# VISUAL WORKING MEMORY AND ATTENTIONAL GUIDANCE

By

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To my lovely wife, Jane.

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LIST OF ABBREVIATIONS

WM........................................................................................... Working memory

VWM.................................................................................. Visual working memory

LTM.........................................................................................Long-term memory

STM........................................................................................ Short-term memory

VLTM............................................................................. Visual long-term memory

# CHAPTER I

# INTRODUCTION

Given that people think, and that people must pay attention to the environment, one of the most basic questions that a psychologist can ask is: what is the relationship between thinking and attention? A general interpretation of this question would ask how tasks that require thinking affect people's ability to perform tasks that require attention. For current purposes, thinking will be broadly construed as any mental operation that requires maintaining or manipulating information in the mind. Thus, holding information in working memory (WM) is considered thinking, as is forming a mental image, or adding and subtracting numbers etc. Selective attention will be construed as differential processing of simultaneously available sources of information, in particular, differential selection of *external* sources of information. Given these definitions, research suggests that thinking can influence the ability to attend to things in the environment. For example, tasks such as subtraction reduce the efficiency with which one can perform concurrent visual searches requiring attention (Han & Kim, 2004), and taxing WM loads can impair one's ability to selectively attend to task-relevant objects (de Fockert, Rees, Frith, & Lavie, 2001; Lavie & de Fockert, 2005). This research suggests that mechanisms involved in WM and attention overlap to some degree, thus implying that thinking about something can hinder one's ability to pay attention to the environment.

A more specific question about the relationship between thought and attention concerns how the *contents* of thought affect mechanisms that control deployment of attention. This is the question on which the current paper will focus. One hypothesis is that thinking about something prepares the cognitive system to attend to things that are similar to what is being thought about. According to this idea, if a person is imagining the new Toyota Camry, then she will likely attend to one if it happens to drive by. The general hypothesis will be referred to as the *automatic guidance hypothesis*, because the idea is that the contents of thought cause attention to deploy to similar objects in the environment by virtue of the fact that there is a similarity between the thought content and the object.

Much of the research investigating whether WM's contents guide attention was inspired by the *biased-competition model* of attention (Desimone, 1996; Desimone & Duncan, 1995; Duncan, 1998). This model describes attention as a series of neural competitions that are biased in favor of stimuli that are currently relevant to behavior. One problem with using neural competition as the basis for an explanatory framework of cognition is that it is difficult to specify which representation will win a given competition independently of the outcome of the competition (Enns & Di Lollo, 2002). For example, if two super-threshold stimuli are presented to an observer, and the observer reports that only one stimulus was present (e.g. Mack & Rock, 1998), then proposing that the reported stimulus "won" some kind of competition provides no explanation as to why. However, positing that WM biases attention in a top-down manner such that objects matching visual WM (VWM) representations tend to win the competitions (Downing, 2000) overcomes this shortcoming. This hypothesis has received support from single-unit recording studies in monkeys performing visual search (Bichot, Rossi, & Desimone, 2005; Chelazzi, Miller, Duncan, & Desimone, 1993; Desimone, 1996) and behavioral studies in humans (Downing, 2000; Pashler & Shiu, 1999; Soto, Heinke, Humphreys, & Blanco, 2005). This research will be discussed in more depth in subsequent chapters.

Of course if one hypothesis is the automatic guidance hypothesis, then another must be the *optional guidance hypothesis*. For example, if the person is imagining a Toyota Camry, and one happens to appear, right next to a Volkswagen Jetta, she might attend to the Volkswagen, while still imagining the Toyota. This idea has not been considered to any great extent in the literature on interactions between thought and attention, but there are good reasons to take it seriously. First, the empirical evidence supporting the automatic guidance hypothesis is ambiguous in important ways (Downing & Dodds, 2004; Woodman & Luck, in press; Woodman, Vogel, & Luck, 2001). Second, there are theoretical reasons why one might predict that thought and visual attention should not be as tightly coupled as implied by the automatic guidance hypothesis.

## Overview of Paper

The current paper reports eight new experiments testing whether the contents of WM guide attention. The experiments are grouped according to the specific questions they were initially intended to address. The experiments in Chapter II (Experiments 1 and 2) examined whether items retrieved from visual long-term memory (VLTM) into working memory guide attention. These experiments adopted a paradigm that has previously yielded findings consistent with the automatic guidance hypothesis (Downing, 2000) and the original impetus for the experiments reported in Chapter II was simply to extend these previous findings. However, Experiments 1 and 2 not only failed to extend prior work in the intended manner, but also failed to provide a strong replication.

Therefore, the experiments in Chapter III (Experiments 3 through 6) were conducted after a careful examination of the assumptions underlying the automatic guidance hypothesis and the conditions necessary for testing it. In essence, the experiments in Chapter III scrutinized the method adopted in Experiments 1 and 2 and by previous researchers, and empirically demonstrated the ambiguity of evidence supporting the automatic guidance hypothesis. Consequently, the experiments reported in Chapter IV (Experiments 7 and 8) were conducted in an attempt to address the methodological concerns of previous work. Finally, Chapter V discusses the implications of these findings for theories proposing a tight link between working memory and attention, and describes ways in which these theories might be modified in order to accommodate the results reported here.

# CHAPTER II

## EXPERIMENTS 1 AND 2

In order for attention to be valuable to a behaving organism, there must be some way to ensure that regions of space containing useful information are selected more often than regions containing less useful information. The mechanism implied by the automatic guidance hypothesis would achieve this end. Moreover, if WM automatically guides attention, then an observer would have to do little more than maintain a representation of a goal-relevant object in WM in order for attention to be deployed to the object in the scene (Downing, 2000; Pashler & Shiu, 1999; Soto et al., 2005). This mechanism would be a time saver, because it takes longer to volitionally direct attention to a specific location than to simply allow attention mechanisms to operate "anarchically" (Wolfe, Alvarez, & Horowitz, 2000).

Downing (2000) sought to empirically test whether WM's contents guided attention. Participants held an object in WM, and over the retention interval two objects (a memory match and a distractor) were simultaneously flashed on the screen (see Figure 1). The *memory match* was the same as the WM item, and the *distractor* was different from the WM item. Soon after the flashed items disappeared, a response probe was presented at the location of the memory match (henceforth, *match trials*), or the location of the distractor (henceforth, *mismatch trials*). If WM's contents guide attention, then participants' probe responses should be faster and more accurate on match trials than mismatch trials; that is, there should be a *match-trial advantage*. This is indeed what

Downing (2000) found. However, using the same presentation parameters, but without a WM requirement, Downing found a *match-trial disadvantage*. These findings are consistent with the ideas that objects matching the current contents of WM attract attention, while objects that match recent contents of WM do not.

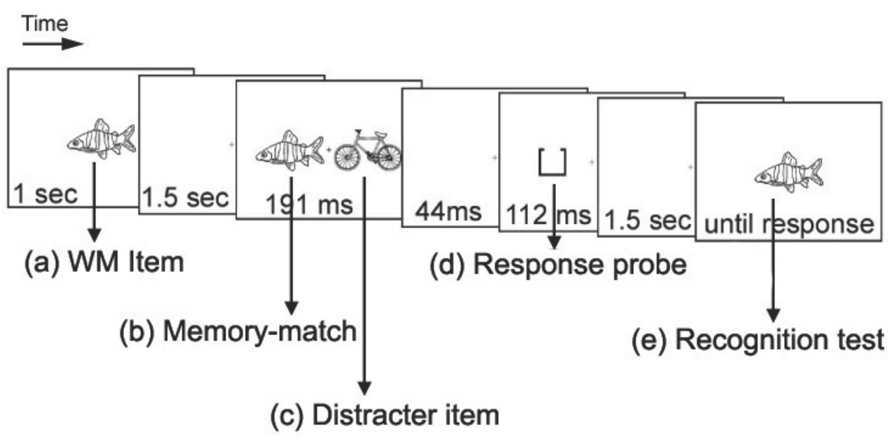


Figure 1. A diagram of the primary task used in the current experiments and Downing (2000)

Experiments 1 and 2 attempted to extend Downing's basic finding. The main question was whether items retrieved from visual-long term memory (VLTM) *into* WM would guide attention to memory matches. If the guidance of attention by WM is a mechanism by which attention is deployed in the real world, then there is reason to expect that items retrieved from VLTM into WM will guide attention. In the real world, observers are not always presented with an object to hold in WM immediately before they should attend to it. Rather, observers often retrieve specific information about an object from VLTM (c.f. Pashler & Shiu, 1999). Thus, if one assumes that WM's guidance of attention operates in a range of real world settings then one should predict that information retrieved from VLTM should guide attention. A question of secondary interest in Experiments 1 and 2 was whether WM would guide attention at a visually specific level. It is sometimes claimed that *visual* working memory (VWM) is critically important for guiding attention (Desimone, 1996; Desimone & Duncan, 1995; Downing, 2000). If this is the case then one might predict that WM would guide attention to memory matches, *even if* the memory match were categorically and visually similar to another visible object. For example, if a person is holding a the new Camry Hybrid in WM, and there happens to be a such a car parked right next to an older-model Camry, then attention should deploy to the Camry Hybrid.

## Experiment 1

There were four conditions in Experiment 1. These resulted from factorially combining two within-subjects factors with two levels each: a memory factor (whether information was retrieved from long-term memory or simply held in short-term memory) and a distractor-category factor (whether the distractor item was from the same category as the memory match or from a different category). If guidance by WM is an important real world mechanism, and if WM guides attention at a visually specific level, then one would expect match-trial advantages in all four conditions.

*Methods*

*Participants*

Seventeen Vanderbilt University undergraduate students participated for course credit. One subject was dropped due to an error rate over 40%, leaving a final N = 16.

*Materials*

Objects were from the Op de Beeck and Wagemans (2001) stimulus set, which has 269 line drawings from 25 basic-level categories. Stimuli were sized to less than 11.7 x 8.7 degrees visual angle viewed from 61 cm (distance assumed for all visual angle reports), but viewing distance was not controlled. The response probe was a 4.5 x 4.5 degree visual angle square with a 2.5-degree gap at the top or bottom. Apple eMac computers (with 15.5-inch monitors, set at 256-level greyscale, 1024 x 768 resolution, and 89 hz refresh) controlled the experiment. Responses were made on the keyboard.

*Design and Procedure*

The two factors of interest were: 1) Memory: whether the WM item was held in short-term memory (STM) or retrieved from long-term memory (LTM), and 2) Distractor: whether the distractor was from the same category as the WM item or a different category. There were four conditions: 1) *LTM/Different category*, 2) *LTM/Same category*, 3) *STM/Different category* and 4) *STM/Same category*. Conditions were blocked. There were 16 practice trials and 96 trials of the primary task in each block.

*Primary Task*. The primary task was almost identical to the task used by Downing (2000; see Figure 1). Participants initiated a trial by pressing the space bar. A fixation-cross (.94 x .94 degrees) appeared at the center of the monitor for one second, followed by the WM item, which was visible for one second at the center of the monitor.

Participants were instructed to hold the WM item "in (their) mind" for the entire trial. After the WM item disappeared, a fixation-cross was presented for 1506 ms, and remained visible when the memory match and distractor were simultaneously flashed on the screen for 191 ms, on the left and right of the fixation-cross, separated by approximately 12.4 degrees visual angle, center-to-center. Participants were told that one of the flashed items would match the WM item, but they did not have to respond to the flashed items. About 44 ms after the flashed items disappeared, the probe appeared for 112 ms, at one of their locations. Participants had to respond by pressing "1" if the gap was at the top, and "2" if the gap was at the bottom of the probe. Both speed and accuracy were stressed. Participants had 1506 ms to respond to the probe, then a recognition test item appeared, replacing the fixation cross, and remained visible until response. For true recognition trials, participants pressed "1", and for recognition foils (i.e. not the WM item), they pressed "2". This basic design was used in all four conditions.

Memory match location (left/right), gap orientation (up/down), probe position (left/right) and recognition test item (true/foil) were crossed 6 times in each block resulting in 96 trials per block. Across participants, each condition appeared in each ordinal position (e.g. 1$^{st}$, 2$^{nd}$, 3$^{rd}$, or 4$^{th}$) twice. Order of blocks was different for each subject. Trial order was random within blocks.

*Same and Different Category Blocks*. In same-category blocks, the distractor was from the same category as the memory match (e.g. both were trees). In different category blocks, the distractor was from a different category than the memory match. Other than these constraints, distractors were selected randomly.

*Short-Term Memory Blocks (STM).* In STM blocks, a line drawing of the WM item was presented at the beginning of the trial. A different WM item was presented on every trial within a given block. Across blocks some randomly selected objects repeated.

*Long-Term Memory Blocks (LTM).* In LTM blocks, participants memorized a set of six objects (3 natural-kind objects and 3 artifacts; a different set on each block) before performing the 96 trials of the primary task. Two randomly selected object sets were used across participants, and each set appeared in the same-category condition for half the participants, and the different-category condition for the other half.

The memorization procedure consisted of a study phase, followed by a yes/no recognition test. For the study phase, each object in the target set was presented one at a time at the center of the monitor at a rate of 2 seconds per item. Participants were instructed to memorize what the objects looked like. For the recognition test, a series of objects was presented one at a time in a random order. There were three same-category foils for each item in the target set. For each item, participants indicated whether it was in their target set. If an observer did not pass the recognition test with 100% accuracy, then the observer had to redo the entire procedure. The criterion for memorization was two consecutive attempts with 100% accuracy. Thus, if a participant failed on any attempt, they had to redo the entire procedure *at least* twice more.

After participants memorized the objects, they performed the primary task. The primary task was modified such that observers had to retrieve visual object information from VLTM; thus, the basic-level name of one object from the memorized set was used to indicate the WM item at the beginning of a trial. Within each LTM block, each item in a memorized set served as a WM item on 16 trials. The distractors (i.e. the item flashed over the retention interval) were the same items that appeared as foils during the memorization phase of the experiment.  In addition, recognition foils (i.e. for the primary task, *not* the memorization procedure) were from same category as the WM item on half of trials (so participants had to remember visual details) and from the memorized set on the other half (so participants had to remember which object from the memorized set was the WM item on a trial).

Note that the LTM retrieval/recognition task required visually specific retrieval of target objects.  If an observer relied on basic level labels, then they would fail when same-category recognition foils were presented.  If participants based responses only on whether an object was in their target set, without remembering which memorized object was the WM item for a given trial, then they would fail when target-set foils were presented (i.e. an object from the target set that was not the WM item for the given trial). Thus, both LTM blocks required visually specific retrieval, while the STM blocks did not.  In the STM/Same category block, observers needed to remember visual details (because only same-category foils were used for the recognition test), but in the STM/Different category condition, observers could rely on a representation of a basic level label (because only different category foils were used).  This confound is present because Downing (2000) used the equivalent of basic level maintenance, and one aim of the current research was to replicate his effect. Experiment 3 addressed this confound.

*Results*

The comparisons of primary interest are between the match- and mismatch- trials within each of the 4 blocks.  Because of the a priori nature of the hypothesis in question, there is justification for focusing exclusively on these. Thus, omnibus ANOVAs are presented after pairwise analyses. Probe reaction time, probe errors, and memory errors (i.e. for the primary task) were each analyzed separately.

*Probe Reaction Time*

Trials on which participants made an error on the probe task or the memory decision were excluded from RT analyses (9.7% of trials removed). In addition, a trial's RT was removed if it was more than 3 standard deviations away from a participant's mean RT for the block from which it came (1.8% of trials removed). Descriptive statistics on errors are presented in Table 1.

For RTs, paired-samples t-tests failed to find reliable match trial advantages in any of the conditions (all $t$'s (15) < 1.74; $p$'s > .10). However, when match and mismatch trials were compared using Wilcoxon Signed Rank (WSR) tests, there was a significant match-trial advantage in the STM/Different category condition (W- = 106; $z = 1.965$; $p = .049$). The discrepancy between the t-test and the WSR test in this condition occurred because one participant exhibited a match-trial *disadvantage* of 71 ms (the next largest match trial disadvantage was 18 ms). However, 12 of the 16 participants showed match-trial advantages, but the largest was only 51 ms. When the outlying participant's data was removed, a paired-samples t-test revealed a significant match-trial advantage for RTs, (match $\underline{M} = 461$, mismatch $\underline{M} = 476$; $\underline{SE}_{Diff} = 5.04$, $t\,(14) = 3.003$; $p = .009$; see Figure 2). The STM/Different category condition is similar to the conditions tested by Downing (2000), and these results replicate his findings. The large match-trial disadvantage that one participant exhibited will be discussed later.

