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What happens to an individual visual working memory representation when it is interrupted?

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Abstract

The present study tested the hypothesis that even the simplest cognitive tasks require the storage of information in working memory (WM), distorting any information that was previously stored in WM. Experiment 1 tested this hypothesis by requiring observers to perform a simple letter discrimination task while they were holding a single orientation in WM. We predicted that performing the task on the interposed letter stimulus would cause the orientation memory to become less precise and more categorical compared to when the letter was absent or when it was present but could be ignored. This prediction was confirmed. Experiment 2 tested the modality specificity of this effect by replacing the visual letter discrimination task with an auditory pitch discrimination task. Unlike the interposed visual stimulus, the interposed auditory stimulus produced little or no disruption of WM, consistent with the use of modality-specific representations. Thus, performing a simple visual discrimination task, but not a simple auditory discrimination task, distorts information about a single feature being maintained in visual WM. We suggest that the interposed task eliminates information stored within the focus of attention, leaving behind a WM representation outside the focus of attention that is relatively imprecise and categorical.

Keywords

working memory; interruption; categorical bias

Introduction

Working memory was originally conceived as a temporary workspace that is used to store information for ongoing cognitive processing, even when that ongoing cognitive processing does not explicitly require memory storage (Baddeley & Hitch, 1974). Much of the evidence for this conceptualization has come from studies of dual-task interference (reviewed by Baddeley, 1986) in which a task of interest (e.g., a language comprehension task) is performed during the delay interval of a working memory task (e.g., a digit span task). The basic logic behind this approach is that if the *interposed task* requires storing information in working memory (WM), then it should be difficult to perform this task when WM is already filled to capacity. Thus, when WM is full because of an explicit WM task, performing an

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interposed task that implicitly requires WM should result in impaired performance of the WM task (relative to a condition in which the WM task is tested alone) and/or impairment performance of the interposed task (compared to when the interposed task is tested alone).

Although this is an appealing approach, data from such dual-task interference experiments can be difficult to interpret because both the WM task and the interposed task will involve many components, making it difficult to determine which components of the tasks are actually responsible for any observed interference. For example, simply maintaining the rules for the interposed task (see De Jong & Sweet, 1994) might lead to a reduction in performance of the WM task even if the target task does not require storing information in WM. Similarly, if the WM task involves maintaining multiple items concurrently in memory, then control processes may be needed to prevent interference between these items (Ahmad, Swan, Bowman, Wyble, Nobre, Shapiro, & McNab, 2017; Emrich, Lockhart, & Al-Aidroos, 2017), and an interposed task may interrupt these control processes even if it does not require storing any information in WM (especially if the interposed task is complex). It is also possible that the mere presentation of an interposed stimulus will disrupt performance of the WM task (e.g., as a result of backward masking or automatic attention capture). Thus, the presence of interference between a WM task and an interposed task does not imply that both tasks involve storing information in the same mental workspace. Indeed, much dual-task research has explicitly focused on the control processes involved in WM rather than storage per se (e.g., Souza & Oberauer, 2017; Vergauwe & Cowan, 2015; Barrouillet, Bernardin, Portrat, Vergauwe, & Camos, 2007). The present study focused on the question of whether performing an interposed task causes interference with WM under conditions that minimize the role of control processes and rule out interference due to the mere presentation of the stimulus.

This was accomplished using the task shown in Figure 1. We asked participants to store a single *sample stimulus* (an oriented teardrop shape) in WM so that it could be reported a few seconds later, and an interposed letter stimulus was present during the retention interval on 50% of trials (*Letter-Present* trials) and was absent on the remaining 50% (*Letter-Absent* trials). In the *Attend-Letter* condition, participants were required to make an immediate buttonpress response to report the identity of the interposed letter on Letter-Present trials (and make no response on Letter-Absent trials). In the *Ignore-Letter* condition, participants were instructed to ignore the interposed stimulus. At the end of the trial, participants reported their memory of the sample stimulus by adjusting a *test stimulus* so that it matched the remembered orientation of the sample stimulus.

This design has several important characteristics. First, the WM task requires storing only one stimulus in WM, which minimizes the role of control processes in the WM task. Some control processes are still necessary (e.g., to avoid interference from the interposed letter target – see Clapp, 2010), but others are not (e.g., those involved in avoiding interference between concurrently WM representations). The interposed task was also extremely simple, further minimizing competition for high-level control processes. As a result, any interference is more likely to reflect the use of a common mental workspace, as envisaged by the original conceptualization of WM as a buffer that is used for performing tasks that do not explicitly require memory storage.

Second, the use of a delayed estimation WM task makes it possible to assess the WM representation in a more fine-grained manner than is possible with most WM tasks. In particular, this task makes it possible to assess three different aspects of the WM representation: a) the probability that the WM representation has been completely eliminated from memory; b) the precision of the WM representation when it has not been eliminated; and c) categorical biases in the WM representation. In the present study, we were particularly interested in categorical biases because representations outside the *focus of attention* may be more categorical than representations inside the focus of attention (as will be discussed in the General Discussion). Moreover, previous research suggests that an interposed task will disrupt noncategorical WM representations (Hardman, Vergauwe, & Ricker, 2017).

The use of a delayed estimation task also made it possible to use a single sample item without running into the ceiling effects that arise in other tasks. For example, Ricker, Cowan, and Morey (2010) examined dual-task interference between a visual change-detection task and several different auditory tasks, and they found that WM performance was not impaired by the interposed tasks when the WM task required storing only a single sample stimulus. This finding could indicate that the WM representations were unaffected by the interposed tasks, but it is possible that the WM representations were degraded but still sufficient for detecting a large change between the sample and test stimuli. In terms of theory development, it is crucial to know whether interference arises only when WM is filled to capacity or whether interference arises even when only a single object (with only one relevant feature value) is being maintained in WM.

A third key characteristic of the present experimental design is that it allows us to distinguish among three types of interference: a) interference caused by task preparation; b) interference caused by the mere presentation of the interposed letter stimulus; and c) interference caused by the actual performance of the interposed task. If merely preparing to perform the interposed letter task impairs WM performance, then WM performance should be impaired on Letter-Absent trials in the Attend-Letter condition compared to the Ignore-Letter condition. If the mere presence of the interposed letter disrupts WM (as a result of masking or automatic attention capture), then WM performance should be impaired on Letter-Present compared to Letter-Absent trials in the Ignore-Letter condition. If actually performing the interposed task interferes with the WM representation, then the effect of letter presence should be larger in the Attend-Letter condition than in the Ignore-Letter condition (which is the same as saying that the effect of the attentional instruction should be greater on Letter-Present trials than on Letter-Absent trials).

