwer, 2017). Consequently in our design, differences in the refresh rate of the task set in WM could drive the differences we see in the magnitude of the prospective task-set-interference effect as a function of task demands. If this were indeed the case, however, future studies should endeavor to detect a main effect on behavioral performance of whether the storage of task sets is intended for implementation or recall. Though not clearly seen in the current data, this would demonstrate the proposed modulation of refresh rates for task sets held in WM as a function of the overall differences in task demands induced by the prospective use for a task set.

It is also interesting to consider what kind of learning process might drive the adjustments in how task sets were being encoded in the present experiments. Because each miniblock involved a unique set of S–R mappings, the adaptation to different proportions of how instructions are to be used clearly cannot be related to low-level S–R learning processes. Instead, one could conceive of the present findings as representing an instance of "control learning," such that high-level cognitive parameters, such as how instructions are encoded in WM, become associated with contextual cues (in the present case, with a temporal context). This type of learning—linking basic associative learning processes with cognitive-control operations—has received considerable attention recently, as it breaks down the traditional dichotomy between top-down control and bottom-up associative processing (e.g., Abrahamse, Braem, Notebaert, & Verguts, 2016; Egner, 2014). The current work arguably contributes a novel finding to this literature by showing that the process of binding declarative to procedural representations in WM can be subject to contextual learning.

Although not especially critical to our hypothesis, the results of the inducer task (i.e., to either recall or implement the instructed task set) displayed an expected pattern. There was an expected main effect of the prospective use, such that when asked to recall an instructed task set, participants were slower in doing so than when asked to implement an instructed task set. We find it interesting that, in the between-participants design of Experiment 1, there was also a main effect of the between-subjects degree of prospective-use-bias manipulation, such that as the certainty of a particular prospective use increased, so did the accuracy in implementing or recalling the task set. This complements the interpretation of the ability to implement proactive strategic control at encoding to adjust the strength of procedural–declarative WM associations. Because an instructed task set was more likely to be implemented than to be recalled, potentially, the simultaneous increasing strength of procedural–declarative WM associations would subsequently facilitate the implementation of that task set, leading to higher accuracy. Conversely, the likely diminishing strength of procedural–declarative WM associations that accompany the increased likelihood of recalling over implementing a task set would lead to higher recall accuracy, as participants would be less reflexively influenced by strong S–R associations. Although only suggestive, a similar pattern of results was also seen in the analysis of RTs during the inducer phase, as well as in the RT and accuracy results of the inducer phase in Experiment 2.

Conclusion

Recent studies have begun to investigate the effects of procedural WM on nominally unrelated behavior by measuring interference elicited from instructed task sets on intervening tasks. Prior work has indicated that maintaining task sets in WM, despite differing prospective uses for the instructed task set, led to behavioral interference—the prospective task-set-interference effect. The present study has demonstrated how strategic control can be proactively implemented at the time of encoding of an instructed task set in WM, based on whether that task set is expected to be recalled or implemented, resulting in the modulation of prospective task-set interference. Within the prevailing model of procedural WM (Oberauer, 2009, 2010), this suggests that the condition–action associations between procedural and declarative WM are updated in accordance with their expected utility, proactively modulating the strength of the associations at encoding on the basis of prospective task demands.

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