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Cognitive Control over Working Memory Biases of Selection

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Abstract

Many studies have found that representations in working memory (WM) can guide visual attention towards items that match the features of the WM contents. While some researchers contend that this occurs involuntarily, others suggest that the impact of WM content on attention can be strategically controlled. Here, we varied the probability that WM items would coincide with either targets or distracters in a visual search task to examine (i) whether participants could intentionally enhance or inhibit the influence of WM items on attention, and (ii) whether cognitive control over WM biases would also affect access to the memory content in a surprise recognition test. We found visual search to be faster when the WM item coincided with the search target, and this effect was enhanced when the memory item reliably predicted the location of the target. Conversely, visual search was slowed when the memory item coincided with a search distracter, and this effect was diminished, but not abolished, when the memory item was reliably associated with distracters. This strategic dampening of the influence of WM items on attention came at a price to memory, however, as participants were slowest to perform WM recognition tests on blocks when the WM content was consistently invalid. These results document that attentional capture by WM contents is partly, but not fully, malleable by top-down control, which appears to adjust the state of the WM content to optimize search behavior. These data illustrate the role of cognitive control in modulating the strength of WM biases of selection, and support a tight coupling between WM and attention.

The influential biased-competition model of attention (Desimone & Duncan, 1995) posits that the active maintenance of an item in working memory (WM) results in top-down biasing of visual processing in favor of matching items over other, competing objects. This model explains how the holding of a search template will facilitate selection of targets, but also suggests that WM-matching stimuli might capture attention when they are not directly task relevant. For instance, if you are mentally rehearsing a reminder to buy milk on your way to the supermarket, you may find your attention caught by a milk advertisement, at the expense of attending to the road.

Many recent studies have found evidence for the guidance of attention by the contents of WM (see Soto et al., 2008 and Olivers et al., 2011 for reviews). These studies required participants to remember an item (such as a colored shape) while performing an intervening visual search task. The critical manipulation was whether the memory item reappeared in the search display, and if so, whether its location coincided with the search target or with an irrelevant distracter. In numerous studies (e.g., Downing, 2000; Soto et al., 2005; Olivers et al., 2006), it has been observed that participants are faster to complete the search when the

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WM item reappears at the location of the search target, and slower to find the target when the WM item reappears at the location of a distracter. This has led to the assertion that attention is captured by items that match the WM contents, even when those are irrelevant for the search task. Because this capture of attention has been observed in search for pop-out targets (Soto et al., 2006), and even when the memory items *never* predicted the search target location, some researchers have suggested that it may be an automatic effect (Soto et al., 2005; Olivers et al., 2006).

Other researchers have found no influence of memory-matching items (Downing & Dodds, 2004; Houtkamp & Roelfsema, 2006; Peters et al., 2008), and others suggest that in certain conditions, participants can strategically avoid a distracting reappearing memory item (Woodman & Luck, 2007; Han & Kim, 2009). The WM capture effect has also been found to be eliminated in the presence of spatial pre-cueing to the search target (Pan & Soto, 2010), and diminished by time constraints on the search task (Dalvit & Eimer, 2011) or high WM processing loads (Soto & Humphreys, 2008). So, there remains some controversy as to whether WM automatically biases the allocation of attention or whether top-down control can modulate the effect.

Carlisle and Woodman (2011) examined the automatic and strategic contributions to WM guidance of attention, following the logic of classic attention studies (Posner & Snyder, 1975). Participants were shown a colored memory cue, then completed a visual search among colored stimuli. After the search, they were shown a memory probe and asked whether it matched the memory cue. Participants experienced 20%, 50%, or 80% valid (e.g. when the WM-matching item corresponded with the search target) conditions. They found that increased probabilities of valid trials amplified the costs of non-matching targets, as well as the benefits of matching targets (though benefits were more than twice as great), but several factors limit the implications of their findings for understanding the interplay between WM, attention, and cognitive control. Like most other studies, they included a memory test after each search array. Thus, even when the WM item is likely to match a distracter, there might be motivation to attend to that item to refresh its representation (cf. Woodman and Luck, 2007). Furthermore, each of their conditions included some probability that the memory item would match the target, creating an incentive for orienting toward that item.