Some previous dual-task research has found little or no load-dependent interference between a WM task and an interposed task when the type of information being processed for the interposed task is different from the type of information being stored in WM (e.g., Fougnie & Marois, 2006; Hollingworth & Maxcey-Richard, 2013; Hyun & Luck, 2007; Woodman, Vogel, & Luck, 2001; Woodman & Luck, 2004). However, many studies have found substantial interference even when the WM task and the interposed task involve different stimulus modalities (e.g., Allen, Baddeley, & Hitch, 2006; Hardman et al., 2017; Makovski, Shim, & Jiang, 2006; Morey & Bieler, 2013; Ricker et al., 2010; Vergauwe, Barrouillet, &

Camos, 2010). One possible explanation for this discrepancy is that almost all of these studies involved maintaining multiple items in WM and/or categorical WM tasks. Cases of cross-modality interference may have been the result of interference between control processes, not the modality of the stimuli used for the WM task and for the interposed task. For example, if the capacity of attention determines the capacity of WM (Cowan, 2001), then filling WM to capacity would reduce the attentional resources needed for processing the interposed stimulus. Such interference results are still important, but they tell us about the control processes rather than competition between different types of WM representations. On the other hand, studies in which little or no interference is found may have been insensitive to the interference owing to the use of categorical WM tasks. In these tasks, the interposed task may have reduced the precision or increased the category bias of the WM representations, but these effects may not have been large enough to produce categorical errors in the WM tasks.

The present experimental design addressed these possibilities by using continuous rather than discrete measures of WM and by requiring participants to store only a single item in WM. Experiment 1 used a visual WM task and a visual interposed task, and Experiment 2 used the same visual WM task combined with an auditory interposed task. If the effect of an interposed stimulus is independent of the modality between that stimulus and the information being remembered, then comparable interference should be observed in Experiments 1 and 2. However, if auditory and visual representations are stored separately—or if dissimilar types of information produce less mutual interference—then the interference should be minimal in Experiment 2. Moreover, if the interference effects in Experiment 1 are the result of modality-independent control processes, then similar interference effects should be obtained in both experiments.

Experiment 1

Experiment 1 examined interference between a visual WM task and an extremely simple visual discrimination task. The WM task consisted of an orientation delayed estimation task that made it possible to assess both precision and categorical biases in the WM representations (Bae, Olkkonen, Allred, & Flombaum, 2015; Hardman et al., 2017; Pratte, Park, Radenmaker, & Tong, 2017). We used an orientation memory task rather than a color memory task because the category structure of color space is complicated and requires special methods to assess (Bae et al., 2015; Hardman et al., 2017). By contrast, the category structure for orientation is quite simple and regular, with category boundaries at each of the cardinal axes (Girshick, Landy, & Simoncelli, 2011). For example, when an oriented teardrop is tilted slightly clockwise from upright, the category boundary at the 12 o'clock orientation causes the orientation representation to be shifted further clockwise, away from the category boundary (Jazayeri & Movshon, 2007; Pratte et al., 2017; van Bergen, Ma, Pratte, & Jehee 2015; Wei & Stocker, 2015).

This task was combined with a simple letter discrimination task in which participants pressed one of two buttons to indicate whether the interposed letter was a C or D (or a P or Q). The interposed letter could be present or absent (varied unpredictably within each block), and participants were instructed either to respond to the letter or ignore it (in separate blocks). If this simple task requires the same mental workspace that is used to maintain the orientation of the teardrop, then memory for the teardrop should become less precise and/or more categorical when the interposed letter is both present and task-relevant compared to when the letter is absent and/or task-irrelevant.

Method

Participants—A group of 16 college students (9 female; age range 18–30 years) with normal or corrected-to-normal visual acuity participated for monetary compensation ($10/hr). This sample size was selected a priori on the basis of our experience with previous experiments using delayed estimation procedures.

Stimuli & Procedure—Stimuli were presented on a Dell U2412M LCD monitor with a gray background (31.2 cd/m^2) at a viewing distance of 70 cm. A black fixation dot was continuously present except during the intertrial interval. The sample stimulus was a teardrop shape (3° long, 1° maximum width) presented at the center of the display. The orientation of a given target was selected with equal probability from 40 equally spaced values (separated by 9°, starting at 0° from horizontal). The interposed stimulus was selected from one of two sets of letters: {C, D} and {P, Q}.

The task is depicted in Figure 1a. Each trial began with the fixation dot. After 500 ms, the sample stimulus was presented for 200 ms, followed by a 350 ms blank interval. Participants were asked to remember the orientation of the sample stimulus as precisely as possible. On *Letter-Present* trials, the interposed letter stimulus (C or D for half the subjects, P or Q for the other half) was then presented for 200 ms. On *Letter-Absent* trials, the letter was replaced by a 200-ms blank period. In the *Attend-Letter* condition, participants were asked to immediately press the left arrow key or the right arrow key on the computer keyboard to report which letter was presented. The letter-response mapping was counterbalanced across participants. In the *Ignore-Letter* condition, a letter from the other letter set (P or Q for half the subjects, C or D for the other half) was presented but was task-irrelevant and required no response. The two letters from a given set were equiprobable. No response was required on Letter-Absent trials in either condition. An equal number of trials with each sample orientation was included for each trial type in each condition. Note that a single object is consolidated very rapidly in visual working memory (Ricker & Hardman, 2017; Vogel, Woodman, & Luck, 2006), so consolidation of the teardrop orientation would have been complete well before the onset of the letter.

After another 750-ms delay interval, a response ring appeared so that the participant could report the orientation of the sample teardrop. However, in the *Attend-Letter* condition, the response ring appeared only if the response to the interposed letter stimulus was correct and occurred before the usual onset time of the response ring. If this response was incorrect or too slow, a feedback message (“Incorrect” or “Slow”) was presented for 500 ms instead of the response ring, and the trial then terminated. This was to motivate participants to prioritize the response to the interposed stimulus.

When the response ring appeared, observers reproduced the remembered orientation of the sample teardrop using a computer mouse. The mouse pointer started at the fixation point at the beginning of the response period. Once the mouse started moving, a teardrop shape appeared at an orientation that matched the current position of the mouse. The observer then adjusted the mouse position until the teardrop matched the memory of the target shape, pressing the mouse button to finalize the report.