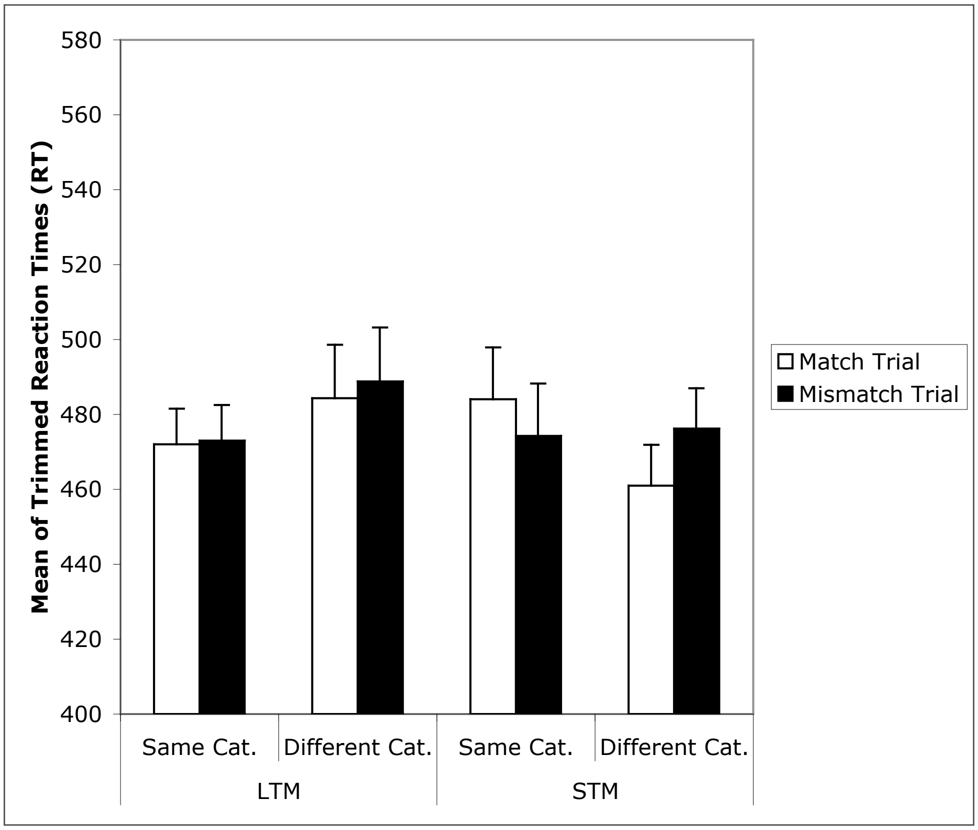


Figure 2. Average trimmed probe RTs from Experiment 1. The outlying subjects's data are not included. Error bars represent 95% margins of error for the within-subjects difference between adjacent bars. Thus, error bars that capture the top of an adjacent mean bar reflect non-significant differences, and error bars that do not capture the top of adjacent mean bars reflect significant differences.

To verify the results of the above analysis, a location (match/mismatch trial) by distractor category (same-category vs. different category) by memory condition (LTM/STM) ANOVA was also run on RTs. With all participants included, there were no significant effects (consistent with the within block pairwise match-mismatch analyses reported above). When the outlying participant from the Different category/STM was removed, the 2-way interaction between location and distractor category was marginally significant ($F$ (1, 14) = 4.352, $p$ = .056), and the 3-way interaction was also marginally significant, *F* (1, 14) = 4.475, *p* = .053. This pattern of interactions is consistent with the results of the pairwise analysis described above.

*Probe Errors*

Probe response errors (3.8% of trials overall) were also analyzed, but there were no differences between match and mismatch trials (see Table 1).

Table 1. Proportion of probe and memory errors for Experiment 1.

| | | Probe Errors | | Memory Errors | |
|---|---|---|---|---|---|
| Memory Condition | Distractor Category | Match | Mismatch | Match | Mismatch |
| LTM | Same-category | .035 | .043 | .034 | .043 |
| | Diff-category | .039 | .043 | .035 | .040 |
| STM | Same-category | .038 | .038 | .103 | .094 |
| | Diff-category | .040 | .034 | .055 | .048 |

*Memory Errors*

A location by distractor category by memory condition ANOVA was conducted on memory errors. Location did not have a main effect (*F* (1,15) <1), nor did it interact with the other factors, *F's* (1, 15) < 1.9, *p*'s > .19. There were fewer errors in the LTM blocks (M = .038, SE = .009) than in the STM blocks (M = .075, SE = .038), *F* (1, 15) = 22.453, *p* < .001. There was also a significant main effect of distractor category (*F* (1, 15) = 4.640, *p* = .048) that was driven by a significant memory by distractor category interaction, *F* (1, 15) = 8.892, *p* < .01. For the STM blocks, there were more memory errors in the same category block (M = .098, SE = .018) than in the different category block (M = .052, SE = .012), *F* (1, 15) = 9.400, *p* < .01. For the LTM blocks, there was no difference in memory errors for same (M = .038, SE = .013) and different (M = .038, SE = .009) category blocks, $F(1, 15) < 1$. These results largely confirm that the memory representation in the LTM blocks was robust, and that it was harder for participants to maintain an object at a visually specific level.

*Discussion*

A match-trial advantage was semi-reliable only in the STM/Different category (see Figure 2). This condition is most similar to that used in Downing (2000). However, this replication depends on removing one participant who showed a large match-trial disadvantage. Thus, the replication was weak at best.

The lack of any effects in the same-category blocks might not be problematic for the automatic guidance hypothesis because it is possible that WM's contents guide attention at a relatively coarse level. For example, items that are visually or conceptually similar to a WM representation could also attract attention (c.f. Duncan & Humphreys, 1989; Moores, Laiti, & Chelazzi, 2003), thus attenuating the competitive advantage an exact memory match might otherwise receive. However, there should have been a match-trial advantage in the LTM/Different-category block, unless 1) WM does not automatically guide attention in a wide range of situations or 2) participants chose not to use WM in the LTM block.

## Experiment 2

In Experiment 1's LTM blocks, it was possible for participants to simply rehearse the one-word label of the WM item when the retrieval cue was presented, continue verbally rehearsing this label while the memory match and distractor were flashed on the screen, and then finally retrieve the visual information about the WM item when their memory was tested. If participants adopted this strategy, then perhaps they were not holding anything in WM. If there was nothing in WM (at least, nothing that matched the flashed items), then it is not surprising that there was no match-trial advantage in either of the LTM conditions. Thus, Experiment 2 took precautions to hinder participant's ability to use this strategy.

### *Method*

Experiment 2 was nearly identical to Experiment 1, except as follows. First for each trial in the primary task, participants started repeating a syllable (e.g. "la") out loud at a rate of about 3-4 syllables per second before the trial started. The purpose of such articulatory suppression was to interfere with participants' ability to rehearse verbal labels, and thereby promote the use of visual representations (e.g. Brandimonte, Hitch, & Bishop, 1992). Second, participants performed 16 practice trials at the beginning of their first block only, and could opt out of subsequent practice trials, because most participants in Experiment 1 seemed not to benefit much from the additional practice.

*Participants*

Seventeen Vanderbilt University undergraduate students participated for course credit. One subject failed to complete the experiment, leaving a final N = 16.

*Results*

*Probe Reaction Time*

For the RT analysis, 9.7% of trials were removed due to response errors and 1.4% of trials were removed as outliers. The data were analyzed in the same way as Experiment 1.

First, the difference between match and mismatch RTs were compared within each condition and no differences were found (all *t's* (15) < 1.35, *p's* > .19; see Figure 3). As depicted in Figure 3, the RT difference between match and mismatch trials in each of the conditions was extremely small. Notice that in the STM/Different category, the difference between match and mismatch trials is in the direction of a match-trial *disadvantage*. Thus, these results do not replicate Experiment 1, or Downing (2000).

A location by distractor category by memory condition ANOVA on RTs yielded a significant 3-way interaction ($F$ (1,15) = 4.565, $p$ = .05), but no significant main effects or 2-way interactions, all *F's* (1,15) < 1.296, *p's* > .27. The 3-way interaction was probably an artifact, because location by distractor category ANOVAs conducted separately on STM and LTM blocks yielded non-significant effects.

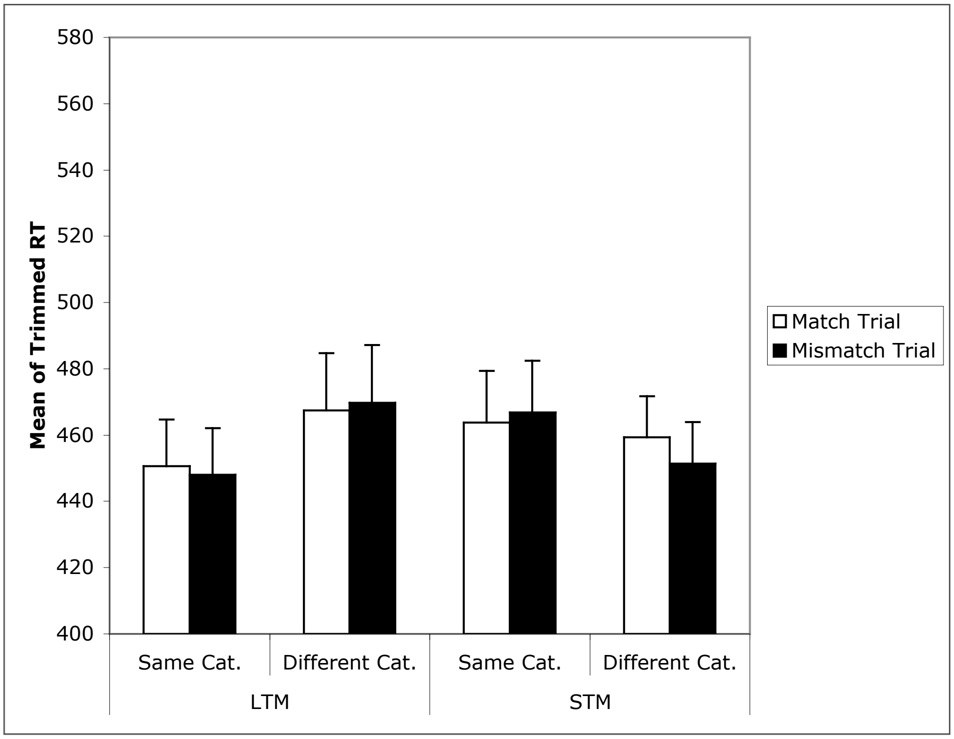


Figure 3. Average trimmed probe RTs from Experiment 2. Error bars represent 95% margins of error for the within-subjects difference between adjacent bars.

*Probe Errors*

Probe errors were generally rare (M = .039). A location by distractor category by memory condition repeated measures ANOVA on probe errors did not yield any significant effects (all *F's* (1,15) < 2.050, *p's* > .17; see Table 2).

Table 2. Proportion of probe and memory errors for Experiment 2.

| | | Probe Errors | | Memory Errors | |
|---|---|---|---|---|---|
| Memory Condition | Distractor Category | Match | Mismatch | Match | Mismatch |
| LTM | Same-category | .044 | .049 | .048 | .043 |
| | Diff-category | .036 | .036 | .072 | .067 |
| STM | Same-category | .035 | .035 | .081 | .086 |
| | Diff-category | .038 | .047 | .031 | .048 |

*Memory Errors*

A location by distractor category by memory condition repeated measures ANOVA on memory errors replicated some, but not all, of the effects that were found in Experiment 1. There were no main or interaction effects involving location, *F's* (1, 15) < 1.5, *p*'s > .2. There was also no main effect of memory condition (*F* (1, 15) < 1) or distractor category, *F* (1, 15) = 2.062, *p* > .17. However, as in Experiment 1, there was a significant memory by distractor category interaction, *F* (1, 15) = 13.551, *p* = .002. For the STM blocks, there were more memory errors in the same category condition (M = .083, SE = .013) than the different category condition (M = .040, SE = .006; *F* (1, 15) = 12.218, *p* < .01), replicating the results from Experiment 1. Surprisingly, for the LTM blocks there were more memory errors in the different category block (M = .069, SE = .017) than in the same category block (M = .046, SE = .010), *F* (1, 15) = 5.041, *p* < .040. Thus, when the distractor and memory match were from the same category, memory decisions were more accurate than when the distractor item was from a different category. This effect is counterintuitive because it seems reasonable to expect that similar distractors should interfere with participants' memory more than dissimilar distractors. One possibility is that participants thought that same category distractors would interfere with their WM representation and therefore overcompensated by trying harder to remember the WM item. A similar hypothesis is that the mere exposure to a same-category distractor strengthens the memory representation.

*Discussion*

In Experiment 2, there were no reliable match-trial advantages *or* disadvantages. Thus, the articulatory suppression task seems to have wiped out the match-trial advantage that was found in Experiment 1's STM/Different category block.

## Discussion: Experiments 1 and 2

Experiments 1 and 2 failed to confirm the automatic guidance hypothesis. In Experiment 1, there was some evidence that memory matches attracted attention in the STM/Different category block. However, this result cannot be interpreted as evidence that WM's contents automatically guide attention for three reasons. First, the result itself depends upon the removal of one participant who seemed to have attended to distractors and *not* memory matches. Second, the effect failed to replicate in Expeirment 2 when participants performed articulatory suppression. Finally, there was no evidence that memory matches attracted attention in any of the other conditions.

If WM matches do not automatically attract attention, then the guidance of attention based upon WM's contents could be strategic. For example, it is possible that WM matches attract attention only when observers intentionally search for them. In other words, when an observer's priority is to search for memory matches, then memory matches might attract attention. Taxing verbal working memory loads can interfere with maintaining attentional priorities (de Fockert et al., 2001; Lavie & de Fockert, 2005; Lavie, Hirst, de Fockert, & Viding, 2004). Accordingly, the articulatory suppression task, coupled with the WM load and/or LTM retrieval, may have interfered with observers' ability to maintain an item in WM as the target of an intentional search. This hypothesis could also explain why other researchers have failed to find evidence that VWM guides attention when WM capacity is taxed (Downing & Dodds, 2004; Woodman et al., 2001). This idea also might explain the outlying participant from Experiment 1 who showed such a large match-trial disadvantage: perhaps he/she did not *want* to search for WM matches.

If participants' self-initiated strategies influence whether WM guides attention, it might be tempting to conclude that the automatic guidance hypothesis is false. However, these conclusions are premature for several reasons, not the least of which is that the experiments reported thus far have not demonstrated that self-initiated strategies play an important role determining whether WM guides attention. The next chapter begins with a detailed analysis of the conditions necessary for testing the automatic guidance hypothesis, then reviews existing literature in light of these conditions, and finally reports four new experiments that were designed to be sensitive to participant initiated strategies.

# CHAPTER III

## EXPERIMENTS 3, 4, 5 and 6

The goal of this chapter is to shed light on the ambiguities of previous research thought to be consistent with automatic guidance. Thus, this section begins with an explication of the conditions necessary for adequately testing the automatic guidance hypothesis. Next, previous research and Experiments 1 and 2 will be discussed in light of these conditions. Finally, four experiments are reported that empirically investigated whether the method adopted in Experiments 1 and 2 and in Downing (2000) would yield evidence consistent with the automatic guidance hypothesis when these conditions are accounted for.

### Four Conditions for Testing the Automatic Guidance Hypothesis

At least four criteria must be met by any experiment in order to distinguish between the automatic guidance hypothesis and the optional guidance hypothesis. These are described below.

#### *First Condition: Know the Contents of Thought*

The first condition is that experimenters should know what participants are thinking about at the time when attention is deployed. If WM's contents are not known, then claims about how WM's contents interact with attention cannot be made.

*Second Condition: Robust Representation*

The second condition is that the representation being maintained WM should be robust. That is, it should not be competing with other representations. In order to test clearly how WM's contents affect the competition among objects in a scene, it is necessary to isolate the competitive interactions of the objects in the scene. If WM is taxed, then WM representations might compete amongst themselves, which could in turn attenuate any content-based attentional guidance that might occur (Downing & Dodds, 2004). This condition is necessary because if the representation is not robust, then a failure to find any interactions between thought and attention could be due to degraded representations that are not fully functional (see discussion by Soto et al., 2005, *p*. 259).

