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Task interference from event-based intentions can be material specific

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Task interference occurs in prospective memory tasks when an intention deleteriously affects performance on an ongoing activity in some way. Several studies have shown that task interference can manifest itself in slower latencies to perform an ongoing task. Recent evidence demonstrates that associating intentions to certain performance contexts affects prospective memory performance (see, e.g., Cook, Marsh, & Hicks, 2005). In the present study, an intention was associated with a particular stimulus class, such as pictures or words. We found that task interference could be reduced when participants could reliably predict that the material about to be processed was irrelevant to the intention. This material-specific interference effect was found on a trial-by-trial basis in a random sequence of two different kinds of materials across two experiments and with blocking manipulation in another experiment. These results demonstrate that task interference is not a monolithic construct; rather, it results from dynamic and flexible attentional allocation strategies that can change on a trial-by-trial basis.

Studies concerned with the issues of how people remember to fulfill their intentions use the umbrella term *prospective memory* to refer to a diverse set of cognitive processes. These processes include assessing the passage of time, detecting a cue related to a previously formed intention, deciding whether one will need a reminder to fulfill the intention, storing the intention in memory, mentally reviewing active intentions, prioritizing and canceling intentions, and so forth. As this incomplete list highlights, everyday prospective memory is a complex and challenging subject that cognitive psychology as a field has barely begun to grapple with. To date, most work has been conducted on event-based prospective memories in which some simple environmental cue can remind a person that an opportunity to fulfill the intention has arisen. In laboratory studies, detecting such a cue requires an overt response by the participant that can be coded as a successful detection. In the real world, such detection does not necessarily mean that the intention will be fulfilled. Rather, the cue can set in motion a whole other set of cognitive processes, including verification that this is the correct opportunity to respond, any additional coordination processes associated with temporarily suspending the current task at hand (see, e.g., Marsh, Hicks, Cook, Hansen, & Pallos, 2003), and, finally, a whole set of output-monitoring processes that can be used to avoid making repetition and omission errors (e.g., Einstein, McDaniel, Smith, & Shaw, 1998; Marsh, Hicks, Cook, & Mayhorn, in press; Marsh, Hicks, Hancock, & Munsayac, 2002).

One issue that has arisen concerning event-based prospective memory is a debate about the degree to which detecting a cue requires more centrally mediated attentional resources or whether detecting a cue can occur automatically. According to the multiprocess view, the interaction between a particular intention and the ongoing task in which prospective cues are encountered produces a continuum of task constellations that at one end are indeed automatic, and at the other end require considerable cue-focused analysis (McDaniel & Einstein, 2000; McDaniel, Guynn, Einstein, & Breneiser, 2004). By contrast, the preparatory attention and memory model (PAM) argues that all event-based tasks require some attention-demanding analysis on the cues in order to detect them (Smith, 2003; Smith & Bayen, 2004). The standard of evidence in this debate has involved examining reaction time (RT) costs on the ongoing task that result from having an intention versus not having one (see, e.g., Einstein et al., 2005; Hicks, Marsh, & Cook, 2005; West, Krompinger, & Bowry, 2005). As such, longer latencies in a condition with an intention—in comparison with those in a condition without an intention—indicate that attention may be required for cue detection, or at least that participants believe that it

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is required because they have changed their attentional allocation policies accordingly. We have labeled this cost to the ongoing task performance *task interference*. Einstein et al. have provided evidence that cue detection can be either more automatic or more effortful. Furthermore, even in cases when it is automatic, high rates of cue detection can nevertheless be obtained.

The issue that transcends the question of which theory (multiprocess vs. PAM) is more accurate concerns specifying the conditions in which a task interference effect is obtained and the conditions in which that cost is not obtained. Recently, we have shown that linking intention completion to a distinct, future context eliminates any task interference during an intervening period before that context appears (Marsh, Hicks, & Cook, 2006). For example, if one expects to deliver a message to a colleague after lunch, having that intention should not produce any significant cost to ongoing activities performed prior to returning from lunch. The purpose of the present study was to examine a different but related research question about whether task interference can exert a material-specific effect rather than a general effect, as has been shown in all previous studies. We approached the question in the following way: When participants are given intentions about either words or pictures, will task interference be specific to that class of items, or will it be more general and exert its influence on both types of items? Our view of task interference has been that it reflects an attentional allocation policy established by participants at the time they encode the intention, with more local variations in the waxing and waning of attention potentially affecting one or both of the ongoing and prospective tasks (Marsh, Hicks, & Cook, 2005). In a simplified form of the argument, we believe that participants make a subjective assessment of how difficult it will be to perform well on both the ongoing task and the prospective memory tasks and then adjust the "weight" of each task accordingly. This policy can continue to be adjusted as experience with the ongoing task is gained, or as attention waxes and wanes more naturally.