Here we further examined the extent to which participants could flexibly use their knowledge about the validity of the memory contents to optimize search performance—namely, to boost attention towards WM-matching targets when the probability of validity is high, and to suppress the WM representation in the search process when the probability of invalidity is high. We varied the probability of search targets occurring at the location of memory-matching stimuli, and we explicitly informed participants about these contingencies. Memory was only probed during surprise memory recognition trials, and never after the search task, so there was no incentive to attend to a reappearing memory item. Further, we included a condition of 100% invalid memory cues, to examine whether WM items may still guide attention, even in the face of top-down effort to prevent their impact when they can only be harmful to the search. In a second experiment, we increased the WM difficulty to explore the give-and-take relationship between the memory and attention components of the task. For the first time, we examined how expectations about the relationship between memory items and search targets would in turn modulate the accessibility of the WM representation. The study provides new insights for understanding *how* the reciprocal interaction between WM content and attention may be modulated by cognitive control.

Experiment 1

Method

Participants—Twenty (11 female) volunteers gave written informed consent and received course credit or $10.00 payment for their participation. The study was approved by the Duke University Institutional review board.

Experimental protocol—The experiment was run on a Dell Optiplex 960 computer using E-prime (Version 2.0; Psychology Software Tools, Pittsburgh, PA, 2007). Stimuli were viewed from approximately 60 cm on an LCD monitor with a 60 Hz refresh rate and a screen resolution of 1280 × 1024 pixels. Each trial began with the presentation of a white fixation dot at the center of the screen on a black background for 1000 msec, followed by a blank screen for 500 msec, then a to-be-remembered colored circle cue, subtending approximately 1.3° in diameter, at the center of the screen for 250 msec (Figure 1). Two thousand msec after the offset of the cue, the target display appeared for 100 msec. The target display was composed of three colored circles—each subtending approximately 1.4° in diameter—at the corners of an imaginary triangle that were each approximately 2.5° from central fixation. Each circle contained a line of .57° length. Two of the lines were vertical and one—the target—was tilted 16° to the left or right. The participants' task was to indicate the orientation of the line using designated button presses to respond “left” or “right”-tilted. Each target location and orientation occurred equally often and in randomized order. To limit any incentive to attend to the memory item when it was irrelevant to the search task—while still ensuring that participants maintained the item in WM—20% of trials were catch trials on which, instead of a visual search display, participants were given a recognition test for the memory item. Memory probes consisted of a colored circle of 1.4° in diameter, at the center of the screen beneath a question mark, for 1500 msec. Participants had to indicate whether the probe was identical to the initial colored circle cue using designated button presses to respond “same” or “different.” Match and non-match probes occurred equally often and their order was randomized. The memory items and search array circles were randomly selected from one of four colors, with RGB values as follows: red (155, 0 0), blue (0, 0, 155), green (0, 165, 35), or yellow (170, 170, 45). Each color occurred equally often as a memory cue, and only one circle of each color appeared in the search display. Individual trials could be valid (memory item reappears surrounding the search target), invalid (memory item reappears surrounding a distracter), or neutral (memory item does not reappear in the search display). The critical manipulation here, however, was that trial conditions were blocked. Blocks could be composed of 100% valid trials, 100% invalid trials, 100% neutral trials, or 50% valid and 50% invalid trials. Participants were given instructions at the beginning of each block informing them of these percentages.

Participants completed a practice session—comprising 5 trials of each condition for a total of 20 trials—with a search duration of 500 msec, and then another practice session at the experimental search duration of 100 msec, to become acclimated to the speed of the search display. Participants then went on to complete 4 blocks of each condition, which occurred in random order. Each block consisted of 20 trials, totaling 320 trials in all.