*Third Condition: Clearly Measure Attention Deployment*

The third condition is that the task used to measure attentional deployment should unambiguously reflect how attention deploys to external objects (to the extent that this is possible). This condition is necessary because some tasks reflect attentional deployment only if particular theoretical assumptions are adopted. If these assumptions are misplaced, the results might not have implications for the hypothesis of interest. In visual search, for example, increased RTs are often assumed to reflect attention deployment. However, increased reaction times might also reflect increased decision time (Huang & Pashler, 2005), or time to compare items serially in memory (Sternberg, 1966).

*Fourth Condition: Account for Participant Initiated Strategies*

The fourth condition is that participant initiated strategies should be accounted for. This condition is necessary, because the hypothesis is that thinking about something *constitutes* preparing to attend to similar inputs. The implication is that inputs that match WM's contents are prioritized for selection by virtue of the fact that there is a match between the two. If an experiment's protocol leads observers to *purposefully* shift attention toward items that match currently maintained representations, then the results might show an aggregate effect that resembles what would happen if guidance were automatic. However, if participants who do not intentionally shift attention do not show the same pattern as those who do, then it is not accurate to claim that WM's contents are automatically controlling deployments of attention. For example, perhaps the outlying participant in Experiment 1 simply chose not to search for memory matches, while most other participants chose to search for memory matches.

With these four conditions in place, the next section will review previous experiments that have tested various versions of automatic guidance hypotheses.

## Evaluation of Previous Research

*Evidence Consistent with Automatic Guidance*

One version of the automatic guidance hypothesis is that deciding to search for an object consists of nothing more than forming a vivid visual image (Pashler & Shiu, 1999). Support for this idea comes from experiments by Pashler & Shiu (1999). In these studies, participants formed a vivid mental image of an object, and then viewed a rapid serial visual presentation (RSVP; Potter & Levy, 1969) stream containing line drawings and a to-be-identified target digit. A drawing that matched the imagined object appeared in the RSVP stream before or after the target digit. The key result is that target detection was impaired if the image-match appeared before the RSVP target. This impairment is similar to the *attentional blink* (AB), which occurs when detecting the first search target in an RSVP stream impairs one's ability to detect a second target (e.g. Chun & Potter, 1995; Raymond, Shapiro, & Arnell, 1992). The difference between Pashler and Shiu's studies and AB studies is that observers in Pashler and Shiu were not instructed to search for the blink-inducing image.

With respect to the four conditions, Pashler and Shiu's study fails to satisfy the First Condition (know the contents of thought) because participants were instructed to *discard* their mental image before the RSVP stream was presented, but there is no way to determine whether participants complied. Thus, the observers' representations were unknown at the time of selection; their thoughts could have turned to *anything*, or they could have maintained the image. It is true that searching for the image-match would have impaired participants' ability to perform the target detection task, but participants did not necessarily know this. Indeed, naïve observers grossly underestimate their susceptibility to other failures of visual awareness (Levin, 2004; Levin & Beck, 2004; Levin, Momen, Drivdahl, & Simons, 2000; Scholl, Simons, & Levin, 2004; Varakin, Levin, & Fidler, 2004). Thus, it is conceivable that participants maintained their image, and simply told the experimenters that they discarded it *because* that is what the instructions specified. Therefore, it is impossible to pinpoint exactly why there was an effect in Pashler and Shiu's studies. One could construct an argument that Pashler's study violates other conditions as well, but because the study clearly violates the most basic condition necessary for testing interactions between WM and attention these will not be discussed.

There are many experiments that do satisfy First Condition (know the contents of thought), by embedding a measure of attention deployment within the retention interval of a WM task (e.g. Downing, 2000; Moores et al., 2003; Soto et al., 2005). Experiments 1 and 2 reported above are an example of this basic design. These experiments also satisfy the Second Condition (robust representation), if the WM task involves only one item per trial.

Another important feature of this design is that the attention task (e.g. the probe discrimination task used here) is constructed such that a WM match's presence will not facilitate participants' performance *on the attention task*. That is, performance would not benefit if the memory match were attended on average (e.g. in Experiments 1 and 2, the probe's position was not predicted by the location of the memory match). In other studies, attending to a memory match would interfere with the attention task on all trials (e.g. Soto et al., 2005).

Some studies have used a WM task/attention task design, but have not adequately measured attention deployment, thus violating the Third Condition (clearly measure attention). For example, Soto et al. (2005) used a visual search task to measure attention deployment, but examination of the search task suggests that it might not measure attention deployment as clearly as the probe discrimination task used by Downing (2000). In Soto et al (2005), the WM items were simple shapes, and the search display (presented during the WM retention interval) contained several shapes. On some trials, one search-display shape matched the WM item. Importantly, the shapes themselves were nominally irrelevant to the search; they merely surrounded a search target (a diagonal line) and distractors (vertical lines). The key results are that search was impaired when the memory match surrounded a distractor and search was facilitated when the memory match surrounded the target. In visual search tasks, the degree to which search times increase as a result searching through arrays with more objects (i.e. a larger set size) is assumed to reflect selective attention's capacity limitations; this assumption is crucial for Soto et al's (2005) interpretation. However, recent research suggests RTs in singleton searches do not necessarily reflect limitations of attention deployment (Huang & Pashler, 2005), and Soto et al's task was a singleton search because the target was defined by a single feature (i.e. diagonal; Wolfe, 1998). One possibility is that all items in singleton searches are selected simultaneously, and RTs increase when post-selective decision processes are inefficient. Thus, it is possible that the RT differences in Soto et al. (2005) were due WM's influence on post-selective processes, and not attention deployment.

The probe discrimination task used by Downing (2000) and in Experiments 1 and 2 seems to measure attention deployment more clearly than visual search. In this task, the memory match and distractor were flashed on the screen *before* the response probe. Thus, the only way in which probe responses could have benefited on match trials is if attention was already focused at the location where the memory match was flashed (c.f. Posner, Snyder, & Davidson, 1980). Any post-selective processing of the memory match that was independent of spatial location would not give rise to a match-trial advantage.

While the procedure used by Downing (2000) and in Experiments 1 and 2 satisfies the First, Second and Third Conditions (know the contents of thought, robust representation and clearly measure attention) it does not satisfy the Fourth Condition (account for participant initiated strategies). In these studies, the item in WM was presented over the retention interval, and although attending to it would not have facilitated responding for the attention task, it could have helped with the WM retention task (the same could be said of Soto et al., 2005). Assuming that participants could easily figure this out, they might have intentionally searched for memory matches. Support for this conjecture comes from the repeated finding that WM is subject to strict capacity limitations (Alvarez & Cavanagh, 2004; Luck & Vogel, 1997; Olsson & Poom, 2005; Todd & Marois, 2004); it is difficult for people to hold even small amounts of visual information active in WM. Indeed, in some situations, the capacity of WM is estimated to be about one object (Alvarez & Cavanagh, 2004; Olsson & Poom, 2005; Rensink, 2000). If attention is deployed to items that match the contents of WM *only* when observers are searching for the item that is in WM, then these results tell us little about WM's contents control of mechanistic and involuntary attention deployment. Thus, the strength of the conclusions that can be made based upon the data reviewed so far is severely compromised.

*Evidence Inconsistent with Automatic Guidance*

A few studies are inconsistent with automatic guidance, but these studies also fail to meet all of the conditions necessary for adequately testing the idea. For example, if visual search involved the continual transfer of information into and out of WM, then loading WM to capacity should impair search efficiency. However, loading WM to capacity does not affect the efficiency of search, even when the memory items' features are similar to the search array items' features (Woodman et al., 2001). Furthermore, visual search for a complex polygon is unaffected when a memory match appears as a distractor compared to when no memory match is present (Downing & Dodds, 2004). These results appear to disconfirm the automatic guidance hypothesis. However, these results are equivocal because they violate the Second Condition (robust representations). WM loads can interfere with spatial and selective attention tasks (de Fockert et al., 2001; Han & Kim, 2004; Lavie & de Fockert, 2005). Thus, loading WM with many items as in Woodman et al., or more difficult items as in Downing and Dodds, could interfere with the effectiveness of any given item to guide attention, perhaps because of competition among items or items' parts in WM.

More recent research has demonstrated that certain visual searches can be executed *faster* when memory matches are present in a search array than when they are absent and when observers made aware of non-target status of memory matches (Woodman & Luck, in press). This finding appears to disconfirm the automatic guidance hypothesis, because the presence of a memory match as a distractor did not slow search times (as happened in Soto et al., 2005), but rather, the memory match allowed faster searches. This finding appears to follow from the idea that memory matches do not automatically attract attention, but the explanation is somewhat paradoxical, because it violates the Third Condition (clearly measure attention). Basically, the claim is that observers were using the contents of WM to *reject* portions of a search display that matched WM's contents, because the participants knew that memory matches were never paired with visual search targets. According to this idea, the presence of an item matching WM's contents allowed observers to avoid deploying attention to that item's location, thereby reducing search time. However, this explanation assumes that a given item at a given location can be rejected *before* it is selected. How can one know to avoid a certain location if they do not know where that location is? An alternative account for this finding that avoids this logical dilemma is that attention deployed to the memory match, but the decision about whether it was the target did not have to be made again because the target-status was already known. If the attention shift to the memory match was automatic, and thus faster than a volitional attention shift (Wolfe et al., 2000), and no time was needed to reject the memory match as the target, then these findings can be reconciled with the idea that memory matches automatically attract attention. Thus, when WM's contents act as a template for rejection, it is plausible that savings in search time are due to savings in decision time, and not attention deployment.

In summary, several results converge in supporting the automatic guidance hypothesis. However, none of these experiments has met all of the conditions necessary for testing it. As such, these interesting results might not be testing involuntary and mechanistic interactions between WM and attention. The few results that run counter to the automatic guidance hypothesis are also ambiguous, because they too fail to meet the all of the criteria necessary for adequately addressing the issue. As such, the automatic guidance hypothesis is still in need of empirical confirmation.

## Experiment 3

Because the procedure used in Experiments 1 and 2 meets most of the criterion outlined above, it was used again in Experiment 3, with a new manipulation. The goal was to remove any incentive for participants to search for memory matches. In the *exact-match* condition, the memory match and the WM item were exactly the same. However, in the *category-match* condition, the memory match was a different object from the same category as the WM item (see Figure 4). In category-match condition, the WM item, memory match and recognition foils were different objects from the same category. Thus, the memory match could not serve as a reminder for the WM item or provide a preview of what a recognition foil might look like. However, if WM automatically guides attention, then the category matches should be attended. Both Experiments 1 and 2 failed to find any evidence that exact matches attract attention in the presence of same category distractors, consistent with the idea that visually and categorically similar items can attract attention (c.f. Duncan & Humphreys, 1989; Moores et al., 2003). Thus, WM should guide attention to memory matches in the category match condition *if* guidance is automatic.

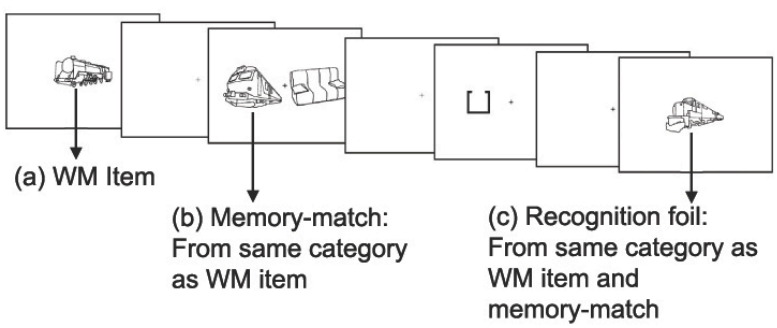


Figure 4. An example of a *category-match* trial from Experiment 3.

In Experiment 3, the WM item and recognition foils were from the same category, so participants had to remember specific details of the target items (i.e. a basic-level label would be insufficient to discriminate the WM item from recognition foils). However, the memory match and distractor items in both conditions were from different basic-level categories. Note, that this addresses the confound between the STM/Different-category and LTM/Different-category blocks that existed in Experiments 1 and 2.

Participants also filled out a post-experiment questionnaire asking them if they intentionally shifted attention to the memory matches in either block, because participants might intentionally attend to memory matches *even though* there is no incentive to do so. If participant's strategies partially determine how WM interacts with attention, then participants who report attending to memory matches might show a match-trial advantage, while those who report otherwise might not.

### *Method*

#### *Participants*

Sixteen Vanderbilt University undergraduate students participated for course credit. Error rates were generally high in this experiment, and four subjects were dropped from all analyses due to chance responding (~50% accuracy) on at least one task in one or both blocks, leaving a final N = 12.

#### *Materials*

The materials and stimuli were the same as Experiment 1.

#### *Design and Procedure*

The conditions in Experiments 3 were blocked and counterbalanced across participants. As in Experiments 1 and 2, which objects repeated as WM items across blocks was determined randomly. In most respects, the composition of each block was similar to STM blocks in Experiments 1 and 2 (i.e. 96 trials; 16 practice trials etc.).

Participants were given full instructions on how to perform the tasks, and were told about the exact and category-match blocks at the beginning of the experimental session, and again at the beginning of each block. They were instructed that they did not have to respond to these items, but were given no information about their purpose.

The post-experiment questionnaire was administered after participants finished both blocks of trials. The questionnaire was paper and pencil. The first question asked participants if they intentionally shifted attention to the location where the memory item was flashed (i.e. if they shifted attention in the exact-match block), and to further explain why they did so and how often. The second question asked participants if they intentionally shifted their attention to the item matching the memory item's category (i.e. in the category-match block), and also asked for explanation.

*Results*

Participants were divided into groups based upon the responses to the post-experiment questionnaire. The *no-search* group (N = 5) unambiguously reported that they did not intentionally search for the memory matches in any block of trials (i.e. their responses were simply 'no', with little or no elaboration). The *yes-search* group (N = 7) intentionally searched for memory matches in at least one block.

*Probe Reaction Time*

For the RT analysis, 15.5% of trials were removed because of errors (see Table 3 for error data), and 1.1% of trials were discarded as outliers. For each block, match-mismatch significance tests were conducted for the yes-search and no-search groups separately. These analyses failed to yield any significant results (see Figure 5).

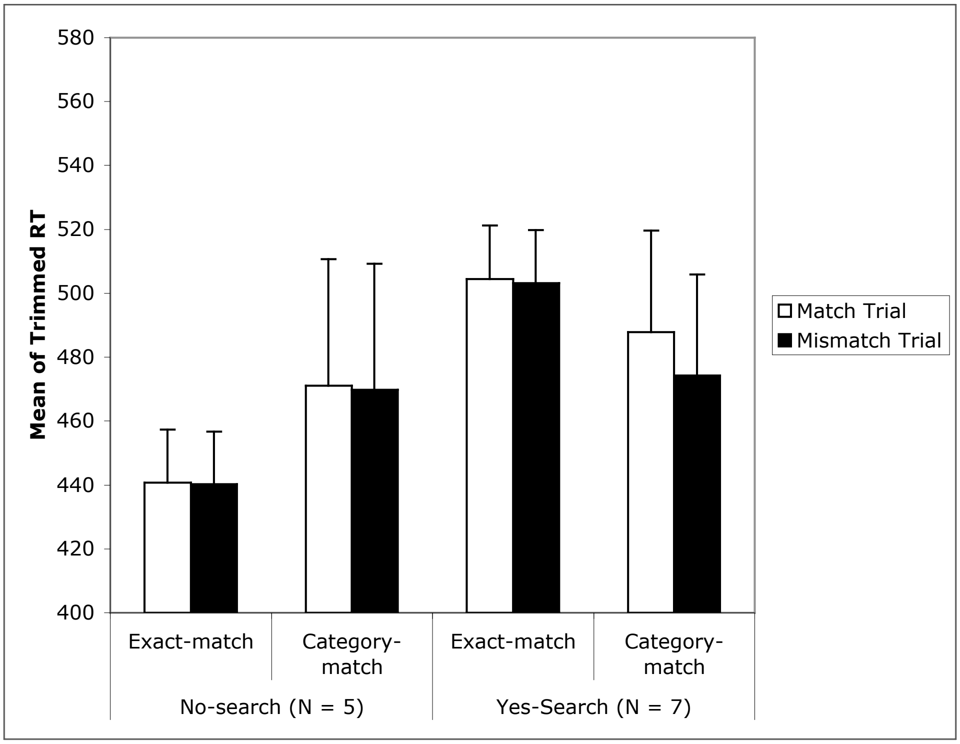


Figure 5. Average trimmed probe RTs from Experiment 3. Error bars represent 95% margins of error for the within-subjects difference between adjacent bars.

RTs were further examined with location (match vs. mismatch) by match-type (exact vs. category match) by strategy (no-search vs. yes-search) ANOVAs. Location and item were within-subjects factors and strategy was a between subjects factor. There were no significant effects in the RT analysis, all $F(1, 10)$*'s* $< 1.3$, $p > .280$.