Guynn (2003) also believes that task interference may have two components. In her work, she cued participants with the numeral 2 on some trials to indicate that participants would be performing an ongoing short-term memory task and tracking an asterisk on the screen. On other trials, she cued them with the numeral 3 to indicate that they would also have to perform the prospective memory task of finding fruit words in the ongoing short-term memory task. She argued that when her participants had an intention, they were in a retrieval mode or a state of readiness for detecting fruit words. This is one component of task interference that we would call a *general interference effect*, and it is entirely consistent with the PAM model that argues for single preparatory allocation of attention away from the ongoing task. By contrast, when the numeral 3 appeared, Guynn found an additional slowing that she attributed to a checking process on that trial in comparison with trials that had been cued with a numeral 2. Thus, like Marsh et al. (2005), Guynn believes in a two-component account of task interference; the main difference in the accounts is in the nature of the specific interference, not the more general component. Guynn believes the interference is due to checking, whereas we believe that it is composed of more local attentional allocation policies that are subject to not only material-specific processing, but also the natural waxing and waning of attention over time. The former—material-specific processing—is what we sought evidence for in the present study.

The question that we specifically addressed in this study was whether task interference is a relatively constant cost to all trials in a laboratory task, or whether participants are able to avoid it when the material being processed is obviously not relevant to the intention at all. For instance, participants in the first three experiments of this study were asked to name a series of pictures and words. In one condition, we asked them to press an extra key if a word denoting a piece of furniture appeared, and in a different condition we asked them to press the key if a picture of a piece of furniture appeared. Although the multiprocess view might not make definitive predictions, the PAM model might clearly assert that task interference would be present on all trials—that is, on both pictures and words—because Smith and Bayen's (2004) model sets a single preparatory attention parameter that is equal for all trial types (see p. 759). That is, there is no mechanism in PAM as of this writing to allow for different amounts of preparatory attention on different trial types. Thus, if conditions can be found in which task interference is reduced either on pictures with an intention about words or on words with an intention about pictures, then that outcome might inform both the current version of the PAM model and future modifications to the multiprocess view. Beyond the theoretical consequences of such an outcome, its empirical contribution can be used to understand when everyday intentions may exert some interference and when they may not. For example, such a material-specific interference effect would suggest that having an intention to send an electronic message may interfere when one is working at the computer or dealing with e-mail tasks, but not when one is reading, teaching, or having a conversation with a student or colleague.

On the basis of the results of Marsh, Hancock, and Hicks (2002), it may be asking too much of prospective memory processes to suspend task interference on a trial-by-trial basis with a randomly mixed list of pictures and words. Marsh, Hancock, and Hicks found that event-based cue detection was worse when participants had to randomly switch back and forth between two ongoing task judgments than when they made one judgment throughout. We argued that the uncertainty of what judgment needed to be made—in combination with randomly loading production rules—exerted a central executive cost that reduced event-based performance. In a similar way, a mixed list of pictures and words would not allow participants to predict what type of stimulus item (picture vs. word) might appear next, thus not allowing the cognitive system any means to avoid a more general task interference effect. By contrast, if the pictures and words were presented in short, alternating blocks, participants could readily predict what stimulus class they were going to have to process, and that situation might potentially enable the task interference effect to be attenuated when participants were processing material that was clearly unrelated to a previously established intention. For these reasons, in Experiment 1A the pictures and words were randomly assigned to trials, whereas in Experiment 1B they were blocked in groups of 10 pictures and 10 words in alternating fashion. If Guynn's (2003) assertions are correct—that task interference comes from poststimulus checking—then interference from checking would be found in Experiment 1A as well as in Experiment 1B. By contrast, if our assertions are correct—that changes in latencies represent a material-specific change in attentional allocation strategies—then no reduction would be found in Experiment 1A for the reasons cited previously in this paragraph; however, they would materialize in Experiment 1B. Experiments 2 and 3 sought to generalize the results from these first two experiments.