Results

Means and standard deviations for all measures are reported in Table 1. We examined response times (RT) for correct visual search responses, according to block and trial condition (Figure 2a). Mean performance on valid and invalid trials, in both 100% and 50% predictability blocks, was normalized to the neutral baseline by subtracting scores for each condition from the mean RT for neutral blocks. These normalized RT scores were entered into a 2 × 2 ANOVA with the factors of Validity (Valid vs. Invalid) and Predictability

(100% vs. 50%). Search was faster overall on valid trials (vs. invalid), $F(1,19) = 47.42$, $p < .001$, $\eta_p^2 = .71$, and faster overall on 100% predictable blocks (vs. less predictable 50% validity) $F(1,19) = 16.97$, $p = .001$, $\eta_p^2 = .47$. In other words, valid cues sped up the search process and this speed-up was enhanced by cognitive control (cue predictability), whereas invalid cues slowed down the search process and this slow-down was attenuated by cognitive control (Figure 2b). The relative benefit of predictability, however, was comparable for valid and invalid trials, $F(1,19) = 1.31$, $p = .267$, $\eta_p^2 = .064$.

Search trial accuracy was at ceiling (mean = 97%) and displayed no main effect of validity ($p = .30$) or predictability ($p = .81$), nor an interaction ($p = .69$). Neither RT nor accuracy for memory catch trials varied significantly according to block condition (all $p > .1$), and accuracy on the memory probes was high (93.4%), confirming that participants were indeed keeping the cues in WM.

Discussion

The results clearly indicate that there are both voluntary and involuntary contributions to the capture of attention by WM contents. While search speed was always fastest on validly cued trials, it was further augmented in the context of 100% valid blocks, when it was known that the WM item would coincide with the search target, and could be strategically enhanced. Conversely, search was always slowest on invalidly cued trials, but the impact of a memory-matching distracter was dampened in 100% invalid blocks, when it was known to be obstructive, and could be intentionally suppressed. It was still the case, however, that 100% invalid blocks were slower than neutral blocks, $t(19) = 2.17$, $p < .05$, indicating that participants could not completely prevent the interference of the memory item. Unlike Carlisle and Woodman (2011), we found the benefits of valid trials and the costs of invalid trials to be of equal magnitude, and to be equivalently modulated by greater predictability. Because we included a 100% anti-predictive condition, and precluded a memory-refreshing account by testing memory only on catch trials, participants may have been able to more effectively dampen the impact of invalid cues—as opposed to the 80% condition used by Carlisle and Woodman (2011), where participants might still have had some incentive to attend to invalid distracters to aid in the subsequent memory test.

We note our 50% valid/50% invalid condition was somewhat predictive relative to a 33% validity condition, which would conform more to an ‘unpredictable’ context given our search set-size of three. Previous research indicates, however, that the effect of invalidity (Invalid RT–Neutral RT) should not necessarily vary with such a difference in probability of validity. For instance, Carlisle and Woodman (2011), Experiment 2, showed comparable costs of invalidity regardless of whether there were 20% or 80% valid trials. Likewise, Soto et al. (2005) observed no difference in the magnitude of invalidity costs regardless of whether trials were 33% valid/33% invalid/33% neutral, or only invalid and neutral. The critical difference that we observe is between a condition that has some probability of validity (50% valid/50% invalid) and one with no valid trials (100% invalid). The slight predictiveness of 50% validity should not undermine the relevance of our observation, namely, that the interference effect from invalid WM-cues was diminished, but not fully abolished, in the context of fully predictable invalid cues which should have maximized the application of cognitive control. We next ask whether cognitive control can modulate the state of the mnemonic content in addition to the strength of the WM-bias.

Experiment 2

Experiment 1 was not designed to investigate WM performance. Memory was tested on only 20% of trials and the task was sufficiently easy that accuracy was very high (93%). Neither accuracy nor RT for memory probes varied significantly with block condition. In

Experiment 2, we sought simply to make the WM component more challenging—by expanding the color space from which stimuli could be drawn—so that accuracy would not be at ceiling, and we might therefore observe variations in memory performance as a function of the strategic modulation of WM biases that occurred in Experiment 1.