*Probe Errors*

For probe errors, participants in the no-search group made significantly fewer errors on mismatch trials ($\underline{M} = .038$) than match trials ($\underline{M} = .096$) in the category-match block (see Figure 6 and Table 3), $\underline{SE}_{Diff} = .0202$, $t(4) = 2.888$, $p = .045$. All remaining match vs. mismatch pairwise comparisons were non-significant ($p > .05$). However, a location by match type by strategy ANOVA further revealed a significant interaction between strategy and location, $F(1, 10) = 10.899$, $p < .01$. None of the other effects were significant, all *F's* $(1, 10) < 2.833$, $p > .122$. Participants in the yes-search group made more probe errors on mismatch trials (M = .054, SE = .019) than match trials (M = .039, SE = .021), while the no-search group made more errors on match trials (M = .088, SE = .025) than mismatch trials (M = .042, SE = .023). Two follow up location by match type ANOVAs were conducted separately for each group. The effect of location was nearly significant in the no-search group ($F(1, 4) = 5.939$, $p = .071$), and was not significant in the yes-search group, $F(1, 6) = 3.333$, $p > .1$. The effect of match type was not significant for either group (both F's < 1). Thus, these results suggest that participants who did not search for the memory matches showed a slight match-trial *disadvantage* in terms of probe discrimination errors (i.e. more errors on match trials than mismatch trials). These data are inconsistent with the automatic guidance hypothesis, but are consistent with the idea that participant initiated strategies influence interactions between WM and attention.

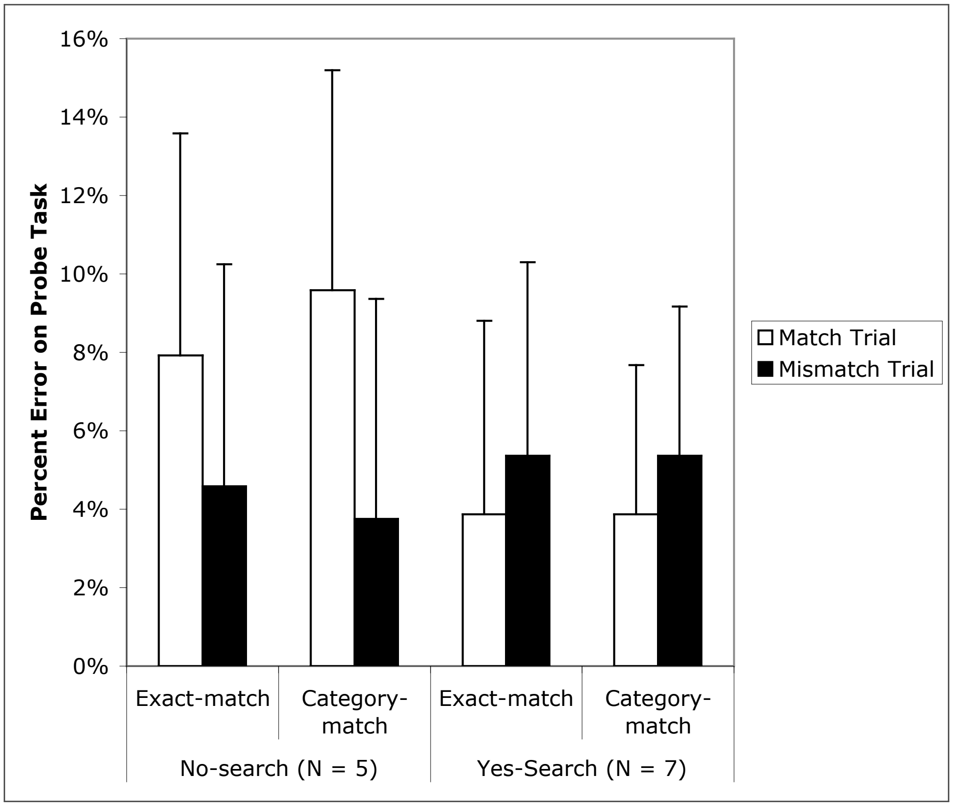


Figure 6. Probe errors from Experiment 3. Error bars represent 95% margins of error for the within-subjects difference between adjacent bars.

Table 3. Proportion of probe and memory errors for Experiment 3.

| | | Probe Errors | | Memory Errors | |
|---|---|---|---|---|---|
| Group | Block | Match | Mismatch | Match | Mismatch |
| Yes-search | Exact-match | .039 | .054 | .113 | .095 |
| (N = 7) | Category-match | .039 | .054 | .170 | .116 |
| | | | | | |
| No-search | Exact-match | .079 | .046 | .092 | .075 |
| (N = 5) | Category-match | .096 | .037 | .121 | .083 |

*Memory Errors*

For memory errors, there was a significant main effect of location, $F(1, 10) = 5.495$, $p < .05$. Fewer memory errors were made on mismatch trials (M = .092, SE = .012) than on match trials (M = .124, SE = .017). This result is difficult to interpret in terms of the hypotheses under investigation here, because the hypotheses concern attention and not memory performance. However, it not obvious why there should be a match-trial disadvantage in terms of memory performance if observers attended to the memory match, as the automatic guidance hypothesis posits. There were no other significant effects on memory errors, *F's* $(1, 10) < 2.252$, $p > .16$. Importantly, there were no differences in memory errors between the yes-search and the no-search groups, $F(1,10) < 1$.

*Discussion*

The most interesting effect in Experiment 3 was the interaction in probe accuracy between participants' strategy reports and the location at which the probe appeared. Participants who reported searching for memory matches showed a slight (but statistically insignificant) match-trial advantage in terms of probe errors. In contrast, participants who did not report intentionally searching for memory matches showed a match-trial disadvantage in probe errors that was significant in the category-match block, and marginally significant overall. The match-trial disadvantage is opposite of what the automatic guidance hypothesis predicts. Thus, these data provide some empirical support for the idea that participants' self-initiated strategies play an important role in how WM interacts with attention, and further hint that objects that do not match WM representations can attract attention in some cases.

Experiment 3's results were born out in errors, rather than RTs, perhaps because the WM task was fairly difficult because it required participants to maintain detailed representations of the WM item. Thus, Experiment 4 used a method similar to Experiment 3's, but relaxed this requirement.

## Experiment 4

Experiment 4 was nearly identical to Experiment 3, except that participants needed only to maintain information at a basic level of abstraction. Thus, memory matches in the category-match condition could potentially serve as reminders for the to-be-remembered *category*, but not the visual details. However, post-experiment questionnaires were used again in Experiment 4, so this experiment allows testing whether participants' strategies could account for match-trial advantages (or disadvantages) in a context more similar to that used in Downing (2000).

### *Method*

Experiment 4 was identical to Experiment 3 with a few exceptions. First, recognition foils were from a different category than WM items. Second, the post-experiment questionnaire was changed to multiple-choice. The first question asked participants to indicate if they intentionally shifted their attention to the location where the memory match was flashed in the exact-match block. The second question asked participants if they intentionally shifted their attention to the item matching the target item's category (i.e. in the category-match block). Both questions had the same four choices. The first three choices were affirmative (i.e. the participant shifted attention to the memory match), and differed in regards to the frequency with which the participant thought they shifted attention (i.e. on almost every trial, on the first few trials only, or only occasionally). The fourth choice was negative (i.e. the participant did not intentionally shift attention). For both questions, if participants answered with one of the affirmative choices, they were asked to choose a statement that best described why they shifted attention. The reasons were, "I couldn't help it, it just happened", "I thought it would help me remember the target" and "I did it just to see if it [the orienting item] really appeared on every trial". These choices were chosen based on an informal evaluation of the open-ended responses provided by participants in Experiment 3.

*Results*

Participants were again divided into two groups based on their responses to the post-experiment questionnaire. There were five no-search participants (i.e. participants who answered negatively to both questions) and eight yes-search participants (i.e. participants who answered affirmatively to one or both questions).

*Probe Reaction Time*

RTs were trimmed (1.1% removed) and error trials were discarded (9.0% removed) as in the previous experiments (see Table 4 for error data). Similar to previous experiments, the first set of analyses compared match and mismatch trials within same item and different item blocks separately for yes-search and no-search groups. For the no-search group, there was a significant match-trial disadvantage in the exact-match block (match $\underline{M}$ = 539, mismatch $\underline{M}$ = 493, $\underline{SE}_{Diff}$ = 10.74), *t* (4) = 4.201, *p* < .02, and no effects in the category-match block (match $\underline{M}$ = 531, mismatch $\underline{M}$ = 539, $\underline{SE}_{Diff}$ = 9.97; *t* (4) = 1.358, *ns*; see Figure 7). For the yes-search group, there was a significant match-trial advantage in the exact-match block (match $\underline{M}$ = 481; mismatch $\underline{M}$ = 504, $\underline{SE}_{Diff}$ = 6.73, *t* (7) = 3.331, *p* < .02) but not in the category-match block (match $\underline{M}$ = 514, mismatch $\underline{M}$ = 525, $\underline{SE}_{Diff}$ = 9.97), *t* (7) = 1.055, *ns*.

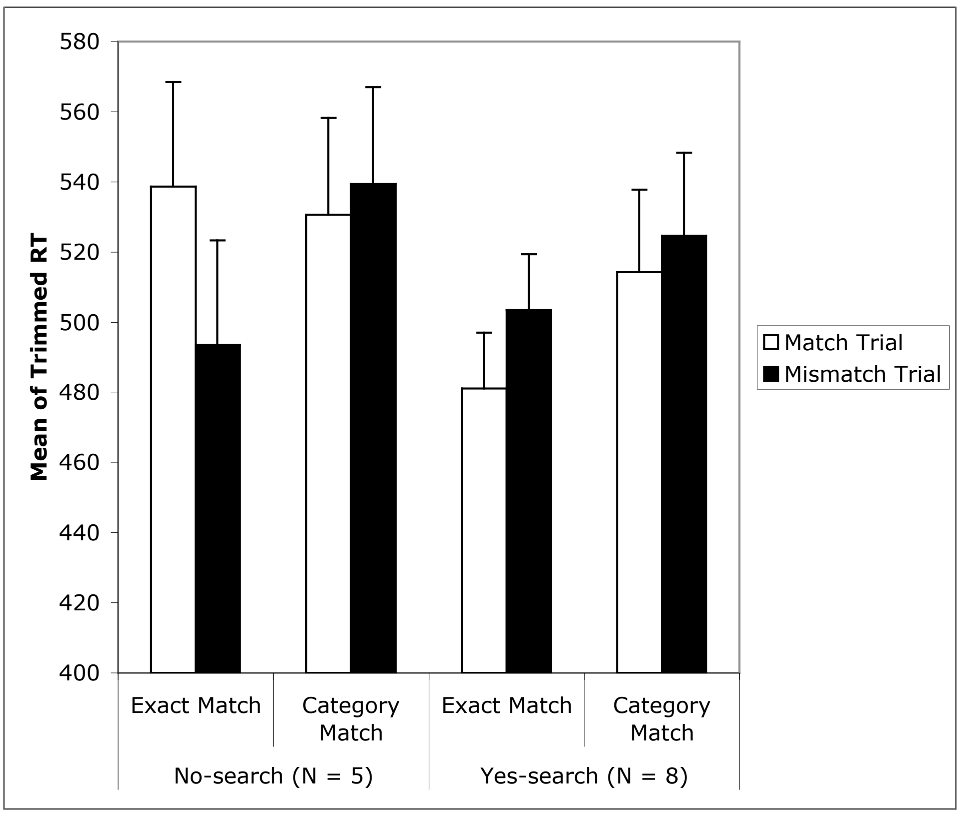


Figure 7. Average trimmed probe RTs from Experiment 4. Error bars represent 95% margins of error for the difference between adjacent bars.

A location (match/mismatch) by match type (exact/category) by strategy (yes-search/no-search) ANOVA was also conducted to double check the initial comparisons. The main effects of location, item and strategy, and the interaction between item and strategy were not significant, *F's* (1,11) < 1.010. However, the location by strategy ($F$ (1, 11) = 12.353, $p < .01$), location by match type ($F$ (1, 11) = 6.597, $p = .026$) and location by match type by strategy ($F$ (1, 11) = 16.320, $p < .01$) interactions were significant, consistent with the results of the match-mismatch pairwise analyses. Because both the yes-search and no-search groups showed a slight match trial advantage in the category-match block, a location by strategy ANOVA was separately conducted on these data only to see if it was significant, but it was not, $F$ (1, 11) = 1.978, $p = .187$.

*Probe Errors*

There were no significant effects in the probe error analyses. Overall, probe errors were rare (M = .048)

Table 4. Proportion of probe and memory errors for Experiment 4.

| | | Probe Errors | | Memory Errors | |
|---|---|---|---|---|---|
| Group | Match Type | Match | Mismatch | Match | Mismatch |
| Yes-search | Exact-match | .031 | .031 | .031 | .056 |
| (N = 8) | Category-match | .067 | .036 | .041 | .051 |
| No-search | Exact-match | .058 | .063 | .096 | .054 |
| (N = 5) | Category-match | .038 | .071 | .080 | .042 |

*Memory Errors*

A location by match type by strategy ANOVA was conducted on memory errors. The main effects, and the match type by strategy, location by match type and the 3-way interaction were all non-significant, $F$ (1, 11) < 1.53, $p$ > .240. The location by strategy interaction was significant, $F$ (1, 11) = 9.764, $p$ = .01. There was a marginally significant match trial advantage for the yes-search group (match M = .036, mismatch M = .053; $F$ (1, 7) = 5.417, MSE < .001, $p$ = .053), while the no-search group showed a slight, but statistically insignificant match-trial disadvantage (match M = .088, mismatch M = .048, $F$ (1, 4) = 3.861, $MSE$ = .002, $p$ = .121). These results lend some support to the idea that the strategy of searching for the memory match can strengthen its WM representation.

*Discussion*

The yes-search group's match-trial advantage in probe RTs in the exact-match block replicated Downing (2000). In contrast, no-search participants showed a reliable *match-trial disadvantage* in RTs in the exact match block. Again, there were no effects in the category-match blocks in either group, and overall memory performance was equivalent in both groups. These results provide more converging evidence that participant initiated strategies play an important role in how WM interacts with attention, at least in the conditions tested by Downing (2000) and in Experiments 1 and 2. Specifically, these results are consistent with the idea that WM *can* guide attention when observers adopt a strategy of searching for memory matches, but when observers are not searching for memory matches, then attention can deploy to objects that are not currently represented in WM.

However, several alternative explanations for match-trial disadvantages should be considered. First, because strategy reports were obtained after the experimental session, it remains possible that strategy is related to individual differences. For example, no-search participants might have a greater WM capacity than yes-search participants, thus, no-searchers do not need to search for memory matches. Alternatively, no-search participants might have less WM capacity, and therefore might have trouble maintaining WM representations as priorities for selection (Lavie & de Fockert, 2005; Lavie et al., 2004). Another possibility is that no-search participants did not use WM. For example, because articulatory suppression was not used in Experiment 4 participants could verbally rehearse items. Furthermore, no-search participants might have transferred items from WM into VLTM, which observers can do fairly easily with stimuli such as the ones used here (Varakin & Levin, 2006), and then based their memory decisions on whether an item had been seen at all. This was possible because most objects were used only once in this experiment, and never more than twice. To address these possibilities, Experiments 5 and 6 attempted to manipulate participants' strategies, reduced the opportunity for verbal rehearsal, and increased the need for participants' to use WM.

## Experiments 5 and 6

Experiments 1-4 failed to find any compelling evidence that WM's contents automatically guide attention, thus justifying further empirical tests of the idea. The discussion for Experiment 4 provided some ways in which the automatic guidance hypothesis could accomodate the results of Experiments 1-4, and Experiments 5 and 6 were designed to address these issues. However, there are also theoretical reasons for questioning whether WM *should* automatically guide attention, and the design of Experiments 5 and 6 was also influenced by such considerations, which are discussed next.

*Should WM's Contents Automatically Guide Attention?*

Consider that one of the primary functions of attention is to select certain information for further processing and transfer into WM (e.g. Schmidt, Vogel, Woodman, & Luck, 2002). If WM's contents likewise controlled what is attended, how would observers avoid getting stuck in a cycle of attending to something, having their attention guided back to it, getting it back into WM, having their attention guided back to it etc.? This simple model is obviously flawed and not intended to represent any particular theory, but it should illustrate one reason why the automatic guidance hypothesis, if it is indeed true, is at least in need of elaboration and limiting conditions: people need to and *do* avoid such attend-represent-attend cycles. Observers need to learn about their environment, and this necessitates paying attention to less familiar items (Johnston & Hawley, 1994). Thus, items in WM (which are somewhat familiar by virtue of the fact that they are already represented) should not always guide attention especially in the presence of novel items, because then observers' ability to attend to new information would be hindered.