EXPERIMENTS 1A AND 1B

Method

Participants. Undergraduate students from the University of Georgia volunteered in exchange for partial credit toward a course research requirement. Each participant was tested individually in sessions that lasted approximately 25 min. A total of 118 participants were randomly assigned to one of two between-subjects conditions in each experiment (cell *n*s are given in Table 1). Within each experiment, one condition was given an intention about pictures, and the other was given an intention about words.

Materials and Procedure. The ongoing activity in these two experiments was a naming task. Either a picture or a word appeared in the center of the computer monitor, and the participant's task was to speak the word or name the picture as quickly and accurately as possible. The stimuli were 180 items taken from the Snodgrass and Vanderwart (1980) norms. We excluded items that—on the basis of our experience over the years—participants found difficult to name (e.g., a picture of a cloud) or whose name agreement had been in question. For a given participant, the presentation of the word or the picture corresponding to a particular concept (e.g., *train*) was entirely random. The software assigned the 180 objects to trials in a random fashion; then it randomly assigned whether the objects would be presented as a picture or word (in Experiment 1A). In Experiment 1B, the assignment of concepts to trials was random, but the software assigned words and pictures in blocks of 10 alternating trial types (10 pictures, 10 words, 10 pictures, etc.). As such, the only difference between Experiment 1A and Experiment 1B was whether the sequence of pictures and words was random or alternating in short blocks of trials, respectively.

The 180-trial experimental sequence was divided into two phases of 90 trials each. The first 90 trials served as a baseline measure of naming latency with no event-based intention being active. During this phase, participants received 50 words and 40 pictures to name. When this phase concluded, an instruction screen appeared that delivered the event-based intention to respond to items that belonged to the category of furniture. In the word intention condition, the instructions clearly indicated that only words denoting furniture should receive a "/" key response after speaking the word. Likewise, in the picture intention condition, the instructions were clear that any picture depicting a piece of furniture should receive the "/" key response. Obviously, with a word intention no pictures of furniture appeared, and with a picture intention no word denoting furniture appeared. Moreover, there were only four event-based cues, and they were the same in each condition: *desk*, *table*, *couch*, *bed*. The example given was *chair*, and it was not subsequently tested. In this second phase—when the intention was active—participants received 40 words and 50 pictures. In this experiment and those that follow, a 3-min distractor task preceded the initiation of the second phase of the experiment to deter the prospective memory task from becoming a vigilance task.

Some readers may be concerned that the task interference effect from having an intention in the second phase of the experiment might have represented an order effect of being fatigued, or that our measure of interference underestimated task interference because of practice effects that increased RTs across the experimental sequence. However, our hypothesis predicted an interaction between the slowing on one kind of trial relative to the other kind of trial. Consequently, any practice or fatigue effect would presumably influence all trial types identically and not affect the relative speed of one trial type as opposed to another. Consequently, the design we chose was appropriate to test the hypotheses that we were investigating.

Each trial began with a message that read *waiting*. Participants were instructed to press the "N" key (for *next*) to indicate that they were ready to name the next stimulus. Doing so initiated a warning tone and fixation point for 250 msec. The fixation point was replaced with the picture or word to be named. Participants spoke their responses into a boom microphone, which had been calibrated at the beginning of the experiment to their normal speaking level. The calibration entailed adjusting the gain on a pre-amplifier that was attached to a Lafayette Instruments voice key. The microphone sat off to the right side of the participant's head so that it would not obstruct the view of the computer monitor. The voice key was attached to the computer that recorded the latency between the stimulus onset and voice key activation. Because each trial was self-initiated, the prospective memory response was to press the "/" key during the waiting message that followed a furniture item.