Participants completed one *Attend-Letter* block and one *Ignore-Letter* block, in counterbalanced order. Each block contained 240 trials Letter-Present trials (120 for each of the two letters) and 120 Letter-Absent trials.

Analysis—The analyses focused on *response error*, which was defined as the angular difference between the actual target orientation and the reported orientation on each trial. Figure 1b shows the distribution of response errors for each trial type, collapsed across all target orientations and participants. The response error was given a positive sign if the reported orientation was away from the nearest cardinal orientation, and it was given a negative sign if the reported error was toward the nearest cardinal orientation. The nearest cardinal orientation was not meaningful for the cardinal orientations themselves (0°, 90°, 180°, 270°) and was undefined for the angles halfway between the cardinals (45°, 135°, 225°, 315°). For example, the 45° orientation was equally distant from the cardinals at 0° and 90°. These orientations were therefore excluded from the primary analyses. For the remaining orientations, the response errors were collapsed for trials with the same relative distance between the sample orientation and the nearest cardinal orientation. For example, the response errors for the 9°, 81°, 99°, 171°, 189°, 261°, 279°, and 351° sample orientations were collapsed together because they were all equally distant from the nearest cardinal orientation. This produced unique 4 relative orientations—9°, 18°, 27°, and 36° from the nearest cardinal orientation—with 48 trials per relative orientation for Letter-Present trials and 24 trials per relative orientation for Letter-Absent trials in each *Attend-Letter* and *Ignore-Letter* block.

To summarize the distribution of responses for each relative orientation, we used a mixture model (Equation 1, Zhang & Luck 2008) in which each trial is selected from a von Mises (circular normal) distribution when the participant is reporting a memory of the orientation or a uniform distribution when the participant has no memory (Equation 1). The model has three free parameters: Kappa (κ) from the von Mises distribution, which represents memory precision (inverse of variance); Mu (μ) from the von Mises distribution, which represents memory bias; and P_{mem}, which represents the proportion of trials on which a memory was present. The proportion of random guesses is $1 - P_{mem}$.

$$P(r) = P_{mem} * vonMises(r, \mu, \kappa) + (1 - P_{mem})\frac{1}{2\pi} \quad (1)$$

The mixture model was fit to the response error distribution for each individual participant, separately for each relative orientation in each condition, using maximum likelihood estimation. To avoid local maxima, we searched for the parameters from multiple starting points and chose the set of parameters that produced the largest likelihood value.

Note that, because the model was fit separately for each relative orientation, the effects of category biases are quantified by examining how the μ parameter—which represents the shift of the distribution of responses toward or away from the nearest cardinal orientation—varies across relative orientations. This made it easier to quantify the amount of category bias without using a specific model that includes a category bias parameter. To confirm that the results were not driven by the specific model we used, we repeated the analyses using the mean response error rather than the μ parameter. The mean response errors showed the same pattern as the μ estimates from the mixture model (see supplementary material).

The κ and μ values were analyzed statistically using analyses of variance (ANOVAs) and *t* tests ($\alpha = .05$). To quantify the strength of evidence for and against the null hypothesis, we also computed Bayes factors using the approach of Rouder, Speckman, Sun, Morey, and Iverson (2009), with the default JZS scaling factor of 0.707. That this approach is limited to comparisons of two cells, and the corresponding approach for ANOVA designs (Rouder, Morey, Speckman, & Province, 2012) is much more complex and has not been as widely adopted and validated. Thus, to provide Bayes factors for our 2 × 2 ANOVA designs, we used averaging or difference scores to create the corresponding *t* tests. For the interaction term, we computed difference scores along one dimension and then compared these two difference scores with the Bayes factor analog of a *t* test. Note that this comparison of difference scores is mathematically equivalent to the interaction term from the 2 × 2 ANOVA. For the two main effects, we averaged across levels of one dimension (e.g., presence versus absence of an intervening stimulus) and then compared the two resulting scores along the other dimension (e.g., attend versus ignore the intervening stimulus). Bayes factors are listed as BF_{10} when the data were more likely under the alternative hypothesis and as BF_{01} when the data were more likely under the null hypothesis.

Results and Discussion

Performance of the Interposed Task—We could examine performance on the interposed task only on Letter-Present trials in the Attend-Letter condition. On these trials, mean reaction time (RT) was 471 ms (95% CI = 423–518 ms), and mean percent correct was 93% (95% CI = 91–95%). Thus, responses in this task were both fast and accurate, consistent with our goal of using a very simple interposed task.

WM Precision—When the response to the interposed stimulus was inaccurate or slow on Letter-Present trials in the Attend-Letter condition (M = 6.56%, 95% CI = 4.5–8.6%), no orientation report was required. Thus, no further analyses of the WM data were possible for those trials. The proportion of guess trials in the WM task was very low (M = .025, SEM = .005). Because guess rates were near floor, we focused on the precision and bias estimates. Analyses of the guess rates are provided in supplementary materials. There were no significant differences in guess rates among conditions.

Figure 2a shows precision (Kappa) averaged across all orientations for each combination of attentional condition (Attend-Letter versus Ignore-Letter) and letter presence (Letter-Present versus Letter-Absent). Precision was reduced when an interposed letter was present and was task-relevant (Attend-Letter/Letter-Present) compared to when the letter was absent and/or ignored. The presence of the interposed letter had little or no effect when it was ignored.

To test this statistically, the data (averaged across relative orientations) were entered into a 2-way ANOVA with factors of attentional condition (Attend-Letter versus Ignore-Letter) and letter presence (Letter-Present versus Letter-Absent). As would be expected from the pattern of means shown in Figure 2a, the attentional condition x letter presence interaction was statistically significant, $F(1,15) = 6.891$, $p = .019$, $\eta_p^2 = .314$. The Bayes factor corresponding to this interaction (computed by calculating Letter-Present minus Letter-Absent difference scores and comparing them across the Attend-Letter and Ignore-Letter conditions), we obtained $BF_{10} = 3.2$. In other words, the data were 3.2 times more likely to arise from a model with an interaction than from a model in which the effect of letter presence was the same in the Attend-Letter and Ignore-Letter conditions. The main effect of attentional condition was not significant, $F(1,15) = 2.939$, $p = .107$, $\eta_p^2 = .164$, $BF_{01} = 1.18$. The main effect of letter presence was significant, $F(1,15) = 7.022$, $p = .018$, $\eta_p^2 = .319$, $BF_{10} = 3.35$, but this appeared to be mainly a side effect of the interaction.