Most prior studies have been conducted from the perspective that WM influences the allocation of attention, and typically only address performance on the search task without scrutinizing memory performance (though see Grecucci et al., 2010; Woodman & Luck, 2007). Here, we seek to more thoroughly understand the relationship between the memory and attention components of the task by probing the role of block-wise cue predictiveness on the memory content itself. This allows us to adjudicate two possible interpretations of the strategic control effects observed in Experiment 1 and previous studies. On the one hand, the role of cognitive control in reducing the impact of WM items on attentional allocation could be seen to demonstrate a decoupling of WM and attention, thus supporting the independence of these processes (Woodman & Luck, 2007; Peters et al., 2008). On the other hand, strategic control may instead reflect modulation of the status of the memory representation itself, which in turn would amplify or diminish its effect on visual search. From the latter perspective, any effect of cognitive control on WM biases of selection would not be an expression of the independence between WM and attention, but of their reciprocity.

In conducting Experiment 2, we reasoned that if the second interpretation is correct, then the strategic modulation of the WM content’s impact on visual search should manifest itself in variations in the speed of memory recognition performance. Such a finding would also be consistent with a recent proposal that reconciles disparate findings on the relationship between WM content and attentional selection. Olivers et al. (2011) theorized that WM items can be designated different statuses or activation states, where only actively held items will influence perception. Other items can be appointed to an accessory state that will not impact visual attention (or at least not as considerably), but can be retrieved into the active state when they become task-relevant. If cognitive control over the impact of WM content modifies the activation state of the critical memory representations, then we would expect that an attempt to suppress a potentially interfering WM item would result in its assignment as an accessory item. This would then require a more time-consuming reinstatement into active memory in the event of a surprise recognition test. Thus, we predicted slower, though not necessarily less accurate, recognition memory responses in the 100% invalid than in the 100% valid cuing condition.

Method

Participants—Twenty-eight (14 female) volunteers gave written informed consent and received course credit or $10.00 payment for their participation. The study was approved by the Duke University Institutional review board.

Experimental protocol—The trial sequence in Experiment 2 was identical to Experiment 1, except that the proportion of memory trials was increased from 20% to 50%, and the memory test was made more difficult by increasing the number of colors from which stimuli could be drawn. After the memory cue, participants completed a visual search on half of trials, and were tested for their memory of the cue color on the other half. The order of these trial types was random. Experiment 1 utilized stimuli of four distinct colors, while Experiment 2 used those same four hues, but with three shades of each hue, totaling twelve colors, with RGB values as follows: three shades of each red (180, 0, 0; 130, 50, 50; 163, 17, 62), blue (7, 186, 249; 1, 104, 255; 7, 70, 249), green (0, 255, 0; 1, 155, 0; 0, 80, 0), and yellow (252, 243, 62; 209, 204, 0; 255, 187, 51). The colors were selected from a range of shades which were determined in informal testing to be subjectively equally distinguishable, and the occurrence of each color was counterbalanced across conditions. When memory was tested for the color of the cue stimulus, the probe shade was either an exact match to the memory color, or a different shade of the same hue, thus necessitating a more fine-grained visual WM representation than the cues in Experiment 1. When a memory color reappeared in the search array, it was only ever an exact match, and never a different shade of the same color.

Participants completed a practice session—comprising 5 trials of each condition for a total of 20 trials—with a search display duration of 500 msec, and then another practice session at the experimental search display duration of 100 msec. The experiment then consisted of 2 blocks of each 100% valid, 100% invalid, and 100% neutral conditions, and 4 blocks of the 50% valid/50% invalid condition, which occurred in random order. Each block constituted 32 trials, totaling 320 trials in all.

Results

Means and standard deviations for all measures are reported in Table 2. The memory difficulty manipulation was successful at decreasing accuracy on memory trials (while keeping it well above chance). While overall memory accuracy in Experiment 1 was 93.4%, it was significantly lower in Experiment 2 at 77.2%, $t(46) = 6.76$, $p < .001$. Despite this dramatic dip in WM accuracy, the pattern of search trial RTs in Experiment 2 was similar to that in Experiment 1 (Figure 3a). Again, scores were normalized—by subtraction from the neutral baseline—and entered into a 2 × 2 ANOVA with the factors of Validity (Valid vs. Invalid) and Predictability (100% vs. 50%). Again, search was faster overall on valid trials (vs. invalid), $F(1, 27) = 85.5$, $p < .001$, $\eta_p^2 = .76$, and faster overall on 100% predictable blocks (vs. 50% validity), $F(1, 27) = 10.24$, $p < .001$, $\eta_p^2 = .28$, replicating the main findings of Experiment 1: valid cues speed up search and this speed-up is enhanced by cognitive control (cue predictability), whereas invalid cues slow down search and slow-down is attenuated by cognitive control. A Validity × Predictability interaction, $F(1, 27) = 7.57$, $p < .05$, $\eta_p^2 = .22$, however, also revealed that the effect of predictability was greater for valid trials (100% valid – 50% valid) than for invalid trials (100% invalid – 50% invalid), $t(27) = 2.75$, $p < .05$ (cf. Carlisle and Woodman, 2011).