Table 1
Proportion of Cues Detected and Average Latencies at Baseline and for Possessing an Intention for Experiments 1A–2, With Interference Effect Calculated as the Latter Minus the Former

			Word Stimuli			Picture Stimuli		
Condition	*n*	Prospective Memory	Baseline	Intention	Interference	Baseline	Intention	Interference
Experiment 1A								
Word intention	30	.91	606	694	88	713	870	156
Picture intention	31	.97	564	643	80	702	860	158
Experiment 1B								
Word intention	28	.86	569	637	67	774	820	46
Picture intention	29	.95	556	573	16	762	848	86
Experiment 2								
Word intention	32	.94	625	698	73	717	863	146
Picture intention	30	.98	619	672	53	696	879	183

Results and Discussion

Unless specifically noted otherwise with a p value, no statistical test had a Type I error that exceeded 5%. Latencies below 300 msec were discarded as mistriggers of the voice key (coughing or throat clearing), and the remaining trials were trimmed by item type on a participant-by-participant basis. Thus, word latencies greater than 2.5 *SD*s from a participant's grand mean on word trials were eliminated, and the same was done for pictures. Prospective memory cue trials were not included in the latencies, and there were no false alarms to exclude in either this experiment or those that followed. Total data loss from these conventions was at an acceptable level of 4.8%. There were exceptionally few late prospective memory responses; therefore, in keeping with our convention, we treated these as undetected cues (however, how they were treated does not change the interpretation of the results). The data are summarized in Table 1. After listing the number of participants, the first column reports the proportion of cues that were detected. The next three columns provide the average latency on word stimuli for the baseline (Phase 1) trials, and then the trials where an intention was active (Phase 2), followed by the task interference effect, which is the difference between the two phases (i.e., Phase 2 latencies − Phase 1 latencies). Finally, the same three measures are provided for the picture stimuli.

Cue detection was analyzed with a 2 (experiment) × 2 (intention: picture vs. word) ANOVA model. Only the main effect of intention was statistically significant [$F(1,114) = 4.91$, $MS_e = 0.034$]. Replicating the results of McDaniel, Robinson-Riegler, and Einstein (1998), cue detection was higher when the cue occurred as a picture than when it occurred as a word. As can be seen in Table 1, baseline latencies were longer for the picture stimuli; thus, they were on the screen longer, which may partially account for the better cue detection. However, the fact that the data corroborate a finding with an unspeeded task suggests that pictures just provide more semantic information and trigger the intention better.

Not surprisingly, the overall latencies were slower in the second stage when the intention was operative than in the baseline stage [$F(1,114) = 336.66$]. Thus, a task interference effect was obtained. The data of primary interest concern the task interference effects that were calculated as the difference between latencies at baseline and when the intention was active (i.e., Phase 2 − Phase 1). These are the two columns labeled *Interference* in Table 1. We analyzed those difference scores with a 2 (experiment) × 2 (intention) × 2 (stimulus class: picture vs. word) ANOVA. The three-way interaction was statistically significant [$F(1,114) = 7.26$, $MS_e = 3,325.32$]. Although many of the other terms in the model were statistically significant, our focus is on the highest order interaction. To explore the nature of this interaction, we analyzed each experiment separately with 2 (intention) × 2 (stimulus class) reduced ANOVA models. In the case of randomly intermixed pictures and words in Experiment 1A, naming pictures produced more interference than did reading words [$F(1,59) = 52.70$, $MS_e = 3,160.61$], but neither the effect of intention nor the critical interaction was statistically significant [$F(1,59) < 1$ for both]. If task interference were due to a poststimulus checking account (Guynn, 2003), we found no evidence for it in this experiment as we should have. We believe that Guynn obtained the results that she did because the numeral 2 versus the numeral 3 served as a reminder of the prospective memory task every time the numeral 3 appeared. Consequently, the checking component could have been induced under such procedures, whereas our participants never received such explicit reminders of the prospective memory component of the task set.

By contrast, in Experiment 1B with the blocked stimuli, the critical interaction between intention and type of stimulus material was statistically significant [$F(1,55) = 16.75$, $MS_e = 3,502.01$]. Therefore, in Experiment 1A with randomly intermixed trials, both the word intention and the picture intention caused the same amount of slowing for a given stimulus class [$t(59) < 1$, n.s., for both intentions]. However, when the stimuli were blocked (Experiment 1B), a word intention interfered more than an intention about pictures for naming the words [$t(55) = 3.89$]. The opposite was true for naming pictures: An intention about pictures caused greater interference on the picture stimulus class than did an intention about words [$t(55) = 2.07$]. In other words, we found evidence that an intention can selectively interfere with materials related to that intention and interfere less with other materials if participants know when stimuli of certain classes are going to be encountered.