To confirm this and show that the Attend-Letter/Letter-Present condition produced the smallest Kappa among the four conditions, we conducted paired *t* tests comparing the Attend-Letter/Letter-Present cell to the other three cells, applying the false discovery rate (FDR) correction for multiple comparisons with an alpha level of 0.05 (Benjamini & Hochberg, 1995). All three comparisons were statistically significant after correction (versus Attend-Letter/Letter-Absent: $t(15) = 3.67$, $p = .002$, $BF_{10} = 18.86$; versus Ignore-Letter/Letter-Present: $t(15) = 3.39$, $p = .004$, $BF_{10} = 11.63$; versus Ignore-Letter/Letter-Absent: $t(15) = 2.91$, $p = .011$, $BF_{10} = 5.14$). These results demonstrate that performing the interposed letter discrimination task while holding an orientation in WM decreased the precision of the WM representation. By contrast, there was no significant difference between the Letter-Present and Letter-Absent trials in the Ignore-Letter condition ($t(15) =.92$, $p = .877$, $BF_{01} = 3.87$). When combined with the significant attentional condition x letter presence interaction in the ANOVA, these results indicate that actively discriminating an interposed stimulus leads to impaired WM precision, but the mere presence of an unattended interposed stimulus leads to little or no impairment. In addition, there was no significant difference between Letter-Absent trials in the Attend-Letter condition versus the Ignore-Letter condition ($t(15) =.97$, $p = .965$, $BF_{01} = 3.91$), indicating that preparing to perform the interposed task in the Attend-Letter condition did not cause substantial disruption of WM performance.

WM Bias—Figure 2b shows bias (μ) as a function of relative orientation for each combination of attentional condition (Attend-Letter versus Ignore-Letter) and letter presence (Letter-Present versus Letter-Absent). Overall, responses were biased away from the nearest cardinal orientation, showing the typical categorical bias in orientation memory (Pratte et al, 2017; van Bergen et al., 2015; Wei & Stocker, 2015). The main focus of our bias analyses was to test how this categorical bias was modulated by the interposed task. To simplify the analyses, we averaged the bias estimates across the relative orientations, excluding 0° and 45° because the nearest cardinal orientations are not defined for those orientations. As summarized in Figure 2c, the bias effect was greater when an interposed letter was present than when it was absent in the Attend-Letter condition, but not in the Ignore-Letter condition.

To test this statistically, the collapsed data were entered into a 2-way ANOVA with factors of attentional condition (Attend-Letter versus Ignore-Letter) and letter presence (Letter-Present versus Letter-Absent). Consistent with the observation that the presence of the letter led to increased bias only when it was attended, the two-way interaction between attentional condition and letter presence was significant, F(1,15) = 10.91, p = .005, $\eta_p^2 = .421$. The Bayes factor for this interaction (derived from difference scores, as in the Kappa analysis) yielded $BF_{10} = 10.0$, indicating that the data were 10 times more likely to arise from a model with an interaction than from a model in which the effect of letter presence was the same in the Attend-Letter and Ignore-Letter conditions. The main effect of letter presence was not significant, F(1,15) = 2.563, p = .13, $\eta_p^2 = .146$, $BF_{01} = 1.37$. The main effect of attentional condition was significant, F(1,15) = 4.737, p = .046, $\eta_p^2 = .240$, $BF_{10} = 1.50$, which may indicate that anticipating and/or preparing for the interposed task influenced memory bias.

To provide additional evidence that the largest categorical bias was observed for Letter-Present trials in the Attend-Letter condition, we conducted paired *t* tests comparing this cell to the other three cells (with FDR correction). We found that the bias was significantly greater for the Letter-Present trials in the Attend-Letter condition than for each of the other three cells (versus Attend-Letter/Letter-Absent: t(15) = 3.08, p = .008, $BF_{10} = 6.82$; versus Ignore-Letter/Letter-Present: t(15) = 3.73, p = .002, $BF_{10} = 20.97$; versus Ignore-Letter/Letter-Absent: t(15) = 2.76, p = .014, $BF_{10} = 4.02$). By contrast, there was no significant difference between the Letter-Present and Letter-Absent trials in the Ignore-Letter condition (t(15) = 1.25, p = .232, $BF_{01} = 2.03$). Moreover, there was no significant difference between Letter-Absent trials in the Attend-Letter condition versus the Ignore-Letter condition (t(15) = .80, p = .434, $BF_{01} = 2.03$, $BF_{01} = 2.95$), indicating that preparing to perform the interposed task in the Attend-Letter condition did not produce a substantial increase in the amount of categorical bias in WM.

Together, the precision and bias results indicate that actively discriminating the interposed stimulus while holding an orientation in WM makes WM performance less precise and more categorical. Remarkably, these effects were observed even though the interposed stimulus required an extremely simple discrimination, with no need to shift attention away from fixation or select a target from among concurrent distractors. However, there was little or no effect of the interposed stimulus when it could be ignored, ruling out a role for backward masking and other automatic effects of the presentation of this stimulus.

Experiment 2

Experiment 1 showed that visual WM representations become less precise and more categorical when observers perform a visual interposed task during the delay interval. To test whether this effect is modality specific, Experiment 2 used an auditory interposed tone stimulus (Figure 3a) and required a simple pitch discrimination. If the pitch discrimination requires the same mental workspace used to store the teardrop orientation, then the orientation reports should be less precise and more categorical when the pitch discrimination is performed. However, if pitch discrimination does not require the same resources as the maintenance of a visual orientation, then performing the pitch task should not make the orientation memory less precise or more categorical.

Method

The methods were the same as those in Experiment 1, except as follows. A new group of 16 college students (7 female; age range 18–30 years) was recruited. A pitch discrimination task replaced the letter discrimination task (Figure 3a). The interposed stimulus was selected from one of two sets of tones: {310 Hz, 510 Hz} and {610 Hz, 810 Hz}. After the 350 ms delay interval, the tone was presented via an external speaker for 200 ms. In the *Attend-Tone* condition, participants were instructed to make an immediate buttonpress response to indicate whether the tone pitch was high or low. In the *Ignore-Tone* condition, a tone from the other tone set (610 Hz or 810 Hz for half the subjects, 310 Hz or 510 Hz for the other half) was presented but no response was required. Tone-Present and Tone-Absent trials were randomly intermixed within the *Attend-Tone* and *Ignore-Tone* blocks.

As in Experiment 1, model-free analyses of mean response errors are provided in the supplementary material and yielded similar results to the analyses of the bias parameter from the mixture model.