Our task design leaves open the possibility that the WM bias in 100% invalid blocks reflects carry-over from having previously experienced blocks with valid trials. To refute this possibility, we conducted a further analysis on the 12 participants who experienced the 100% invalid condition as their first block. We conducted the same ANOVA as above and again found a main effect of Validity, $F(1, 11) = 56.21$, $p < .001$, $\eta_p^2 = .84$, and Predictability, $F(1, 11) = 5.34$, $p < .05$, $\eta_p^2 = .33$, as well as an interaction between the two, $F(1, 11) = 6.88$, $p < .05$, $\eta_p^2 = .39$. A two-tailed t-test confirmed that even these 12 participants were significantly slower than neutral on 100% invalid blocks, $t(11) = -5.74$, $p < .001$. Thus, the capture of attention by the contents of WM in 100% invalid blocks cannot be explained by carry-over effects.

Search accuracy was sensitive to the WM validity condition in Experiment2. While the ANOVA revealed no main effect of Predictability ($p = .25$), nor a Validity × Predictability interaction ($p = .7$), there was a main effect of Validity, $F(1, 27) = 12.05$, $p < .01$, $\eta_p^2 = .31$, reflecting better overall search accuracy when the WM cue coincided with the search target, as opposed to a distracter.

Finally, our main goal for Experiment 2 was to examine whether WM performance, now exposed to higher demands, would be affected by the experimental manipulations. Indeed, memory probe RTs did fluctuate with block condition. In a repeated measures ANOVA with the 3-level factor of Block Condition[1] (100% Valid, 50% Valid/50% Invalid, and 100%

Invalid) there was a main effect of Condition, $F(2, 54) = 3.42$, $p < .05$, $\eta_p^2 = .11$, and a significant linear trend, $F(1, 27) = 4.99$, $p < .05$, $\eta_p^2 = .16$. In line with our prediction that memory responses would be faster in the 100% valid blocks than in 100% invalid blocks, a two-tailed t-test confirmed this to be the case, $t(27) = -2.23$, $p < .05$. Memory recognition for accurate trials was fastest in 100% valid blocks, then 50% validity blocks, and was slowest in the 100% invalid blocks (Figure 3c). Memory accuracy, on the other hand, did not differ according to block condition ($p = .32$).

Discussion

Although the memory task was more difficult in Experiment 2, participants displayed the same general search RT pattern as Experiment 1, with one important difference: trial validity and block predictability interacted. When memory demands were greater, the impact of predictability was diminished for invalid trials. We propose that the requirement to maintain a more precise visual memory representation magnified its impact when it was valid, but somewhat prevented the strategic attenuation of its impact when it was invalid. This enhanced WM representation in Experiment 2 may have also interfered on invalid trials beyond just slowing down the response, but to the point that it increased error rates.

Of more direct concern to our hypothesis, however, is the memory performance. While memory accuracy was not modulated by validity or predictability, participants took longer to retrieve and report memory items when they were strategically attempting to inhibit their impact on visual search. We found no significant difference, however, in memory RT between 100% valid and neutral blocks ($p = .9$). This may well reflect a ceiling effect in the speed with which participants can respond to the memory probe. We predicted that the memory cues in an invalid context should be relegated to a different state, leading to slower RTs relative to the valid cues which should remain accessible. This framework, however, makes no strong prediction about what should occur in the neutral context.