In final analyses, we compared the interference effects to zero using one-sample t tests (which were equivalent to the intercept analyses from the reduced ANOVA models). In all cases, we found statistically significant interference. These analyses thus support the PAM model, because even when the interference effect was reduced to 16 msec it was still statistically above zero. What does not support PAM is the variation among the means of the task interference effect. That model has a single attentional parameter—albeit derived in the context of a multinomial model for cue detection—and the latency data argue that attentional allocation policies changed in Experiment 1B as a function of the material being processed.

EXPERIMENT 2

The results of Experiments 1A and 1B suggest that when people can predict what type of material is going to be processed—and when that material is not related to an intention—task interference can be reduced modestly. Notice that task interference was not entirely absent; consequently, only a portion of the task interference effect is material specific. The remaining portion seems to be more or less a fixed cost on the ongoing activity. The blocking manipulation in Experiment 1B allowed people to predict on the vast majority of trials whether the material was relevant to their intention. As we said earlier, it was probably too much to ask for in Experiment 1A that a reduction be found on a trial-by-trial basis given only the material presented on that trial. However, if participants were warned before the trial began as to what kind of material they were about to process, we predicted that we might be able to find the reduction in material-specific interference even when the trial sequence was random, as it was in Experiment 1A. In Experiment 2, we replaced the asterisk fixation point with the word *picture* or *word* to indicate what kind of material would have to be named on that trial. If this warning reduced task interference when the upcoming trial was not associated with the intention, then it would suggest that people can somewhat flexibly change their attentional allocation policies on a trial-by-trial basis under some circumstances.

Method

Participants. Undergraduates from the University of Georgia volunteered in exchange for partial credit toward a course requirement. Each participant was tested individually in sessions that lasted approximately 25 min. Participants were randomly assigned to either the word intention condition or the picture intention condition, with 32 and 30 volunteers tested, respectively.

Materials and Procedure. The procedural details of this experiment were identical to those in Experiment 1A in every manner except one. The asterisk fixation point was replaced with the word *picture* or *word* to indicate the type of material that was to be named. In addition, participants were instructed that these cues would allow them to anticipate what kind of material they would have to name on the upcoming trial. The fixation point had been displayed for 250 msec in Experiments 1A and 1B, but this duration was increased to 750 msec in Experiment 2 to ensure that participants had time to read the word and engage in any anticipatory cognitive processing that this manipulation was intended to evoke.

Results and Discussion

The results can be found at the bottom of Table 1, and they are summarized in the same manner as was used for Experiments 1A and 1B. Under normal circumstances, the 4% difference in event-based prospective memory performance would not even approach significance as it did here [$F(1,60) = 3.58$, $p = .06$, $MS_e = 0.01$]. However, performance is at ceiling, so there is a restriction in the amount of variability. The task interference effect in moving from baseline to the performance interval of having an intention was statistically significant, as it was in Experiments 1A and 1B [$F(1,58) = 332.97$, $MS_e = 2{,}347.37$]. We analyzed the task interference effect in the same manner as before, using a 2 (intention: word vs. picture) × 2 (stimulus class: picture vs. word) ANOVA model. The critical interaction between the intention held and the type of material being processed was statistically significant [$F(1,60) = 8.58$, $MS_e = 2{,}376.66$]. This interaction arose for the same reason that it did in Experiment 1B; namely, task interference was greater on the word stimuli for participants who held an intention about words, and task interference was greater on picture stimuli for participants who held an intention about pictures. We are dealing with a very subtle effect, because the simple effects analysis on word interference was not statistically significant [$t(60) = 1.46$] and was only marginally significant for the picture stimuli [$t(60) = 1.91$, $p = .06$]. Although Smith (2003) found large effects on the order of hundreds of milliseconds, our effect sizes have always been more modest. Because the pattern of data replicated that of Experiment 1B, we are not that concerned that the simple effects were meager with the modest sample sizes used here.[1] As in Experiment 1A, the task interference effect was greater with pictures than with words [$F(1,60) = 135.86$].