Indeed, the phenomenon of *novel popout* (Johnston & Hawley, 1994; Johnston, Hawley, & Farnham, 1993; Johnston, Hawley, Plewe, Elliott, & DeWitt, 1990; Johnston & Schwarting, 1997) is consistent with the idea that attention is automatically attracted to relatively novel stimuli. In typical novel popout study (e.g. Johnston et al., 1993;

Johnston & Schwarting, 1997), observers viewed a briefly flashed array of words on each trial, and were subsequently probed as to where on the monitor one of the words had been flashed. On most trials, the word arrays contained the same four words, but every once in a while a new word would be presented with the repeated words, or an array of all new words would be presented. Not surprisingly, performance was better for arrays of all old words than all new words. However, the relevant finding is that in arrays of one new word and three old words, observers were more accurate at localizing the *new* word relative to the old words, as if the new (i.e. novel) word popped out of the array containing old words.

While these and subsequent findings certainly show that novel items receive benefits on mixed novel/old arrays, localization accuracy as assessed by such recognition tests is not ideal for studying rapid shifts of attention (Christie & Klien, 1996). For example, a novel item advantage could arise based upon post-selective processing, or retrieval processes, rather than attentional selection per se (as defined here). Furthermore, other researchers have found that *familiar* items are localized better when they appear with all novel items (Diliberto, Altarriba, & Neill, 2000), as are categorically distinct words appearing in arrays containing all novel words (Diliberto, Altarriba, & Neill, 1998). Despite ambiguities surrounding the evidence that novel items capture attention, the general idea remains a viable alternative (or at least supplement) to the automatic guidance hypothesis. Indeed, the match-trial disadvantages in Experiments 3 and 4, and Downing (2000) can be viewed as novel popout based upon a more direct measure of attention deployment. However, as mentioned previously, it is possible that these results occurred *because* participants were not maintaining information in WM.

Indeed, when Downing (2000) obtained a match-trial disadvantage, there was no WM load. Thus, Experiments 5 and 6 sought to test whether novel items could attract attention in the presence of memory matches in addition to attempting to manipulate strategies and address concerns about whether participants utilize WM.

Experiments 5 and 6 used the same task as in previous experiments, with changes intended to, 1) increase the need for participants to use WM, 2) minimize observers' chance to intentionally search for items matching their WM representation and 3) investigate whether novel objects could attract attention in the presence of memory matches. Experiments 5 and 6 are reported together, because attempts to discourage participants from intentionally attending to memory matches did not work.

*Method*

*Participants*

Fifteen undergraduates participated in Experiment 5 for course credit. One participant was dropped because she reported using sign language on each trial to aid WM. Error rates for other subjects were below 20%. Seven undergraduates participated in Experiment 6 for course credit. One subject was dropped because of an excessive error rate on critical trials (> 20%).

*Materials*

The materials and stimuli were the same as Experiments 1-4.

*Design and Procedure*

The basic task was identical to previous experiments, but the design was changed in important ways. First, in Experiments 5 and 6, there were no memory matches item on 5/6 of the trials (320 trials total), and on the remaining 1/6 of the trials (64 trials total) there were memory matches. Trials without memory matches will be referred to as *baseline trials*, and trials that contained a memory match will be referred to as *critical trials*. Baseline and critical trials were randomly intermixed. On baseline trials, two task-irrelevant objects were flashed on screen. The same two objects were used for each trial, thus providing a familiar context. On critical trials, a memory match and a novel distractor were flashed on the screen. To maximize the novelty of distractors, these items only appeared in the experiment once and they were drawn from different categories than WM items. The purpose of the baseline trials was to minimize participants' opportunity, and perhaps inclination, to attend to memory matches.

Second, a randomly selected set of four objects served as WM items. Each item appeared an equal number of times in baseline and critical trials. The purpose of this manipulation was to provide observers with a greater degree of familiarity with the memory items. To ensure that observers had to maintain information in WM, the recognition foil items were drawn from the same set of items as the WM items. Thus, participants could (in principle) rely on a coarsely coded WM representation to succeed on the recognition test, but they did have to remember which specific item was the WM item on each trial. Further, an articulatory suppression task was implemented both Experiments 5 and 6 in the same way as in Experiment 2 to hinder participants' ability to engage verbal rehearsal.

The instructions given to observers in Experiments 5 and 6 were slightly different than the instructions given to participants in previous experiments. One difference is that observers were not told that memory matches would appear during the retention interval, and they were told to ignore the flashed items. In Experiment 6, observers were also given an additional instruction adapted from Lleras and von Muehlenen (2004). These investigators used instructions that effectively reversed the repeated-array advantage typically found when observers perform visual search tasks (e.g. Chun & Jiang, 1998; Chun & Jiang, 1999; Chun & Nakayama, 2000). The instructions, which were adapted for the current task, were read to the participants verbatim, and elaborated upon to ensure that participants understood them. The critical portion of the instructions were as follows:

> "The best strategy for this task, and the one we want you to use, is to be as receptive as possible to the locations where the box might appear and to let the box just 'pop' into your mind as you look at the screen. The idea is to let the display and your intuition determine your response."

Two horizontal grey lines were added to the background in Experiment 6, to give participants something to be receptive to besides the flashing objects. The bars were spaced such that the top and bottom of the response probe overlapped with the bars.

*Results*

There were 6 no-search participants (4 from Experiment 5; 2 from Experiment 6). The no-search group included participants who indicated not attending to memory matches at all, and three participants who indicated attending at first, but stopping (2 from Experiment 5; inclusion of these subjects in the no-search group is admittedly post hoc, but there were no subjects who reported this strategy exclusively in Experiments 3 or 4). There were 14 yes-search participants (10 from Experiment 5; 4 from Experiment 6), who reported attending to memory matches throughout the experiment.

*Probe Reaction Times*

For the RT analysis, 11% of trials were removed due to errors (see Table 5) and 1% of trials were removed as outliers (critical trial outliers were defined with respect to a participant's critical trial mean and baseline trials with respect to the baseline mean so that an equal percentage of trials was trimmed from each). An ANOVA on trial type (baseline, match or mismatch) as a within-subjects factor, and with strategy and experiment as between-subjects factors yielded a significant effect of trial type, Wilks' Lambda = .211, $F$ (2, 15) = 28.011, $p < .001$, and a significant trial type by strategy interaction, Wilks' Lambda = .594, F (2, 15) = 5.131, $p = .020$. There were no other significant effects, within subjects factors: F's (2, 15) < 1.140, $p > .345$; between subjects factors: F (1, 16) < 1.

Since there was no effect of Experiment, each trial type was compared to the other trial types in a pairwise fashion separately for yes- and no-search groups (the pattern of significance is the same when experiments are analyzed separately). For the yes-search group, baseline trials ($\underline{M}$ = 387) were faster than match ($\underline{M}$ = 465, $\underline{SE}_{Diff}$ = 9.20, $t$ (13) = -8.460, $p < .001$) and mismatch trials ($\underline{M}$ = 477, $\underline{SE}_{Diff}$ = 11.44, $t$ (13) = -7.880, $p < .001$), but there was no difference between match and mismatch trials ($\underline{SE}_{Diff}$ = 7.79), $t$ (13) = -1.590, $p = .136$. For the no-search group, baseline trials ($\underline{M}$ = 381) were faster than match trials ($\underline{M}$ = 446, $\underline{SE}_{Diff}$ = 11.80, $t$ (5) = -5.493, $p = .003$), and marginally faster than mismatch trials (M = 418, $\underline{SE}_{Diff}$ = 16.43), *t* (5) = -2.223, *p* = .077. Critically, match trials were significantly slower than mismatch trials (see Figure 8, $\underline{SE}_{Diff}$ = 6.57), *t* (5) = -4.297, *p* = .008. These results largely replicate Experiment 4, in that the difference between match and mismatch trial RTs depended on participants self-reported strategies. Yes-search participants showed a match-trial advantage (not significant), and the no-search group showed a match-trial disadvantage.

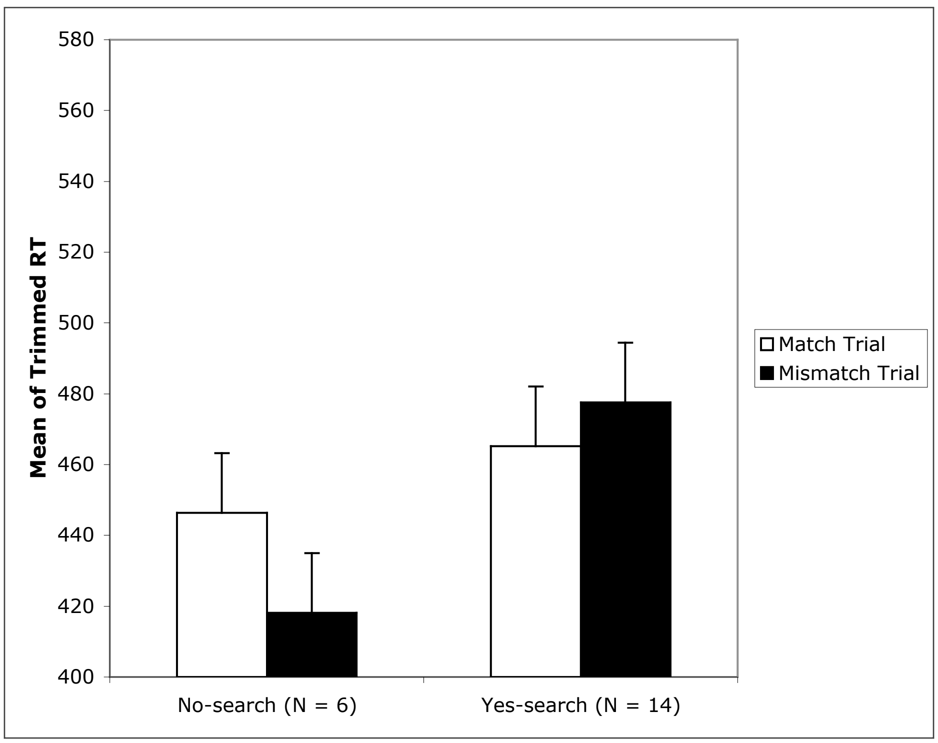


*Figure 8*: Average trimmed probe RTs from Experiments 5 and 6 combined. Baseline trials are not included, because error bars are based on pairwise comparisons. Error bars represent 95% margins of error for the within-subjects difference between adjacent bars.

*Probe Errors*

An ANOVA on probe errors with strategy and experiment as between subjects factors and trial type (baseline, match or mismatch) as a within subjects factor yielded a marginally significant trial type by strategy interaction, Wilks' Lambda = .708, $F$ (2, 15) = 3.095, $p$ = .075, and a significant trial type by experiment interaction, Wilks' Lambda = .596; $F$ (2, 15) = 5.082, $p$ = .021. The other effects were not significant: within subjects factors *F's* (2, 15) < 1; between subjects factors *F's* (1,16) < 1680, $p$ > .212.

Table 5. Proportion of probe and memory errors for Experiments 5 and 6.

| | Probe Errors | | | Memory Errors | | |
|---|---|---|---|---|---|---|
| Group | Baseline | Match | Mismatch | Baseline | Match | Mismatch |
| Yes-search (N = 14) | .032 | .043 | .049 | .071 | .053 | .058 |
| No-search (N = 6) | .040 | .020 | .036 | .087 | .046 | .063 |

Because trial type interacted with both strategy and experiment, two sets of pairwise analyses were conducted. The first compared each trial type to every other separately for yes- and no-search groups. For the no-search group, there were no differences between baseline (M = .04) match (M = .02) and mismatch trials (M = .036), all *t's* (5) < 1.597, *p*'s > .17. The yes-search group made significantly fewer probe errors on baseline trials (M = .032) than match trials (M = .046; $SE_{Diff}$ = .007; $t$ (13) = -2.172, $p$ = .049), but mismatch trials (M = .049) did not differ from baseline ($SE_{Diff}$ = .015; $t$ (13) = 1.202, $p$ = .251), or match trials ($SE_{Diff}$ = .016), $t$ (13) < 1. It might seem odd that the estimated proportion of probe errors was higher in the yes-search group for mismatch than match trials, but only the match trials were significantly different from the baseline trials. This owes to the fact that the SE of the difference between match and baseline trials was half the size than the SE of the difference between mismatch and baseline trials. This result is therefore difficult to explain, but does not have any clear implications for the hypotheses in question. Importantly, there was no difference between the match and mismatch trials in the yes- or no-search groups.

Trial types were also compared separately for each experiment, collapsing over strategy. In Experiment 5, there were no significant differences. Participants in Experiment 6 however made more errors on match trials (M = .042) than on baseline trials (.019) ($SE_{Diff}$ = .009), $t$ (5) = 2.575, $p$ = .05, but mismatch trials (M = .0625) did not differ from match or baseline trials, *t's* (5) < 1.974, *p*'s > .104. Again, the SE of the difference between match and baseline trials was less than half the SE of the differences for the other comparisons.

*Memory Errors*

An ANOVA on memory errors with strategy and experiment as between subjects factors and trial type (baseline, match or mismatch) as a within subjects factor yielded a significant effect of trial type, Wilks' Lambda = .639, $F$ (2,15) = 4.239, $p$ = .035. No other effects were significant: within subjects effects *F*'s (2, 15) < 1.025, $p$ > .382; between subjects effects *F*'s (1, 16) < 1. Pairwise analyses revealed that more memory errors were made on baseline trials (M = .075) than on match trials (M = .051) ($SE_{Diff}$ = .008), $t$ (19) 3.263, $p$ < .01. Mismatch trials (M = .059) did not differ from match or baseline trials, *t's* (19) < 1.51, *p*'s > .147. Interestingly, this effect did not depend on strategy, as might be expected if participant initiated strategies affected attentional selection. Nonetheless, this finding does not seriously compromise the idea that participant initiated strategies influence whether WM guides attention, because recognition accuracy is a poor measure of rapid shifts of attention (e.g. Christie & Klien, 1996). It is possible, for example, that this enhancement of WM performance was due to semantic priming and not attention deployment, which are thought to be dissociable (Dark, Vochatzer, & VanVoorhis, 1996).

*Discussion*

Experiments 5 and 6 provide further evidence against the automatic guidance hypothesis. First, the results again suggest that participant initiated strategies play an important role in determining whether memory matches attract attention, because the difference between match and mismatch trials depended on participants' self-reported strategies. Second, the results again show that attention can deploy to novel items even in the presence of a memory match, because no-search participants showed a reliable match-trial disadvantage. Third, the results suggest that presenting memory matches rarely and unpredictably reduces the efficacy of searching for memory matches, because participants in the yes-search group did not show a reliable match-trial advantage. Thus, although participants' self-reported strategy suggests that searching for memory matches was a priority, the data suggest that memory matches were not effectively prioritized for selection.

# CHAPTER IV

# EXPERIMENTS 7 AND 8

The results of Experiments 1-6 are inconsistent with proposals that the contents of WM automatically guide attention to memory matches. However, there remain several important ways in which WM contents might guide attention automatically. Experiments 7 and 8 were designed to address two of these ways. Experiment 7 investigated the effect of increasing WM load in Downing's paradigm, and Experiment 8 investigated whether WM's contents might guide attention when WM items are presented and maintained in WM for briefer periods of time.

## Experiment 7

Although Experiments 1-6 were designed to test the effects of WM's contents on attention deployment, it is possible that participants did not actively maintain information in WM. In each of the previous experiments, participants had to remember only one item per trial, thus they could have stored it in LTM for a short time. The LTM blocks in Experiment 2, and Experiments 5 and 6 would have additionally required participants to encode a time stamp in LTM, because WM items were drawn from a small set of items and therefore recognition foils would also be present in LTM. However, it is not inconceivable that participants might do this.

Ideally, the WM task should require *active* maintenance of information in WM. There is a large body of research and theory suggesting that people must increasingly rely on active maintenance of information in WM as load increases (Alvarez & Cavanagh, 2004; Cowan, 2000; Luck & Vogel, 1997; Vogel, Woodman, & Luck, 2001). Thus, in Experiment 7, participants had to remember 1, 2, or 3 objects for each trial (see Figure 9).