Results and Discussion

Performance of the Interposed Task—We could examine performance on the interposed task only on Tone-Present trials in the Attend-Tone condition. On these trials, mean RT was 464 ms (95% CI = 410–518 ms), and mean percent correct was 88% (95% CI = 84–92%).

WM Precision—When the response to the interposed stimulus was inaccurate or slow on Tone-Present trials in the Attend-Tone condition (M = 12.37%, 95% CI = 8.7–16.0%), no orientation report was required. Thus, no further analyses were possible for those trials. The proportion of guess trials in the WM task was very low (M = .034, SEM = .01). As in Experiment 1, we focused our analyses on precision and bias, and analyses of guess rates are provided in the supplementary materials. Guess rates were near floor, and there were no statistically significant differences among conditions.

Figure 4a shows precision (Kappa) estimates averaged across all relative orientations for each combination of attentional condition (Attend-Tone versus Ignore-Tone) and tone presence (Tone-Present versus Tone-Absent). Overall, there was no visible difference in the precision across the combinations of attentional condition and tone presence. We tested this statistically using a 2-way ANOVA with factors of attentional condition (Attend-Tone versus Ignore-Tone), and tone presence (Tone-Present versus Tone-Absent). No significant main effect or interaction was observed (main effect of attentional condition, F(1,15) = .109, p = .746, BF_{01} = 3.73; main effect of tone presence, F(1,15) = .037, p = .849, BF_{01} = 3.85; 2-way interaction, F(1,15) = .067, p = .799). The Bayes factor corresponding to the interaction (calculated as in Experiment 1) was BF_{01} = 3.8, indicating that the data were 3.8 times more likely to arise from a model in which the effect of letter presence was the same in the

Attend-Letter and Ignore-Letter conditions (a null interaction) than from a model with an interaction.

To be consistent with Experiment 1, we also compared Tone-Present trials in the Attend-Tone condition to the other three cells in the design using pairwise *t* tests with FDR correction (Benjamini & Hochberg, 1995). The precision for Tone-Present trials in the Attend-Tone condition was not significantly different from any of the other three cells in the design (versus Attend-Tone/Tone-Absent: t(15) = .03, p = .976, BF_{01} = 3.91; versus Ignore-Tone/Tone-Present: t(15) = .04, p = .692, BF_{01} = 3.64; versus Ignore-Tone/Tone-Absent: t(15) = 0.15, p = .879, BF_{01} = 3.87). These results demonstrate that performing an auditory interposed task does not impact the precision of visual representations in WM. There was also no significant difference between the Tone-Present and Tone-Absent trials in the Ignore-Tone condition (t(15) = .470, p = .645, BF_{01} = 3.55), and there was no significant difference between Tone-Absent trials in the Attend-Tone condition versus the Ignore-Tone condition (t(15) = .151, p = .882, BF_{01} = 3.88).

WM Bias—Figure 4b shows bias (μ) estimates as a function of relative orientation for each combination of attentional condition (Attend-Tone versus Ignore-Tone) and tone presence (Tone-Present versus Tone-Absent). Overall, responses were biased away from the nearest cardinal orientation, replicating the categorical bias in orientation memory. To simplify the analyses, we averaged the bias estimates across relative orientations as in Experiment 1. As shown in Figure 4c, categorical bias was greater for the Attend-Tone condition than for the Ignore-Tone condition, but the bias effect was similar for Tone-Present and Tone-Absent trials.

To test this statistically, the collapsed data were entered into a 2-way ANOVA with factors of attentional condition (Attend-Tone versus Ignore-Tone), and tone presence (Tone-Present versus Tone-Absent). The main effect of tone presence was not significant, F(1,15) = .106, p = .749, BF_{01} = 3.53 but the main effect of attentional condition was significant, F(1,15) = 5.589, p = .032, η_p^2 = .271, BF_{10} = 1.48, which indicates that anticipating and/or preparing for the interposed task influenced the memory bias. Importantly, the two-way interaction between attentional condition and tone presence was not significant, F(1,15) = .865, p = .367. The Bayes factor corresponding to the interaction (calculated as in Experiment 1) was BF_{01} = 2.7, indicating that the data were 2.7 times more likely to arise from a model in which the effect of letter presence was the same in the Attend-Letter and Ignore-Letter conditions (a null interaction) than from a model with an interaction. Together, these results demonstrate that performing an auditory interposed task while holding a visual representation has little or no impact on the magnitude of categorical bias in the visual WM representation. In addition, an across-experiment comparison described below shows that the interference effect was significantly smaller in Experiment 2 than in Experiment 1.

To parallel the analyses of Experiment 1, we also compared Tone-Present trials in the Attend-Tone condition to the other three cells in the design using pairwise *t* tests with FDR correction (Benjamini & Hochberg, 1995). The categorical bias in the Attend-Tone condition did not differ between Tone-Present and Tone-Absent trials, t(15) = .55, p = .590, BF_{01} = 3.41. This result further confirms that performing the tone discrimination task had no impact on the magnitude of the categorical bias. However, the categorical bias for Tone-Present trials in the Attend-Tone condition was significantly greater than the bias on Tone-Present trials in the Ignore-Tone condition, t(15) = 3.015, p = .009, BF_{10} = 6.14, and was marginally greater than the Tone-Absent trials in the Ignore-Tone condition, t(15) = 1.992, p = .065, BF_{10} = 1.23. However, there was no significant difference between the Tone-Present and Tone-Absent trials in the Ignore-Tone condition (t(15) = .730, p = .476, BF_{01} = 3.10), indicating that the mere presence of an interposed tone had little or no effect on WM. There was no significant difference between the Tone-Absent trials in the Attend-Tone condition and the Ignore-Tone condition (t(15) = 1.638, p = .476, BF_{10} = 1.3).

Together, these results demonstrate that performing an auditory interposed task has little or no impact on visual representations in WM. There was no impact of the auditory task at all on the precision of the visual WM representations, and the pattern observed for bias indicates that the visual WM representation was altered by anticipation of the auditory task but not by actually performing the task. In other words, the fact that bias was increased in the Attend-Tone condition by the same amount on Tone-Present and Tone-Absent trials suggests that bias was impacted by the participants' task set but not by the act of discriminating the pitch of the tone. This contrasts with the bias effect observed in Experiment 1, in which the bias within the Attend-Letter condition was greater when the letter was present than when it was absent.