One way to think about the more specific versus general slowing is to compare the reduction in interference within the column labeled *Interference* with the overall magnitude of interference. In Experiment 2, we found a 20-msec reduction on words (73 − 53) and a 37-msec reduction on pictures (183 − 146). This represented approximately a 20% reduction, in comparison with an average reduction of approximately 60% in Experiment 1B. Thus, the blocking manipulation used in Experiment 1B was more effective at reducing material-specific slowing than was the fixation-point warning that was used in the random trial sequence of this experiment. We attribute the difference to the residual uncertainty of the random trial sequence in this experiment, and we have observed a similar (unpublished) effect with interference in a time-based intention. Participants who are asked to respond to animal words when a clock reads an odd minute will exhibit less interference during even minutes. However, the same intention to respond when the 10-min column of a digital clock is odd does not result in any less interference. There must be a limit to the advantage of switching attentional allocation policies, and it is obviously more advantageous with blocking.[2] In the one-sample *t* tests against zero, all four interference effects were statistically significant. Consequently, the existence of overall task interference is consistent with the PAM model; however, what again seems to be inconsistent is the attentional allocation difference due to material-specific processing.

EXPERIMENT 3

The results of Experiments 1B and 2 suggest that under certain circumstances participants can adjust their attentional allocation policies flexibly. In this next experiment, we sought to generalize our findings to other tasks and conditions in which we might reasonably expect to find that task interference would covary with being able to predict the sort of material that would appear on an upcoming trial. In this experiment, we gave participants an intention to respond to animals (changed from the category of furniture) printed in a red or a green font. The ongoing task was a lexical decision task during which the color of the asterisk fixation point was perfectly correlated with the color of the letter string on the upcoming trial. If the results from Experiments 1B and 2 would generalize to this new type of intention involving color, then task interference should be reduced on green stimuli when holding an intention about animals printed in red, and on red stimuli when holding an intention about animals printed in green.

Because all stimuli were presented in either a red or green font, this new design had a different sort of baseline—namely, the RT to the other color not specified in the intention about animals. In the previous experiments, the reduction in interference was attributable to a difference in the intention, and in this next experiment, the reduction would be attributable to the kind of stimulus being processed. Both reductions reflected a material-specific processing effect, and the interaction was critical—not how it was obtained. All that was lost by not testing a neutral baseline without an intention was a demonstration that task interference occurred. However, we have repeatedly shown this pattern elsewhere (e.g., Hicks et al., 2005; Marsh et al., 2003) as have others (e.g., Smith, 2003), and we have shown it herein across Experiments 1A–2. Consequently, we feel that this design did generalize the results beyond the conditions tested in Experiments 1B and 2.

Method

Participants. University of Georgia undergraduates volunteered in exchange for partial credit toward a course research requirement. Each participant was tested individually in sessions that lasted approximately 30 min. Approximately half were given an intention to respond to animal words printed in red, whereas the other half were given the same intention but the specified font color was green. The data from two participants with an aberrantly high error rate were discarded. Sample sizes are located in Table 2.

Materials and Procedure. The parameters of the lexical decision task conformed closely to our many previous uses of it in our prospective memory research (e.g., Hicks et al., 2005; Marsh et al., 2003; Marsh, Hicks, & Watson, 2002). Although the details can be found in these previously published reports, we will briefly recapitulate them here. There were 210 trials, with 105 of them using valid English words and the other 105 using pronounceable nonwords. Each trial began with a red or green asterisk that was consistent with the color of the letter string on the upcoming trial. The appearance of the asterisk was accompanied by a warning tone for 250 msec; then the asterisk remained alone for an additional 500 msec (i.e., 750 msec total, as in Experiment 2). The asterisk was replaced with a letter string, and the participant had been instructed to press one of two labeled keys to indicate whether the string was a valid English word or not. After a response, the string disappeared immediately and a 1.5-sec intertrial interval (ITI) with a blank screen ensued. The length of this ITI was chosen because it allowed participants enough time to comfortably make their prospective memory response before the next trial was initiated. They had been instructed that when an animal word of the appropriate color appeared, they should make their word response and press the "/" key afterward.

There were eight animal words, and all eight appeared in the correct color of the participant's assigned intention (i.e., there were no lure trials as in West & Craik, 1999). These were assigned to the trials numbered 25, 50, 75, and so on, through trial 200. Participants read instructions from the computer monitor, which were then verbally reiterated by the experimenter. Consistent with the vast majority of our prospective memory experiments, a distractor task was administered for 4 min before the ongoing task was commenced without any reminder of the prospective task.