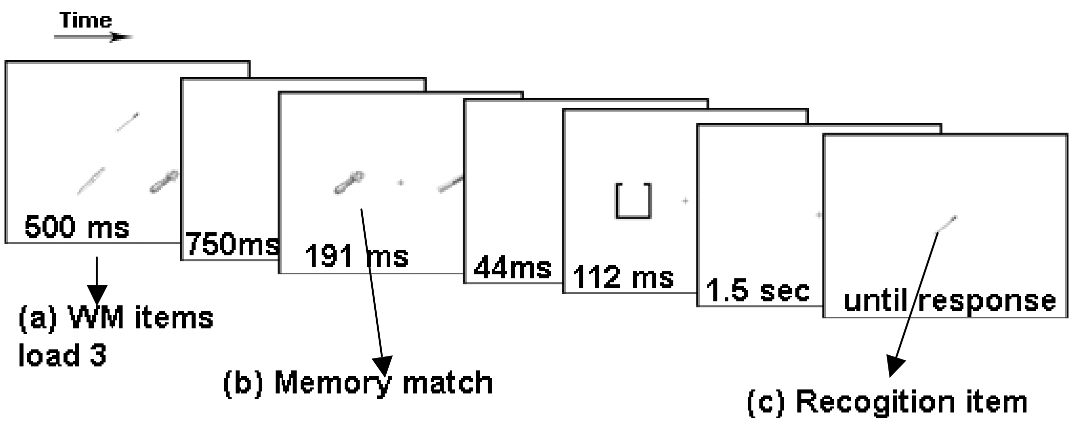


Figure 9. An example of a load-3 match trial from Experiment 7.

This manipulation of WM load appears to violate one of the conditions for testing the automatic guidance hypothesis from in Chapter III. Recall, the Second Condition was that the information in WM should be robustly represented, and should not compete with other representations in WM. The reason for including this condition revolved around concerns that competitive interactions among items in WM could attenuate top down biasing signals from WM, thus eliminating guidance (c.f. Downing & Dodds, 2004). However, because several WM loads were used in Experiment 7, it was possible to track WM load's effect on top down guidance of attention. Specifically, if competition in WM attenuates attentional guidance, one would predict that the guidance by WM should be reduced, perhaps even eliminated, as WM load increases. If guidance is automatic, then this attenuation should be evident regardless of whether participants report searching for memory matches. If WM's guidance is not automatic, then the attenuation should be evident only for participants who report searching for memory matches.

*Method*

*Participants*

Thirteen undergraduates participated in Experiment 7 for course credit. Two participants performed at chance on the probe task and were excluded, leaving an N = 11.

*Materials*

The materials were the same as previous experiments except as follows. Objects were Snodgrass and Vanderwart (1980) images used by Alvarez and Cavanagh (2004). Stimuli were sized to 3.76 x 3.29 degrees visual angle. These objects were used because previous research suggests that WM performance should decline from load-2 to load-3, and further, 191 ms (i.e. the amount of time memory matches and distractors are visible) should be long enough for these specific objects to be discriminated (Alvarez & Cavanagh, 2004).

*Design and Procedure*

The primary task was similar to previous experiments in most respects. The important change to the method was the manipulation of memory load. On any given trial, participants were presented with a WM set of 1, 2, or 3 objects. Because memory errors were expected to increase as WM load increased, there were more load-3 and load-2 load trials than load-1 trials (96 at load-3, 128 at load-2 and 64 at load-1; there were more load-2 trials than load-3 trials so that participants would not become too frustrated and give up). There were also 18 randomly selected practice trials (due to experimenter error, there were 10 match trials and 8 mismatch trials in the practice period; there were equal numbers of each trial type in the experiment proper). For each level of load, each of the 6 objects was used an approximately equal number of times as either a WM item, a memory match, a flashed distractor or a recognition test foil. For load-2 and load-3 trials, the flashed memory match was never presented at the end of the trial on the recognition test to remove any incentive to use it as a reminder (no participants indicated noticing this). In addition, the flashed distractor was never presented as a recognition foil.

For load-1 trials, the only thing that was different from previous experiments was the presentation time of the WM item at the beginning of a trial (reduced to 500 ms), and the interval between the offset of the WM item and the onset of the flashed items (reduced to 750 ms). Load-2 and load-3 trials also used these presentation parameters.

For load-3 trials, objects were presented at the corners of an invisible isosceles triangle, that was 8.76 x 7.15 degree visual angle, horizontally and vertically centered on fixation, with the base either below or above fixation. For load-2 trials, objects were presented in one of four configurations that corresponded to the legs of the triangles. Each configuration appeared an equal number of times, and was uncorrelated with memory match location, probe location, gap position and whether the recognition item was a foil (and all of these latter variables were uncorrelated as well, as in previous experiments).

In Experiment 7, participants were not informed about memory matches, although all participants noticed them. Articulatory suppression was used, as in Experiments 2, 5 and 6.

The post-experiment questionnaire was changed slightly. Separate questions asked whether participants searched for memory matches on load-1, load-2 and load-3 trials. As in previous experiments, participants were included in the no-search group only if they indicated not intentionally attending memory matches in any trials, or if they indicated stopping early for all types of trials.

*Results*

There were five no-search participants and six yes-search participants.

*Probe Reaction Time*

For the RT analysis, 1.1% of trials were eliminated as outliers, 7% due to probe errors, and 20% due to memory errors (elevated error rates for memory decisions were expected because of the load manipulation).

Because the hypotheses were again a priori, match and mismatch trials were compared in a pairwise fashion for the yes- and no-search groups separately (see Figure 10). For the yes-search group, load-1 match trials ($\underline{M}$ = 506) were marginally faster than mismatch trials ($\underline{M}$ = 526; $\underline{SE}_{Diff}$ = 7.79; $t$ (5) = 2.529, $p$ = .053); at load-2 match and mismatch trials were equivalent (both $\underline{M}$'s = 535, $\underline{SE}_{Diff}$ = 9.38, $t$ (5) < 1); at load-3, match trials ($\underline{M}$ = 530) were slower than mismatch trials ($\underline{M}$ = 510; $\underline{SE}_{Diff}$ = 7.35), $t$ (5) = 2.771, $p$ < .05. An ANOVA on the yes-search group's data with load and location (match vs. mismatch) as factors also yielded a significant load by location interaction (Wilks' Lambda = .113, $F$ (2,4) = 15.637, $p$ = .013), but the main effects were not significant, *F's* < 1. For the no-search group, at load-1 match ($\underline{M}$ = 496) and mismatch trials ($\underline{M}$ = 511) were not different ($\underline{SE}_{Diff}$ = 13.73; $t$ (4) = 1.11, $p$ > .3), at load-2 match ($\underline{M}$ = 554) and mismatch trials (M = 549) were not different ($SE_{Diff}$ = 14.40, $t$ (4) < 1), and at load-3, match trials (M = 526) and mismatch trials (M = 528) were not different ($SE_{Diff}$ = 18.91), $t$ (4) < 1. An ANOVA on the no-search group's data with load and location as factors yielded a significant main effect of load (Wilks' Lambda = .124, $F$ (2,3) = 10.589, $p$ = .044), but the other effects were not significant, $F's$ < 1.

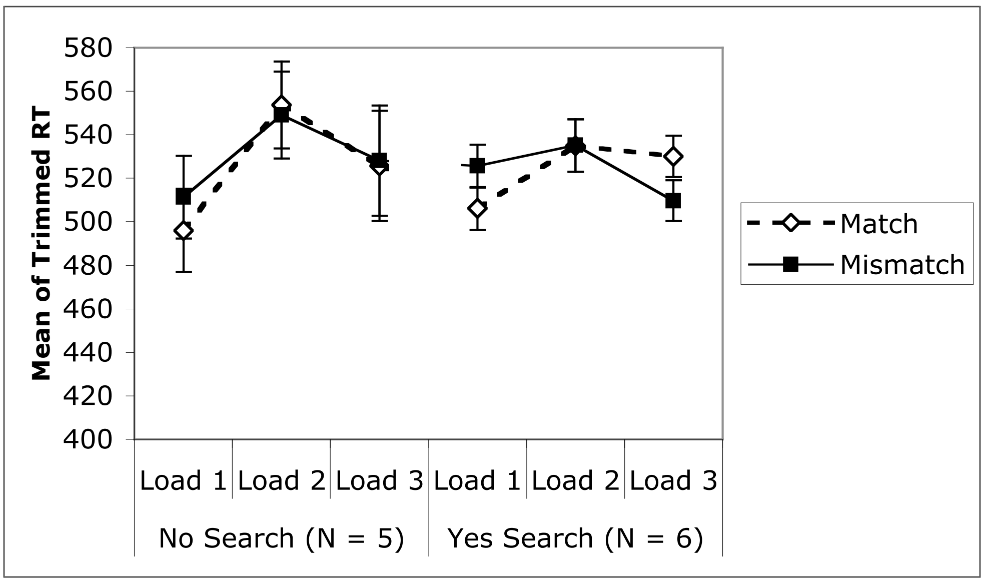


Figure 10. Average trimmed probe RTs from Experiment 7. Error bars represent one half of the 95% margin of error for the difference between match and mismatch trials at a each level of load within each group

An omnibus ANOVA with load and location as within-subjects factors and strategy as a between subjects factor yielded a significant effect of load (Wilks' Lambda = .468, $F$ (2,8) = 5.545, $p$ = .048), but no other significant effects $F's$ < 1.862.

These results largely replicate previous findings and conform to the predictions discussed earlier. That is, WM load modulates the match-trial advantage in probe RTs only for participants who report using WM to attend to memory matches. The failure to find the expected interactions between strategy, location and match in the omnibus ANOVA would be problematic if these were not a priori hypotheses; however, the hypotheses were a priori and the effects are significant at the pairwise and individual group levels.

*Probe Errors*

Probe errors were analyzed in a manner analogous to RTs, but there were no significant effects (see Table 6). Overall, probe errors were rare (M = 7%).

Table 6. Proportion of probe and memory errors for Experiment 7.

| | | Probe Errors | | Memory Errors | |
|---|---|---|---|---|---|
| Group | | Match | Mismatch | Match | Mismatch |
| Yes-search | Load-1 | .089 | .099 | .089 | .078 |
| (N = 6) | Load-2 | .063 | .047 | .162 | .180 |
| | Load-3 | .066 | .069 | .288 | .247 |
| No-search | Load-1 | .088 | .063 | .094 | .106 |
| (N = 5) | Load-2 | .066 | .066 | .228 | .210 |
| | Load-3 | .058 | .092 | .330 | .308 |

*Memory Errors*

Memory errors were analyzed in a manner analogous to RTs, but there were no effects related to strategy or location. However, as expected, there were effects related to load, omnibus Wilks' Lambda = .043, $F$ (2,8) = 88.025, $p < .001$ (see Table 6). As expected, there were more memory errors at load-3 (M = 29%) than at load-2 (M = 19%) or load-1 (M = 9%), and at load-2 than at load-1 (all $p$'s < .003).

Memory errors were also analyzed in terms of the Cowan's K (2000). Cowan's K provides an estimate of the number of objects worth of information (K) that observers have stored based upon observed hit (H) and correct rejection (CR) rates, and load (N): K = [H +CR –1]*N. For each level of load, the maximum capacity estimate is N (e.g. for load-1, $K_{MAX} = 1$; for load-2, $K_{MAX} = 2$, etc.). Thus, K is expected to increase with increases in load until observers' capacity is reached, at which point K estimates should level off. In the current experiments, capacity estimates were estimated at .8 ($K_{YES\text{-}SEARCH} = .8$, $K_{NO\text{-}SEARCH} = .8$), 1.2 ($K_{YES\text{-}SEARCH} = 1.3$, $K_{NO\text{-}SEARCH} = 1.1$), and 1.2 for ($K_{YES\text{-}SEARCH} = 1.4$, $K_{NO\text{-}SEARCH} = 1.1$) for load-1, load-2 and load-3 respectively. An ANOVA with strategy, load and location as factors yielded a significant effect of load, Wilks' Lambda = .300, $F$ (2,8) = 9.344, $p < .01$. Importantly, K estimates for load-1 differed from loads-2 and load-3 ($t$ (10) = 3.93 and 4.05 respectively, *p's* < .01), but load-2 and load-3 did not differ, $t$ (10) < 1. Overall, these results suggest that participants' capacity for these objects was exceeded at load-3 for both yes- and no-search groups.

*Discussion*

The results of Experiment 7 strengthen the optional guidance hypothesis because no-search participants showed a pattern of memory performance that is indicative of a strict WM capacity limit (memory performance declined as load increased, and capacity estimates leveled off) yet at each load there was no difference between match and mismatch trials on probe response. These results also suggest that WM might not automatically play a role in deploying attention at all, because the match-trial disadvantage that was present in Experiments 3, 4, 5 and 6 was conspicuously absent in Experiment 7's no-search group. However, in previous experiments the flashed distractors were always *novel* objects, whereas in Experiment 7 flashed distractors were as familiar as WM items. Thus, although Experiment 7 was not intended to address the degree to which WM items guide attention to *mismatches,* the results suggest that WM contents do not do this. Rather, the match-trial disadvantages in previous experiments likely occurred primarily because of distractors' relative novelty, and not the fact that they did not match the WM item.

In addition to strengthening optional guidance hypothesis, the results of Experiment 7 also provide interesting insights into how WM's contents might affect attention deployment when participants *are* using it to guide attention. Specifically, the match trial advantage evident at load-l for the yes-search group became a match-trial disadvantage at load-3. This result can be explained in terms how competitive interactions among items in WM affect competitive interactions among items in a scene. The match trial advantage at load-1 can be explained in terms of top-down signals from WM's contents conferring a competitive advantage upon memory matches (e.g. the standard explanation for match trial advantages). However, at load-3, there would be three objects providing top-down signals, each for a different memory match. If objects in WM compete with each other by reducing the top-down signals provided by other objects in WM, the net result could plausibly be a *disadvantage* for any stimulus that matches only one item in WM. Basically, the top-down signal provided to a single memory match could be eliminated and possibly reversed by the top-down inhibition from other items in WM. The distractor item, which would not be part of this competition, could then receive attention.

## Experiments 8a and 8b

In Experiments 1-7, the stimulus-onset-asynchrony (SOA) from the WM item to the flashed items was more than 1 second. In terms of the time scales in visual cognitive processing (Hayhoe, 2000), this is enough time for participants to encode an item, maintain it, and perhaps make a decision about how to *use* the information in WM. Thus, any automatic guidance based on WM's guidance could have plausibly worn off by the time the flashed items appeared. Most of the research reviewed thus far has used a time course similar to the one adopted here. One exception is the experiments reported by Soto et al. (2005). In Soto et al.'s experiments, the WM item was presented for about 100 ms, and the memory match was presented about 200 ms later. At this time course, participants should have enough time to fully encode the WM item (Woodman & Vogel, 2005). Also, 300 ms enough time to set up a target template for an exact match, but it is not enough time for participants to *change* a target template; visual search experiments suggest that a search template for an exact match can be set up in about 200-500ms, but a more abstract template cannot be (Vickery, King, & Jiang, 2005). If WM's contents automatically guide attention during early portions of maintenance, then automatic effects should be found at Soto et al.'s time course.

Thus, Experiment 8a used the same basic method as Experiment 4, except that the presentation of WM items was limited to 100 ms, the ISI between WM item offset and presentation of the flashed items was limited to 200ms (to approximate the time used by Soto et al., 2005). In addition, participants engaged in articulatory suppression. If WM's contents automatically guide attention, then match-trial advantages should be observed for both yes- and no-search participants. To preview the results, both yes- and no-search participants showed match-trial advantages, thus, Experiment 8b was conducted to see if it was WM's contents or priming driving the effects. Experiment 8b was identical to Experiment 8a, except there was no WM load.

*Method: Experiment 8a*

The method was identical to Experiment 4, except that the WM item was presented for 100ms, and was followed by the memory match and distractor 200ms later.

*Participants*. Ten Vanderbilt University undergraduates participated for course credit. One participant performed at chance on the probe discrimination task, and was excluded from all analyses leaving a final N = 9

*Results: Experiment 8a*

There were four no-search participants and five yes-search participants.

*Probe RTs*

For the RT analysis, 1.4% removed as outliers and 8% of trials were removed because of errors.