General Discussion

Experiment 1 confirmed that there are both purposeful and unintentional contributions to the capture of attention by the contents of WM. As in many other studies (Downing, 2000; Soto et al., 2005; Olivers et al., 2006), search performance was delayed in the presence of WM-matching distracters and expedited by WM-matching targets. The magnitude of these costs and benefits, however, was modulated by the probability that WM cues would coincide with targets vs. distracters. Despite the confidence with which participants could predict—and presumably attempt to avoid—invalid cues in 100% invalid blocks, however, they were still significantly slowed by them. In line with previous studies, these results suggest that WM biases of selection are modifiable by cognitive control (Han & Kim, 2009; Woodman & Luck, 2007; Carlisle & Woodman, 2010). This top-down influence over WM biases of selection is limited, however, because invalid cue costs are still incurred under conditions of 100% anti-predictive cues.

Experiment 2 further extends our understanding of the nature of the relationship between WM and attention. We showed that memory recognition speed was sensitive to the validity/predictability of the WM item for visual search, further supporting the reciprocity of attention and WM processes; WM not only influenced the allocation of attention but the status of the memory itself was modulated based on goals. This pattern of memory

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[1] When analyzing search RTs, we conducted a 2 × 2 (Validity × Predictability) ANOVA. Memory trials, however, could not be analyzed in this way. Memory was only tested during catch trials, so there were no valid or invalid memory trials within the 50% validity blocks. Rather, we could only look at memory performance in the context of predictable vs. unpredictable blocks. Consequently, in the memory analysis, we have entered each block condition of interest as a level in the ANOVA.

performance indicates a possible means by which strategic control over the WM-attention interaction may be implemented for optimal performance. Specifically, it appears that control can occur at the level of the memory representation to alter its subsequent impact, and this may be accomplished through manipulation of the status of the WM trace.

As recently suggested by Olivers and colleagues (2011), WM items may be maintained in different states of activation, and that status may determine how extensively they influence the allocation of attention. This possibility was first suggested by Downing and Dodds (2004) who found that an irrelevant memory item did not interfere with search, and supported by Olivers and Eimer (2011) who found that the extent of memory guidance depended on the imminence of the memory test. The current findings suggest that predictably helpful memory cues will be strategically, actively maintained in the focus of attention to promote faster search for a matching target, and will also be immediately accessible if memory is probed. Predictably invalid memory items, on the other hand, will be shifted to an accessory status so as not to impede search performance, and will therefore require more time to restore to the active state for a memory probe response. This is consistent with the proposal made by Oberauer (2002) that there is an internal focus of attention within WM which can hold a single representation at a time, and a separate store which can simultaneously contain several items for direct access. We propose shifting between the internal focus of attention and the zone of direct access may be driven by the relevance of the WM contents vis-a-vis intermittent task goals.

Two important implications stem from these results. First, both the visual search and memory performance profiles indicate that there is a give-and-take relationship between the attention and WM components of this dual-task paradigm. When task demands necessitate a richly maintained memory representation, there are consequences for visual search performance, which may be harmful or helpful depending on the relationship between the memory content and search target. Likewise, when cognitive control is exerted to limit the impact of irrelevant memory contents on search performance, there are consequences for memory recognition. Thus, WM and attention processes appear to be tightly connected, and not easily dissociable. Secondly, this cognitive control over WM biases is implemented (at least partly) through modification of the memory representation itself, and specifically, via context-dependent enhancement or attenuation of its impact by shifting its WM state.