Table 2
Proportion of Cues Detected and Average Latencies for Red and Green Stimuli in Experiment 3, With Interference Effect Calculated as the Color Specified in the Intention Minus the Other Color

Intention	*n*	Prospective Memory	Red	Green	Interference
Red animals	27	.89	769	747	22
Green animals	28	.82	804	822	18

Results and Discussion

The data were trimmed in the same manner as that described for Experiments 1A and 1B. Incorrect responses were not analyzed. Data loss from these procedures was at an acceptable 3.6%. The data are summarized in Table 2. There was no difference in cue detection as a function of holding the red versus the green intention about animal words [$t(53) < 1$, n.s.]. The column labeled *Interference* in Table 2 is provided as a comparable figure to those displayed in Table 1; however, in this experiment we mainly analyzed the raw latencies to green and red stimuli (i.e., not the derived interference score). In the 2 (intention: red vs. green) × 2 (stimulus: red vs. green) ANOVA, the two-way interaction between stimulus type and intention was statistically significant [$F(1,53) = 11.91$, $MS_e = 907.81$]. When holding an intention to respond to red animals, red stimuli received slower latencies than did green stimuli [$t(26) = 3.09$]. When holding the intention to respond to green animals, green stimuli were responded to more slowly than red [$t(26) = 1.96$, $p = .06$]. Although one of the simple effects analyses was in the marginal range, there was no difference in the amount of interference created by the two intention conditions (see the column labeled *Interference* in Table 2) [$t(53) < 1$]. The amount of interference was statistically above zero with the red intention [$t(26) = 3.09$], but with the green intention it was marginally significant [$t(27) = 1.96$, $p = .06$]. In sum, material-specific slowing was demonstrated once again, albeit under different conditions.

GENERAL DISCUSSION

The general message suggested by these experiments is that task interference is not a monolithic construct; rather, it can be influenced in subtle ways by subtle changes in what appear to be flexible and dynamically changeable attentional allocation policies. Experiment 1A simulated a task situation where participants were unable to modify their attentional policies. In this case, we believe that they set some policy at the outset of Phase 2 after receiving the intention, and this policy remained in effect for the duration of the experiment. That sort of policy is what Marsh et al. (2005) labeled a more global, metacognitive strategy for dealing with the entire task set (i.e., both the naming and prospective tasks), and it is consistent with Smith's (2003) PAM account. As such, we are arguing that part of the task interference effect comes from participants' beliefs about how easy or difficult it will be to successfully complete the ongoing activity and the prospective memory task together (i.e., the entire task set). Metacognitive beliefs that the task will be difficult result in more interference, and beliefs that it will be easy result in less interference. Undoubtedly, this component of attentional policy setting is subject to individual differences. Perhaps this is why Einstein et al. (2005) were able to identify two groups of people who either showed no task interference or showed it significantly; nevertheless, both groups detected equivalent numbers of prospective cues.

In addition to what may be a more constant or "sticky" component of an attentional allocation policy, the results of Experiments 1B–3 demonstrate that there is another, more flexible component of that policy. When one is able to predict what sort of material is about to be processed and when that material is clearly unrelated to an existing and active intention, it can be processed more rapidly. Whether this is a conscious or unconscious change in people's approach to the task is unclear until further research can be conducted. The greater interference on intention-relevant materials might be caused by some unconscious activation of the intention when the upcoming material is relevant. That activation consequently leaves fewer resources to perform the ongoing task.

Another means by which the effect could be unconscious might occur through a specific decision at the outset to process one class of items preferentially (i.e., to take more time with the intention-related material). This account seems less likely, however, given the results of Experiment 1A where the same material-specific interference should have been found, but was not. As we argued earlier, the decision to take more time for a certain class of material is consistent with the account offered by Guynn (2003) for a general retrieval mode causing some interference that is augmented by a poststimulus processing check that also slows latencies. Note that the magnitude of slowing in Experiment 1A is greater than that in Experiment 1B. Therefore, not only was there no stimulus-specific slowing that would be predicted by a checking account, the overall slowing was greater—indicating that this outcome is more consistent with the general component of task interference. To be clear, we are not saying that Guynn's account is incorrect, only that it does not seem to be operative in the current paradigm.

The greater interference on intention-related material could reflect a conscious strategy of processing the material more carefully, as would be the case if the ability to predict the intention served as an explicit reminder about the intention. The very high level of cue detection in these experiments is consistent with such an account of being reminded. However, when people are stopped and asked what they are thinking about in an event-based prospective memory study, they rarely report thinking about the intention (Reese & Cherry, 2002), which undercuts this version of the locus of the effect's being conscious. Another finding also undercuts the effect's being due to a conscious reminding effect; namely, only very specific reminders that contain both the cue and the target action to be performed actually change detection performance (Guynn, McDaniel, & Einstein, 1998).