To provide more direct statistical evidence for this difference between visual and auditory interposed tasks, we compared the effect of interposed tasks on the orientation memory between Experiment 1 and 2. Specifically, we subjected the precision and bias estimates to separate 3-way ANOVAs with within-subject factors of attentional condition (Attend versus Ignore) and interposed stimulus presence (letter/tone present versus absent), and a between-subject factor of interposed stimulus modality (visual versus auditory). These analyses yielded a significant 3-way interaction for bias, F(1,30) = 4.897, p = .035, η_p^2 = .14, and a marginally significant 3-way interaction for precision, F(1,30) = 3.294, p = .080, η_p^2 = .01. These results indicate that categorical bias was more strongly impacted by a visual interposed task than by an auditory interposed task, with suggestive support for a modality effect on precision.

In sum, we found evidence that a simple auditory interposed task produced little or no interference with a single visual WM representation. The Bayes factors indicated that the data were more consistent with the null hypothesis than with an interference effect, and the across-experiment ANOVA indicated that the disruption of visual WM was greater for the visual task in Experiment 1 than for the auditory task in Experiment 2. However, it is important to note that the visual and auditory interposed tasks used in Experiments 1 and 2 also differed along other dimensions. For example, the visual task involved a letter discrimination whereas the auditory task involved a pitch discrimination. Thus, additional research is needed to conclusively demonstrate that the different WM effects observed in the two experiments are a result of modality per se. Nonetheless, these results provide initial evidence that performing a simple pitch discrimination task does not disrupt the representation of a single visual feature, consistent with the use of separate mental workspaces.

General Discussion

The present study sought to investigate the role of WM as a buffer for performing simple cognitive tasks that do not explicitly require memory storage. If performing a simple visual discrimination requires storing the to-be-discriminated information in WM, then this should disrupt other information being held in WM. Consistent with this hypothesis, we found that orientation memory was less precise and more categorical when a letter was discriminated during the delay period compared to when the letter was absent and/or ignored. These results are consistent with the hypothesis that performing a simple letter discrimination task requires storage of information in WM even though the discrimination task does not explicitly require memory. In contrast, this pattern was not observed when the interposed task required an auditory pitch discrimination rather than a visual letter discrimination. This result suggests that WM representations are modality-specific and that performing an interposed task does not always disrupt visual WM. Moreover, the lack of an interference effect in Experiment 2 indicates that the interference effect in Experiment 1 was unlikely to be a consequence of central (modality-general) control processes, increasing the likelihood that the interference was the result of the storage of the visual interposed stimulus in the same mental workspace used to store the sample item.

Previous studies that investigated the interaction between visual WM and an interposed task have typically involved the storage of multiple items in WM (Allen et al., 2006; Fougnie & Marois, 2006; Hardman et al., 2017; Hollingworth & Maxcey-Richard, 2013; Makovski et al., 2006; Morey & Bieler, 2013; Morey & Cowan, 2004; Ricker et al., 2010; Saults & Cowan, 2007; Vergauwe et al., 2010; Woodman & Luck, 2004; Woodman et al., 2001). Moreover, many of these studies used relatively complex interposed tasks, such as verbal memory search (Ricker et al., 2010), visual search (Woodman et al., 2001), mental rotation (Hyun & Luck, 2007) or word categorization (Vergauwe et al., 2010). Such studies can test whether *WM performance* is disrupted by the interposed task, but they are not ideal for investigating whether the same *WM storage buffer* is used for the WM task and the interposed task because the interference is likely to reflect shared control processes rather than a shared mental workspace. For example, the storage of multiple concurrent WM representations requires the individuation of these representations (Balaban, Drew, & Luria, 2018), and the individuation process may be disrupted by the interposed task. In addition, more complex interposed tasks would require multiple cognitive processes that may be involved in maintaining representations in WM. Thus, to investigate the role of WM storage for performing the interposed task, we combined WM for a single feature with simple interposed tasks, using a delayed estimation WM task to avoid the ceiling effects that might mask interference effects in other tasks. Although our results may appear to conflict with the results of other studies of dual-task interference, the present study and the previous studies are largely addressing different aspects of WM and are therefore complementary rather than contradictory.

Visual information Inside versus Outside the focus of attention in WM

Why might visual WM performance become less precise and more categorically biased when a simple visual discrimination is performed during the maintenance interval? We predicted this result on the basis of the growing evidence that information can be held in at least two distinct states in WM, which are often described as being inside versus outside the focus of attention (Cowan, 2012; Oberauer, 2002; Shipstead & Engle, 2012). We assumed that visual information inside the focus of attention is maintained by means of sustained neural activity within visual cortex and therefore provides a relatively precise, metric representation of the visual features of the stimulus. This kind of neural storage has been observed in monkey single-unit recordings, in human EEG recordings, and in human fMRI experiments (Bae & Luck, 2018; Foster et al., 2016; Harrison & Tong, 2009; Pasternak & Greenlee, 2005). In contrast, we assumed that information outside the focus of attention would be maintained either outside visual cortex (e.g., in prefrontal cortex; Stokes, 2005; Goldman-Rakic, 1995) or by means of "activity-silent" synaptic mechanisms (Rose et al., 2016; Stokes, 2015; Olivers, Peters, Houtkamp, & Roelfsema, 2011; Mongillo, Barak, & Tsodyks, 2008). Without the benefits of active neural processing within visual cortex, WM representations would likely be less precise and more categorical. Thus, we predicted that performing even a simple visual discrimination task during the retention interval of a WM task would flush the sample stimulus out of the focus of attention, eliminating the precise metric representation of this stimulus provided by active coding within visual cortex, but would leave intact a less precise and more categorical representation of the sample stimulus outside the focus of attention.

Although these ideas motivated the predictions of the present study, we have no independent evidence that performing the interposed letter task resulting in a flushing of the orientation information from the focus of attention. Future research—likely using neural measures of active WM storage—will be needed to verify this. Nonetheless, the present results do show that performing a very simple visual discrimination task during the maintenance interval of a visual WM task leads to subtle but systematic changes in the WM representations, whereas an auditory interposed task did not lead to such effects.