As in Experiment 4, data were first analyzed by comparing match- and mismatch trials in the exact and category match blocks separately for yes- and no-search groups. For the no-search group, match trials ($\underline{M} = 538$) were significantly faster than mismatch trials ($\underline{M} = 554$) in the exact match block ($\underline{SE}_{Diff} = 3.784$, $t\ (3) = 4.157$, $p < .03$), but not in the category match block (match $\underline{M} = 571$, mismatch $\underline{M} = 566$, $\underline{SE}_{Diff} = 14.06$, t (3) < 1). For the yes-search group, match trials ($\underline{M} = 558$) were significantly faster than mismatch trials ($\underline{M} = 592$) in the exact match block ($\underline{SE}_{Diff} = 9.39$, $t\ (3) = 3.720$, $p < .03$), but not in the category match block (match = 553, mismatch = 553, $\underline{SE}_{Diff}$ = 3.09, t (3) < 1). An ANOVA with match type (exact or category), location (match or mismatch) and strategy report (yes or no search) yielded a main effect of location ($F$ (1, 7) = 14.039, $p$ < .01) and an interaction between match-type and location, $F$ (1, 7) = 14.039, $p$ < .01. The remaining effects were not significant, $F$'s (1, 7) < 3.6, $p$'s > .1. Unlike each of the previous experiments in which strategy reports were obtained, match-trial advantages were obtained for *both* yes- and no-search groups. This result is consistent with automatic guidance.

*Probe Errors*

Probe errors were analyzed in a manner analogous to RTs. There were no significant effects between match- and mismatch trials, (see Table 7), all t's < 1.5. An ANOVA with match-type, location, and strategy report yielded a significant match-type by strategy interaction ($F$ (1, 7) = 6.105, $p$ < .05), and a marginal effect of match-type ($F$ (1, 7) = 3.741, $p$ = .094) that appears to have been driven by the interaction. Participants in the no-search group made more errors in the category match block ($\underline{M}$ = 15%) than the exact match block ($\underline{M}$ = 6%; marginally significant, $F$ (1,3) = 7.442, $p$ = .072), whereas there was no significant difference between exact ($\underline{M}$ = 7%) and category ($\underline{M}$ = 6%) matches for yes-search participants, $F$ (1, 4) < 1. That accuracy seems to have been facilitated in general by the presence of an exact memory match for no-search participants is further evidence that attention was affected in no-search participants at a shorter time-course.

*Memory Errors*

There were no significant effects in terms of memory errors (see Table 7).

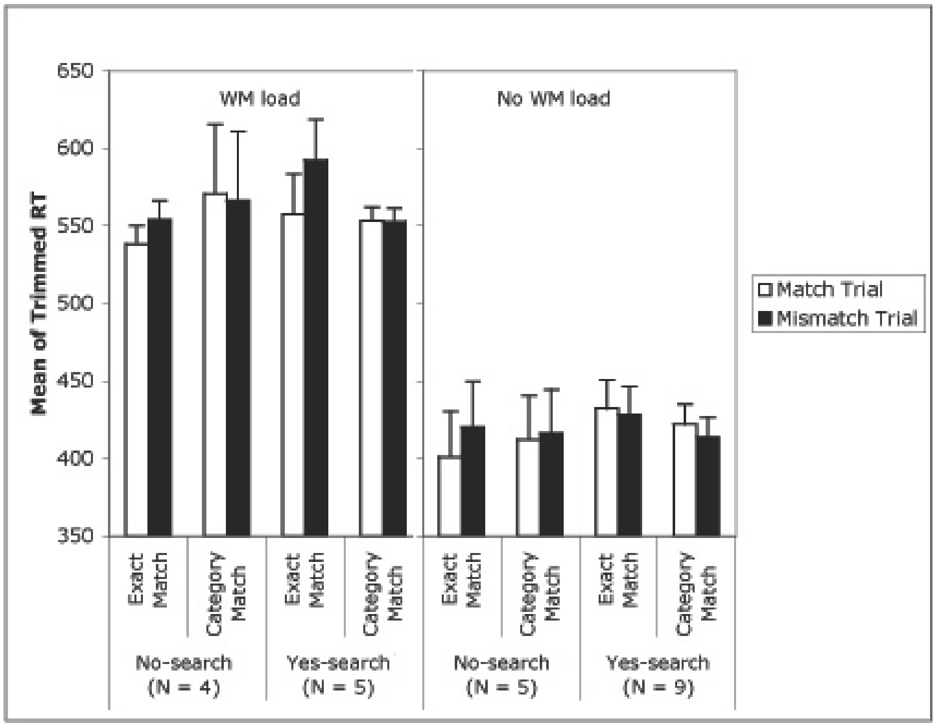


Figure 11. Average trimmed probe RTs from Experiments 8a (with WM load) and 8b (no WM load). Error bars represent 95% margins of error for the within-subjects difference between adjacent bars.

Table 7. Proportion of probe and memory errors for Experiments 8a and 8b.

| | | Probe Errors | | Memory Errors | |
|---|---|---|---|---|---|
| WM Load (Exp 8a) | Match Type | Match | Mismatch | Match | Mismatch |
| Yes-search (N = 5) | Same-item | .061 | .074 | .064 | .067 |
| | Different-item | .043 | .071 | .065 | .054 |
| No-search (N = 4) | Same-item | .055 | .069 | .095 | .111 |
| | Different-item | .150 | .154 | .136 | .092 |
| No WM Load (Exp 8b) | | | | | |
| Yes-search (N = 9) | Same-item | .066 | .076 | - | - |
| | Different-item | .060 | .030 | - | - |
| No-search (N = 5) | Same-item | .063 | .048 | - | - |
| | Different-item | .037 | .066 | - | - |

*Method*: *Experiment 8b*

Experiment 8a is consistent with the automatic guidance hypothesis; however, it is necessary to demonstrate that WM contents were responsible for the guidance of attention, and not mere exposure to two objects that were the same (i.e. priming). Thus, in Experiment 8b, the presentation parameters were identical to Experiment 8a, but there was no WM requirement. The WM item was simply presented, and at the end of a trial, participants simply pressed any button to clear the recognition test item (which served as a place holder only) off the screen in order to keep the presentation parameters exactly the same.

Also, the wording of the questionnaire had to be changed because there was no WM item. Thus, participants were asked if they attended to the flashed items when they matched the previously flashed item. Participants were also asked if they tried to remember the flashed items (all but one participant did not).

*Participants*. Fourteen Vanderbilt University undergraduates participated for course credit.

*Results: Experiment 8b*

There were 5 no-search participants and 9 yes-search participants.

*Probe RTs*

For probe RT's, 1.4% of trials were eliminated as outliers and 5.6% of trials were removed due to response errors. In the category match block, the no-search group's match ($\underline{M}$ = 412) and mismatch ($\underline{M}$ = 417) trial RTs were not different, ($\underline{SE}_{diff}$ = 10.15; $t(4) < 1$) and the yes-search group's match ($\underline{M}$ = 422) and mismatch ($\underline{M}$ = 414) trial RTs were not different, ($\underline{SE}_{diff}$ = 5.50; $t(8) = 1.524$, $p > .1$). In the exact match block, the no-search group's match ($\underline{M}$ = 401) and mismatch ($\underline{M}$ = 420) trials were not different, ($\underline{SE}_{diff}$ = 10.15; $t$ (4) < 1), and the yes-search group's match ($\underline{M}$ = 432) and mistmatch ($\underline{M}$ = 428) trials were not different ($\underline{SE}_{diff}$ = 7.92, $t$ (8) < 1).

Notice that the match-trial advantage in Experiment 8a's no-search group was about 16 ms and was significant, whereas in Experiment 8b's no-search group it was about 19 ms and was not significant. An explanation for this result lies in the difference between the variance of the difference scores ($\underline{s}^2_{diff}$) for each group. The variance in Experiment 8a ($\underline{s}^2_{diff}$ = 57.3) was significantly less that the variance in Experiment 8b ($\underline{s}^2_{diff}$ = 561.7), $F$ (4,3) = 9.802, $p < .05$. This difference between the variances implies that significance tests on differences among means across experiments should be interpreted cautiously.

An ANOVA with location, match-type, strategy and experiment as factors yielded a significant main effect of location ($F(1, 19) = 4.405$, $p = .049$) and experiment ($F$ (1,19) = 10.427, $p < .01$), a significant match-type by location interaction ($F(1,19) = 10.014$, $p < .01$) and a significant location by strategy by experiment interaction, $F$ (1,19) = 4.722, $p = .043$. For current intents and purposes, the most interesting effect is the 3-way interaction, and this was followed up match-type by location by experiment ANOVAs conducted separately on the yes- and no-search groups. As mentioned previously, the variances of the difference scores were different between the two experiment's no-search groups, thus, the fact there was no effect of location ($F$ (1,7) = 2.053, $p = .195$), only a marginal interaction between item and location ($F$ (1,7) = 4.131, $p = .082$), and no 3-way interaction ($F$ (1,7) < 1) should not be taken to imply that the WM had equivalent effects in both experiments' no search groups. For the yes-search group, there was a significant location by experiment interaction ($F$ (1, 12) = 9.294, $p$ = .01), a significant match-type by location interaction ($F$ (1,12) = 7.482, $p$ < .02) and a nearly significant 3-way interaction, $F$ (1,12) = 4.492, $p$ = .056. The match- vs. mismatch-trial pairwise analyses mentioned above are consistent with this pattern of effects. That is, there was an effect of location (i.e. a match-trial advantage), but only when participants were holding an item in WM, and the memory match was an exact match.

*Probe Errors*

In terms of probe errors for Experiment 8b, there were no significant differences between match- and mismatch trials in the yes- or no-search groups for either the exact or category match blocks, *t's* < 1.7. An ANOVA with match-type, location, strategy and experiment as factors yielded significant match-type by strategy ($F$(1,19) = 11.099, $p$ < .01), match-type by experiment ($F$(1,19) = 8.582, $p$ < .01), and match-type by strategy by experiment interactions ($F$(1,19) = 4.6, $p$ <.05). This pattern of interactions is consistent with the analyses already reported; no-search participants made more errors in the category match block, but only in Experiment 8a, and there were no effects in the yes-search group.

*Discussion: Experiments 8a and 8b*

The results of Experiments 8a and 8b are consistent with the automatic guidance hypothesis. In Experiments 8a, robust match trial advantages were found in both yes- and no-search participants, and in Experiment 8b, when there was no WM requirement, there was no difference between match- and mismatch trials for yes- or no-search participants.  Thus, without having encoded something into WM, there is no match-trial advantage even when participants report attending to memory matches.

# CHAPTER V

# SUMMARY AND IMPLICATIONS

The experiments described here provide evidence that WM's content's control over attention deployment is severely limited. Experiments 1 and 2 demonstrated that information retrieved from long-term memory into WM does not automatically guide attention to memory matches, and furthermore failed to replicate previous experiments in which WM's contents did appear to guide attention (i.e. Downing, 2000). Experiments 3 through 6 provided empirical evidence that WM's contents guide attention only when observers report that they intentionally attended to memory matches and that attention can deploy to novel items, even when WM's contents match another object in a display. Experiments 7 and 8 both demonstrated that WM's contents *can* affect attentional deployment. The results of Experiment 7 suggest that competition among items already in WM can influence an individual item's effectiveness as a guider of attention deployment, but only when participants report intentionally attending to memory matches. Finally, the results of 8a and 8b suggest that WM's contents might automatically guide attention at early stages of encoding and/or maintenance. Combined, these results support key aspects of both the automatic and optional guidance hypotheses and highlight the importance of considering the time course over which visual cognitive processes operate.

## Automatic Guidance

Although most of the experiments reported here are inconsistent with the automatic guidance hypothesis, there is at least one important way in which the current results are consistent. Experiments 8a and 8b suggest that guidance by WM's contents might be more or less automatic at early stages of encoding or maintenance. This idea is consistent with the claims of Soto et al. (2005). Based on an analysis of only the fastest RTs, these authors found that memory matches in a visual search array interfered with participants' ability to respond to the target. However, when all RTs were considered, the memory match's effect was attenuated, and in some cases eliminated. Soto et al. (2005) assumed that the fastest RTs reflected early, automatic processes, and slower RTs reflected voluntary processes. Thus, they concluded that guidance by WM was involuntary during early portions of the search processes and the current results largely support this claim. Additionally, the current results are consistent with the idea that WM's contents automatically guide attention to memory matches even when an observer is not intentionally engaged in visual search.

The current results also leave open the possibility that certain visual features, such as color, might guide attention more or less automatically. Evidence from previous research suggesting that color automatically guides attention is equivocal, for reasons similar to those outlined in Chapter III, but there are a few findings that suggest color and shape information in WM differ in terms of their efficacy at guiding attention. For example, Soto et al. (2005) found that color-only memory matches attracted attention more reliably than shape-only memory matches did. Recent single-unit recording studies in monkeys also suggest that color and shape information have different effects

(Bichot et al., 2005). In these studies, cells in area V4 were monitored while monkeys searched for objects defined by a conjunction of color and shape information. The relevant finding is that neural responses were stronger and more coherent when the stimulus in a cell's receptive field was the cell's preferred stimulus *and* happened to match the visual search target. Moreover, this effect was stronger for color-only matches than for shape-only matches. These results suggest that at the neural level, top-down biasing signals for color and shape are different. The current experiments did not use color stimuli, however, the results of previous research provides reason to suspect that color and shape information in WM might be utilized differently.

## Optional Guidance

Although WM's contents might guide attentional deployment during the early phase of maintenance (i.e. at least the first 300 ms), WM's contents do not seem to automatically guide attention for the entire maintenance period. This claim is supported by the observation that participants who reported not attending to memory matches were slower (or less accurate) at responding to probes presented at the location of a memory match (Experiment 3, 4, 5, and 6). The attempts to manipulate participants' strategies were unsuccessful, and based on this alone, it is possible that stable individual differences (rather than unstable strategic differences) differentiate yes- and no-search participants. Indeed, there are individual-differences accounts that an automatic guidance hypothesis could readily incorporate. However, the current experiments provide no strong evidence in favor of an individual differences account, and furthermore, post-hoc analyses of the order in which match and mismatch trials happened to appear for the yes- and no-search groups' lend support to a strategic differences account. Nonetheless, the current experiments cannot rule out an individual differences account in favor of purely strategic accounts, thus, each is reviewed in the next section.

*Individual Differences*

If the difference between the yes-search and no-search groups in these experiments is based upon individual differences, then it could be the case that WM capacity limits are one determining factor in whether WM's contents guide attention. For example, it could be the case that when an individual's WM capacity is exceeded, then WM's contents cannot effectively guide attention. This account assumes that 1) competition among items in WM can affect lower-level competitions among items in a scene and 2) that no-search participants' capacity is less than yes-search participants' capacity.

This hypothesis is appealing for several reasons. First, it would enable observers to avoid the attend-represent-attend cycle in a fairly straightforward way, that is, by holding as much information in WM as possible. For example, if a person was thinking about a Toyota Camry very deeply (i.e. using most of WM capacity to think about the car), then WM might not guide attention to a Camry that happens to appear. If however, the person is thinking about the Camry in a relatively superficial manner (i.e. not exhausting WM capacity), then attention could be deployed to any Camry that happened to appear. In the former case, WM capacity, including any putative "executive" mechanisms (c.f. Baddeley, 2003) would be tied up maintaining a representation of the car suitable for whichever cognitive task is being performed. In the later case, these mechanisms would be free, and by default, might bias attention towards matching objects in a scene. Another reason why this account is appealing is that it makes straightforward predictions that are easily testable, because it implies that the "decision" to search for memory matches is nothing more than having spare capacity in WM. Working memory capacity has been extensively studied over the past few decades and is easily manipulated (Baddeley, 2003; Cowan, 2000). Thus, placing the explanatory burden on the capacity of WM's peripheral storage systems (e.g. visual storage) creates an experimentally tractable solution to the problem of how WM's contents guide attention deployment.