The fact that attentional allocation policies change depending on the material being processed seems to us to be somewhat inconsistent with the PAM model. In that theory, two parameters are determined from a multinomial model of cue detection: one for memory and one for preparatory attention. Manipulating variables that should affect one or the other component does indeed influence these parameters in reliable and sensible ways. Variations in preparatory attention that happen over the course of the experiment are not currently captured in the model. However, the model is not intended to predict RT differences. Marsh et al. (2005) manipulated the amount of attention participants had to exert on certain trials and found cases where detection was affected and cases where it was not. PAM could be used to model that data with two different preparatory attention parameters if there were enough degrees of freedom to fit the model. Regardless of whether this material-specific slowing is ultimately determined to be conscious or unconscious, its presence argues that there are subtle but important influences on task interference that probably augment or change any attentional allocation policies that are established by metacognitive beliefs at the time of intention formation. This flexibility is what PAM cannot currently handle.

The reduction in interference for task-irrelevant material bears some resemblance to Marsh et al.'s (2006) demonstration that task interference can be eliminated during an intervening context before an intention can be fulfilled. In that study, the experiments were set up into three distinct stages. Participants were given either an event-based task or a time-based task. Some participants were not told when the cues or the time-based response window would occur. In actual fact, both occurred in the third stage. These people exhibited task interference in both the first and third stages of the experiment where response latencies were taken. By contrast, participants who were told that the intention would be relevant only in the third stage were able to avoid any task interference during the first stage, despite having an intention. The analogous effect in the present study is that people were able to reduce task interference when the intention was irrelevant for the current task demands. Marsh et al. (2006) concluded that linking an intention to a specific context alleviated task interference in other contexts. In the present study, the context can be argued to be more local when items were blocked in Experiment 1B or when the material on the upcoming trial could be predicted perfectly. One main difference between the two studies is that complete elimination of task interference was observed in the Marsh et al. (2006) study, as opposed to the mere reduction of it in the present study. Theoretically, this makes great sense. In the present case, the intention could need to be executed imminently, so it would be of some value to keep it slightly activated or available. In the Marsh et al. (2006) study, knowledge that the relevant context was temporally distal meant that keeping the intention activated was of no fundamental use until that context was encountered.

Associating intentions to specific contexts has only recently surfaced as a potentially important area of inquiry for scholars interested in how people complete their intentions (for a summary, see Marsh, Hicks, & Cook, in press). Nowinski and Dismukes (2005) had people learn one of two tasks that they would have to perform, and they embedded the prospective memory instructions in that task. After performing a block of trials of that task, participants learned about how to perform a second ongoing task. When event-based cues were embedded in each of the two tasks, more cues were detected in the task associated with intention encoding. Cook, Marsh, and Hicks (2005) had some participants associate a time-based intention with a response in a third phase of their experiment, and other participants were given the same intention but were not told when the response window would open. If the window opened when it was expected, performance was better than when participants did not know in what phase it would occur. Moreover, when the response window occurred before the expected context arrived, time-based performance was quite detrimentally affected. Against the backdrop of these two studies showing that performance can be affected by linking an intention to a particular context, there are now two demonstrations that such an association can affect task interference (the Marsh et al., 2005, study described in the previous paragraph and the present one). To the extent that task interference is correlated with prospective memory performance in some situations, understanding what influences the degree to which an intention is active enough to affect ongoing task performance should eventually allow us to specify remedies for populations that exhibit deficits in prospective memory. Of course, a necessary precursor will be to first ascertain whether these populations respond to contextual associations in the same manner as did the young healthy adults tested in the reports summarized here.

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NOTES

1. Some readers may feel still uneasy about the simple effect analysis failing to reach conventional significance. To assuage any concerns, we pooled the results of Experiment 1B and Experiment 2 and reran these analyses. With an intention about words, task interference to words (M = 70.11) was greater than when holding an intention about pictures [M = 35.21, $t(117) = 3.89$]. With an intention about pictures, task interference was greater on pictures (M = 135.43) than it was on words [M = 99.46, $t(117) = 2.28$].

2. We thank an anonymous reviewer for suggesting that we discuss this issue.

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