Attentional prioritization within WM

Because the present study used only a single orientation item and a single interposed letter stimulus, one might have expected that sufficient WM capacity would have been available for both tasks (Cowan 2001; Luck & Vogel, 1997; Zhang & Luck 2008). However, there is considerable evidence that only a single object can be held within the focus of attention in WM (Hardman et al., 2017; McElree, 2001; Oberauer, 2002; Olivers et al., 2011). This possibility is consistent with a recent study in which attentional priority was directly manipulated between two orientation representations in WM (Bae & Luck, 2017). Participants in this study were shown two serially presented orientations and were directly instructed to prioritize one of the two items. The representation of the high priority item was not influenced very much by the orientation of the low-priority item, but the representation of the low-priority item was strongly influenced by the orientation of the high priority item. However, neural measures of active maintenance suggest that approximately three items can be actively maintained at a given time (Vogel & Machizawa, 2004), and an eye tracking study indicated that at least two WM representations can concurrently control feature-based visual scanning (Beck, Hollingworth, & Luck, 2012). These findings could potentially be reconciled by proposing that multiple items can be simultaneously active in WM, but only one of the representations is precise and metric. This will be an important topic for future research.

Interestingly, some of the effects of attentional prioritization in the present experiment appeared to reflect anticipation of the upcoming interposed stimulus as opposed to the actual processing of that stimulus. In both Experiment 1 and 2, WM representations exhibited greater categorical bias during blocks in which the interposed stimulus was task-relevant, even on trials in which this stimulus was not presented. A plausible explanation for this effect would be that participants were anticipating the need to discriminate the interposed stimulus, and they prepared by preemptively flushing the orientation information from the focus of attention. This is plausible given the proposal that recognizes the brain as a 'predictive organ' that anticipates incoming sensory stimulation to guide perception and memory (Nobre & van Ede, 2018). However, the present study does not provide direct evidence the orientation representation was flushed from the focus of attention, so this possibility requires additional research.

Modality-specific interruption of visual WM

The finding that visual WM was not interrupted by a concurrent auditory interposed task suggests that attentional processing of auditory information can be achieved independently from the active maintenance of visual information in WM (at least under conditions that minimize the role of control processes). This conclusion is consistent with the general view that WM provides independent buffers for different types of information (Baddeley & Hitch, 1974). However, this could also reflect a single distributed storage system in which the degree of interference depends on the similarity between the items. The auditory tones and visual orientations used in Experiment 2 were highly dissimilar, and this may explain the lack of interference from the interposed task. Moreover, it would not be appropriate to conclude that all aspects of WM are modality-specific, because there are numerous studies showing interactions between visual WM and auditory/verbal WM (Allen et al., 2006; Hardman et al., 2017; Makovski et al., 2006; Morey & Bieler, 2013; Morey & Cowan, 2004; Ricker et al., 2010; Saults & Cowan, 2007; Vergauwe et al., 2010). Thus, a reasonable synthesis of the existing literature would be that different types of information do not interfere directly with each other in WM, but content-independent control mechanisms play an important role under many or even most conditions (e.g., when multiple items must be retained, when the retention interval is long, when complex tasks must be performed).

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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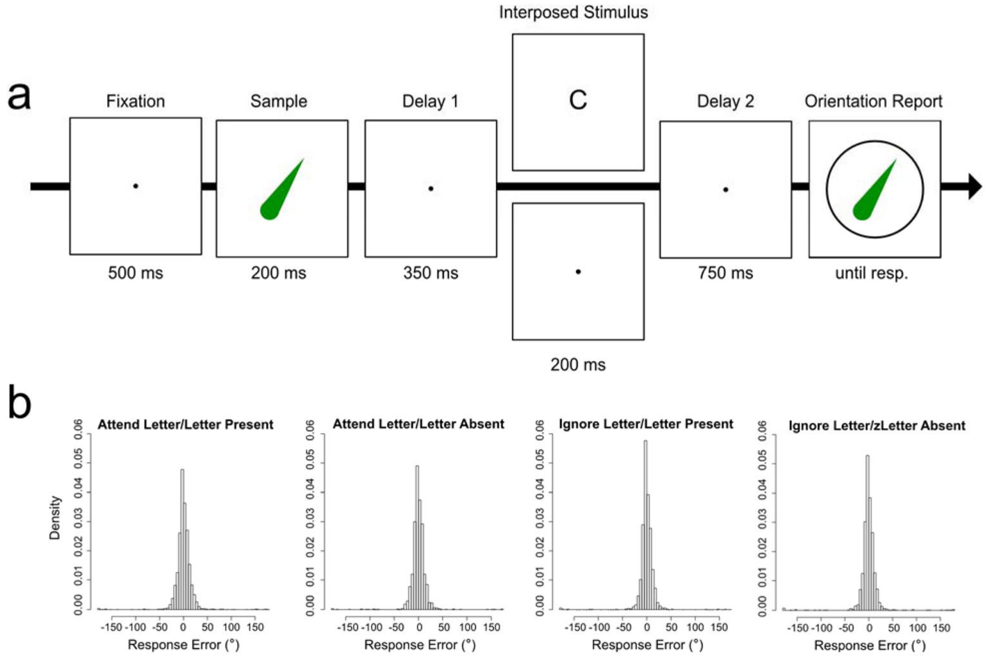

Figure 1.

(a) Example of a single trial in Experiment 1. Observers remembered the orientation of the sample teardrop and, after a delay, reproduced the remembered orientation by adjusting the orientation of the test teardrop. An intervening letter stimulus or an equivalent-duration blank period was presented during the delay. In the *Attend-Letter* condition, observers were asked to report which of two letters (e.g., C or D) was presented by means of an immediate button-press response. In the *Ignore-Letter* condition, a letter from another set (e.g., P or Q) was presented but it was task-irrelevant and required no response. No response was required when the intervening stimulus was absent, which occurred on 1/3 of trials of each condition. However, the remembered orientation of the teardrop was reported whether the intervening letter was attended or ignored and present or absent except for trials with inaccurate or slow responses for the intervening task in the Attend-Letter/Letter-Present trials. In such trials, a warning message either “Inaccurate” or “Slow” replaced the orientation report. (b) Response error distribution for the four combinations of trial type (intervening letter present versus absent) and attentional condition (attend letter versus ignore letter) collapsed across all sample orientations and participants in Experiment 1. Response error is the difference between the true sample orientation and the reported orientation.

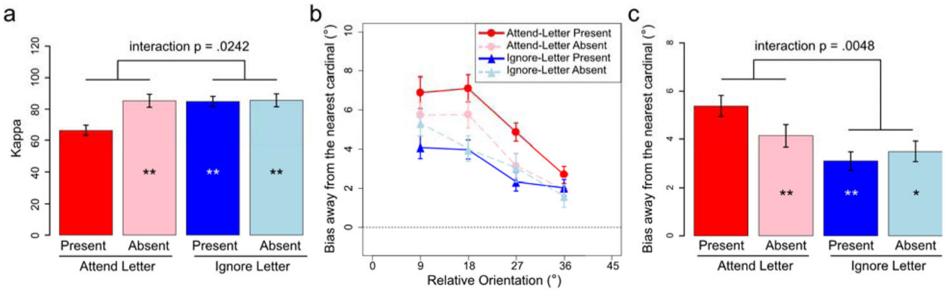

Figure 2.