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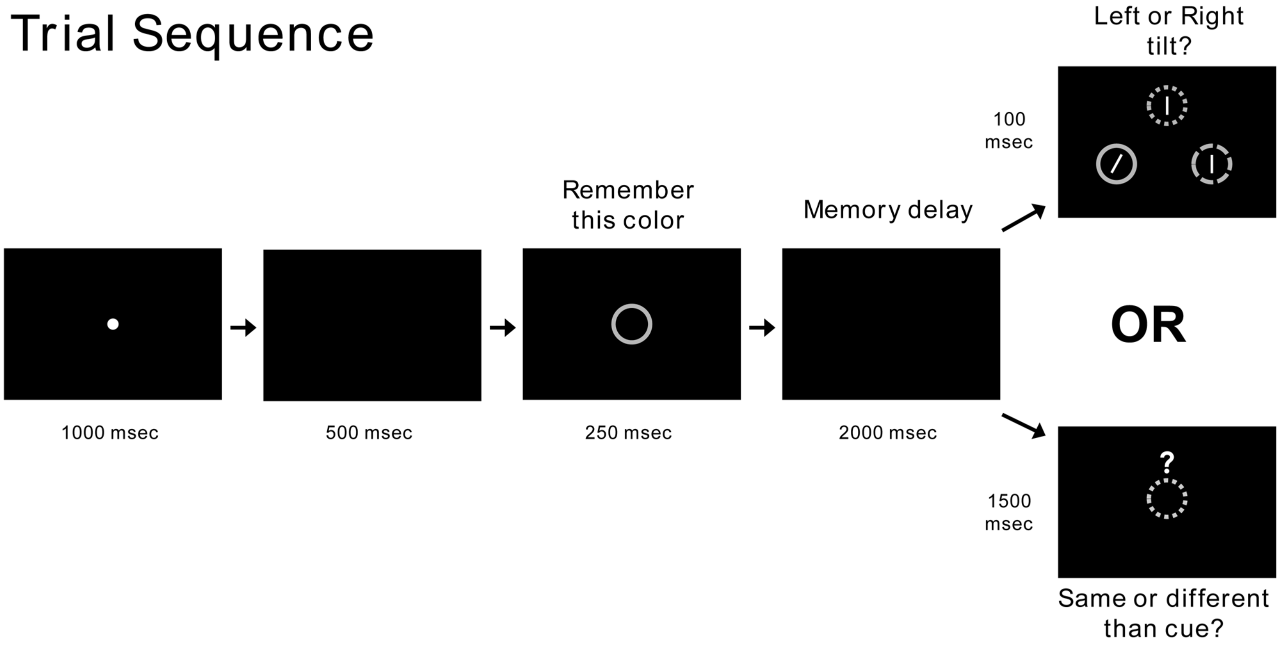

Figure 1.

Example trial sequence. Solid, dotted, and dashed lines represent different colors. Participants were asked to remember a colored circle over a delay and were then shown either an array of three circles for a visual search, or were given a recognition test for the original cue. The memory test never occurred after the visual search display.

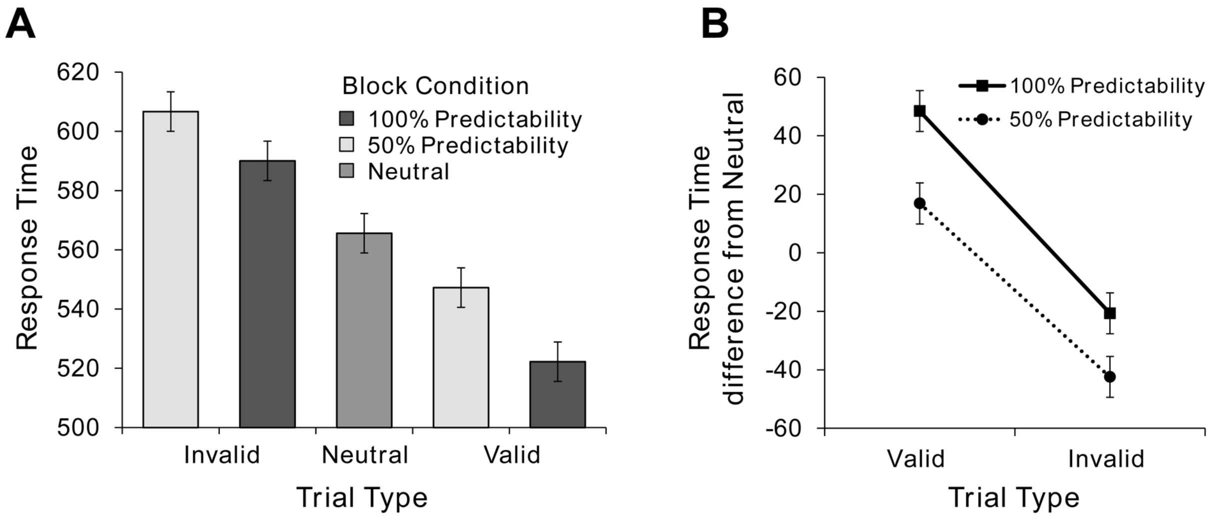

Figure 2.