The trouble is that the current experiments provide minimal support for this hypothesis and there are certain patterns in the data that are not readily explained by appealing to WM capacity. Minimal support for the idea that capacity limits underlie the "decision" to search or not to search for memory matches comes from Experiment 7, but this support is weak at best. From Experiment 7, recall that WM's contents did not guide attention to matches as WM load increased for the yes-search group, and also that capacity estimates for the no-search group were numerically lower than for the yes-search group. However, any difference in terms of WM capacity between yes- and no-search groups must remain speculative at this point because the sample size was too small to effectively test for differences between the groups. Furthermore, this account does not readily explain why there were no match trial disadvantages in Experiment 7's no-search group, but there was in the yes-search group at load-3. Presumably, the effect of exceeding WM capacity should have the same effect on both groups (although it is possible that WM capacity interacts with attentional guidance). Moreover, the idea that capacity limits underlie the "decision" to search for memory matches does not easily explain patterns that emerge when all of the experiments are considered. For example, it does easily explain the difference between the match-trial advantage found in Experiment 1 and the one found in Experiment 4. Recall that Experiment 1's STM/Different category condition was virtually identical to Experiment 4's exact match condition. However, a match-trial advantage was found in Experiment 1 (at least, with non-parametric tests), but in Experiment 4, a match-trial advantage was found only after participants were divided into yes- and no-search groups. Of course, it is possible that there were fewer low-capacity individuals in Experiment 1. If so, the capacity hypothesis would predict that WM memory performance should be related in someway to attentional deployment, but there was no evidence for this proposal anywhere in the current data. For example, individuals' match-trial advantages (i.e. mismatch RT minus match RT) were uncorrelated with the memory errors ($r$ = .12, $p$ = .305)[1], and strategy reports were also uncorrelated with memory errors ($r_{pb}$ = -.02, $p$ = .9)[2]. Notice that the correlation between match-trial advantages and memory errors, although not significant, is not even in the direction predicted by the capacity equals strategy hypothesis. Thus, these data suggest that memory performance (at least, as measured here) is unrelated to self-reported strategic differences and attentional deployment. Of course, the WM task used here is ill suited to uncover individual differences in WM capacity. After all, most participants only had to maintain a single object, thus individual differences in WM capacity might not be evident.

[1] Based on data from Experiments 1 and 2 (STM/Different category blocks), Experiment 4 (exact match block), Experiments 5 and 6 (critical trials only) and Experiment 7 (load-1 trials), total N = 76. These conditions were chosen because participants were retaining a single object at a basic level, and flashed distractors were from a different category than memory matches. The significance does not change with other conditions included.

[2] This correlation is based on data from Experiments 3 and 4 (exact match blocks), Experiments 5 and 6 (critical trials) and Experiment 7 (load-1 trials), N = 56. The significance does not change with other conditions included.

In summary, the idea that capacity limits play some role in the decision to search for memory matches is an attractive hypothesis that should be followed up with more direct tests of the idea. However, equating strategy with capacity cannot readily explain all of the findings in the current data. As such, it is possible (perhaps even likely) that other factors underlie the differences between yes- and no-search groups.

*Strategic Differences*

Another plausible hypothesis is that guidance by WM is related to participant's strategies during the experiment. The idea here is that yes-search participants decided to attend to memory matches, while no-search participants did not. A purely strategic account is appealing because it provides a natural explanation for the self-reported strategies. The question is: what could have caused some participants to decide to attend to memory matches and not others? Experiments 5 and 6 attempted to manipulate strategies by reducing the base rate of memory matches and through subtle instruction, but these manipulations did not work. This suggests that the base rate of match and mismatch trials (within limits) is not a critical factor in determining whether a participant attends to memory matches. However, post hoc analyses of the order in which match and mismatch trials happened to appear revealed that trial-to-trial contingencies are related to whether a participant reports attending to memory matches.

Recall, that in all experiments the order in which trials were presented was randomized. Thus, the percent of trials that followed a trial of the same type (e.g. a match trial following a match trial) varied somewhat from participant to participant. Although this ultimately resulted in about 50% of the trials following a trial of the same type, it is well known that people are poor at making judgments about random sequences. For example, people often falsely attribute a short run in which the probability of a repetition exceeds chance to non-randomness (e.g. “hot hands”; Ayton & Fischer, 2004; Gilovich, Vallone, & Tversky, 1985) and even in cases where people know that events are random, they sometimes believe they can predict what will happen next (e.g. the gambler’s fallacy; Ayton & Fischer, 2004). Thus, it seems plausible that the way in which trials happened to cluster during early portions of the experiment led participants to search, or not search, for memory matches.

Indeed, in Experiments 3, 4, and 7, the percentage of practice trials that followed a trial of the same type (e.g. a match trial followed by a match trial) differentiated the yes- and no-search groups. In Experiment 4, trial types were more likely to follow a trial of the same type in the practice period for the yes-search group (Mismatch = 56%, Match = 53%) than the no-search group (Mismatch = 45%, Match = 45%), $F$ (1, 11) = 5.686, $p$ = .036. However, there was no difference in the percent of repetition in the first 16 non-practice trials, $F$ (1, 11) = 2.403, $p$ = .149. This suggests that the number of repetitions, regardless of whether the repetition is match or mismatch, might lead participants to intentionally search for memory matches. This general pattern of repetition in the practice trials held in Experiment 3 (yes-search: Match = 45%, Mismatch = 43%; no-search: Match = 35%, Mismatch = 40%), but the difference was not significant. In Experiment 7, there was a different pattern of trial repetitions during the practice trials that differentiated the yes- and no-search groups. In Experiment 7, there was no overall difference between the percent of repetition between yes- and no-search groups in the practice trials ($F$ (1, 9) < 1), however, there was a significant interaction between probe location (i.e. match or mismatch) and strategy report, $F$ (1, 9) = 7.765, $p$ = .021. In the yes-search group, match and mismatch trials followed a trial of the same type on 45% and 25% of trials respectively, while in the no search group it was 44% and 35%. Thus, match trials repeated more often than mismatch trials ($F$ (1, 9) = 53, $p$ < .001), but the difference between match and mismatch repetitions was smaller in the no-search group. It should be noted that the overall difference between match and mismatch trial repetitions in the practice period is itself not interesting, because there were more match trials (10) than mismatch trials (8) during the practice trials (and only in the practice trials) in Experiment 7 due to experimenter error. Nonetheless, it seems plausible that a relatively greater percentage of match trial repetitions than mismatch trial repetitions could cue a person to search for memory matches.

In Experiments 5 and 6, the practice trials did not contain any match or mismatch trials, and only one-sixth of the non-practice trials contained memory matches. Thus, the percent of critical trials that followed a trial of the same type was extremely small for both groups. However, the yes- and no-search groups differed in terms of how many total trials they performed before encountering a critical trial that had two or fewer baseline trials before it and the previous critical trial of the same type (e.g. t - 3 = match; t-2 = baseline; t-1 = baseline; t = match). In the yes-search group, the first such clustering of match trials occurred after M = 35 trials, and for mismatch trials after M = 50 trials. In the no-search group, the first such match cluster occurred after M = 72 trials, and for mismatch trials after M = 29 trials, (interaction between trial type and strategy, $F$ (1, 18) = 6.682, $p$ = .019). Again, it seems plausible that because the yes-search group experienced a cluster of match trials significantly sooner than the no-search group ($t$ (18)

= 2.387, $p < .05$) that they would be more inclined than the no-search to attend to memory matches. Alternatively, no-search participants could have been more inclined to search for novel objects because they did not experience a cluster of match trials.

It should be noted that over the course an entire experiment, the clustering of trials was statistically equivalent for yes- and no-search groups. That is, in Experiments 3, 4, and 7, the percentage of trials that followed a trial of the same type was the same for yes- and no search groups, and in Experiments 5 and 6 the number of trials following a trials of the same type by fewer than three trials was the same. This is to be expected, because the trial orders were determined randomly.

These different patterns of trial repetitions during the practice period and early portions of the experiment proper lend support to a strategic account. However, because of the analyses' post-hoc nature there are important caveats that should be mentioned. For example, it is unknown whether the participants actually formed explicit beliefs about the probability of repetition. Thus, it could be the case that the strategy reports were based upon participants' experience of attention being guided to certain objects, and not a deliberate decision to search for certain objects. The results of Experiment 8a argue against this report-from-experience account, because no-search participants had a match trial advantage. However, in Experiment 8a, stimuli were presented very rapidly, which plausibly affected participants' subjective impressions.

Nonetheless, if the decision to search for memory matches is purely strategic and based upon the probability of repetition, then an explicit (albeit not necessarily correct) belief about the probability of repetition should have been formed. It seems likely that participants would form such a belief, but there is reason to suspect they might not have.

For example, certain conditioned eye blink responses depend on participants' conscious expectations about when a conditioned stimulus (CS) will be paired with an unconditioned stimulus (US; e.g. trace conditioning; Clark, Manns, & Squire, 2001; Manns, Clark, & Squire, 2000). However, very similar forms of eye blink conditioning depend upon the number of trials since the CS-US was last presented, but not participants' expectations (e.g. delay conditioning; Clark et al., 2001). Obviously, the mechanisms controlling such learned eye blink responses are vastly different from the mechanisms under investigation here. Nevertheless, the point is that subtle variations in participants' conscious expectations about what will happen next can sometimes affect very low-level processes, such as eye blinks and perhaps attention deployment, but in other cases, the clustering of events seems to be more important than participants' expectations (or lack thereof). Without knowing participants' conscious expectations during the experiment, it is impossible to determine whether a deliberate choice to attend to memory matches mediates attention deployment. However, given that participants could report upon their experience, and that trial clusters in later portions of the experiments did not differ between yes- and no-search groups, it seems likely that conscious strategies are an important factor in determining whether memory matches are attended.

## Time Scales in Visual Cognition

Although most of the experiments reported here were not initially concerned with questions about the time course over which cognitive processes operate, the results are consistent with the idea that visual representations function differently at different time courses, specifically, that processes at brief time scales are more automatic and than processes at longer time scales. Indeed, a temporal hierarchy of cognition of the sort discussed by Hayhoe (2000) fits well with the pattern of automatic and strategic effects found in these experiments.

According to Hayhoe's scheme, the relevant time scale for cognitive operations such as reasoned decision-making is approximately tens of seconds. In the current experiments, examples of such processes might include forming a general strategy for completing the WM and the probe discrimination tasks, and it is at this time scale where the yes- and no-search participants plausibly differ. Yes-search participants may have decided (or were otherwise predisposed) to attend to memory matches on each trial, whereas no-search participants may have decided not to attend to the distractors (or perhaps, made no decision at all). If the yes- and no-search groups differed at this gross time scale, it would have affected the operation of processes that occur at shorter time scales, including: maintaining priorities in WM (~1+ sec); the kinds of visual routines (i.e. processes that extract relevant information; Ullman, 1984) that are executed (~100+ ms); and basic/neural operations (~10+ ms). These considerations highlight a crucial difference between attention deployment and WM: these processes operate on different time scales.

Figure 12 is an illustration of how the inclination to search for memory matches could have affected the processes occurring at shorter time scales in Experiment 4. Of principle importance are the visual routines. Visual routines are specialized sets of basic operations (e.g. index location, shift processing focus etc.; see Hayhoe, 2000) that extract specific task-relevant information, and are executed on an as-needed basis. In the current task, the first visual routine would have been attending to the initial presentation of the WM item ("get WM item"). As can be seen in the figure, the yes- and no-search groups both must initiate this routine at the beginning of a trial, and because it is a visual routine, the participants have little conscious control over how long it takes to fully execute once it is initiated (Hayhoe, 2000). In Experiment 4 the memory match was presented 2.5 seconds after the onset of the WM item, so the first routine had plenty of time to run its course (estimated at ~500ms, based on Vickery et al., 2005) before the time the memory match was presented. Thus, a second visual routine had to be initiated if the memory match or distractor was to receive attention. The nature of this second visual routine presumably depends on the priorities that an observer is maintaining in WM, which in turn depends on their general strategy (or individual differences). Hence, yes-search participants' second routine was something such as "get memory match", whereas the no-search participants might have executed a routine such as "get novel item". Also notice that the second routine (for both groups) overlaps in time with the third routine, "get probe". This temporal overlap could account for the match-trial advantages and disadvantages, because different visual routines are thought to share mechanisms (e.g. index location) if they both need to use it (Ullman, 1984).

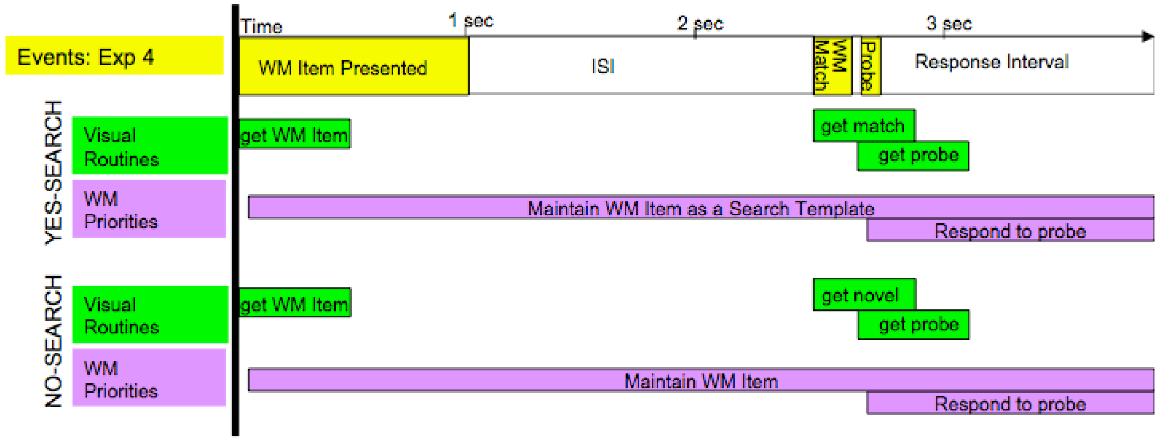


Figure 12. Diagram of the visual routines and WM priorities of the yes- and no-search groups in Experiment 4.

Figure 13 illustrates an analogous situation for Experiment 8a, which was identical to Experiment 4 in all respects except for stimulus presentation times. In this experiment, the match-trial advantage did not depend on whether participants reported attending to memory matches, which could be because the first visual routine ("get WM item") did not have time to run its course by the time the memory match was presented. However, the routine had begun, thus the WM was already encoded to some degree (Woodman & Vogel, 2005) which could have caused an obligatory shift of attention to any input that matched the WM item (Soto et al., 2005). This obligatory attention shift to matching inputs during the course of a disrupted encoding routine might be useful as a way to further refine WM representations in cases where the encoding routine is somehow disrupted.

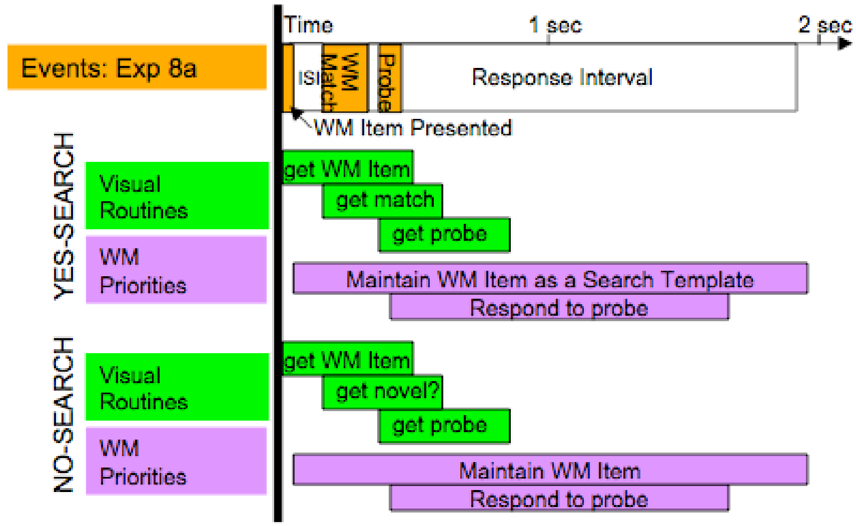


Figure 13. Diagram of the visual routines and task relevant thoughts of the yes- and no-search groups in Experiment 8a.

Of course, as discussed earlier, it is possible that individual differences, or differences in clustering of match and mismatch trials affect whether an observer attends to memory matches. However, these considerations do not take away from the importance of recognizing the importance of time scales in visual cognition. Visual cognition consists of processes that extend over both space and time, therefore, any complete account of these phenomena must account for the different time scales over which processes such as attention, working memory and decision making operate.

## Conclusion

The experiments reported here have repeatedly demonstrated that the contents of WM do not always guide attention. Rather, people can at times avoid attending to memory matches, and instead attend to information that is not already represented in WM. This suggests that attentional priorities and WM's contents are not one in the same (c.f. Baddeley, 2003). At short time scales however, attention might deploy to memory matches, not necessarily because priorities are to attend to memory matches, but because of the way in which encoding routines adapt to disruption.

Temporal limitations and strategic flexibility in terms of how WM's contents affect attention deployment can be advantageous. If WM's contents always guided attention, then observers would have difficulty attending to objects that had never been experienced, and had therefore never been held in WM. Conversely, if WM's contents never influenced attention deployment, then observers would have difficulty finding and maintaining focus on task relevant objects.

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