Kappa and bias estimates from the mixture model in Experiment 1. (a) Kappa estimates averaged across all relative orientations for each combination of trial type (intervening letter present versus absent) and attentional condition (attend letter versus ignore letter). (b) Bias estimates as a function of the relative orientation of the sample teardrop for each combination of trial type (intervening letter present versus absent) and attentional condition (attend letter versus ignore letter). The data were aggregated across trials on the basis of the distance from the nearest cardinal orientation, excluding 0° and 45° because the nearest cardinal orientation is not defined for those orientations. The X axis indicates the orientation of the sample relative to the nearest cardinal orientation (0°, 90°, 180°, and 270°). Positive values indicate that responses were biased away from the nearest cardinal orientation. (c) Bias averaged across relative orientations, again excluding 0° and 45°. Error bars represent the within-subject standard error of the mean (Morey, 2008). Asterisks inside a given bar indicate a significant difference between that condition and the Attend-Letter/Letter-Present condition (paired t tests) after applying the false discovery rate correction (Benjamini & Hochberg, 1995). ** p < .01, * p < .05.

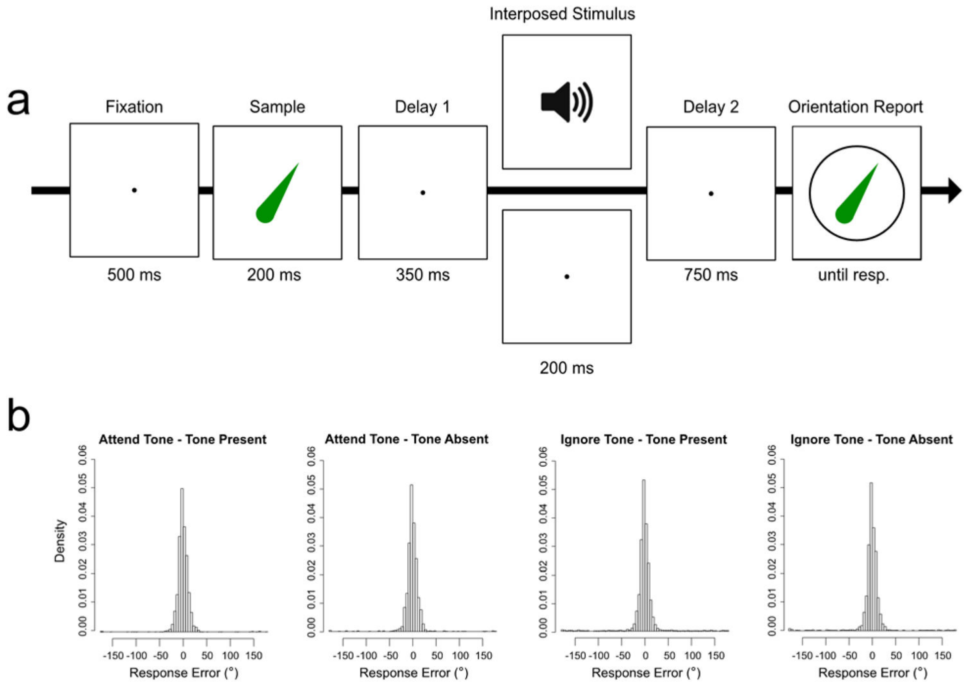

Figure 3.

(a) Example of a single trial in Experiment 2. The task was identical to that in Experiment 1, except that the intervening stimulus was an auditory tone. In the *Attend-Tone* condition, observers reported which of two tones (e.g., 310 or 510 Hz) was presented. In the *Ignore-Tone* condition, a tone from another set (e.g., 610 Hz or 810 Hz) was presented but was task-irrelevant and required no response. *Tone-Absent* trials were also included. Participants reported the teardrop orientation at the end of every trial except for trials with inaccurate or slow responses for the intervening task in the Attend-Tone/Tone-Present trials. In such trials, a warning message either “Inaccurate” or “Slow” replaced the orientation report. (b) Response error distribution for the four combinations of trial type (intervening tone present versus absent) and attentional condition (attend tone versus ignore tone) collapsed across all sample orientations and participants in Experiment 2.

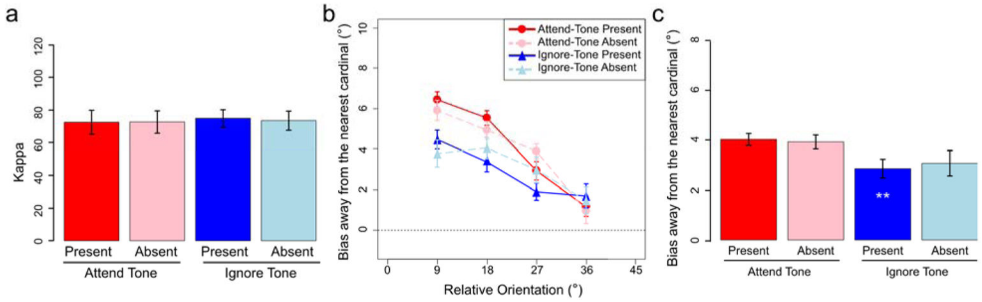

Figure 4.

Kappa and bias estimates from the mixture model in Experiment 2. (a) Kappa estimates averaged across all orientations for each combination of trial type (intervening tone present versus absent) and attentional condition (attend tone versus ignore tone). (b) Bias estimates as a function of the relative orientation of the sample teardrop for each combination of trial type (intervening tone present versus absent) and attentional condition (attend tone versus ignore tone). The data were aggregated across trials on the basis of the distance from the nearest cardinal orientation, excluding 0° and 45° because the nearest cardinal orientation is not defined for those orientations. Positive values indicate that responses were biased away from the nearest cardinal orientation. (c) Bias averaged across relative orientations, again excluding 0° and 45°. Error bars represent the within-subject standard error of the mean (Morey, 2008). Asterisks inside a given bar indicate a significant difference between that condition and the Attend-Tone/Tone-Present condition (paired t tests) after applying the false discovery rate correction (Benjamini & Hochberg, 1995). ** $p < .01$.