A) Experiment 1 visual search RT (msec) as a function of block and trial condition. B) Scores on each condition subtracted from the neutral baseline. Positive values reflect RTs faster than neutral and negative values reflect RTs slower than neutral. Error bars reflect mean standard errors.

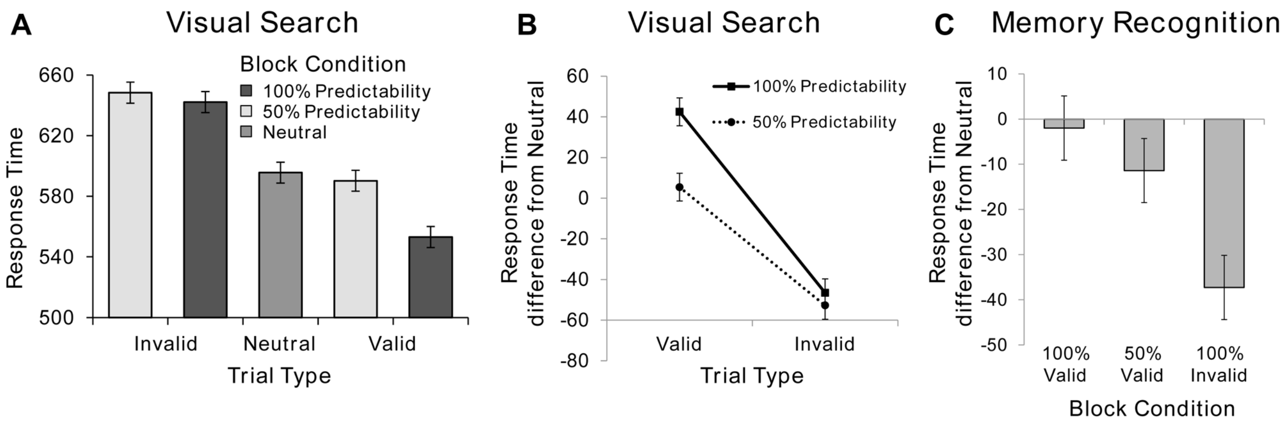

Figure 3.

A) Experiment 2 visual search RT (msec) as a function of block and trial condition. B) Visual search RT on each condition subtracted from the neutral baseline. Positive values reflect RTs faster than neutral and negative values reflect RTs slower than neutral. (C) Memory probe RT for each block condition, subtracted from the neutral baseline. Error bars reflect mean standard errors.

Table 1

Means and Standard Deviations for all conditions in Experiment 1

		Visual Search Task		Working Memory Task	
Block Condition	**Trial Condition**	**RT (msec)**	**Accuracy (%)**	**RT (msec)**	**Accuracy (%)**
100% Neutral	Neutral	566 (157)	97.5 (15.5)	834 (206)	92.0 (27.1)
100% Valid	Valid	522 (150)	97.3 (16.3)	821 (203)	94.0 (23.8)
100% Invalid	Invalid	590 (147)	97.0 (17.0)	838 (214)	94.2 (23.3)
50% Valid	Valid	547 (152)	98.1 (13.7)	844 (219)	93.4 (24.9)
	Invalid	607 (152)	96.7 (17.8)		

Table 2

Means and Standard Deviations for all conditions in Experiment 2

		Visual Search Task		Working Memory Task	
Block Condition	**Trial Condition**	**RT (msec)**	**Accuracy (%)**	**RT (msec)**	**Accuracy (%)**
100% Neutral	Neutral	596 (94)	96.3 (6.8)	738 (96)	77.3 (10.6)
100% Valid	Valid	553 (74)	97.4 (4.4)	740 (99)	76.4 (9.6)
100% Invalid	Invalid	642 (87)	95.3 (7.3)	775 (97)	78.7 (7.9)
50% Valid	Valid	590 (73)	96.6 (6.2)	750 (91)	78.3 (8.5)
	Invalid	648 (86)	93.9 (8.3)		